Epsilon-Ergodicity and the Success of Equilibrium Statistical Mechanics*

Peter B. M. Vranas†‡
Department of Philosophy, University of Michigan

Why does classical equilibrium statistical mechanics work? Malament and Zabell (1980) noticed that, for ergodic dynamical systems, the unique absolutely continuous invariant probability measure is the microcanonical. Earman and Rédei (1996) replied that systems of interest are very probably not ergodic, so that absolutely continuous invariant probability measures very distant from the microcanonical exist. In response I define the generalized properties of epsilon-ergodicity and epsilon-continuity, I review computational evidence indicating that systems of interest are epsilon-ergodic, I adapt Malament and Zabell's defense of absolute continuity to support epsilon-continuity, and I prove that, for epsilon-ergodic systems, every epsilon-continuous invariant probability measure is very close to the microcanonical.

1. Introduction.[1] The fact that classical equilibrium statistical mechanics (SM) works is deeply puzzling. Take an isolated system in equilibrium; e.g., a gas in an insulated box. Define the *phase space* of this system as the set of all possible microstates of the gas. Choose a macroscopic observable, like pressure, and define the corresponding *phase function* as the function that gives the value of the observable for each microstate in the phase space. Compute the *phase average* of this function;

*Received September 1997.

†Send requests for reprints to the author, Department of Philosophy, The University of Michigan, 2215 Angell Hall, Ann Arbor MI 48109, USA; e-mail: vranas@umich.edu.

‡I am very grateful to Robert Batterman, James Joyce, and Lawrence Sklar for extensive discussion. Thanks also to Stephen Leeds, David Malament, and Peter Railton for help. Versions of this paper were presented at the seventy-second annual meeting of the Pacific Division of the American Philosophical Association (Los Angeles, March 1998) and at the Twentieth World Congress of Philosophy (Boston, August 1998).

1. This section presents the issues in a simplified and slightly imprecise way, eschewing formalism and omitting details. Rigor is introduced in later sections.

Philosophy of Science, 65 (December 1998) pp. 688–708. 0031-8248/98/6504-0008$2.00

i.e., its average over the phase space with respect to a specific probability measure, the so-called *microcanonical* measure. Lo and behold, the result of the computation reliably predicts the measured value of the observable. What is amazing about this method is that it seems to proceed in blithe ignorance of the equations of motion that govern the dynamical evolution of the microstate of the gas. Why does the method work for gases composed of various kinds of molecules interacting with various potentials? What justifies the use of the microcanonical measure? What is so special about taking averages? The recipe works, but its success cries out for explanation.

A traditional attempt to supply an explanation relied on the assumption that the systems for which SM works are *ergodic*. A system is ergodic if, given almost any point of the phase space, the trajectory that goes through that point sooner or later crosses every region of the phase space that has positive measure. The crucial feature of ergodic systems is given by *Birkhoff's theorem*: given almost any trajectory, the infinite time average of any phase function along that trajectory exists and is equal to the phase average (and is thus independent of the trajectory). But how does the equality of the phase average with the infinite time average explain the equality of the phase average with the measured value? Here is an attempt to bridge the gap between measured values and infinite time averages: given that every measurement extends over a time interval that is very long with respect to microscopic time scales, a measured value is equal to a finite time average that closely approximates the infinite time average. Critics were quick to react to this attempt: if measurements always lasted long enough for finite time averages to approximate infinite time averages, then how could measurements ever reveal that a system is not in equilibrium? This explanation has thus fallen into disrepute.

Khinchin (1949) proposed an alternative explanation that was intended to dispense with ergodicity. Khinchin relied on two fundamental observations. First, the systems for which SM works are composed of *large* numbers of particles. Second, the phase functions that correspond to macroscopic observables have a special form: they are sums of further functions, each function in the sum depending on the state of a single particle. Khinchin then proved that, as the number of particles goes to infinity, the microcanonical measure of the set of points in the phase space for which the value of the phase function is very close to the phase average goes to one. The appeal to infinite time averages (and to ergodicity) became thus redundant: the measured value is equal to *some* value of the phase function (or to some finite time average) and is thus with high microcanonical probability approximately equal to the phase average. But why should high microcanonical probability correspond to high "physical" probability? Khinchin's account is silent on this point, and this is its fundamental weakness.

Enter Malament and Zabell (1980). They attempt to clinch Khinchin's argument by providing a justification for the microcanonical measure. Their justification reintroduces ergodicity. Here are some details. Suppose that the probabilities of finding an equilibrium system in various regions of the phase space are given by some *invariant measure* (i.e., a measure that assigns the same value to a region and to its images under the dynamical flow). Malament and Zabell argue that, because we can prepare systems with only limited precision, this *equilibrium measure* must be "translation-continuous"; i.e., roughly, it must assign approximately equal values to any two regions one of which is a small displacement of the other. It follows that the equilibrium measure must assign value zero to every region of Lebesgue or, equivalently, microcanonical measure zero; i.e., it must be *absolutely continuous* (with respect to Lebesgue or to the microcanonical measure). But a corollary of Birkhoff's theorem says that, if a system is ergodic, then the *unique* absolutely continuous invariant probability measure is the microcanonical! Is this at last the sought-for justification?

Earman and Rédei are not convinced. They raise two main worries. First, they claim that most systems for which SM works are very probably not ergodic. Second, they point out that, "even if the system is just a 'little bit' non-ergodic, the uniqueness result fails—and it fails entirely, not just a little bit" (1996, 71). To appreciate the second point, notice that the phase space of a non-ergodic system contains an *invariant set* (i.e., a union of trajectories) of microcanonical measure greater than zero (and less than one). If the system is just a little bit non-ergodic, then this invariant set has tiny measure. But absolute continuity provides constraints only for sets of measure exactly zero, not for sets of positive but tiny measure. Intuitively, then, one expects that absolutely continuous invariant probability measures very distant from the microcanonical will exist. Disaster?

Something strange is going on. Mathematically, the reasoning of Earman and Rédei is impeccable; intuitively, however, why should it matter if there is an invariant set of such tiny measure that it cannot be detected by measurements? This is not an argument, but it is the motivation behind the present paper.

In this paper I carry the debate one step further by proposing a modification of the Malament-Zabell explanation that avoids the Earman-Rédei objections. My strategy has five ingredients. (1) I make rigorous the notion of a system's being just a little bit non-ergodic. Here is a rough definition: a system is ε-ergodic if its phase space has an invariant subset of measure $1 - \varepsilon$ such that, given almost any point in that subset, the trajectory that goes through that point sooner or later crosses every region of the subset that has positive measure. (2) I review computational evidence indicating that the systems for which SM works are ε-ergodic with ε tiny or zero. (3) I generalize the notion of absolute continuity so that I can get constraints even from sets of positive measure: a measure is $\varepsilon_1/\varepsilon_2$-continuous (with respect to the microcanonical measure) if it assigns value *at most* ε_2 to every region of microcanonical measure *at most* ε_1. (4) I prove that a condition of "translation-closeness" entails that the equilibrium measure is $\varepsilon/\varepsilon_2$-continuous with ε_2 small. The translation-closeness condition is similar to (but different from) Malament and Zabell's translation-continuity condition and says roughly that, if a region of the phase space is a small displacement of another region, then the probability of finding the system in the first region is close to the probability of finding the system in the second region. I justify this condition by appealing to the "coarse-graining" (discretization) of the phase space that results from the limited precision of measurements. (5) I prove that, if a system is ε-ergodic with ε small or zero, then every $\varepsilon/\varepsilon_2$-continuous invariant probability measure with ε_2 small is very close to the microcanonical.

Besides escaping the Earman-Rédei objections, my amendment of the Malament-Zabell explanation has the virtue of avoiding the implausibly strong conclusion that *only* the microcanonical measure works. Gibbs (1902) points out that the so-called *canonical* measure works just as well as the microcanonical. Leeds (1989) argues that the equilibrium measure is not *exactly* the microcanonical. Batterman (1998) adduces theoretical reasons for expecting that many measures besides the microcanonical will work. My approach is consonant with these insights, since I only conclude that the equilibrium measure is *very close* to the microcanonical.

In Section 2 I examine what exactly it is that we are trying to explain. In Section 3 I present Malament and Zabell's explanation, Earman and Rédei's objections, and an outline of my response. In Section 4 I defend epsilon-ergodicity. In Section 5 I defend epsilon-continuity and translation-closeness. In Section 6 I provide some concluding remarks.

2. The Explanandum. Several issues in the foundations of statistical mechanics are of philosophical interest (Sklar 1993), but the program of which this paper is a continuation focuses on a severely circumscribed explanandum (Malament and Zabell 1980, 339). The aim is to explain why, *for certain physical systems and for certain macroscopic observables, whenever a system is in equilibrium and we measure the value of an observable, we reliably get a number approximately equal to the* *microcanonical phase average, with respect to a mathematical model of the system, of a phase function that corresponds to the observable.* I will comment briefly on five aspects of this explanandum: (1) the physical systems, (2) the mathematical models, (3) the macroscopic observables, (4) the microcanonical phase average, and (5) the notion of equilibrium.

(1) The physical systems of interest are those for which SM (*classical equilibrium* statistical mechanics) works; i.e., those for which the approximate equality of phase averages with measured values mentioned in the explanandum holds. Standard examples include classical fluids and high-temperature solids (Reichl 1998; cf. Quay 1978, 53–54).[2]

(2) The mathematical models of interest are known as autonomous Hamiltonian dynamical systems. I define a *dynamical system* as a pair $\langle X, T_t \rangle$.[3] The *phase space* X is a set whose members (the *phases*) correspond to the possible microstates of the physical system. The *flow* $T_t (t \in \mathbf{R})$ is a one-parameter group of one-to-one maps of X onto itself that describes the dynamics: if the phase at time t_0 is $x \in X$, then the phase at time $t_0 + t$ is $T_t(x) \in X$. A dynamical system is *Hamiltonian* if $X \subseteq \mathbf{R}^{2n}$ and the flow is generated by Hamilton's equations: $dq_i/dt = \partial H/\partial p_i$, $dp_i/dt = -\partial H/\partial q_i$ $(i = 1, \ldots, n)$. n is the (enormous) number of degrees of freedom (e.g., the number of particles), q_i are generalized position coordinates, p_i are generalized momenta, and the Hamiltonian H is a function of q_i, p_i, and t whose value (under certain conditions) is equal to the energy of the physical system. A Hamiltonian system is *autonomous* if $\partial H/\partial t = 0$; then it can be shown that $dH/dt = 0$, so that the energy is a constant E and motion in X is confined to the *energy surface* S_E (i.e., the subset of X defined by $H = E$).

(3) The macroscopic observables of interest are those that can be represented by *phase functions* (functions of X into $\mathbf{R}$); i.e., those that can be defined with respect to a single microstate (e.g., pressure; cf. Farquhar 1964, 22).

(4) Given a probability measure μ on (a σ-algebra of *measurable* subsets of) X, the *phase average* $\langle f \rangle_\mu$ of a phase function f is defined as $\int_X f d\mu$; i.e., as the expectation (first moment) of f when f is considered as a random variable. The *microcanonical phase average* $\langle f \rangle_m$ of f is the phase average of f with respect to a particular probability measure, the *microcanonical measure* μ_m, which is confined to an energy surface S_E

2. My characterization of the physical systems of interest does not make the explanandum tautological: asking why SM works for the systems for which it works is like asking, e.g., why people who have blue eyes have blue eyes.

3. I depart from the usual definitions of a dynamical system (Cornfeld et al. 1982, 6), which mention explicitly a measure on X. My reason is that I want to call *measures* rather than (except derivatively) *systems* "ergodic," in order to emphasize that (infinitely) many ergodic measures can correspond to the same physical system.

(i.e., $\mu_m(S_E) = 1$), and whose formula is given in the Appendix (Theorem 3).

(5) The physical systems of interest exhibit thermodynamic behavior (i.e., essentially, approach to equilibrium), but the fact that they do is not part of the explanandum. From the point of view of the explanandum, "equilibrium" must be defined operationally, without reference to a probability measure (Leeds 1989, 328): a physical system is in equilibrium at time t if, e.g., measurements reveal that the values of macroscopic observables remain approximately constant during a sufficiently long time interval that contains t.

3. The Malament-Zabell Explanation and the Earman-Rédei Objections. Consider any physical system of interest and let $\langle X, T_t\rangle$ be a corresponding (autonomous Hamiltonian) dynamical system. Consider any macroscopic observable of interest and let f be a corresponding phase function. Let t be any time at which the physical system is in equilibrium. Let E be the energy of the physical system and let S_E be the corresponding energy surface. Let A be any measurable subset of S_E. Let $P_t(A)$ be the probability that the microstate of the physical system at t corresponds to a phase in A. Then, according to Malament and Zabell (1980):

(P1) There is a small $\varepsilon > 0$ such that $\mu_m(\{x \in S_E\colon |f(x) - \langle f\rangle_m| \leq \varepsilon\}) \cong 1$;
(P2) $P_t(A) = \mu_m(A)$.

Loosely speaking, P1 says that each phase function of interest is approximately constant over microcanonically-almost-all the energy surface and approximately equal to its microcanonical phase average, and P2 says that the microcanonical measure gives the "physical" probabilities of finding an equilibrium system at given microstates. P1 and P2 together entail that $P_t(\{x \in S_E\colon |f(x) - \langle f\rangle_m| \leq \varepsilon\}) \cong 1$, from which the explanandum of Section 2 follows (assuming that measurements are sufficiently accurate; I return to this point in Section 5).

P1 was proven by Khinchin (1949) for systems of noninteracting particles and by Mazur and van der Linden (1963) and Lanford (1973) for some systems of interacting particles. Thus Malament and Zabell focus on P2. Before I give their argument for P2, I need to introduce four definitions. (1) A measure μ is *invariant* exactly if, for every (measurable) A and for every t, $\mu(A) = \mu(T_t(A))$; i.e., μ assigns the same value to A and to the images of A under the flow. (2) A set A is *invariant* exactly if, for every t, $T_t(A) = A$; equivalently, the trajectory $\{T_t(x)\colon t \in \mathbf{R}\}$ through any member x of A is a subset of A (i.e., A is a union of trajectories). (3) An invariant probability measure μ is *ergodic* exactly if, for any invariant set A, $\mu(A)$ is 0 or 1. Equivalently, X is *μ-indecomposable*: X cannot be partitioned into invariant sets more than one of which have positive μ-measure. Still equivalently, the trajectory through μ-almost every member of X intersects every subset of X that has positive μ-measure; formally, $\mu(\{x \in X\colon (\forall A \subseteq X)\ (\mu(A) > 0) \rightarrow (\{T_t(x)\colon t \in \mathbf{R}\} \cap A \neq \varnothing)\}) = 1$. (4) A measure μ_2 is *absolutely continuous* with respect to (henceforth abbreviated as "a.c. wrt") a measure μ_1 exactly if, for every A, $\mu_2(A) = 0$ if $\mu_1(A) = 0$. Now Malament and Zabell's argument for P2 can be formulated as follows. (Some of the universal quantifications that precede P1 above are also implicit here. μ_1 and μ_2 are any probability measures.)

(P3) If μ_1 is ergodic and μ_2 is a.c. wrt μ_1 and invariant, then $\mu_2 = \mu_1$;
(P4) μ_m is ergodic;
(P5) P_t is a.c. wrt μ_m;
(P6) P_t is invariant.
Therefore: (P2) $P_t = \mu_m$.

Clearly, the argument is valid. P3 is uncontroversial: it is a theorem of ergodic theory (Cornfeld et al. 1982, 15). I will examine P5 in Section 5; now I turn to P4.

Earman and Rédei (1996) argue that, for typical systems of interest, μ_m is very probably not ergodic. I am not convinced, but in this paper let me grant for the sake of argument that P4 is false. Then (see definition 3 above) there is some invariant set B with $\mu_m(B) \notin \{0, 1\}$. But suppose that μ_m is just a little bit non-ergodic, in the sense (roughly) that $\mu_m(B)$ is tiny. Then one might salvage an explanation of the Malament-Zabell type by establishing, not that (P2) P_t and μ_m are *equal*, but rather that (Q2) P_t and μ_m are *ε-close* for some small $\varepsilon > 0$ (in other words: P_t and μ_m are *epsilon-close*), where two measures μ_1 and μ_2 are said to be ε-close exactly if, for every A, $|\mu_2(A) - \mu_1(A)| \leq \varepsilon$. P1 and Q2 together would entail the explanandum (assuming, again, that measurements are sufficiently accurate).

How would one establish Q2? Maybe by replacing P3 with (P3*): If μ_1 is just a little bit non-ergodic and μ_2 is a.c. wrt μ_1 and invariant, then μ_1 and μ_2 are ε-close for some small $\varepsilon > 0$. Earman and Rédei vaguely anticipate such a move, and one of their arguments (1996, 72) can provide a reply. Let μ' be any invariant probability measure confined to B (i.e., $\mu'(B) = 1$) that is a.c. wrt μ_m. Let μ'_m be the normalized restriction of μ_m to $X \backslash B$ (i.e., $\mu'_m(\bullet) = \mu_m(\bullet \cap B^c)/\mu_m(B^c)$). Then $\mu_\delta = \delta\mu'_m + (1 - \delta)\mu'$ $(0 \leq \delta \leq 1)$ is an invariant probability measure that is a.c. wrt μ_m. But $|\mu_\delta(B) - \mu_m(B)|$ is equal to $|1 - \delta - \mu_m(B)|$ and is thus close to 1 for small δ (recall that $\mu_m(B)$ is tiny), so that μ_δ and μ_m are not ε-close for small ε, and P3* is false.

Absolute continuity is the culprit. Because $\mu_m(B)$ is *positive* (though tiny), $\mu_\delta(B)$ can be close to 1 even though μ_δ is a.c. wrt μ_m: absolute continuity would restrict $\mu_\delta(B)$ only if $\mu_m(B)$ were *exactly* 0. So I propose to replace absolute continuity with $\varepsilon_1/\varepsilon_2$-*continuity*: given numbers ε_1 and ε_2 in [0, 1), I will say that μ_2 is $\varepsilon_1/\varepsilon_2$-continuous with respect to μ_1 exactly if, for every A, $\mu_2(A) \leq \varepsilon_2$ if $\mu_1(A) \leq \varepsilon_1$. Now my amendment of the Malament-Zabell explanation replaces P2 through P6 with:

(Q3) If μ_1 is ε-ergodic and μ_2 is $\varepsilon/\varepsilon_2$-continuous wrt μ_1 and invariant, then μ_1 and μ_2 are ε_3-close with $\varepsilon_3 = 2\varepsilon_2 + \varepsilon(1 - \varepsilon)^{-1}$;
(Q4) μ_m is ε-ergodic for some tiny or zero ε;
(Q5) P_t is $\varepsilon/\varepsilon_2$-continuous wrt μ_m for some small $\varepsilon_2 > 0$ (ε is the same as in Q4);
(Q6) P_t is invariant.
Therefore: (Q2) P_t and μ_m are ε_3-close for some small $\varepsilon_3 > 0$.

Although I have not yet defined ε-ergodicity (Section 4), it should be clear that the above argument is valid. Thus my amendment of the Malament-Zabell explanation avoids the Earman-Rédei objections. Q3 follows from a theorem which I prove in the Appendix (Theorem 1). In the next two sections I defend Q4 and Q5 respectively. (See fn. 12 for some remarks on Q6.)

4. Epsilon-Ergodicity and its Ubiquity. We saw that an invariant probability measure μ is ergodic on a phase space X exactly if X is μ-indecomposable. Similarly, I will say that μ is *ergodic on an invariant subset* Y of X that has positive μ-measure exactly if Y is μ-indecomposable; i.e., Y cannot be partitioned into invariant sets more than one of which has positive μ-measure. (Equivalently, the trajectory through μ-almost every member of Y intersects every subset of Y that has positive μ-measure. Still equivalently, every invariant subset of Y has μ-measure 0 or $\mu(Y)$.) Now given a number ε in [0, 1), I will say that μ is ε-*ergodic* on X exactly if μ is ergodic on an invariant subset Y of X with $\mu(Y) = 1 - \varepsilon$. I will say that μ is *epsilon-ergodic* if μ is ε-ergodic with ε tiny or zero. (Note that I distinguish between ε-ergodicity and epsilon-ergodicity. Strict ergodicity, corresponding to $\varepsilon = 0$, is a special case of ε-ergodicity and of epsilon-ergodicity.) Thus Q4 says that the phase space (equivalently—because $\mu_m(S_E) = 1$—, the energy surface) of every dynamical system that corresponds to a physical system of interest has a microcanonically indecomposable invariant subset of microcanonical measure almost or exactly one. Why should we accept Q4?

In contrast to systems with two degrees of freedom, Hamiltonian systems with many degrees of freedom are still poorly understood (Lichtenberg and Lieberman 1992, 439). Nevertheless, some computational evidence indicates that nonintegrable[4] Hamiltonian systems approach ergodicity (with respect to the microcanonical measure) when the number of degrees of freedom increases. For example, a one-dimensional system of N self-gravitating plane parallel sheets of uniform density has been studied computationally by several investigators (Froeschlé and Scheidecker 1975; Froeschlé 1978; Benettin, Froeschlé, and Scheidecker 1979; Wright and Miller 1984; Reidl and Miller 1992, 1993). It seems that, as N increases, the system approaches very quickly strict ergodicity and reaches it for $N = 11$. As a second example, a system of N coupled "symplectic maps" (which is much easier than Hamiltonian systems to study computationally but is expected to share many features with Hamiltonian systems) was studied by Falcioni et al. (1991). It seems that the volume of the non-ergodic portion of the phase space is quite small and decreases *exponentially* with N (for N ranging from 10 to 200). A similar conclusion was reached by Hurd et al.[5] (1994; cf. Györgyi et al. 1989).

It might be objected that other nonintegrable Hamiltonian systems fail to approach ergodicity when the number of degrees of freedom increases. Take the Fermi-Pasta-Ulam (FPU) system, a one-dimensional chain of N particles with weakly nonlinear nearest-neighbor interaction (Fermi et al. 1965; Ford 1992). Computational studies for N up to 1024 suggest that every FPU-like system has an energy threshold (the so-called *equipartition threshold*) such that equipartition of energy among the degrees of freedom (a necessary condition for ergodicity) is or is not approached according to whether the energy of the system falls above or below the threshold respectively (Livi et al. 1985a, 1985b; Kantz 1989; Galgani et al. 1992; Kantz et al. 1994; Benettin 1994, 219–220). It can be shown that at least *approximate* equipartition is necessary for *epsilon*-ergodicity; thus it seems that an FPU-like system with large N but with energy below the threshold is not epsilon-ergodic. It turns out, however, that the equipartition threshold is in a sense fictitious. Recent computational studies suggest

4. Roughly, an *integrable* Hamiltonian system displays quasi-periodic motion (Ford 1992, 276–279; Hénon 1983, 74–82). Generic Hamiltonian systems are neither integrable nor ergodic (Markus and Meyer 1974).

5. Earman and Rédei (1996, 70) raise an objection along the following lines. The fact that the non-chaotic portion of the phase space has small measure does not suffice for epsilon-ergodicity; the indecomposability of the chaotic portion is also needed but was not confirmed by Hurd et al. (1994). But Hurd et al. examined cases with N only up to 7; their results are consistent with the possibility (confirmed by Falcioni et al. 1991, 2266) that epsilon-ergodicity is reached for much larger N.

that, at least for large *N*, at least approximate equipartition is always achieved; only the *time* to equipartition depends on the energy, and is short or very long according to whether the energy falls above or below the threshold respectively (Pettini and Landolfi 1990; Pettini and Cerruti-Sola 1991; De Luca, Lichtenberg, and Lieberman 1995; cf. De Luca, Lichtenberg, and Ruffo 1995). I conclude that FPU-like systems provide no counterexample to the conjecture that nonintegrable Hamiltonian systems with many degrees of freedom are epsilon-ergodic.

The existence of an energy threshold has also been documented for Lennard-Jones (LJ) gas systems (Bocchieri et al. 1970; Diana et al. 1976; Galgani and Lo Vecchio 1979; Benettin et al. 1980; Kazumasa and Tomohei 1996); i.e., systems of *N* particles in a box interacting via the Lennard-Jones potential, a "potential which gives a very good approximation to the interaction between atoms" and "is perhaps the most widely used interparticle potential" in SM (Reichl 1998, 502, 505). I am not aware of any studies suggesting that the energy threshold for LJ systems is fictitious in the sense explained above for FPU-like systems. So suppose, for the sake of argument, that LJ systems with large *N* but with energy below the threshold are not epsilon-ergodic. It does not follow that Q4 is false, because Q4 refers only to Hamiltonian systems that correspond to (i.e., adequately model) physical systems of interest. The energy threshold corresponds to very low temperatures (Cercignani et al. 1972; e.g., about 4.3 K for argon: Kazumasa and Tomohei 1996, 4688), at which quantum effects become important (Reichl 1998, 488), so that the physical systems at lower temperatures which are the candidates for corresponding to the LJ systems in question are not adequately modeled by Hamiltonian systems as defined in Section 2 and thus do not correspond to the LJ systems in question.[6] Moreover, there is evidence that, above the energy threshold, LJ systems with large *N* are epsilon-ergodic. For example, Stoddard and Ford (1973) studied computationally a two-dimensional LJ gas system with $N = 100$ and found ergodic behavior ("exponential separation with time of most initially close phase-space trajectories"[7]) "over a

6. (It seems thus that it is irrelevant for my purposes whether LJ systems—similarly, FPU-like systems—with large *N* but with energy below the threshold are epsilon-ergodic or not.) Are there physical systems that are adequately modeled by Hamiltonian systems but for which SM does not work? If there are, then none of them should be epsilon-ergodic if epsilon-ergodicity suffices to explain the success of SM. (A related point is made by Sklar: "the system of *two* hard spheres in a cubical box is ergodic" (1973, 209), but does SM work for systems with a small number of degrees of freedom? Actually, it may: see Berdichevsky and Alberti 1991.)

7. Such exponential separation, usually measured by computing the so-called Maximal Lyapunov Exponent (Benettin et al. 1976), indicates ergodicity.

fairly wide density range . . . from very dilute, ideal-gas densities up to 15% of the liquid density" for the case of neon. These results "cannot rule out stable, nonergodic phase-space regions; however, they do indicate that such regions, if they exist, are likely to be quite small" (1973, 1511). In other words, the system may be ε-ergodic for tiny but positive ε. (Wightman (1985, 19) claims that most experts regard LJ gas systems as unlikely to be strictly ergodic.) Given that ε is tiny for $N = 100$, one can expect ε to be fantastically small for realistic-size systems (N around 10^{23}); this observation will prove crucial in the next section.

The above evidence is admittedly scant. Nevertheless, it seems reasonable to reach the tentative conclusion that the dynamical systems of interest are epsilon-ergodic. (This conclusion is compatible with the possibility that these systems are strictly ergodic.[8]) My defense of Q4 is now complete. In the next section I defend Q5.

5. Epsilon-Continuity and Translation-Closeness. Before I give my argument for Q5, it will help to review Malament and Zabell's argument for P5. Malament and Zabell start with the following intuition: "Given two measurable sets on the constant energy surface, if one is but a small displacement of the other, then . . . the probability of finding the exact microstate of the system in one set should be close to that of finding it in the other" (1980, 346). Malament and Zabell formalize this intuition by claiming that P_t is "translation-continuous." Denoting by $d(A, t)$ the "displacement" of a set A by t, a measure μ is *translation-continuous* exactly if, for every A, $\lim_{t\to 0} \mu(d(A, t)) = \mu(A)$; i.e., $\forall \varepsilon > 0\ \exists \delta > 0\ \|t\| < \delta \rightarrow |\mu(d(A, t)) - \mu(A)| < \varepsilon$. Now Malament and Zabell's argument for P5 can be formulated as follows:

(P7) If μ is translation-continuous, then μ is a.c. wrt μ_L (Lebesgue measure);
(P8) If μ is a.c. wrt μ_L, then μ is a.c. wrt μ_m;
(P9) P_t is translation-continuous.

8. Strict ergodicity has turned out to be surprisingly difficult to prove even for relatively simple dynamical systems. Contrary to what is sometimes asserted, the system of N elastic hard balls moving in a cubical box with hard reflecting walls has *not* yet been proven to be ergodic for *arbitrary N*—only for $N \leq 4$ (Szász 1996; cf. Simányi and Szász 1994, 587–590). Nevertheless, it seems that mathematicians are coming increasingly closer to a proof (Sinai 1970; Sinai and Chernov 1987; Krámli et al. 1991, 1992; Simányi 1992a, 1992b), and computational evidence suggests that this system is indeed ergodic (Zheng et al. 1996; cf. Ackland 1993). Note that systems of N elastic hard balls moving in boxes of very special types have been proven to be ergodic for arbitrary N (Bunimovich et al. 1992); some of these systems correspond to "a periodic Lorentz gas with a kind of a bounded free path (finite horizon) condition," and are thus intermediate "between the gas of hard balls and the Lorentz gas model" (1992, 358).

(Strictly speaking, Malament and Zabell omit P8 and define absolute continuity only wrt μ_L.) The conjunction of P7, P8, and P9 entails that P_t is a.c. wrt μ_m; i.e., P5. P7 follows from a theorem proven by Malament and Zabell (but in the context of $\mathbf{R}^n$ rather than of S_E, so that $d(A, t) = A + t$). P8 follows from the definition of μ_m (Appendix, Theorem 3). Thus the only controversial premise is P9.

P9 is not the only possible way of formalizing the intuition with which Malament and Zabell start. Another possible way is by claiming that P_t is "translation-close." Given numbers δ_0 and ε_0 in (0, 1), I will say that a measure μ is δ_0/ε_0-*translation-close* exactly if, for every A, $\|t\| < \delta_0 \rightarrow |\mu(d(A, t)) - \mu(A)| < \varepsilon_0$. I will say that μ is *translation-close* if μ is δ_0/ε_0-translation-close with ε_0 small. It can be shown that translation-closeness is neither weaker nor stronger than translation-continuity. Now my argument for Q5 replaces P7 through P9 with:

(Q7) If μ is δ_0/ε_0-translation-close and $\gamma\varepsilon < (\varepsilon_2 - \varepsilon_0)\delta_n$, then μ is $\gamma\varepsilon/\varepsilon_2$-continuous wrt μ_L;
(Q8) If μ is $\gamma\varepsilon/\varepsilon_2$-continuous wrt μ_L, then μ is $\varepsilon/\varepsilon_2$-continuous wrt μ_m;
(Q9) P_t is δ_0/ε_0-translation-close for some small ε_0.

(δ_n and γ are positive numbers defined in the Appendix, Theorems 2 and 3 respectively.) The conjunction of Q7, Q8, and Q9 entails that P_t is $\varepsilon/\varepsilon_2$-continuous wrt μ_m for $\gamma\varepsilon < (\varepsilon_2 - \varepsilon_0)\delta_n$; i.e., for any $\varepsilon_2 > \varepsilon_0 + \varepsilon\gamma\delta_n^{-1}$. Assuming that ε is sufficiently small, Q5 follows. (The minuteness of ε, argued for in Section 4, is crucial here.[9]) Q7 follows from a theorem which I prove in the Appendix (Theorem 2; following Malament and Zabell, I formulate this theorem in the context of $\mathbf{R}^n$ rather than of S_E). Q8 follows from the fact that μ_L is $\varepsilon/\gamma\varepsilon$-continuous wrt μ_m (Appendix, Theorem 3). Thus the only controversial premise is Q9.

Why should we accept that P_t is translation-continuous or translation-close? Malament and Zabell offer no argument for the translation-continuity of P_t; they only appeal to "the method by which the system is prepared or brought to equilibrium" (1980, 347). Leeds attempts to flesh out this appeal by arguing that the translation-continuity of P_t follows from the assumptions that "the microstate of the system is a continuous function of the parameters of preparation" and that "the probability distribution of these parameters is . . . translation-

9. Using the definitions of δ_n and γ from the Appendix, one gets the impression that ε should decrease faster than exponentially with the number of degrees of freedom if $\varepsilon_0 + \varepsilon\gamma\delta_n^{-1}$ is to be small. Lacking quantitative estimates of ε, I do not know whether this apparently stringent requirement is satisfied. My derivation of Q5 is thus incomplete; this is a topic for future research.

continuous in parameter space" (1989, 327).[10] Any such attempt, however, faces difficult questions. "What exactly do constraints on our ability to delimit an ensemble have to do with real probabilities in the world?" (Sklar 1993, 184.) Does SM work only for systems that we have "prepared"? What counts as preparation?

It seems to me that the key to justifying translation-closeness is *measurement* rather than preparation. Note, first, that *some* assumption about measurement is needed, given that the explanandum refers to measured values. At the very least, the assumption is needed that measurements are sufficiently accurate: if measured values had a large systematic deviation from values of phase functions, then the explanandum would not follow from the almost certain equality of phase functions with microcanonical phase averages.

Measurements, however, are not perfectly accurate. For a given macroscopic observable and a given method of measurement, let $g_1, \ldots, g_k$ be the (finitely many) possible measured values of the observable. (If, for instance, we are measuring pressure, then these values might range from 0.5 to 50 MPa, with increments of 0.01 MPa.) Let g be the function of S_E onto $\{g_1, \ldots, g_k\}$ that gives the measured value which corresponds to every possible microstate. Then the energy surface can be partitioned into a collection of *phase cells* $G_i = \{x \in S_E: g(x) = g_i\}$; i.e., sets of phases that correspond to the same measured value. Let the *coarse-grained* (cf. Farquhar 1964, 17–21; Friedman 1976, 153) equilibrium measure P_{Gt} be the restriction of P_t to (the smallest σ-algebra that contains all) phase cells; i.e., $P_{Gt}(A) = P_t(A)$ if A is a union of phase cells and $P_{Gt}(A)$ is undefined otherwise. *For purposes of the explanandum, it suffices to establish the epsilon-closeness to the microcanonical measure of the coarse-grained equilibrium measures* P_{Gt} *that correspond to the macroscopic observables of interest and to the available methods of measurement*; the epsilon-closeness of the "fine-grained" equilibrium measure P_t would also be sufficient but is not necessary. (Indeed, let $G_\varepsilon = \{x \in S_E: |g(x) - \langle f \rangle_m| \leq \varepsilon\}$. Given the assumption that measurements are sufficiently accurate, P1 entails that $\mu_m(G_\varepsilon) \cong 1$ for some small $\varepsilon > 0$. Given that every G_ε is a union of phase cells, the explanandum can be established by showing that $P_{Gt}(G_\varepsilon) \cong 1$ for some small $\varepsilon > 0$. Therefore, it suffices to show that P_{Gt} and the restriction

10. I do not think that this argument is valid. The continuity of the microstate as a function of the parameters of preparation excludes the existence of distant microstates resulting from close values of the parameters but is compatible with the existence of close microstates resulting from distant values of the parameters, hence with the existence of close microstates having distant probabilities.

of μ_m to unions of phase cells are epsilon-close.) Now P_{Gt} is invariant if P_t is; thus it suffices to establish that (every) P_{Gt} is translation-close.

What have we gained by moving from P_t to P_{Gt}? Three things, I submit. First, now we have a reason for choosing between translation-continuity and translation-closeness, these two different but confusingly similar ways of formalizing the same intuition. P_{Gt} need not be translation-continuous: as long as we exclude "displacements" that leave a union of phase cells unchanged, any (no matter how small) displacement of a union of phase cells will result in the addition or deletion of at least one phase cell, and will thus result in a discontinuous probability change if it results in a probability change at all (because there are only finitely many possible combinations of phase cells). Second, now we have a rough argument for the translation-closeness of P_{Gt}. Sufficiently small displacements (δ_0 small) will result in the addition or deletion of only a few phase cells, and thus in a probability change at most equal to a small multiple k of $\max_i P_{Gt}(G_i)$. This maximum probability change, $k\max_i P_{Gt}(G_i)$, which corresponds to ε_0 in the definition of δ_0/ε_0-translation-closeness, will be small (and translation-closeness will be established) if $\max_i P_{Gt}(G_i)$ is small. But $\max_i P_{Gt}(G_i)$ will in general decrease as the accuracy of measurements increases, and should thus be small if measurements are sufficiently accurate (i.e., if the coarse-graining is not "too coarse"). (Here the assumption is needed that $\max_i P_{Gt}(G_i)$ is small for perfectly accurate measurements; i.e., when the set of all possible measured values is $f(S_E)$, the set of all possible values of the phase function f. This assumption does not look implausible (given that $f(S_E)$ is in general infinite) and is much weaker than Q9, but I do not know how to justify it.) Third, the value of ε_0 ($= k\max_i P_{Gt}(G_i)$) depends on the observable, because different observables in general yield different partitions of the energy surface into phase cells. Thus we have the beginning of an explanation for the fact that SM is more successful for some observables than for others (Livi et al. 1987; cf. Leeds 1989, 334–335, talking about nonequilibrium), because P_{Gt} may be closer to μ_m for some observables than for others.

The bottom line of my proposal is that a justification for the microcanonical measure is to be found in the coarse-graining (discretization) of the phase space that results from the limited precision of measurements. Interestingly, a similar proposal has been defended in a related context. A dynamical system can have uncountably many ergodic measures, resulting in different values of phase averages. Computational studies, however, suggest that the Lebesgue measure μ_L holds a privileged position, in the sense that, whenever a unique ergodic measure a.c. wrt μ_L exists, numerical computations of time averages yield a value equal to the phase average which corresponds to that measure (Benettin, Casartelli, et al. 1979). The reason seems to be that, "since digital computers are finite-state machines, they effectively discretize the continuous phase space" (Beck and Roepstorff 1987, 173). In fact, Góra and Boyarsky have proven that, for dynamical systems with $X = [0, 1]$ that satisfy a certain condition, "the very process of discretization of the space forces the computer orbits to display only the" ergodic measure that is a.c. wrt μ_L (1988, 322; cf. Boyarsky 1984, 1990; Corless 1994, 112).[11] There is thus converging evidence that a satisfactory justification for the microcanonical measure may at last be at hand.

6. Conclusion. Though not lacking in detail, the explanation of the success of SM that I proposed in this paper is in some respects programmatic. Here is a list of open questions. First, the argument for the translation-closeness of the coarse-grained equilibrium measures that I outlined in Section 5 needs to be made more precise. We need a definition of displacements with respect to unions of phase cells, a more rigorous relation between the accuracy of measurements and the maximum probability of a phase cell, and an argument for the assumption that I was unable to justify. Second, an argument is needed for Q6 (the invariance of the equilibrium measure).[12] Third, epsilon-ergodicity needs to be investigated in more detail. It would be nice to have theo-

11. It should not be thought, however, that computational studies are worthless or that the evidence for epsilon-ergodicity that I adduced in Section 4 is suspicious. Precautions against roundoff errors are routinely taken by investigators (Lichtenberg and Lieberman 1992, 310–312). Although in artificial cases "numerical methods can introduce spurious chaos or even suppress actual chaos", such discrepancies are rare in practice (Corless 1994, 107–109). A partial explanation of this rarity is provided by *shadowing* theorems: even though the computer orbit that passes through a given point of the phase space after sufficiently many iterations becomes very distant from the phase-space trajectory, for some kinds of dynamical systems the computer orbit is always closely "shadowed" by (i.e., is uniformly close to) *some* phase-space trajectory (Lanford 1983, 30–37; Benettin et al. 1978; contrast: Palmore and McCauley 1987; Corless 1994, 110–113).

12. Q6 does not follow from my definition of equilibrium (Leeds 1989, 332 fn. 3). Actually, Q6 is false. Here is why. Given the definition of invariance, Q6 becomes: $\forall t' \forall A(P_t(T_{t'}(A)) = P_t(A))$. Given that $\forall t' \forall t'' \forall A(P_{t'}(A) = P_{t''}(T_{t''-t'}(A)))$ (which follows from the definition of the flow T_t), Q6 is equivalent to: $\forall t' \forall A(P_t(A) = P_{t'}(A))$ (*stationarity*). Therefore, Q6 entails that the equilibrium measure P_t is equal to the initial measure P_0 that corresponds to the time of preparation of the system (when such a time exists). But then P_0 must be epsilon-close to the microcanonical if P_t is, whereas one can prepare systems so that P_0 is very distant from the microcanonical. To avoid this problem, I replace Q6 with: $\forall \tau < t - t^* \forall A(|P_t(T_\tau(A)) - P_t(A)| \leq \varepsilon$ for small $\varepsilon > 0)$, where t^* is a time at which the physical system has settled down to equilibrium. It can be shown that this premise is equivalent to *forward epsilon-stationarity*: $\forall t' > t^* \forall A(|P_t(A) - P_{t'}(A)| \leq \varepsilon$ for small $\varepsilon > 0)$. Theorem 1 of the Appendix (hence also P1) can be similarly modified.

retical results showing both that ergodicity is approached when the number of degrees of freedom increases and that ε decreases faster than exponentially with the number of degrees of freedom. Fourth, the theorem that translation-closeness entails epsilon-continuity (Theorem 2 of the Appendix) needs to be generalized from Euclidean spaces to energy surfaces. Fifth, larger questions need to be addressed. Why does an equilibrium probability measure exist? What kind of probabilities are we talking about? Ultimately, why does equilibrium exist (cf. Sklar 1978, 192)?

In spite of its shortcomings, my proposal has several appealing features. First, it captures the widely held (though not yet rigorously justified) insight that large Hamiltonian systems are almost but not exactly ergodic. Second, it avoids the implausibly strong conclusion that *only* the microcanonical measure works. Third, it finds a place for the oft-repeated idea that a justification is needed only for a restricted set of phase functions, those that correspond to the macroscopic observables of interest. Fourth, it gives a hint as to why SM works better for some observables than for others. Fifth, it coheres with independently arrived at results in computer theory.

I conclude by expressing the hope that the present paper will stimulate further work in the direction of epsilon-ergodicity.

Appendix

Theorem 1 (epsilon-ergodicity and epsilon-continuity entail epsilon-closeness). *Let* $\langle X, T_t\rangle$ *be a dynamical system. Let* ε, ε_1, *and* ε_2 *be numbers in* $[0, 1)$ *such that* $\varepsilon \leq \varepsilon_1$ *and let* $\varepsilon_3 = 2\varepsilon_2 + \varepsilon(1 - \varepsilon)^{-1}$. *Let* μ_1 *and* μ_2 *be probability measures on the same* σ*-algebra of subsets of* X. *If* μ_1 *is* ε*-ergodic and* μ_2 *is invariant and* $\varepsilon_1/\varepsilon_2$*-continuous with respect to* μ_1, *then* μ_1 *and* μ_2 *are* ε_3*-close.*

Proof. I will consider the case of discrete-time dynamical systems, as do Cornfeld et al. (1982, 16) in their proof of an analogous theorem for *strict* ergodicity and *absolute* continuity (the theorem I called P3 in Section 3).

Since μ_1 is ε-ergodic, there is an invariant, μ_1-indecomposable subset Y of X with $\mu_1(Y) = 1 - \varepsilon$. Let μ'_1 be the normalized restriction of μ_1 to Y (i.e., $\mu'_1(\bullet) = \mu_1(\bullet \cap Y)/\mu_1(Y)$). Let A be any measurable subset of X, let χ_{AY} be the indicator function of the set $AY = A \cap Y$ (i.e., $\chi_{AY}(x)$ is 1 if $x \in AY$ and is 0 if $x \notin AY$), and let f_n, $n \in \mathbf{N}$, be the finite time averages of χ_{AY}: $f_n(x) = \frac{1}{2n+1} \sum_{t=-n}^{n} \chi_{AY}(T_t(x))$. Define $F = \{x \in Y: \lim_{n\to\infty} f_n(x) = \mu'_1(AY)\}$. Then the Birkhoff-Khinchin ergodic theorem (Cornfeld et al. 1982, 11) gives: $\mu'_1(F) = 1$. Thus $\mu_1(F) = \mu_1(Y) = 1 - \varepsilon$, so that $\mu_1(F^c) = \mu_1(Y^c) = \varepsilon \leq \varepsilon_1$, and $\varepsilon_1/\varepsilon_2$-continuity gives: $\mu_2(F^c) \leq \varepsilon_2$, $\mu_2(Y^c) \leq \varepsilon_2$.

The Lebesgue dominated convergence theorem (Rudin 1976, 321) gives: $\lim_{n\to\infty} \int_F f_n\, d\mu_2 = \int_F \mu'_1(AY) d\mu_2 = \mu_2(F)\mu'_1(AY)$. Now for any t: $\int_Y \chi_{AY}(T_t(x))\, d\mu_2 = \mu_2(YT_{-t}(AY)) = \mu_2(T_{-t}(AY)) = \mu_2(AY)$, because Y is an invariant set and μ_2 is an invariant measure. Thus: $\lim_{n\to\infty} \int_Y f_n d\mu_2 = \mu_2(AY)$. Therefore: $\lim_{n\to\infty} \int_Y f_n\, d\mu_2 - \lim_{n\to\infty} \int_F f_n\, d\mu_2 = \mu_2(AY) - \mu_2(F)\mu'_1(AY) = \lim_{n\to\infty} \int_{Y\setminus F} f_n\, d\mu_2 \in [0, \mu_2(Y\setminus F)] \subseteq [0, \mu_2(F^c)] \subseteq [0, \varepsilon_2]$. In conclusion: (*) $\mu_2(AY) - \mu_2(F)\mu'_1(AY) \geq 0$; (#) $\mu_2(AY) - \mu_2(F)\mu'_1(AY) \leq \varepsilon_2$.

Now $\mu_1(AY) = \mu_1(A) - \mu_1(AY^c) \geq \mu_1(A) - \mu_1(Y^c) = \mu_1(A) - \varepsilon$. Thus, using (*) and $\mu_2(F) \geq 1 - \varepsilon_2$, we get: $\mu_2(A) \geq \mu_2(AY) \geq (1 - \varepsilon_2)\mu_1(AY)(1 - \varepsilon)^{-1} \geq (1 - \varepsilon_2)(\mu_1(A) - \varepsilon)(1 - \varepsilon)^{-1}$. Therefore: $\mu_2(A) - \mu_1(A) \geq (1 - \varepsilon_2)(\mu_1(A) - \varepsilon)(1 - \varepsilon)^{-1} - \mu_1(A) = (\mu_1(A)(\varepsilon - \varepsilon_2) - \varepsilon(1 - \varepsilon_2))(1 - \varepsilon)^{-1}$. If $\varepsilon_2 \geq \varepsilon$ we get: $\mu_2(A) - \mu_1(A) \geq (\varepsilon - \varepsilon_2 - \varepsilon(1 - \varepsilon_2))(1 - \varepsilon)^{-1} = -\varepsilon_2 \geq -\varepsilon_3$. On the other hand, if $\varepsilon_2 < \varepsilon$ we get: $\mu_2(A) - \mu_1(A) \geq -\varepsilon(1 - \varepsilon_2)(1 - \varepsilon)^{-1} \geq -\varepsilon(1 - \varepsilon)^{-1} \geq -\varepsilon_3$, and half of ε_3-closeness is proven.

Similarly, $\mu_2(AY) = \mu_2(A) - \mu_2(AY^c) \geq \mu_2(A) - \mu_2(Y^c) \geq \mu_2(A) - \varepsilon_2$. Thus, using (#): $\mu_2(A) - \varepsilon_2 \leq \varepsilon_2 + \mu_1(A)(1 - \varepsilon)^{-1}$. Therefore: $\mu_2(A) - \mu_1(A) \leq 2\varepsilon_2 + \mu_1(A)((1 - \varepsilon)^{-1} - 1) \leq 2\varepsilon_2 + \varepsilon(1 - \varepsilon)^{-1} = \varepsilon_3$, and the theorem is proven. □

Theorem 2 (translation-closeness entails epsilon-continuity). *Let μ be Lebesgue measure on* $\mathbf{R}^n$ *and v a probability measure on* $\mathbf{R}^n$. *Let δ_0 and ε_0 be numbers in* $(0, 1)$ *and let δ_n be the volume of an n-sphere of radius δ_0 (i.e., $\delta_n = (\pi^{1/2}\delta_0)^n/\Gamma(n/2+1)$, where Γ is the gamma function). Then* (1) *entails* (2):

(1) *For every open set $A \subseteq \mathbf{R}^n$ and every $t \in \mathbf{R}^n$, $\|t\| < \delta_0 \rightarrow |v(A+t) - v(A)| < \varepsilon_0$;*

(2) *For every $\delta \in (0, \delta_n)$, every $\varepsilon_2 \in (\varepsilon_0, 1)$, and every $B \subseteq \mathbf{R}^n$, $\mu(B) \leq (\varepsilon_2 - \varepsilon_0)\delta \rightarrow v(B) \leq \varepsilon_2$.*

Proof. Suppose that (2) fails. Then there are $\delta \in (0, \delta_n)$, $\varepsilon_2 \in (\varepsilon_0, 1)$, and $B \subseteq \mathbf{R}^n$ such that $\mu(B) \leq (\varepsilon_2 - \varepsilon_0)\delta$ but $v(B) > \varepsilon_2$. Then (see Malament and Zabell 1980, 348) there is a closed subset C of B such that $v(C) > \varepsilon_2$ and $\int_{\mathbf{R}^n} v(C+t)d\mu(t) = \mu(C) \leq \mu(B) \leq (\varepsilon_2 - \varepsilon_0)\delta$. Let $T = \{t \in \mathbf{R}^n\colon v(C+t) \geq \varepsilon_2 - \varepsilon_0\}$. Then $(\varepsilon_2 - \varepsilon_0)\,\delta \geq \int_T v(C+t)d\mu(t) \geq (\varepsilon_2 - \varepsilon_0)\mu(T)$; thus $\mu(T) \leq \delta$. Now let $A = C^c$; then A is open, $A+t = (C+t)^c$, and $T = \{t \in \mathbf{R}^n\colon v(A+t) \leq 1 - (\varepsilon_2 - \varepsilon_0)\}$. Now $v(C) > \varepsilon_2$ gives $v(A) < 1 - \varepsilon_2$, so that $v(A) + \varepsilon_0 < 1 - (\varepsilon_2 - \varepsilon_0)$. Thus T is a superset of $S = \{t \in \mathbf{R}^n\colon v(A+t) < v(A) + \varepsilon_0\}$, so that

$\mu(S) \leq \mu(T) \leq \delta < \delta_n$, contradicting (1) (because (1) entails $\mu(S) \geq \delta_n$). □

Theorem 3 (epsilon-continuity of Lebesgue with respect to the microcanonical measure). *Let S be an energy surface of an autonomous Hamiltonian dynamical system with Hamiltonian H and n degrees of freedom. Let μ_m be the microcanonical measure on S; i.e., $\mu_m(A) = \int_A |\nabla_x H|^{-1}\, d\mu_L(x) \,/\, \int_S |\nabla_x H|^{-1}\, d\mu_L(x)$, where μ_L is Lebesgue measure on $\mathbf{R}^{2n-1}$. Let $\gamma = (\sup_{x \in S} |\nabla_x H|) \int_S |\nabla_x H|^{-1}\, d\mu_L(x)$. For any $\varepsilon \in [0, 1)$, μ_L is $\varepsilon/\gamma\varepsilon$-continuous with respect to μ_m.*

Proof. Suppose $\mu_m(A) \leq \varepsilon$. Then $\varepsilon \geq (\sup_{x \in A} |\nabla_x H|)^{-1} \int_A d\mu_L(x) \,/\, \int_S |\nabla_x H|^{-1}\, d\mu_L(x) \geq \gamma^{-1}\mu_L(A)$. Thus $\mu_L(A) \leq \gamma\varepsilon$. □

REFERENCES

Ackland, Graeme J. (1993), "Equipartition and ergodicity in closed one-dimensional systems of hard spheres with different masses", *Physical Review E* 47: 3268–3275.

Batterman, Robert W. (1998), "Why equilibrium statistical mechanics works: Universality and the renormalization group", Philosophy of Science 65: 183–208.

Beck, C. and G. Roepstorff (1987), "Effects of phase space discretization on the long-time behavior of dynamical systems", *Physica D* 25: 173–180.

Benettin, Giancarlo (1994), "Ergodicity and time-scales for statistical equilibrium in classical dynamical systems", *Progress of Theoretical Physics Supplement* 116: 207–234.

Benettin, Giancarlo, M. Casartelli, Luigi Galgani, Antonio Giorgilli, and Jean-Marie Strelcyn (1978), "On the reliability of numerical studies of stochasticity. I: Existence of time averages", *Nuovo Cimento B* 44: 183–195.

———. (1979), "On the reliability of numerical studies of stochasticity. II: Identification of time averages", *Nuovo Cimento B* 50: 211–232.

Benettin, Giancarlo, C. Froeschlé, and J. P. Scheidecker (1979), "Kolmogorov entropy of a dynamical system with an increasing number of degrees of freedom", *Physical Review A* 19: 2454–2460.

Benettin, Giancarlo, Luigi Galgani, and Jean-Marie Strelcyn (1976), "Kolmogorov entropy and numerical experiments", *Physical Review A* 14: 2338–2345.

Benettin, Giancarlo, Guido Lo Vecchio, and Alexander Tenenbaum (1980), "Stochastic transition in two-dimensional Lennard-Jones systems", *Physical Review A* 22: 1709–1719.

Berdichevsky, V. L. and M. V. Alberti (1991), "Statistical mechanics of Hénon-Heiles oscillators", *Physical Review A* 44: 858–865.

Bocchieri, P., Scotti, A., Bearzi, B., and Loinger, A. (1970), "Anharmonic chain with Lennard-Jones interaction", *Physical Review A* 2: 2013–2019.

Boyarsky, Abraham (1984), "On the significance of absolutely continuous invariant measures", *Physica D* 11: 130–146.

———. (1990), "Discontinuity of physical measures", *Physics Letters A* 149: 12–16.

Bunimovich, Leonid, Carlangelo Liverani, Alessandro Pellegrinotti, and Yurii Suhov (1992), "Ergodic systems of *n* balls in a billiard table", *Communications in Mathematical Physics* 146: 357–396.

Cercignani, C., Luigi Galgani, and A. Scotti (1972), "Zero-point energy in classical nonlinear mechanics", *Physics Letters A* 38: 403–404.

Corless, R. M. (1994), "What good are numerical simulations of chaotic dynamical systems?" *Computers and Mathematics with Applications* 28: 107–121.

Cornfeld, I. P., Sergei V. Fomin, and Yakov G. Sinai (1982), *Ergodic theory*. New York: Springer.

De Luca, J., Allan J. Lichtenberg, and Michael A. Lieberman (1995), "Time scale to ergodicity in the Fermi-Pasta-Ulam system", *Chaos* 5: 283–297.

De Luca, J., Allan J. Lichtenberg, and Stefano Ruffo (1995), "Energy transitions and time scales to equipartition in the Fermi-Pasta-Ulam oscillator chain", *Physical Review E* 51: 2877–2885.

Diana, E., Luigi Galgani, M. Casartelli, G. Casati, and A. Scotti (1976), "Stochastic transition in a classical nonlinear dynamical system: A Lennard-Jones chain", *Theoretical and Mathematical Physics* 29: 1022–1027.

Earman, John and Miklós Rédei (1996), "Why ergodic theory does not explain the success of equilibrium statistical mechanics", *British Journal for the Philosophy of Science* 47: 63–78.

Falcioni, M., U. Marini Bettolo Marconi, and Angelo Vulpiani (1991), "Ergodic properties of high-dimensional symplectic maps", *Physical Review A* 44: 2263–2270.

Farquhar, I. E. (1964), *Ergodic theory in statistical mechanics*. New York: Wiley.

Fermi, Enrico, John Pasta, and S. Ulam (1965), "Studies of non linear problems", in E. Segré (ed.), *Enrico Fermi: Collected papers*, vol. II. Chicago: University of Chicago Press, pp. 978–988. [Original circulated 1955 as Document LA-1940.]

Ford, Joseph (1992), "The Fermi-Pasta-Ulam problem: Paradox turns discovery", *Physics Reports* 213: 271–310.

Friedman, K. S. (1976), "A partial vindication of ergodic theory", *Philosophy of Science* 43: 151–162.

Froeschlé, C. (1978), "Connectance of dynamical systems with increasing number of degrees of freedom", *Physical Review A* 18: 277–281.

Froeschlé, C. and J. P. Scheidecker (1975), "Stochasticity of dynamical systems with increasing number of degrees of freedom", *Physical Review A* 12: 2137–2143.

Galgani, Luigi, Antonio Giorgilli, Andrea Martinoli, and Stefano Vanzini (1992), "On the problem of energy equipartition for large systems of the Fermi-Pasta-Ulam type: Analytical and numerical estimates", *Physica D* 59: 334–348.

Galgani, Luigi and Guido Lo Vecchio (1979), "Stochasticity thresholds for systems of coupled oscillators", *Nuovo Cimento B* 52: 1–14.

Gibbs, J. Willard (1902), *Elementary principles in statistical mechanics*. New York: Charles Scribner's Sons.

Góra, P. and Abraham Boyarsky (1988), "Why computers like Lebesgue measure", *Computers and Mathematics with Applications* 16: 321–329.

Györgyi, G., F. H. Ling, and G. Schmidt (1989), "Torus structure in higher-dimensional Hamiltonian systems", *Physical Review A* 40: 5311–5318.

Hénon, Michel (1983), "Numerical exploration of Hamiltonian systems", in G. Iooss, R. H. G. Helleman, and R. Stora (eds.), *Chaotic behaviour of deterministic systems*. New York: North-Holland, pp. 53–170.

Hurd, Lyman, Celso Grebogi, and Edward Ott (1994), "On the tendency toward ergodicity with increasing number of degrees of freedom in Hamiltonian systems", in J. Seimenis (ed.), *Hamiltonian mechanics: Integrability and chaotic behavior*. New York: Plenum, pp. 123–129.

Kantz, Holger (1989), "Vanishing stability thresholds in the thermodynamic limit of nonintegrable conservative systems", *Physica D* 39: 322–335.

Kantz, Holger, Roberto Livi, and Stefano Ruffo (1994), "Equipartition thresholds in chains of anharmonic oscillators", *Journal of Statistical Physics* 76: 627–643.

Kazumasa, Shinjo and Sasada Tomohei (1996), "Hamiltonian systems with many degrees of freedom: Asymmetric motion and intensity of motion in phase space", *Physical Review E* 54: 4685–4700.

Khinchin, Aleksandr I. (1949), *Mathematical foundations of statistical mechanics*, trans. G. Gamow. New York: Dover.

Krámli, A., Nándor Simányi, and Domokos Szász (1991), "The K-property of three billiard balls", *Annals of Mathematics* 133: 37–72.

———. (1992), "The K-property of four billiard balls", *Communications in Mathematical Physics* 144: 107–148.

Lanford, Oscar E., III (1973), "Entropy and equilibrium states in classical statistical mechanics", in A. Lenard (ed.), *Statistical mechanics and mathematical problems*. New York: Springer, pp. 1–113.

———. (1983), "Introduction to the mathematical theory of dynamical systems", in G.

Iooss, R. H. G. Helleman, and R. Stora (eds.), *Chaotic behaviour of deterministic systems*. New York: North-Holland, pp. 3–51.
Leeds, Stephen (1989), "Malament and Zabell on Gibbs phase averaging", *Philosophy of Science* 56: 325–340.
Lichtenberg, Allan J. and Michael A. Lieberman (1992), *Regular and chaotic dynamics* (2nd ed.). New York: Springer.
Livi, Roberto, Marco Pettini, Stefano Ruffo, Massimo Sparpaglione, and Angelo Vulpiani (1985a), "Equipartition threshold in nonlinear large Hamiltonian systems: The Fermi-Pasta-Ulam model", *Physical Review A* 31: 1039–1045.
Livi, Roberto, Marco Pettini, Stefano Ruffo, and Angelo Vulpiani (1985b), "Further results on the equipartition threshold in large nonlinear Hamiltonian systems", *Physical Review A* 31: 2740–2742.
———. (1987), "Chaotic behavior in nonlinear Hamiltonian systems and equilibrium statistical mechanics", *Journal of Statistical Physics* 48: 539–559.
Malament, David B. and Sandy L. Zabell (1980), "Why Gibbs phase averages work—The role of ergodic theory", *Philosophy of Science* 47: 339–349.
Markus, L. and K. R. Meyer (1974), "Generic Hamiltonian dynamical systems are neither integrable nor ergodic", *Memoirs of the American Mathematical Society* 144.
Mazur, P. and J. van der Linden (1963), "Asymptotic form of the structure function for real systems", *Journal of Mathematical Physics* 4: 271–277.
Palmore, Julian I. and Joseph L. McCauley (1987), "Shadowing by computable chaotic orbits", *Physics Letters A* 122: 399–402.
Pettini, Marco and Monica Cerruti-Sola (1991), "Strong stochasticity threshold in nonlinear large Hamiltonian systems: Effect on mixing times", *Physical Review A* 44: 975–987.
Pettini, Marco and Marco Landolfi (1990), "Relaxation properties and ergodicity breaking in nonlinear Hamiltonian dynamics", *Physical Review A* 41: 768–783.
Quay, Paul M. (1978), "A philosophical explanation of the explanatory functions of ergodic theory", *Philosophy of Science* 45: 47–59.
Reichl, L. E. (1998), *A modern course in statistical physics* (2nd ed.). New York: Wiley.
Reidl, Charles J., Jr. and Bruce N. Miller (1992), "Gravity in one dimension: Stability of periodic orbits", *Physical Review A* 46: 837–843.
———. (1993), "Gravity in one dimension: The critical population", *Physical Review A* 48: 4250–4256.
Rudin, Walter (1976), *Principles of mathematical analysis* (3rd ed.). New York: McGraw-Hill.
Simányi, Nándor (1992a), "The K-property of *N* billiard balls I", *Inventiones Mathematicae* 108: 521–548.
———. (1992b), "The K-property of *N* billiard balls II. Computation of neutral linear spaces", *Inventiones Mathematicae* 110: 151–172.
Simányi, Nándor and Domokos Szász (1994), "The K-property of 4D billiards with non-orthogonal cylindric scatterers", *Journal of Statistical Physics* 76: 587–604.
Sinai, Yakov G. (1970), "Dynamical systems with elastic reflections", *Russian Mathematical Surveys* 25: 137–189.
Sinai, Yakov G. and N. I. Chernov (1987), "Ergodic properties of certain systems of two-dimensional disks and three-dimensional balls", *Russian Mathematical Surveys* 42: 181–207.
Sklar, Lawrence (1973), "Statistical explanation and ergodic theory", *Philosophy of Science* 40: 194–212.
———. (1978), "Comments", in P. D. Asquith and I. Hacking (eds.), *PSA* 1978, vol. 2. East Lansing, MI: Philosophy of Science Association, pp. 186–193.
———. (1993), *Physics and chance: Philosophical issues in the foundations of statistical mechanics*. Cambridge: Cambridge University Press.
Stoddard, Spotswood D. and Joseph Ford (1973), "Numerical experiments on the stochastic behavior of a Lennard-Jones gas system", *Physical Review A* 8: 1504–1512.
Szász, Domokos (1996), "Boltzmann's ergodic hypothesis, a conjecture for centuries?" *Studia Scientiarum Mathematicarum Hungarica* 31: 299–322.
Wightman, A. S. (1985), "Regular and chaotic motions in dynamical systems: Introduction

to the problems", in G. Velo and A. S. Wightman (eds.), *Regular and chaotic motion in dynamical systems*. New York: Plenum, pp. 1–26.

Wright, H. L. and Bruce N. Miller (1984), "Gravity in one dimension: A dynamical and statistical study", *Physical Review A* 29: 1411–1418.

Zheng, Zhigang, Gang Hu, and Juyuan Zhang (1996), "Ergodicity in hard-ball systems and Boltzmann's entropy", *Physical Review E* 53: 3246–3252.