Ergodicity, ensembles, irreversibility in Boltzmann and beyond. [!]

Giovanni Gallavotti[*]

Abstract: *the implications of the original misunderstanding of the etymology of the word "ergodic" are discussed, and the contents of a not too well known paper by Boltzmann are critically examined. The connection with the modern theory of Ruelle is attempted.*

§1 *The etymology of the word "ergodic" and the heat theorems.*

Trying to find the meaning of the word "ergodic" one is led to a 1884 paper by Boltzmann, [B84].[1] This paper by Boltzmann is seldom quoted [2] and no english translation is available yet. But I think that this is one of the most interesting papers of Boltzmann: it is a precursor of the work of Gibbs, [G], on the ensembles, containing it almost entirely (if one recalls that the equivalence of the canonical and microcanonical ensembles was already established (elsewhere) by Boltzmann himself, at least in the free case [B66],[B68]), and I will try to motivate such statement.

The paper stems from the fundamental, not too well known, work of Helmholtz, [He1], [He2], who noted that *monocyclic* systems[3] could be used to provide models of thermodynamics in a sense that Boltzmann undertakes to extend to a major generalization.

After an introduction, whose relative obscurity has been probably responsible for the little attention this paper has received, Boltzmann introduces the notion of "stationary" probability distribution on the phase space of N interacting particles enclosed in a vessel with volume V. He calls a family $\mathcal{E}$ of such probabilities a *monode*, generalizing an "analogous" concept on monocyclic systems.[4]

In fact the orbits of a monocyclic system can be regarded as endowed with a probability distribution giving an arc length a probability proportional to the time spent on it by the motion: hence their family forms a family of stationary probability distributions.

Etymologically this undoubtedly[5] means a family of stationary distributions with a "unique nature", (each consisting of systems with a "unique nature", differing only by the initial conditions), from $\mu\acute{o}\nu o\varsigma$ and $\varepsilon\tilde{\iota}\delta o\varsigma$,

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[*] Dipartimento di Fisica, Università di Roma, P.le Moro 2, 00185 Roma. E–mail 40221::gallavotti. This paper is archived in *mp_arc@math.utexas.edu*, updated copies (in Postscript) can also be obtained by sending request to the author, by E–mail.

[1] see the footnote of S. Brush in his edition, [Bo2], of the Lectures on Gas Theory, on p. 297 (§32): here the Boltzmann's paper is quoted as the first place where the word is introduced, although the etymology is taken from the Erhenfests' paper, which is incorrect on this point: see [EE], note #93, p.89, (where also the first appearance of the word is incorrectly dated and quoted).

[2] I found only the Brush's reference in [1], and a partial account in [Br1], p.242 and p. 368, before my own etymological discussion, appeared in print in [G1] after several years of lectures on the subject. My discussion was repeated in [G2] and [G3]. More recently the paper has been appropriately quoted by [Pl], unaware of my analysis. The paper was discussed also by [Ma], see footnote [9] below.

[3] this is what we call today a system whose phase space contains only periodic orbits, or cycles: *i.e.* essentially a one dimensional conservative system.

[4] in fact Boltzmann first calls a monode just a single stationary distribution regarded as an ensemble. But sometimes later he implicitly, or explicitly, thinks of a monode as a collection of stationary distributions parameterized by some parameters: the distinction is always very clear from the context. Therefore, for simplicity, I take here the liberty of calling "monode" a collection of stationary distributions, and the individual elements of the collection will be called "elements of the monode". The etymology that follows, however, is more appropriate for the elements of the monodes, as they are thought as consisting of many copies of the same system in different configurations. By reading the Boltzmann's analysis one can get the impression, see p. 132 of [B84], that the word monode had been already introduced by Maxwell, in [M]: however the reference to Maxwell is probably meant to refer to the notion of stationarity rather than to the word monode which does not seem to appear in [M].

[5] of course one can doubt (on this as well as on many other things).

with a probable reference to Plato and Leibnitz.

Then the following question is posed. Given an element μ of a monode $\mathcal{E}$, also called a monode by Boltzmann, we can compute the average values of various observables, *e.g.* average kinetic energy, average total energy, average momentum transfer per unit time and unit surface in the collisions with the vessel walls, average volume occupied and density, denoted, respectively:

$$T = \frac{1}{N}\langle K\rangle_\mu, \quad U = \langle K+\Phi\rangle_\mu, \quad p, \quad V, \quad \rho = \frac{N}{V} \tag{1.1}$$

where Φ denotes the potential interaction energy. We then imagine to vary μ in the monode $\mathcal{E}$, by an infinitesimal amount. **Question:** *is it true that the corresponding variations dU and dV are such that*:

$$\frac{dU + p\,dV}{T} \quad \text{is an exact differential} \quad dS\ ? \tag{1.2}$$

In other words is it true that the above quantities, defined in purely mechanical terms, verify the same relation that would hold between them if, for some thermodynamic system, they were the thermodynamic quantities bearing the same name, with the further identification of the average kinetic energy with the absolute temperature?[6] If so the monode would provide a "mechanical model of thermodynamics" extending, by far, the early examples of Helmholtz on monocyclic systems.

Thus Boltzmann is led to the following definition:

Definition: *a monode $\mathcal{E}$ is called an* orthode *if the property described by (1.2) holds.*

Undoubtedly the etymology of "orthode" is ὀρθός and εἶδος, *i.e.* "right nature".

I find it almost unbelievable that such a deep definition has not been taken up by the subsequent literature. This is more so as Boltzmann, in the same paper, proceeds to discuss "examples" of mechanical models of thermodynamics, *i.e.* examples of orthodic monodes.

It has, certainly, not escaped the reader that an orthodic monode (or orthode) is what we call today an *equilibrium ensemble*. And the above orthodicity concept is still attributed to Gibbs, see [Br1], p. 242).

The examples of orthodes discussed by Boltzmann in his paper are the *holode* and the *ergode* which are two ensembles whose elements are parameterized with two parameters β, N or U, N, respectively. Their elements are:

$$\mu_{\beta,N}(d\,\vec p\,d\,\vec q) = \frac{d\,\vec p_1\dots d\,\vec p_n d\,\vec q_1\dots d\,\vec q_n}{const}\, e^{-\beta(K+\Phi)} \tag{1.3}$$

and:

$$\mu_{U,N}(d\,\vec p d\,\vec q) = \frac{d\,\vec p_1\dots d\,\vec p_n d\,\vec q_1\dots d\,\vec q_n}{const}\, \delta(K(\vec p) + \Phi(\vec q) - U) \tag{1.4}$$

Boltzmann proves that the above two ensembles are both orthodes! thus establishing that the canonical and the microcanonical ensembles (using our modern terminology) are equilibrium ensembles and provide mechanical models of thermodynamics.[7]

Boltzmann's proof makes use of the auxiliary (with respect to the above definition) notion of heat transfer: in the canonical case it yields exactly the desired result; in the microcanonical it is also very simple but somehow based on a different notion of heat transfer. An analysis of the matter easily shows, [G4], that the correct[8] statement becomes exact only in the limit as $N, U \to \infty$, keeping of course $\frac{U}{V}, \frac{N}{V}$ constant, *i.e.* in what we call today the "thermodynamic limit".

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[6] that the temperature should be identified with the average kinetic energy per particle was quite well established (for free gases) since the paper by Clausius, [C], and the paper on the equipartition of kinetic energy by Boltzmann, [B68] (in the interacting cases); see the discussion of it in Maxwell's last scientific work, [M]. The latter paper is also very interesting as Maxwell asks there whether there are other stationary distributions on the energy surface, and tries to answer the question by putting forward the ergodic hypothesis.

[7] he also studies other ensembles, for instance in a system in which angular momntum is conserved, *e.g.* a gas in a spherical container, he considers the stationary distributions with fixed energy and fixed total angular momentum $\vec K$. Such monodes are called, by Boltzmann, *planodes* (form the "area law"); and he remarks that in general they are not orthodic (in fact one needs the extra condition that $\vec k = \vec 0$).

[8] there is a problem only if one insists in defining in the same way the notion of heat transfer in the two cases: this is a problem that Boltzmann does not even mention, possibly because he saw as obvious that the two notion would become equivalent in the thermodynamic limit.

Undoubtedly the word "holode" has the etymological origin of ὅλος and εἶδος while "ergode" is a shorthand for "ergomonode" and it has the etymological root of ἔργον and εἶδος, meaning a "monode with given energy", [G1]. [9]. The word "holode" is probably a shorthand for "holomonode", meaning a "global monode" (perhaps a monode involving states with arbitrary energy, *i.e.* spread over the whole phase space).

This is not what is usually believed to be the etymology of "ergode": the usual belief comes from the Erhenfests' statement that the etymology is ἔργον and ὁδός, with the meaning of "unique path on the surface of constant energy, see [EE] note #93. This absurd etymology has been taken up universally and has been attached to the subject of "ergodic theory", which is instead a theory dealing with time evolution properties.

§**2** *The ergodic hypothesis, continuous and discrete phase space.*

The etymological error of the Erhenfests could be just an amusing fact: but it had a rather deep negative influence in the development of the 20-th century Physics. They present their etymology in connection with the discussion (amounting to a *de facto* rejection) of the ergodic hypothesis of Boltzmann. In fact Boltzmann had come to the ergodic hyptothesis in his attempts to justify, *a priori*, that the ergode, as a model of thermodynamics, had to produce **the** thermodynamics of a system with the given hamiltonian function, (and not just a model).

Boltzmann had argued that the trajectory of any initial datum evolves on the surface of constant energy, visiting all phase space points and spending equal fractions of time in regions of equal Liouville measure.

The Ehrenfests criticize such a viewpoint on surprisingly abstract mathematical grounds: basically they say that one can attach to each different trajectory a different label, say a real number, thus constructing a function on phase space constant on trajectories. Such a function would of course have to have the same value on points on the same trajectory (*i.e.* it would be a constant of motion). This is stated in the note #74, p. 86 where the number of different paths is even "counted", and referred to in the note #94, p. 89. Therefore, they conclude, it is impossible that there is a single path on the surface of constant energy, *i.e.* the ergodic hypothesis is inconsistent (except for the monocyclic systems, for which it trivially holds).[10]

Having disposed of the ergodic hypothesis of Boltzmann, the Erhenfests proceed to formulate a new hypothesis, the rather obscure (and somewhat vague as no mention is made to the frequency of visit to regions in phase space) "quasi ergodic hypothesis", see notes #98 and #99, p.90, in [EE]: it led the physicists away from the subject and it inspired the mathematicians to find the appropriate definition giving birth to ergodic theory and to its first non trivial results.

The modern notion of ergodicity is not the quasi ergodicity of the Erhenfests. It is simply based on the remark that the Erhenfests had defined a non trivial constant of motion very abstractly, by using the axiom of choice. In fact from the definition, consisting in attaching a different number, or even $6N-2$ different numbers, to each distinct trajectory, there is *in principle* no way to construct a table of the values of the function defined in order to distinguish the different trajectories. In a system ergodic in the modern sense the Ehrenfests' construction would lead to a non measurable function; and to a physicist dowed with common sense *such a function, which in principle cannot be tabulated, should appear as non existent, or as non interesting.* Thus the motion on the energy surface is called ergodic if there are no *measurable* constants of

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[9] the word "ergode" appears for the first time on p. 132 of [B84]: but this must be a curious misprint as the concept is really introduced on p. 134. On p. 132 the Author probably meant to say "holode", instead: this has been correctly remarked by [Pl]. See also footnote 13. The above etymology was probably proposed for the first time by myself in various lectures in Roma, and it was included in the first section of [G1]. The date of the preprint of [G1] is june 1980, the publication date is 1981: a year later a reference to the same new etymology appears, see [Ja],[Ma], attributed to Mathieu. I find it obviously possible, even likely, that independently two scientists may reach the same conclusion: even with only a few years of delay. Nevertheless *no reference* is made to my book in the paper of 1988 by Mathieu, in [Ma]. In fact I gave a series of lectures in august 1979 in Cortona which were attended by prof. R. Nagel who had access to (and, as all the partecipants, a copy of) my manuscript [G1] already including the etymology section in its present form; he informed me in a subsequent letter that he had discussed the matter with his student Mathieu, sending me a manuscript by him on the subject.

[10] The abstract mathematical nature of this argument, see also below for a critique, was apparently remarked only by a mathematician, see [Pl] p. 86, althoug a great one (Borel, 1914); but it escaped many physicists. It is worrying to note how seriously the mathematicians took the ergodic hypothesis and how easily they disposed of it, taking for granted that the Ehrenfests formulation was the original formulation by Boltzmann and Maxwell, see [Br1], p. 383.

motion: here measurable is a mathematical notion which essentially states the possibility of a tabulation of the function.

It is surprising that a generation of physicists could be influenced (in believing that the ergodic hypothesis of Boltzmann had to be abandoned as a too naive viewpoint) by an argument of such an exquisitely abstract nature, resting on the properties of a function that could not be tabulated (and not even defined if one did not accept the sinister axiom of choice).[11]

Therefore it is worth, perhaps, to try understanding what could have possibly meant Boltzmann when he formulated the ergodic hypothesis. Here one cannot fully rely on published work, as the question was never really directly addressed by Boltzmann in a critical fashion (he might have thought, rightly, that what he was saying was clear enough). The following analysis is an elaboration of [G1], [G2]: in some points it gets quite close to [Pl]. It will not escape the reader that [Pl] has a somewhat different point of view on several key issues, although we seem to share the main thesis that the [EE] paper is responsible for most of the still persisting misunderstandings on Boltzmann's work. Including the exclusive attribution to Gibbs of Boltzmann's ideas on ensembles, so clearly elaborated in [B84].

My point of view is that of those who believe that Boltzmann always conceived the phase space as a discrete space, divided into small cells, see [B68]. He always stressed that the continuum must be understood as a limit, see [Br], p. 371, and [Kl1,2,3],[D]. The book of Dugas, [D], is particularly illuminating (also) on this respect (see for instance ch. 1 and the quotations of Boltzmann presented there, where he seems to identify the discrete viewpoint with the atomistic conceptions).

Although Boltzmann seems to have been, sometimes, quite apologetic about such a viewpoint (even calling it a "mathematical fiction", [Ba], p.18, from [B72]; see also [Pl], p. 75), he took advantage of it to a point that one can say that most of his arguments are based on a discrete conception of phase space, followed at the end by a passage to the continuum limit. It should be however understood that the discretization that Boltzmann had in mind is by no means to be identified with the later concept of coarse graining: see §4 where a modern version of Boltzmann's discretization is considered and where a distinction has to be made between cells and volume elements, see also [Pl] and [G3].

It is easier for us, by now used to numerical simulations, to grasp the meaning of a cell: in the numerical simulations a cell is nothing else but an element of the discrete set of points in phase space, each represented within computer precision (which is finite). One should always discuss how much the apparently harmless discreteness of the phase space affects the results. This is, however, almost never attempted: see [G3] for an attempt. A volume element has, instead, a size much larger than the machine resolution, so that it looks a continuum (for some purposes).

Hence one can say that an essential characteristics of Boltzmann's thought is to have regarded a system of N atoms, or molecules, as described by a *cell* of dimension δx and δp in each position and momentum coordinates. He always proceeded by regarding such quantities as very small, avoiding to enter into the analysis of their size, but every time this had some importance he must have regarded them as positive.

A proof of this is when he refutes the Zermelo's paradoxes by counting the number of cells of the energy surface of $1cm^3$ of normal hydrogen, [B96], a feat that can only be achieved if one considers the phase space as discrete.

In particular this point of view must have been taken when he formulated the ergodic hypothesis: in fact conceiving the energy surface as discrete makes it possible to assume that the motion on it is "ergodic", *i.e.* it visits *all* the phase space points, compatible with the given energy (and possibly with other "trivial" constants of motion) behaving as a monocyclic system (as all the motions are necessarily periodic).

The passage to the continuum limit, which seems to have never been made by Boltzmann, of such an assumption is of course extremely delicate, and it does not lead necessarily to the interpretation given by the Erhenfests. It can easily lead to other interpretations, among which the modern notion of ergodicity: but it should not be attempted here, as Boltzmann himself did not attempt it.

And in general one can hardly conceive that by studying the continuum problem could lead to really new information, that cannot be obtained by taking a discrete viewpoint. Of course some problems might still be easier if studied in the continuum, [S]: and the few results on ergodicity of physical systems do in fact rely explicitly on continuum models. However I interpret such results rather as illustrations of the complex nature of the discrete model: for instance the ergodicity theory of a system like a billiards is very enlightening as it allows us to get some ideas on the question of whether there exist other ergodic distributions (in the sense of ergodic theory) on the energy surface, and which is their meaning, [BSC].

[11] we recall, as it is quite an irony, the coincidence that the recognition and the development of the axiom of choice was due essentially to the same Zermelo who was one of the strongest opponents of Boltzmann ideas on irreversibility, see also [Sc].

And the theory of the continuum models has been essential in providing new insights in the description of non equilibrium phenomena, [R], [CELS].

Finally the fruitfulness of the discrete models can be even more appreciated if one notes that they have been the origin of the quantum theory of radiation: it can be even maintained that already Boltzmann had obtained the Bose Einstein statistics, [Ba].

The latter is a somewhat strong intepretation of the 1877 paper, [B77]. The most attentive readers of Boltzmann have, in fact, noted that in his discretizations he really thinks always in terms of the continuum limit as he does not discuss the two main "errors" that one commits in regarding a continuum formulation as an approximation (based on integrals instead of sums)[12] with respect to a discrete one.

The above "oversight" might simply be a proof that Boltzmann never took the discretization viewpoint to its extreme consequences. Among which there is that the equilibrium ensembles are *no longer* orthodic in the sense of Boltzmann (see [G3],[G4]), (although they still provide a model for thermodynamics provided the temperature is no longer identified with the average kinetic energy): a remark that very likely was not made by Boltzmann *in spite of his consideration and interest on the possibility of finding other integrating factors for the heat transfer dQ*, see the footnote on p. 152 in [B84].[13]

The necessity of an understanding of this "oversight" has been in particular clearly advocated by Kuhn referring to Boltzmann's "little studied views about the relation between the continuum and the discrete", [K], for instance.

§**3** *The ergodic hypothesis and irreversibility.*

The reaction of the scientific world to the ergodic hypothesis was, "on the average", a violently negative one, also as it was intended to provide further justification to the irreversibility predicted by the Boltzmann equation, derived earlier.

The great majority of the scientists saw absurd and paradoxical consequences of the hypothesis, without apparently giving any importance to the "unbelievable" fact that on the basis of a maximal simplicity assumption (*i.e.* only one cycle on the energy surface) Boltzmann was obtaining not only the possibility of explaining, mechanically, the classical equilibrium thermodynamics but also that of explaining it in a quantitative way. It allowed, for the first time, the theoretical calculation of the equations of state of many substances (at least in principle) like imperfect gases, and even other fluids and solids.

The success of the highly symbolic but very suggestive formula of Boltzmann, see [EE], p.25:

$$\lim_{T\to\infty}\frac{dt}{T}=\frac{\sigma\,ds}{\int\sigma\,ds} \tag{3.1}$$

(where σ is the microcanonical density on the energy surface, whose area element is ds) in the calculation of the equilibrium properties of matter led quickly the physicists to accept it in the "minimal interpretation". Such interpretation demanded that the r.h.s. be used to compute the equilibrium averages and the l.h.s. ignored, togheter with the atomic hypothesis. This is regarded as a *law of nature*, in spite of the persistent skepticism (or deep doubts) on its deducibility from the laws of mechanics. A point of view usually attributed to Gibbs, referring to [G], and which is still around us, although we assit, since the mid fifties, to a slow but inexorable inversion of tendency.

Immediately after the first critiques Boltzmann elaborated answers often very clear and simple by our modern understanding: but they were very frequently ill understood not only by the opponents of Boltzmann and their epigones, but also by those who were closest to him. The above quoted critique to the ergodic hypothesis by the Erhenfests is a shocking example.

Another example is the recurrence paradox, based on the simple theorem of Poincaré. Boltzmann was finally led to the calculation of the number of cells on the energy surface, [B96], thus to a superastronomical estimate of the recurrence time: which, nevertheless, did not seem to impress many.

It is also clear that Boltzmann himself became aware of the fact that, after all, the ergodic hypothesis might have been unnecessarily strong and perhaps even useless to explain the approach to equilibrium in

[12] and which amount to the identification of the Maxwell Boltzmann statistics and the Bose Einstein statistics, and to neglecting the variation of physically relevant quantities over the cells: see the lucid analysis in [K], p.60; for a technical discussion see [G3],[G4].

[13] I have profited, in checking my understanding of the original paper as partially exposed in [G1], from an english translation that Dr. J. Renn kindly provided, while being ny student in Roma (1984). I could note this footnote in [B94], and insert a few new remarks in the present paper, because of his translation, (unfortunately still unpublished).

physical systems. The latter in fact reach equilibrium, normally, within times which are microscopic times, not at all comparable with the recurrence time. He asserted repeatedly that the (very few) macroscopic observables of interest had essentially the *same* value in most of the energy surface, and the time spent in the "anomalous phase space cells" is therefore extremely small: a quantitative understanding of this is provided by the Boltzmann equation. This remark also frees (3.1) from the ergodic hypothesis: it might well be that the r.h.s can be used to evaluate the average values, in equilibrium, of the few observables which are of interest, although there might be observables (*i.e.* functions on phase space) for which the (3.1) fails.

It is well known that Boltzmann went quite far in this direction, by providing us with a concrete method to estimate the true times of approach to equilibrium: the Boltzmann's equation (historically developed well before the 80's).

Finally it is worth noting that the methods used by By Boltzmann in deriving the theory of the ensembles and the ergodic hypothesis are quite modern and in fact are most suited to illustrate the new developments on non equilibrium theory: as I shall try to prove in the next section.

§**4** *Non equilibrium. Ruelle's principle. Outlook.*

I cannot resist the temptation of at least mentioning some recent new developments which look to me exciting and very likely to remain as important progress in the field.[14]

The (3.1), in its minimal interpretation of providing, via the r.h.s. (*i.e.* the microcanonical distribution), the law for the evaluation of the "relevant" macroscopic observables, starting from the energy function of the system, "solves" the problem of the equilibrium theory. Completely, as far as we know (in Classical Physics).

Is a similar theory possible for systems in non equilibrium, but in a stationary state? What (if anything) replaces the microcanonical distribution in such cases? As an example of "cases" we mean the motion of a gas of particles subject to a constant force ("electric field") setting them in motion, while the energy produced is dissipated into a reservoir.

The answer seems affirmative, at least in some cases. The problem lies in the fact that the motion of such systems is dissipative, hence the volume element of the energy surface is not conserved even in the simple case in which the thermostat is such that it keeps the total energy of the system constant (as I shall suppose, to simplify the discussion), *i.e.* the microcanonical distribution cannot describe the stationary state. Taking the continuum viewpoint we can imagine that the motion is essentially concentrated, after a transient time, on a set A which has zero measure with respect to the Liouville measure on the energy surface.

To avoid giving the impression that the discussion is abstract (hence possibly empty) let me declare explicitly one, among many, models that one should have in mind. We consider a system of N particles interacting with a potential energy Φ and subject to an external constant force field $\vec{E}$, (*e.g.* electric field):

$$\dot{\vec{q}}_i = \frac{1}{m}\,\vec{p}_i, \qquad \dot{\vec{p}}_i = -\partial_{\vec{q}_i}\Phi + \vec{E} - \alpha(\vec{p})\,\vec{p}_i \tag{4.1}$$

where $\vec{E}$ is the external constant force and α is defined so that the energy $\sum_{i=1}^N \frac{\vec{p}_i^{\,2}}{2m} + \Phi$ is constant (*i.e.* $\alpha = \frac{\vec{E}\cdot\sum\vec{p}_i}{\sum\vec{p}_i^{\,2}}$). The term $\alpha\,\vec{p}_i$ is a model of a thermostat (this should be called a *gaussian thermostat* as it is related to the Gauss' principle of "least constraint", see [CELS]). The system is considered enclosed in a box with periodic boundary conditions: hence we expect that a current parallel to $\vec{E}$ will be established and the system will reach a stationary state. The volume in phase space contracts at a rate $(3N-1)\alpha$, (which is positive, in the average): hence the motion will asymptotically develop on some "attractor", which is a set of 0 Liouville measure.

What follows will lead to a unified theory of the equilibrium as well as the non equilibrium, for system (4.1).

The discrete viewpoint is also possible: the enegy surface consists of cells which are relevant (for the study of the asymptotic properties) forming a set A in phase space, and of cells which are irrelevant. The motion can be regarded to develop on the set of cells which are in A, which is strictly smaller than the set of all the cells: in fact far smaller (and in the continuum limit the fraction of cells in A approaches 0).

Since the volume of the cells is not conserved care must be exercised in regarding the dynamics as a permutation of the cells of A. This is in fact also true in the equilibrium case because, even if the cells do not change in volume, they are deformed being squeezed in some directions and dilated in others. In

[14] I like to think that Boltzmann his listening to the celebration of his birthday: he would certainly be bored by hearing a, presumably poor, exposition dealing only with things that he knew far better.

equilibrium it is possible to conceive situations in which the deformation can be neglected (this leads to restrictions on the region of temperature and density in which the consideration of the dynamics as a cell permutation is acceptable: a discussion which we have not begun above and which we avoid here as well, see [G3] for a quantitative analysis). And a similar analysis can be carried in the present case.

Basically one has to think that the system is observed at time intervals τ_0 which are not too small (so that something really happens) and not too large (so that the cell's deformations can be either neglected or controlled, at least for a large majority of cells): see [G3] for a quantitative analysis of what this means in the equilibrium cases and of when this might lead to inconsistencies. Let S_{τ_0} denote the transformation of A describing the dynamics on A over the time τ_0. By making the cells small enough we can take τ_0 larger.

We shall imagine the set A as a surface in phase space of dimension roughly $\frac{6N}{2}$ at least if the external force is small (so that the friction α, *i.e.* the phase space volume contraction, is also small): in fact if there is no external force the dimension of A should be $1+\frac{6N-2}{2}$.[15] The surface A can fold itself on the energy surface filling it up completely (in the $\vec{E}=\vec{0}$ case) or not (in the general case).[16] We can assume the following extension of the ergodic hypothesis: *on A the dynamics is a one cycle permutation of the cells.*

Then the motion of a randomly chosen initial datum, randomly with respect to a distribution with some density on the energy surface, will simply consist in a fast approach to the surface A; at the same time data which are on A itself and close to each other will separate from each other at some exponential rate, because on A all the directions are dilated, by definition. To fix the ideas we take the initial data with constant density in some little ball U. If we assume, for simplicity, the above ergodic hypothesis, the layer is, over times multiples of the recurrence time, a set of cells each visited with equal frequency. However the surface A will, in general, not be a monolayer of cells but it will have a large "width", *i.e.* a (macrospcopic) area element $d\sigma$ will contain many (microscopic) cells. [17]

The number of cells per unit area can be deduced by remarking that after a time $\tau=M\tau_0$ the density of cells around $x\in A$, initially distributed with constant density in the region U (where the initial data are randomly chosen), has to be proportional to the inverse of the area expansion rate of the transformation S_τ. This means that we expect that the distribution on A which has to be used to compute the stationary averages is described by a suitable density with respect to the area element on A.

With this intuitive picture in mind, [R], ECM2], we see that a little ball U in phase space evolves becoming a thin layer around A: the density of the layer, after a large time T, is proportional to the expansion rate of the surface area on A under the transformation S_T generating the time evolution over the given time.

In the case of no external forces one has that the surface A folds itself on the energy surface coming back to a given phase space volume element V_0 (not to be confused with a cell, which has to be thought as much smaller); just enough times, and with enough volume around, so that the fraction of the volume initially in U and falling in the volume element V_0 is proportional to V_0 itself (this is consistent because of the equality of the total expansion rate and the total contraction rate, due to the hamiltonian nature of the equations of motion). But in general the fraction of volume U falling into a volume element will be far different from the volume element fraction of the energy surface.

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[15] because there are as many contracting directions as expanding ones (the volume being conserved in the $6N$ dimensional phase space); and there are two "neutral" directions (the direction orthogonal to the energy surface and the direction of the phase space motion) one of which lies on the energy surface (the direction of motion), see [Dr], [ECM1], [SEM]. Of course the existence of other conserved quantities, as in (4.1) when the linear momentum is conserved, affects this calculation: in (4.1), when $\vec{E}=\vec{0}$, this brings down the dimension to $1+\frac{6N-8}{2}$. Furthermore we are assuming here that there are no "neutral" directions other than the ones possibly provided by the obvious conservation laws: *i.e.* that our system has strong instability properties (hence this does not *directly* apply to the free gas, for instance.

[16] in the continuum point of view we can proceed as follows: we fix an approximation ε and we identify the points on A which are very far on any path that joins them *along* A, but which are close within ε as points on the energy surface. Then A becomes a finite surface A_ε. This surface depends on the point that we initially choose for the construction: but the results should be independent on the choice. The latter is in fact an assumption which essentially replaces the ergodicity assumption of the conservative cases. The above "viewpoint" will imply ergodicity in the case of the conservative systems: this non trivial fact is a consequence of the hidden assumption that the description does not depend on which surface A_ε we choose as an approximation for A. In fact the choice of A_ε suffers from an arbitrariness which consists in deciding that one given point is actually on A_ε: choosing another point leads, in general, to a different A_ε. In concrete cases it will, however, be very difficult to show that the results are independent on A_ε (a manifestation of the conservation of difficulties).

[17] this can perhaps be clarified if one thinks of the numerical experiments in which the computer representatives of the phase space points are regarded as cells, while the unstable manifolds of the motion are regarded as surfaces built with computer points, *i.e.* cells.

One is thus led to the following unified "principle" to describe the stationary states of non equilibrium systems, [R]:

Principle: *the average values of the observables in the stationary state describing the asymptotic behaviour of systems like (4.1), is computable from a probability distribution on A which has a density, with respect to the surface element of A.*[18]

This principle can be more mathematically stated (a problem into which we refrain to enter here), and is due to Ruelle, [R], who based himself also on the results of Anosov, Sinai, Bowen on the theory of a class of dynamical systems known as "hyperbolic systems" (which play in some sense, for non equilibrium statistical mechanics, the role of the monocyclic systems of Helmholtz). The probability distributions selected by the above principle (which in "good cases" is unique) are called SRB measure, [R].

What is the predictive value of the above statements? in the cases without external forces we have already mentioned that this principle leads to the microcanonical distribution and, therefore, implies the classical thermodynamics, [B84]. Life is made easy by the fact that although A may be very difficult to identify, still the stationary distribution is just the microcanonical ensemble because A folds on the energy surface filling it up completely, with no gaps.

In the dissipative cases it seems that we have little control on A and hence on the stationary distribution.

Yet this might not be really so: we simply have to learn how to extract informations from such an abstract principle. After all it now seems natural that the Gibbs distribution predicts all the phenomena of equilibrium statistical mechanics (from the phase coexistence, to the critical point, to cristallization). But this was far from clear only a few decades ago, and many decades after the original formulations of Maxwell, Gibbs and Boltzmann (as many of us certainly recall).

That the principle might have predictive value is indicated by the first attempts at its use in problems of statistical mechanics, see [ECM2], (see also [CELS]), who were somewhat inspired by previous papers, see also [HHP]. In fact only recently the principle started being considered in the theory of non equilibrium, as it was developed originally by Ruelle mainly as an attempt to a theory of turbulent phenomena. This is not the appropriate place to discuss the paper [ECM2] in the perspective of the above principle: the discussion is rather delicate (as [ECM2] should be regarded as a pioneering work).

A simpler example of a quantitative (yet quite abstract) consequence of the above principle is the determination of the density function mentioned in the principle: the latter is in fact essentially determined. If we are interested in stationary distributions phenomena which are observable by measurements that take place in a fixed time τ we can just take averages over A with respect to a distribution with density over A proportional to $\Lambda^{-1}_{\tau'}(x)$, with $\tau'=M'\tau_0\gg\tau$ (where the expansion rate is the jacobian determinant of the transformation $S_{\tau'}$ at x, *i.e.* $\Lambda^{-1}_{\tau'}(x)\equiv\prod_{-M'}^{M'}\Lambda^{-1}_{\tau_0}(S^j_{\tau_0}x)$). So that two equal area elements of A around x and y have a *relative probability* of visit equal to $\Lambda^{-1}_{\tau'}(x)/\Lambda^{-1}_{\tau'}(y)$.

Of course τ' cannot be taken too large: if τ' is taken of the order of the recurrence time the ratio becomes 1. The natural upper bound on τ' has to be such that the cells in U ending in the considered area elements are still in a large number. This sets an upper limit to the values of τ for which the above remark applies.[19]

The example (4.1) is very special.[20]

It is however generalizable: many generalizations have already been considered in the literature, [PH]. Still it should be stressed that the models to which the above principle can be applied form a rather small class of deterministic models. It is not immediately clear how it can be applied to stationary non equilibrium phenomena in which the thermostat is realized in a different way, *e.g.* by some stochastic boundary conditions. Nor it is obvious that the different thermostats are physically equivalent.

In my opinion there is, also, some misunderstanding in the literature about the fact that the set A has zero measure (in the non equilibrium cases this has been sometimes associated with the questions related

[18] It is extremely important to think, to avoid trivial contradictions, that the cells on A must be regarded as much smaller than the surface elements of A that we consider in talking about the density.

[19] this means that the ratio between the linear dimension of U and the linear dimension of the cells has to be large compared to the maximal linear expansion rate over the time τ, a condition that can be expressed in terms of the largest Lyapunov exponent.

[20] this is shown also by the fact that the operation i mapping $x=(\vec p,\vec q)$ to $ix=(-\vec p,\vec q)$ is such that $t\to ix(-t)$ is a solution of the equation of motion if $t\to x(t)$ is such: a time reversal symmetry. This has several implications, among which the properties that both initial data x and ix evolve towards the same attractor A, in the future, and to the attractor iA in the past. In general A and iA are different, except in the case $\vec E=\vec 0$ (because A is the full energy surface).

to irreversibility) and about the fact that A, regarded as a folded surface on the equal energy manifold, has a fractal dimension (thereby representing a "strange attractor"). Such facts may be quite misleading. The above analysis shows that A should be more conveniently regarded as a smooth non fractal surface of dimension about $6N/2$: its fractal dimension arises from the folding of A on the surface of constant energy (rising from $6N/2$ to about $6N$ if $\vec{E}$ is small).

Furthermore in the assumption that the stochastic thermostats and the gaussian thermostat (or other thermostats, [PH]) are equivalent one sees clearly a problem related to attaching importance to the set A as a fractal with zero measure. In fact we expect that stochastic thermostats lead to stationary distributions which have a density in phase space, hence which cannot be concentrated on a set of 0 measure.

The contradiction disappears if one thinks that, in a stationary state, there may be several distributions which, in the limit as $N \to \infty$, become equivalent. A distribution concentrated on a set of zero measure might well be equivalent to one distributed on the whole energy surface, or on the whole phase space. A much simpler, but very familiar, example of such a situation is provided by the microcanonical distribution which is concentrated on a set of zero measure, but it is equivalent (in the thermodynamic limit) to the canonical distribution, which is concentrated on the whole phase space.

Finally it should be clear that the problem of approach to stationarity will show up exactly in the same terms as in the equilibrium cases. The "ergodicity" assumptions above cannot in any way justify the use of the distribution verifying the Ruelle principle: the time necessary for a phase space point to visit the full set of cells building A will be of the order of magnitude of the recurrence time. And as in the equilibrium cases we can expect that the rapidity of the approach to equilibrium is rather due to the fact that we are interested only in very few observables, and such observables have the same value in most of phase space.

I hope to have shown, or at least given arguments, that the point of view, see for instance [Pl], whereby Boltzmann was a XIX century physicist judged by his interpreters with XX century mathematical standards is not exactly correct: today's way of thinking is not too different from his and most problems the physicists had with his work were problems with the understanding of his Physics and *not* of his Mathematics, see also [L]. The misunderstandings about his ideas are, in my opinion, largely due to the unwillingness of studying the original publications and to the unfounded belief that they were forwarded with fidelity by the reviewers that wrote about his achievements.

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