Ergodic and chaotic hypotheses: nonequilibrium ensembles in statistical mechanics and turbulence

Giovanni Gallavotti
Fisica, Università di Roma "La Sapienza"
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The ergodic hypothesis outgrew from the ancient conception of motion as periodic or quasi periodic. It did cause a revision of our views of motion, particularly through Boltzmann and Poincaré: we discuss how Boltmann's conception of motion is still very modern and how it can provide ideas and methods to study the problem of nonequilibrium in mechanics and in fluids. This leads to the chaotic hypothesis, a recent interpretation of a very ambitious principle conceived by D. Ruelle: it is a possible extension of the ergodic hypothesis and it implies general parameterless relations. Together with further ideas, it appears to be consistent with some recent experiments as we discuss here.

I. ERGODIC HYPOTHESIS

Since Galileo's *"Philosophy is written in this great book which is continuously open before our eyes"*, 377 years ago [Ga65], p.38, we deciphered a few more pages of the great book, beyond the ones that had already been read in the 3000 previous years. The substantial conceptual identity between the problems met in the theoretical study of physical phenomena is absolutely unexpected and surprising, whether one studies equilibrium statistical mechanics, or quantum field theory, or solid state physics, or celestial mechanics, harmonic analysis, elasticity, general relativity or fluid mechanics and chaos in turbulence. I discuss here a few aspects of the developments of the theory of chaos as a paradigm of the stability of our processes of understanding natural science.

In the Renaissance Copernicus started anew the theoretical foundations of astronomy: since Ptolemy the technical ability to understand the "world", *i.e.* the motion of the planets, had been essentially lost. With Copernicus comes the rediscovery of the technical meaning of the Greek conception of motion as generated by many *uniform circular motions*: in his youth he undertakes to improve Ptolemy's great work by restoring the simplicity of the Aristotelian conception that he thought Ptolemy had betrayed.[1]

At the end of his work he left us a system of the world apparently more orderly than that of Ptolemy, if not more precise. I say apparently because it seems to me that Ptolemy's *Almagest* is more an astronomical almanac than a book in which the theory of celestial motions is discussed. It would be difficult, if not impossible, to extract from the modern *Astronautical Almanac*, [AA89], informations about the three body problem: it is not impossible that we simply ignore, as Copernicus did, the theory at the base of the compilation of the Almagest whose "*explanation did not seem sufficiently complete nor sufficiently conform to a rational criterion*", [Co30] p.108. Often ancient science has been misinterpreted because its original purpose had been forgotten or had become corrupted, [Ru98].

I would say that Copernicus' contribution, far greater than "just" setting the Earth aside by the second postulate of his *Commentariolus*, [Co30], was to show how a consistent "system of the world" could be developed from scratch (*i.e.* from raw observations): the method that he followed generated a systematic rethinking of the structure of the "world" (in this case the system of the planets) which led or at least guided the works of Galileo, Kepler and many others until the Newtonian synthesis was achieved, whose all encompassing power is expressed in the work of Laplace. With Laplace's work the Greek conception of motion had again become very clear and understood. With the addition of methods to *deduce* cycles and epicycles starting from very simple first principles (the law of gravitation): the enthusiasm of the new scientists was so overwhelming that the classical names became (and remain) obsolete with, for instance, the epicycles and deferents becoming the austere Fourier modes that could be read from the tables of Le Verrier.

It was at this moment of triumph of the orderly and simple motion by cycles, deferents and epicycles, with the inebriating sense of power that must have been felt when (for instance) the periods of the Moon became (easily) computable from first principles, that the atomic hypothesis started being investigated beyond its first steps. Boltzmann's attempts to derive thermodynamics from mechanics and the atomic hypothesis really began under-

[1] "*Nevertheless, what Ptolemy and several others legated to us about such questions, although mathematically acceptable, did not seem not to give rise to doubts and difficulties*" ... "*So that such an explanation did not seem sufficiently complete nor sufficiently conform to a rational criterion*" ... "*Having realized this, I often meditated whether, by chance, it would be possible to find a more rational system of circles with which it would be possible to explain every apparent diversity; circles, of course, moved on themselves with a uniform motion*", [Co30], p.108.

mining the conceptions of motions that all scientists had maintained for millennia, with relatively minor changes. At the beginning all seemed to indicate just a new addition to the old Aristotelian views: in his early papers Boltzmann is perhaps somewhat uneasy with nonperiodic motions: he prefers to think of a nonperiodic motion as of "a periodic motion with infinite period", [Bo66].[2]

And Boltzmann's mechanical definition of entropy addresses the conflicting notions of "measure of disorder" on the one hand, and of a property of systems whose motions are very ordered, necessarily "periodic". His fundamental work, [Bo84], where he lays down the theory of statistical ensembles, in a form that is astoundingly modern and almost identical to the one we use today, associates entropy with the mechanical properties of periodic motions: he even starts the paper by showing that one can associate a function "with the properties of the entropy" to the motion of a Saturn ring, regarded as a rigidly rotating circle (which is so unusual an example that it is likely to be the reason why such a fundamental paper has been little noted).

But, as Boltzmann himself had to argue against the objections of Zermelo, [Ce99], nothing could be less ordered that the motions to which he was trying to attach a quantity to be identified with entropy, which also appeared to play a rather different role in his theory of approach to equilibrium via the Boltzmann equation.

These were the years when Poincaré had noted the recurrence theorem, from which some wanted to derive the proof of the alleged inconsistency of the atomic hypothesis, [Ce99], viewing matter as an assembly of particles obeying Newton's equations, because of its conflict with macroscopic thermodynamics. At the same time Poincaré had for the first time given incontrovertible evidence that planetary motions *could not always be explained* in terms of cycles and epicycles (as Laplace theory of the world hinted): I refer here to his theorem, [Po87], "of nonintegrability" of the three body problem. The inescapable consequence, of which Poincaré was well aware, was that not all motions could be quasi periodic, *i.e.* compositions of circular motions.

Nevertheless one of the achievements of Boltzmann was the *heat theorem*: to a system endowed only with periodic motions, a *monocyclic system* after Helmoltz, [Bo84], one could associate *mechanical* quantities, that could be given a name familiar from macroscopic physics, like "temperature" T, "energy" U, "volume" V, "pressure" p, so that by changing infinitesimally the parameters describing the system the consequent changes dU and dV of U and V would be such that

$$\frac{dU + pdV}{T} = \text{ exact differential} \tag{1}$$

which can be used to define entropy as the integral of the exact differential: and eq. (1) is the analytic form of the second law of thermodynamics in equilibrium. In the late work of Boltzmann, [Bo84], where it is proved in maximum generality, this theorem appears as a consequence of his *ergodic hypothesis*: an hypothesis that has a double nature. On the one hand it is usually interpreted as saying that the motion (in phase space) is rather random; on the other hand it rests on an essential idea of Boltzmann, that in fact phase space can be regarded as discrete (basically because we cannot suppose that the world is a continuum: see ([Bo74]), p. 169,

Therefore if we wish to get a picture of the continuum in words, we first have to imagine a large, but finite number of particles with certain properties and investigate the behavior of the ensemble of such particles. Certain properties of the ensemble may approach a definite limit as we allow the number of particles ever more to increase and their size ever more to decrease. Of these properties one can then assert that they apply to a continuum, and in my opinion this is the only non-contradictory definition of a continuum with certain properties.

A similar view was held for the phase space in which atoms are described, [Ce99], [Ga95].

If phase space is regarded as discrete then every motion is a permutation of its discrete points, called "cells", hence it *must* be periodic and it is then reasonable that it is just a one cycle permutation of the cells on the energy surface. Thus Boltzmann hypothesizes that motion, viewed as a permutation of cells with the same energy, has one cycle, *i.e.* that every cell visits all the others before returning to itself.

Hence we see the duality mentioned above: to derive thermodynamics we assume that the motion is periodic, but at the same time such that the motion of the system is so irregular to fill the whole energy surface. It is not surprising that many scientists were shocked by arguments and theories built on apparently conflicting assumptions: they disregarded Boltzmann's discrete approach and, identifying cells with points of a *continuous* energy surface, pointed out the mathematical inconsistency of the ergodic hypothesis strictly interpreted as saying that a point representing the system in phase space wanders around passing eventually through *every* point of the energy surface (quite absurd in general, indeed), see p. 22 and notes 98, 99 at p. 90 in [EE11].

A key point that is often overlooked is that the relation (1) is a property that holds for arbitrary mechanical systems of identical particles no matter how small or large they are, [Bo84] and Ch. I of [Ga99b]. Only for assemblies of atoms that can be considered to form a macroscopic system the quantities U, T, V, p acquire the

[2] See p. 30: "... *das man die bahnen, falls sie in keiner endlichen Zeit geschlossen sind, doch in einer unendlichen Zeit als geschlossen ansehen darf.*" (!) In fact if one looks at the context in which the above statement is made one realizes that it is completely justified and that Boltzmann never really changed his mind in the later work.

interpretation that is suggested by our familiarity with their names, and therefore only for such systems eq. (1) can be regarded as the second law of equilibrium thermodynamics, [Bo84]. A "trivial" general identity can then be interpreted as a very important law of nature, [Ga99b].

The long discussions on the matter, led initially by Boltzmann who began to explain to ears unwilling to listen why there was no contradiction in his discoveries, continued until today with every new generation bringing up the same old objections against Boltzmann's theories often (though not always, of course) still refusing to listen to the explanations (for a modern discussion of Boltmann's views see [Le93], [Ga95]).

But one can say that after Boltzmann there was no substantial progress, at least no better understanding was gained on the foundations of Statistical Mechanics other than the theorem of Lanford proving Grad's conjecture on the possibility of deriving rigorously the Boltzmann equation from a microscopically reversible dynamics, [La74] (a result which unfortunately does not seem to be as well known as it should).

The achievement of Boltzmann was to have proposed a *general assumption* from which one could derive the prescription for studying properties of large assemblies of particles (I refer here to the ergodic hypothesis and, [Bo84], to the microcanonical and canonical ensembles theory) and that had immediately proved fruitful through its prediction of the second law, (1). This remained an isolated landmark while interest concentrated on the derivation of further consequences of the new theory: namely to understand phase transitions and their critical points, or the basic quantum statistical phenomena: black body radiation, superconductivity or superfluidity are, perhaps, the clearest examples.

Also the parallel efforts to understand phenomena out of equilibrium were far less successful. Yet in a sense such phenomena too *must be understood*, not only because of their obvious interest for the applications, which most often deal with systems in stationary nonequilibrium states, like a turbulent flow of a liquid in a pipe or a stationary current kept in a circuit by an electromotive force, but also because their understanding promises to bring light on the mentioned duality between *orderly motions*, *i.e.* periodic or quasi periodic, and *chaotic motions*, as we now call motions that are *neither* periodic *nor* quasi periodic.

It is not until the 1960's, under the powerful solicitation of new experimental techniques and the rapid growth of digital computers, that the problem began to be attacked. Existence of chaotic motions became known and obvious even to those who had no familiarity with the work of Poincaré and with the results, [Si77], of Hopf, Birkhoff, Anosov and more recently of Kolmogorov, Sinai and many others that developed them further.

Works on chaotic motions started to accumulate until their number really "exploded" in the 1970's and it continued to grow rapidly, since. The goal of the research, or at least one of the main goals, was to understand how to classify chaotic phenomena whose existence had become known and visible to the (scientific and not scientific) general public which seemed quite surprised for not having noted them before. Perhaps the main aim was to find out whether there was any extension to nonequilibrium systems of the statistical ensembles that were at the basis of the applications of equilibrium statistical mechanics.

The problem has two aspects which initially seemed uncorrelated: indeed chaotic motions arise both in many particles systems typical of statistical mechanics and in fluids (and in other fields not considered here, for lack of space).

It is in the theory of fluids that the last attempt to an Aristotelian interpretation of motion had survived to these days. The book of Landau and Lifschitz, [LL71], presents a remarkable theory of fluid turbulence based on quasi periodic motions: basically a fluid in a container of fixed geometry put in motion by external constant (non conservative) forces would settle in a stationary state which would look at first, under weak forcing, static ("laminar"), then periodic (in Greek terms it would be described by "one epicycle") then quasi periodic with two periods (in Greek terms it would be described by "two epicycles") then periodic with three periods ("three epicycles") and so on until the number of epicycles had grown so large and, hence, the motion so complex to deserve the name of "turbulent".[3]

Through the work of Lorenz, [Lo63], and of Ruelle–Takens, [RT71a], it became clear that the quasi periodic view of the onset of turbulence was untenable: a conclusion which also several Russian scientists had apparently reached, [RT71b], independently.

The works making use of the new point of view stem also, and mainly, from the innovative ideas that Ruelle later wrote or simply exposed in lectures. There he developed and strongly stressed that the mathematical theory of dynamical systems, as developed in this century, would be relevant and in fact it would be the natural framework for the understanding of chaotic phenomena.

The impact on experimental works was profound. Already the very fact, [RT71a], that a study of the onset of turbulence could be physically interesting had been new at the time (the 1960's and early 1970's). And one

[3]Unfortunately the quoted chapter on turbulence has been removed from the more recent editions of the book and replaced by a chapter based on the new ideas: a choice perhaps useful from the commercial viewpoint but quite criticizable from a philological viewpoint. Of course keeping the original version and adding the new one as a comment to it would have been more expensive: a saving that might generate a lot of work a thousand years from now and that continues a long tradition which makes us wonder even what Euclid really wrote and what might have been added or changed later.

can say that after the first checks were performed, some by skeptical experimentalists, and produced the expected results a stage had been achieved in which the "onset of turbulence" was so well understood that experiments dedicated to check the "Ruelle–Takens" ideas on the onset of turbulence were no longer worth being performed because one would know what the result would be.

In this respect, before proceeding to the (developed) turbulence problems we stress that there remains still a lot to be done: the phenomena appearing at the onset of turbulence are in a sense too fine and detailed, and besides telling us that motions can be far more complex than one would have imagined a priori they give us little perspective on the theory of developed turbulence, admittedly more difficult.

Understanding the onset of turbulence is perhaps analogous to understanding the atomic system and classifying the spectra. The variety of the atomic spectra is enormous and its classification led quite naturally to quantum theory: but in itself it is of little help in understanding the mechanical properties of gases or of conducting metals, for instance.

Likewise we should expect that the analysis of the onset of turbulence will eventually lead to a more fundamental understanding of how the basic chaotic motions (that appear in a, so far, imperscrutable way at the onset of turbulence) are in fact predictable on the basis of some general theory: we have many experiments and a wide corpus of phenomena that have been studied and recorded and the situation is similar to the one at the beginning of the century with the atomic observations. *We see a few types* of "bifurcations", *i.e.* changes in the stationary behavior of systems, that develop in many different systems, as the strength of forcing is increased, but we do not know how to predict the order in which the different bifurcations arise and why they do so.

In a way it is deceiving that this understanding has not yet been achieved: this is certainly a goal that we should have in mind and that perhaps will be attained in a reasonable time in view of its practical importance. But we cannot expect that the solution, much desired as it is by all, can by itself solve the problems that we expect to meet when we study the stationary behavior of a *strongly turbulent* fluid or a gas of particles out of equilibrium. Much as understanding the two body problem gives us little *direct* information on the behavior of assemblies of 10^{19} particles (corresponding to $1\,cm^3$ of Hydrogen in normal conditions).

In the light of the above considerations it is important to note that Ruelle's view, besides reviving the interest in Lorenz' work which had not been appreciated as it should have been, was noticed by physicists and mathematicians alike, and had a strong impact, because it was *general and ambitious in scope* being aimed at understanding from a fundamental viewpoint a fundamental problem.

In 1973 he proposed that the probability distributions describing turbulence be what are now called "Sinai-Ruelle-Bowen" distributions. This was developed in a sequence of many technical papers and conferences and written explicitly only later in 1978, [Ru78], see also [Ru99]. It had impact mostly on numerical works, but it proposes a fundamental solution to the above outstanding theoretical question: what is the analog of the Boltzmann–Gibbs distribution in non equilibrium statistical mechanics? his answer is a general one valid for chaotic systems, be them gases of atoms described by Newton's laws or fluids described by Navier–Stokes equations (or other fluid dynamics equations).

It is not simple to derive predictions from the new principle which, in a sense, is really a natural extension of Boltzmann's ergodic hypothesis. Systems under nonconservative forcing must be subject also to forces that take out the energy provided to the system by the external forces: otherwise a stationary state cannot be reached. Assuming that the forces are deterministic the equations of motion *must be dissipative*: this means that the divergence of the equations must have a negative time average and, therefore, the statistics of the stationary state will be concentrated on a set of *zero volume* in phase space. In other words the motions will evolve towards an *attractor* which has zero volume.

It is precisely the fact that the attractor has zero volume that makes it difficult to study it: we are not used to think that such singular objects may have a physical relevance.

However from a point of view similar to the discrete viewpoint of Boltzmann such a situation is *not really different* from that of a system in equilibrium. One has to think of the attractor as a discrete set of points and of the dynamics as a permutation of them which has only one cycle. Then of course the stationary state will be *identified with the uniform distribution on the attractor*, giving equal probability to each of its points.

The difficulty is that we do not know where the attractor is. In equilibrium the problem did not arise: because the attractor was simply the entire surface of constant energy.

In the next section I discuss from a more technical viewpoint the meaning of the principle arguing that it is a natural and deep extension of the ergodic hypothesis. I will then analyze the potentialities of the hypothesis that, in a form slightly broader than the original, I will call *chaotic hypothesis*, following [GC95], by showing (§4) that it is capable of yielding general universal results, *i.e.* "parameterless laws", and perhaps even to shed some light (§3) on the very controversial question: "what is the proper extension of the notion of entropy to nonequilibrium systems?". In §5 I discuss the notion of dynamical statistical ensembles and the possibility of equivalence between time reversible and time irreversible dynamics in "large" systems. In §6 I attempt at an application of the ideas in §5 to the interpretation of an experiment and in §7 I collect a few conclusions and comments. I try to avoid technicalities, yet I try not to hide the problems, which means that for instance in §6 I must refer to some equations that might be not familiar to the reader. The references should help the readers interested in a more technical understanding.

II. CHAOTIC SYSTEMS FROM A TECHNICAL VIEWPOINT.

Observing motion at steps over short time intervals is a natural way to study time evolution: indeed this is what is normally done in experiments where observations are always timed in coincidence with some event of special interest (*e.g.* with the passage of a clock arm though the "12" position or, more wisely, with the realization of some event characteristic of the phenomenon under investigation, *e.g.* a collision between two particles if the system is a system of balls in a vessel).

Any simulation represents phase space as discrete and time evolution as a map S over the discrete points. It is assumed that the small size of the cells (of the order of the machine precision) is so small that errors, due to the fact that the size is not strictly zero, do manifest themselves over time scales that are negligible with respect to the ones over which the phenomena of interest naturally take place. Following Boltzmann we shall take the same viewpoint even when considering real (*i.e.* not simulated) systems and we shall suppose the phase space to consist of a discrete set of points, also called "cells".

We consider a "chaotic system" under external forcing and subject to suitable "thermostats", *i.e.* forces that forbid unlimited transformation into unreleased "heat" (kinetic energy) of the work performed on the system by the external forces, so that the system *can* reach a stationary state (*i.e.* does not "boil out of sight"). The evolution will then be described by a map S of the discrete phase space.

The map S will not, however, be in general a permutation of cells. Because the effect of the thermostating forces will be that dynamics will be effectively dissipative, *i.e.* the divergence of the equations of motion will not vanish and will have a negative average (unless the system is conservative and therefore the thermostating forces vanish). Hence a small ball U in phase space will evolve in time becoming a set S^TU at time T which has a much smaller volume than the original U and in fact has a volume that tends to 0 as $T \to \infty$, usually exponentially fast.

As mentioned in §1 we must understand better the structure of the attractor and the motion on it. To visualize the attractor we imagine, for simplicity of exposition, that the evolution map S has at least one fixed point O (*i.e.* a configuration in phase space that, observed with the timing that defines S, reproduces itself because it generates a motion whose period is exactly that of the timing): this turns out to be not really an assumption[4] but it is useful, at first, for expository purposes as it eliminates a number of uninteresting technical steps).

We get a good approximation of the attractor simply by identifying it with the set S^TU into which a small ball around O evolves in a large time T. The ball will expand strongly along the unstable manifold of the fixed point O and it will strongly contract along the stable manifold (as we shall see the point O has to be hyperbolic, "together with all the others", for the picture to be consistent).

If T is large the image S^TU so obtained will be a very wide and thin layer of points around a wide portion of the unstable manifold of O, and this layer will be a good approximation of the attractor. The assumption that the system is thermostated is translated technically into the property that the region of phase space that the trajectories starting in U will visit is finite: therefore the unstable manifold will necessarily "wound around" in meanders and the layers will locally look as stacks of surfaces thinly coated by the points of S^TU.

The layers however will in general *not be equispaced* (not even very near a given point: the case of a conservative system being essentially the only notable exception) so that a cross section of the stack of layers will usually remind us more of a "Cantor set" than of a pile of sheets. Furthermore the width of the layer will not be constant along it but it will change from point to point because the expansivitity of the unstable manifold is not uniform, in general (not even in the conservative cases).

We now think phase space, hence also the region S^TU, as consisting of very tiny cells. The picture of the dynamics will then be the following: cells which are outside the region S^TU will eventually evolve into cells inside S^TU while cells inside S^TU will be simply permuted between themselves.[5]

One should think that the region S^TU is invariant under the application of S in spite of the fact that this apparently contradicts the invertibility of the evolution S (when S is generated by a differential equation). The point being that a dynamics that evolves contracting phase space cannot be represented as an invertible permutation of cells: so that we cannot any more regard the

[4] Because a chaotic system will always have a lot, [Sm67], of periodic orbits and a a periodic orbit can play the same role plaid here by the fixed point.

[5] If we take $T' \gg T$ and consider instead as a model for the attractor the set $S^{T'}U$ the picture is unchanged. Even though the volume of the region $S^{T'}U$ is much smaller than that of the region S^TU because the layer around $S^{T'}U$ is much thinner than that around S^TU while the portion of surface of the unstable manifold of the fixed point O coated by $S^{T'}U$ is much wider than that covered by S^TU. Of course since contraction prevails over expansion the thinning of the layer far outweighs the widening of the surface coated (by $S^{T'}U$).

map S as strictly invertible once we decide to approximate it with a map on a discrete space. By replacing T with $T' \gg T$ the approximation improves but it can become exact only when we reduce the cells size to points, *i.e.* when we use a continuum representation of phase space.

Therefore it is clear that in order that the above picture be rigorously correct (*i.e.* in order to be able to estimate the errors made in the predictions derived by assuming it correct) one needs assumptions. It is interesting that the "only" assumptions needed are that the continuum system be "chaotic" in the sense that pairs of points initially very close get far apart at a constant rate as time evolves (*i.e.* exponentially fast) with the exception of very special pairs.

More precisely if we follow the motion of a point x in phase space so that it looks to us as a fixed point $\overline{x}$, then the action of the map on the nearby points generates a motion relative to $\overline{x}$ like that of a map having $\overline{x}$ as a hyperbolic fixed point with nontrivial Lyapunov exponents (*i.e.* with exponents uniformly, in x, away from 1, some of which larger and some smaller than 1).

One says that *in a chaotic system instability occurs at every point in phase space*, [Si79], and that a system is chaotic if the attractors[6] have the above property whose formal mathematical definition can be found in [Sm67] and is known as the "axiom A property": it is the formal mathematical structure behind the simplest chaotic systems.

Finally the discrete evolution on the attractor should be "ergodic", i.e. the permutation of the cells in $S^T U$ should be a one cycle permutation.

The latter property remarkably follows from the chaoticity assumption and the principle of Ruelle, that I interpret as "empirical chaoticity manifests itself in the technical sense that one can suppose, for the purposes of studying the statistical properties of systems out of equilibrium, that they have the mathematical structure of systems with axiom A attractors" has, therefore, conceptually very strong consequences.

III. THE CHAOTIC PRINCIPLE. ENTROPY AND THERMOSTATS.

The principle discussed in the previous sections was originally formulated for models of (developed) fluid turbulence: here I shall discuss a slightly different form of it, introduced and applied in [GC95]

Chaotic Hypothesis: A chaotic system can be regarded, on its attractor and for the purpose of evaluating statistical properties of its stationary states, as a transitive Anosov system.

This is stronger than Ruelle's formulation because it replaces axiom A system by Anosov system (a transitive Anosov system can be thought of as a dynamical system on a smooth surface which is also an axiom A attractor). Intuitively one is saying that the attracting set is a smooth surface rather than a generic closed set.

Implicitly the hypothesis claims that "fractality" of the attractor must be irrelevant in systems with 10^{19} or with just many degrees of freedom.

The hypothesis allows us immediately to say that the stationary state is uniquely determined and therefore we are in a position similar to the one in equilibrium where also, by the ergodic hypothesis, the statistics of the equilibrium state was uniquely determined to be the microcanonical one. And if applied to a system in equilibrium (*i.e.* to a system of particles subject to conservative forces) it gives us again that the statistics of the motions is precisely the microcanonical one.

In other words the chaotic hypothesis is a strict extension of the ergodic hypothesis and it provides us with a formal expression ("uniform distribution on the attractor")[7] for the analogue of the microcanonical ensemble in systems out of equilibrium but stationary. The new distribution is called the SRB distribution of the system.

As discussed in §2 the hypothesis amounts to assuming that the motion is periodic. Hence the dualism between periodic and chaotic motions persists in the same sense as in the case of Boltzmann's equilibrium theory. And, as in that case, one should not confuse the periodic motion on the attractor with the periodic motions of "Aristotelian" nature: the latter are motions with short, observable periods, on the same time scale of the observation times. This is also the case in Laplace's celestial mechanics and in the Landau–Lifshitz theory of turbulence. The periods of the motions involved in the chaotic hypothesis are unimaginably larger; in the case of a model of a gas (in equilibrium or in a stationary nonequilibrium state) the period is estimated by Boltzmann's well known estimate to be "about" $10^{10^{19}}$ *ages of the universe*, [Bo95] p. 444.

We begin to explore the consequences of the chaotic hypothesis by looking at the notion of *entropy*. To fix the ideas we imagine a Hamiltonian system of N particles in a box $\mathcal{B}$ which is forced by a constant external force and is in contact with s heat reservoirs $\mathcal{R}_k$, $k = 1, \ldots, s$. We assume that the box opposite sides in the direction parallel to the force field $\mathbf{E}$ are identified and that inside the box there are fixed scatterers (*e.g.* on a regular array),

[6]In general there can be several, as a system can consist of several non interacting systems represented by points of sets located in different regions of phase space.

[7]Or, as one says more technically but equivalently for Axiom A attractors, "distribution absolutely continuous along the unstable manifolds".

enough so that there is no stright path parallel to $\mathbf{E}$ which does not hit a scatterer. A symbolic illustration of the situation is in the following picture with two reservoirs

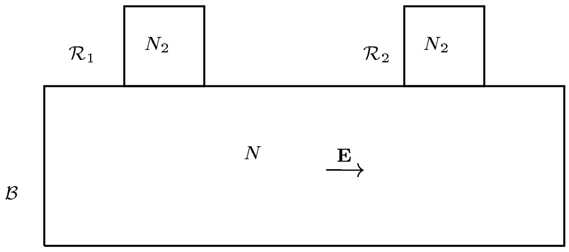

Fig.1: *The scatterers in the box $\mathcal{B}$ are not drawn; the particles in the reservoirs interact between themselves and with those of the system. The opposite sides perpendicular to $\mathbf{E}$ are identified.*

This means that the equations of motion are

$$m\ddot{\mathbf{x}}_i = \mathbf{f}_i + \mathbf{E} + \vartheta_i, \qquad \mathbf{f}_i = \sum_{j\neq i} \mathbf{f}(\mathbf{x}_i - \mathbf{y}_j) \tag{2}$$

where $\Phi f(\Xi x - \mathbf{y})$ is the (conservative) force that a particle at $\mathbf{y}$ exerts over one at $\mathbf{x}$, $\vartheta_i(t)$ are the forces due to the thermostats and m is the mass.

The particular form of the thermostating forces should be, to a large extent, *irrelevant*. Therefore we make the following model for the thermostats. Each of the s thermostats is regarded as an assembly of N_k, $k = 1, 2, \ldots, s$ particles which are kept at constant temperature:

$$\vartheta_i^{(k)} = \sum_{j=1}^{N_k} \tilde{\mathbf{f}}^{(k)}(\mathbf{x}_i - \mathbf{y}_i^{(k)}) - \alpha\,\dot{\mathbf{x}}_i \tag{3}$$

where $\tilde{\mathbf{f}}^{(k)}(\mathbf{x} - \mathbf{y}^{(k)})$ is the (conservative) force that the thermostat particle at $\mathbf{y}$ exercises over the system particle at $\mathbf{x}$, while the particles of the k–th reservoir satisfy the equation

$$m\ddot{\mathbf{y}}_i = \mathbf{f}_i^{(k)} - \alpha^{(k)}\,\dot{\mathbf{y}}_i^{(k)} \tag{4}$$

where $\mathbf{f}_i^{(k)}$ is the (conservative) force that the particle at $\mathbf{y}_i^{(k)}$ feels from the other particles of the k–th thermostat or from the system particles.

The multipliers $\alpha, \alpha^{(k)}$ are so defined that the temperatures of the system and that of each reservoir is fixed in the sense that, if k_B denotes Boltzmann's constant,

$$\begin{aligned} &\tfrac{1}{N_k}\textstyle\sum_{j=1}^{N_k} \tfrac{m}{2}(\dot{\mathbf{y}}_j^{(k)})^2 = \tfrac{3}{2}k_BT_k, \\ &\tfrac{1}{N}\textstyle\sum_{j=1}^{N} \tfrac{m}{2}(\dot{\mathbf{x}}_j^{(k)})^2 = \tfrac{3}{2}k_BT \end{aligned} \tag{5}$$

are exactly constant along the motions. The model is obtained by requiring that the constraints (5) are imposed by exerting a force that satisfies the *principle of minimum constraint* of Gauss (see appendix in [Ga96a]), just to mention a possible model of a thermostat widely used in applications then the multipliers take the values

$$\begin{aligned} \alpha^{(k)} &= \frac{\sum_j^{N_k} \mathbf{f}_j^{(k)}\cdot\dot{\mathbf{y}}_j^{(k)}}{\sum_j (\dot{\mathbf{y}}_j^{(k)})^2} = \frac{\dot{Q}_k}{3k_BT_k} \\ \alpha &= \frac{\sum_j^{N} (\mathbf{f}_j^{tot}+\mathbf{E})\cdot\dot{\mathbf{x}}_j}{\sum_j \dot{\mathbf{x}}_j^2} = \frac{\dot{Q}}{3k_BT} + \frac{\mathbf{E}\cdot\mathbf{J}}{3k_BT} \end{aligned} \tag{6}$$

where $\mathbf{J} = \sum_j^N \dot{\mathbf{x}}_i$ is the "*current*" and $\mathbf{f}_i^{tot} = \mathbf{f}_i + \sum_k\sum_j \tilde{\mathbf{f}}^{(k)}(\mathbf{x}_i - \mathbf{y}_j^{(k)})$ is the total force acting on the i–th particle.

If we compute the divergence of the equations of motion in the phase space coordinates $(\mathbf{p}, \mathbf{q})$ with $\mathbf{p}_i = m\dot{\mathbf{x}}_i$, $\mathbf{p}_i^{(k)} = m\dot{\mathbf{y}}_i^{(k)}$ we get, as noted in eq. (3.4) of [Ga96a],

$$\sum_{k=1}^{s} \frac{\dot{Q}_k}{k_BT_k} + \frac{\dot{Q}}{k_BT} + \frac{\mathbf{J}\cdot\mathbf{E}}{k_BT} \tag{7}$$

up to corrections of order N_k^{-1} and N^{-1}.[8] If there is no external field $\mathbf{E}$ or if the temperature T is not kept fixed, we only get

$$\sum_{k=1}^{s} \frac{\dot{Q}_k}{k_BT_k} \tag{8}$$

still up to corrections of order N_k^{-1} and N^{-1}.

The quantities $-\dot{Q}_k$, $-\dot{Q} - \mathbf{E}\cdot\mathbf{J}$ represent the work done over the system (including the thermostats) to keep the temperatures fixed: this means that if the system is in a stationary state the same quantities changed in sign must represent the heat that the thermostats cede to "the outside" in order to function as such. So (7) or (8) represent the *rate of increase of the entropy* of the "Universe" (in the sense of thermodynamics).

It is gratifying that, as proved in wider generality by Ruelle, [Ru96], a system verifying the chaotic hypothesis must necessarily satisfy the inequality

$$\langle \sum_{k=1}^{s} \frac{\dot{Q}_k}{k_BT_k} + \frac{\dot{Q}}{k_BT} + \frac{\mathbf{J}\cdot\mathbf{E}}{k_BT} \rangle \geq 0 \tag{9}$$

where $\langle\cdot\rangle$ denotes time average over the stationary state. One can check that the contribution to the average due to the internal forces between pairs of particles in $\mathcal{B}$ *vanishes*, therefore $\langle\dot{Q}\rangle$ receives contributions only from the forces exerted by the thermostats on the system.

Hence from the above example, which is in fact very general, the entropy creation rate when the system is in the phase space point x should be defined *in general* and in deterministic, finite, thermostated systems to be

$$\sigma(x) = -\,\mathrm{div}\, F(x) \tag{10}$$

[8] The exact value is obtained by multiplying the k–th term in the sums by $1 - \frac{1}{3N_k}$ and the other terms by $1 - \frac{1}{3N}$.

where x denotes a phase space point describing the microscopic state of the system and its evolution is given by the differential equation $\dot{x} = F(x)$ for some vector field F.[9]

It appears therefore reasonable (or *it may* appear reasonable) to set the following definition *in general*:

Definition: In a finite deterministic system the instantaneous entropy creation rate is identified with the divergence of the equations of motion in phase space evaluated at the point that describes the system at that instant.

A strong argument in favor of this ambitious definition is the following, [An82]. Suppose that a finite system is in a equilibrium state at some energy U and specific volume v. At time $t = 0$ the equations of motion are changed because the system is put in contact with heat reservoirs and subject to certain external forces whereby it undergoes an evolution at the end of which, at time $t = +\infty$, the system is *again* governed by Hamiltonian equations and settles into a new equilibrium state. If ρ_0 is the density in phase space of the distribution representing the initial state, ρ_t the density of the state at intermediate time t and ρ_∞ is the density over phase space of the final state then we can study the evolution of $S(t) = -\int \rho_t \log \rho_t \, dpdq$ which evolves from $S_0 = -\int \rho_0 \log \rho_0 \, dpdq$ to $S_\infty = -\int \rho_\infty \log \rho_\infty \, dpdq$ and one checks that:

$$S_\infty - S_0 = \int_0^\infty \frac{d}{dt} S(t) = \int_0^\infty dt \int \rho_t \, \mathrm{div}\, F \, d\mathbf{p} \, d\mathbf{q} = \\ = \int_0^\infty \langle\, \mathrm{div}\, F \,\rangle(t) \, dt \tag{11}$$

so that we see that also in this case $\langle\, \mathrm{div}\, F \,\rangle(t)$ can be interpreted as (average) entropy creation rate, [An82].

An argument against the above definition is that it does not seem to be correct in systems in which the thermostats are modeled by infinite systems initially in equilibrium at a given temperature and interacting with the particles of the system that is thermostated: in such systems there will be a flow of heat at infinity and the above considerations fail to be applicable, in a fundamental way, as shown in [EPR98]. However I see no arguments against the definition when one uses finite thermostats and finite systems.

In nonequilibrium statistical mechanics the notion of entropy and of entropy creation are *not* well established. New definitions and proposals arise continuously.

Hence a fundamental definition is highly desirable. By fundamental I mean a definition, like the one above, that should hold for very general systems in stationary states: *and* it should not be restricted to (stationary) systems close to equilibrium. This means that it should be defined even in situations where the other fundamental thermodynamics quantity, the *temperature*, may itself be also in need of a proper definition. And *furthermore* it should be a notion accessible to experimental checks, on numerical simulations and possibly on real systems.

IV. FLUCTUATIONS AND TIME REVERSIBILITY.

The definition of entropy creation rate in §3 points in the direction of an adaptation of the very first definitions of Boltzmann and Gibbs and relies on the recent works on chaotic dynamics, both in the mathematical domain and in the physical domain.

The theory of the SRB distributions together with Ruelle's proposal that they may constitute the foundations of a general theory of chaotic motions, provides us with *formal expressions* of the probability distributions describing stationary states (namely equal probability of the attractor cells). This is a surprising achievement[10] and the hope is that such formal expressions can be used to derive relations between observable quantities whose values there is no hope to ever be able to compute via the solution of the equations of motion (much as it is already the case in equilibrium statistical mechanics).

I have in mind general relations like Boltzmann's heat theorem $\frac{dU+pdV}{T} =$ exact, (1), which involves averages U, p (computed, say, in the canonical ensemble), where V, T are "parameters") that we cannot hope ever to compute, but which nevertheless is a very important, non trivial and useful relation. Are similar relations possible between dynamical averages in *stationary nonequilibrium states*? after all a great part of equilibrium statistical mechanics is dedicated to obtaining similar (if less shiny) relations, from certain N–dimensional integrals (with N very large) representing partition functions, correlation functions, *etc.*

[9] One should note, however, that in general switching on thermostating forces does not necessarily imply that the system will reach a stationary state: for instance in the above example with a field $\mathbf{E}$ but no thermostat acting on the bulk of the system (*i.e.* without fixing the bulk temperature T) it is by no means clear that the system will reach a stationary state: in fact the energy exchanges with the thermostats could be so weak that the work done by the field $\mathbf{E}$ could accumulate in the form of an ever increasing kinetic energy of the particles in the container. We need a nonobvious (if at all true) Appl. Phys. estimate of the energy of the bulk which tells us that it will stay bounded uniformly in time. The result will strongly depend on the nature of the forces between thermostats particles and system particles $\tilde{\mathbf{f}}^{(k)}$ and on the interparticle forces.

[10] Relying, for a more technical and usable formulation, on the basic work of Sinai on Markov partitions, [Si77].

Of course such results are difficult: but they might be not impossible. In situations "close" to equilibrium there are, in fact, *classical examples* like Onsager's reciprocal relations and Green–Kubo's transport coefficients expressions: these are parameterless relations essentially independent of the model used, as long as it is time reversible at least at zero forcing (*i.e.* in equilibrium).

By "close" we mean that the relations are properties of derivatives of average values of suitable observables *evaluated at zero forcing*: one says that they are properties that hold *infinitesimally close* to equilibrium.

A system is said to be time reversible under a time reversal map I if I is an isometry of phase space with $I^2 = 1$ which anticommutes with the time evolution S_t, (or S if the evolution is represented by a map), *i.e.*

$$IS_t = S_{-t}I, \qquad \text{or} \qquad IS = S^{-1}I \tag{12}$$

where $S_t x$ denotes the solution of the equations of motion at time t with initial datum x. Clearly $S_t S_{t'} = S_{t+t'}$.

The thermostat models that are derived, as in §3, from the Gauss' principle have the remarkable property of generating *time reversible* equations of motion. The importance and interest of such models of thermostats has been discovered and stressed by Hoover and coworkers, [PH92]. This has been an important contribution, requiring intellectual courage, because it really goes to the heart of the problem by stressing that one can (and should) study irreversible phenomena by only using reversible models: *microscopic reversibility has nothing to do with macroscopic irreversibilty*, as Boltzmann taught us and getting rid of spurious microscopically irreversible models can only help our understanding.

In a finite deterministic system verifying the chaotic hypothesis entropy creation rate fluctuations can be conveniently studied in terms of the *average* entropy creation rate σ_+, evaluated on the stationary state under exam and assumed > 0, and of the *dimensionless* entropy creation rate

$$p = \frac{1}{\tau}\int_{-\tau/2}^{\tau/2} \frac{\sigma(S_t x)}{\sigma_+} dt \tag{13}$$

The quantity p is a function of x (and of course of τ). Therefore if we observe p as time evolves, in a stationary state, it fluctuates and we call $\pi_\tau(p)dp$ the probability that it has a value between p and $p + dp$. On general grounds we expect that $\pi_\tau(p) = \exp \tau\zeta(p) + o(\tau)$ for τ large, [Si77]. The function $\zeta(p)$ is a suitable model dependent function with a maximum at $p = 1$ (note, in fact, that by the normalization in (13) the infinite time average of p is 1).

The theorem that one can prove under the only assumption of the chaotic hypothesis and of revesibility of the dynamics is

$$\zeta(-p) = \zeta(p) - p\sigma_+ \tag{14}$$

where $-\infty \le \zeta(p) < +\infty$.

The (14) is the analytic form of the *fluctuation theorem*, [GC95]: it is a *parameterless relation, universal* among the class of systems that are time reversible and transitive. It was first observed experimentally in a simulation, [ECM93]; it was proved, and its relation with the structure of the SRB distributions was established, in [GC95]. See also [Ru99] for a general theory and [CG99] for some historical comments.

It is a general relation that holds whether the system is at small forcing field or not, *provided* the system remains transitive *i.e.* the closure of the attractor is the full phase space in the sense that it is a time reversal invariant surface (as it is the case at zero forcing when the attractor is dense on the full energy surface).[11]

It is interesting to remark that at least in the limit case in which the forcing tends to 0, hence $\sigma_+ \to 0$, the relation (14) becomes degenerate, but by dividing both sides by the appropriate powers of the external fields *one gets a meaningful nontrivial limit* which just tells us that Green–Kubo relations and Onsager reciprocity hold so that (14) can be considered an extension of such relations, [Ga96b].

In nonequilibrium, unlike in the equilibrium case, we do not have any well established nonequilibrium thermodynamics, but at least we have that the (14) is an extension valid in "great generality" which coincides with the only universally accepted nonequilibrium thermodynamics relations known at 0 forcing.

V. REVERSIBLE VERSUS IRREVERSIBLE THERMOSTATS.

One would like more: it would be nice that (14) could be regarded as a general theorem also valid for systems which are not time reversible. In fact there are many cases, in particular in the theory of fluids, in which the thermostating effects are modeled by "irreversible" forces like friction, viscosity, resistivity, *etc.*

This is an important problem and it deserves further analysis: a proposal which has been advanced, [Ga96a], is that one can imagine to thermostat a system in various ways which are "physically equivalent" (*e.g.* one can use different models of thermostating forces). This means that the stationary state distribution μ that describes the

[11] This condition will be verified automatically at small forcing , if true at zero forrrcing, as a reflection of a property called "*structural stability*" of chaotic (*i.e.* transitive Anosov) systems. It is important that "small forcing" does not mean infinetismal forcing but just not too large forcing, so that we are really out of equilibrium. How far out of equilibrium will depend on the model: in simple models it turns out, experimentally, to be a property that holds for very strong forcing in the relevant physical units.

statistics of the system will depend on the special equations of motions used (which reflect a particular thermostating mechanism). However by "suitably", see[Ga97a], [Ga99b], changing the equations the averages of the interesting observables will not change, at least if the number of degrees of systems is large (*i.e.* "in the thermodynamic limit").

And it is possible that the *same system* can be equivalently described in terms of reversible or of irreversible equations of motion. This may at the beginning be very surprising: but in my view it points at one of the more promising directions of research in nonequilibrium: if correct this means that in various cases we could use reversible equations to describe phenomena typically described by irreversible equations: at least as far as the evaluation of several averages is concerned. This is very close to what we are used to, since the classical (although scarcely quoted and scarcely known) paper of Boltzmann [Bo84] where we learned that one could use *different equilibrium ensembles* to describe the *same system*: canonical or microcanonical ensembles give the same average to all "local observables".

In equilibrium the ensembles are characterized in terms of a few parameters (in the microcanonical ensemble one fixes the total energy and the specific volume, in the canonical ensemble one fixes the temperature and the specific volume, *etc.*): the distributions in phase space that correspond to the elements of such ensembles are very different. However, if the parameters that characterize the distribution are correctly correspondent, then the distributions give the same averages to large classes of observables.

Philosophically this is rather daring and physically it seems to bear possibly important consequences: namely it might be that the general results that apply to reversible systems do apply as well to irreversible systems because the latter may be just equivalent to reversible ones. We again fall dangerously close to the paradoxes, that were used to counter the new equilibrium statistical mechanics, by Loschmidt and Zermelo, [Ce99]. Except that now we have learnt, after Boltzmann, why they may be in fact circumvented, [Le93].

Of course an idea like the one above has to be supported by some further evidence and requires further investigations. I think that some rather strong evidence in favor of it, besides its fascination, is that a number of experiments on computer simulations of fluids motion in turbulent states have been already carried out (for other purposes) but their results can be interpreted as evidence in favor of the new idea, [SJ93]: see [Ga97a], [Ga97b], [Ga99b].

More recently there have been attempts to perform *dedicated simulations* to observe this property in fluid dynamics systems. These experiments are interesting also because the application of the chaotic hypothesis to fluids may look less direct than to nonequilibrium statistical systems. In fact trying to perform experiments, even in simulations, is quite promising and perhaps we may even be close to the possibility of critical tests of the chaotic hypothesis in real fluids. The preliminary results of simulations are encouraging but more work will have to be done, [RS98]; in the next section we shall examine a real experiment and attempt a theoretical explanation of it.

VI. AN EXPERIMENT WITH WATER IN A COUETTE FLOW.

A most interesting experiment by Ciliberto–Laroche, [CL98], on a physically macroscopic system (water in a container of a size of the order of a few deciliters), has been performed with the aim of testing the relation (14).

This being a real experiment one has to stretch quite a bit the very primitive theory developed so far in order to interpret it and one has to add to the chaotic hypothesis other assumptions that have been discussed in [BG97], [Ga97a].

The experiment attempts at measuring a quantity that is eventually interpreted as the difference $\zeta(p)-\zeta(-p)$, by observing the fluctuations of the product ϑu^z where ϑ is the *deviation* of the temperature from the average temperature in a small volume element Δ of water at a fixed position in a Couette flow and u^z is the velocity in the z direction of the water in the same volume element.

The result of the experiment is in a way quite unexpected: it is found that the function $\zeta(p)$ is rather irregular and lacking symmetry around $p=1$ *but the function* $\zeta(p)-\zeta(-p)$ *seems to be strikingly linear.* As discussed in [Ga97a], predicting the slope of the entropy creation rate would be difficult but if the equivalence conjecture considered above and discussed more in detail in [Ga97a] is correct then we should expect linearity of $\zeta(p)-\zeta(-p)$. In the experiment of [CL98] the quantity ϑu_z does not appear to be the divergence of the phase space volume simply because there is no model proposed for the theory of the experiment. Nevertheless Ciliberto–Laroche select the quantity $\int_\Delta \vartheta\, u^z\, d\mathbf{x}$ on the basis of considerations on entropy and dissipation so that there is great hope that in a model of the flow this quantity can be related to the entropy creation rate discussed in §3.

Here we propose that a model for the equations, that can be reasonably used, is Rayleigh's model of convection, [Lo63], [Ga97b] sec. 5. An attempt for a theory of the experiment could be the following.

One supposes that the equations of motion of the system in the whole container are written for the quantities t, x, z, ϑ, u in terms of the height H of the container (assumed to be a horizontal infinite layer), of the temperature difference between top and bottom δT and in terms of the phenomenological "friction constants" ν, χ of viscosity, dynamical thermal conductivity and of the thermodynamic dilatation coefficient α. We suppose that the fluid is 3–dimensional but stratified, so that velocity and temperature fields do not depend on the coordinate y, and gravity is directed along the z–axis: $\mathbf{g}=g\,\mathbf{e}$, $\mathbf{e}=(0,0,-1)$.

In such conditions the equations, including the boundary conditions (of fixed temperature at top and bottom and zero normal velocity at top and bottom), the convection equations in the Rayleigh model, see [Lo63] eq. (17), (18) where they are called the *Saltzman equations*, and [Ga97b], become

$$
\begin{aligned}
&\partial \cdot \mathbf{u} = 0\\
&\dot{\mathbf{u}} + \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \mathbf{u} = \nu \Delta \mathbf{u} - \alpha \vartheta \mathbf{g} - \partial p'\\
&\dot{\vartheta} + \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \vartheta = \chi \Delta \vartheta + \tfrac{\delta T}{H} u_z \qquad (15)\\
&\vartheta(0) = 0 = \vartheta(H), \quad u_z(0) = 0 = u_z(H),\\
&\textstyle\int u_x d\mathbf{x} = \int u_y d\mathbf{x} = 0 \qquad (16)
\end{aligned}
$$

The function p' is related, *but not equal*, to the pressure p: within the approximations it is $p = p_0 - \rho_0 g z + p'$.

It is useful to define the following adimensional quantities

$$
\begin{aligned}
&\tau = t \nu H^{-2},\ \xi = x H^{-1},\ \eta = y H^{-1},\ \zeta = z H^{-1},\\
&\vartheta^0 = \frac{\alpha \vartheta}{\alpha\, \delta T},\ \mathbf{u}^0 = (\sqrt{g H \alpha\, \delta T})^{-1}\, \mathbf{u}\\
&R^2 = \frac{g H^3 \alpha\, \delta T}{\nu^2}, \qquad R_{Pr} = \frac{\nu}{\kappa} \qquad (17)
\end{aligned}
$$

and one checks that the Rayleigh equations take the form

$$
\begin{aligned}
&\dot{\mathbf{u}} + R \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \mathbf{u} = \Delta \mathbf{u} - R \vartheta \mathbf{e} - \partial p,\\
&\dot{\vartheta} + R \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \vartheta = R_{Pr}^{-1} \Delta \vartheta + R u_z,\\
&\partial \cdot \mathbf{u} = 0\\
&u_z(0) = u_z(1) = 0, \qquad \vartheta(0) = \vartheta(1) = 0,\\
&\textstyle\int u_x d\mathbf{x} = \int u_y d\mathbf{x} = 0 \qquad (18)
\end{aligned}
$$

where we again call $t, x, y, z, \mathbf{u}, \vartheta$ the adimensional coordinates $\tau, \xi, \eta, \zeta, \mathbf{u}^0, \vartheta^0$ in (17). The numbers R, R_{Pr} are respectively called the *Reynolds and Prandtl numbers* of the problem: $R_{Pr} =\sim 6.7$ for water while R is a parameter that we can adjust, to some extent, from 0 up to a rather large value.

According to the principle of equivalence stated in [Ga97a] here one should impose the constraints

$$
\int \big((\partial \underset{\sim}{u}\ )^2 + \frac{1}{R_{Pr}} (\partial \vartheta)^2 \big)\, d\mathbf{x} = C \qquad (19)
$$

on the "frictionless equations", (*i.e.* (18) without the terms with the laplacians) obtaining

$$
\begin{aligned}
&\partial \cdot \mathbf{u} = 0\\
&\dot{\mathbf{u}} + R \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \mathbf{u} = R \vartheta\, \mathbf{e} - \partial p' + \tau_{th}\\
&\dot{\vartheta} + \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \vartheta = R u^z + \lambda_{th}\\
&\vartheta(0) = 0 = \vartheta(H), \quad \textstyle\int u_x d\mathbf{x} = \int u_y d\mathbf{x} = 0 \qquad (20)
\end{aligned}
$$

where the frictionless equations are modified by *the thermostats forces* τ_{th}, λ_{th}: the latter impose the nonholonomic constraint in (19). Looking only at the bulk terms we see that the equations obtained by imposing the constraints via Gauss' principle, see [Ga96a], [Ga97a], become the (18) with coefficients in front of the Laplace operators equal to $\nu_G, \nu_G R_{Pr}^{-1}$, respectively, with the "gaussian multiplier" ν_G being an *odd* functions of $\mathbf{u}$, see [Ga97a]: setting $\tilde{C} = \int \big((\Delta \mathbf{u})^2 + R_{Pr}^{-1} (\Delta \vartheta)^2\big) d\mathbf{x}$ one finds

$$
\begin{aligned}
\nu_G = \tilde{C}^{-1} \Big(\textstyle\int \big((\Delta \mathbf{u} \cdot (\underset{\sim}{u} \cdot \underset{\sim}{\partial}\ ) \mathbf{u}) + &\\
+ R_{Pr}^{-2} (\Delta \vartheta \cdot (\underset{\sim}{u} \cdot \underset{\sim}{\partial}\ ) \vartheta) + &\\
+ R (1 + R_{Pr}^{-1})\, u^z \vartheta \big)\ d\mathbf{x} \Big) & \qquad (21)
\end{aligned}
$$

which we write $\nu_G = \nu^i + R \nu^e$. And the equations become, finally

$$
\begin{aligned}
&\partial \cdot \mathbf{u} = 0\\
&\dot{\mathbf{u}} + R \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \mathbf{u} = R \vartheta\, \mathbf{e} - \partial p' + \nu_G \Delta \mathbf{u}\\
&\dot{\vartheta} + \underset{\sim}{u} \cdot \underset{\sim}{\partial}\ \vartheta = R u^z + \nu_G \tfrac{1}{R_{Pr}} \Delta \vartheta\\
&\vartheta(0) = 0 = \vartheta(H), \quad \textstyle\int u_x d\mathbf{x} = \int u_y d\mathbf{x} = 0 \qquad (22)
\end{aligned}
$$

One has to tune, [Ga97a], the value of the constant C in (19) so that the average value of ν_G is precisely the physical one: namely $\langle \nu_G \rangle = 1$ by (18). This is the same, in spirit, as fixing the temperature in the canonical ensemble so that it agrees with the microcanonical temperature thus implying that the two ensembles give the same averages to the local observables.

The equations (22) are time reversible (unlike the (18)) under the time reversal map:

$$
I(\mathbf{u}, \vartheta) = (-\mathbf{u}, \vartheta) \qquad (23)
$$

and they should be supposed, by the arguments in [Ga97a], "equivalent" to the irreversible ones (18).

The (22) should have, by the general theory of [Ga97a], a "divergence" $\sigma(\mathbf{u}, \vartheta)$ whose fluctuation function $\zeta(p)$ verifies a linear fluctuation relation, *i.e.* $\zeta(p) - \zeta(-p)$ should be linear in p similar to (14). And the divergence of the above equations is proportional to ν_G if one supposes that the high momenta modes can be set equal to 0 so that the equation (22) becomes a system of finite differential equations for the Fourier components of $\mathbf{u}, \vartheta$. The Lorenz' equations, for instance, reduced the number of Fourier components necessary to describe (18) to just three components, thus turning it into a system of three differential equations.

By the conjectures in [Ga97a] a fluctuation relation should hold for the divergence; except that the slope of the differenxe $\zeta(p) - \zeta(-p)$ should not necessarily be σ_+ as in (4).

Proceeding in this way the divergence of the equations of motion is a sum of two integrals one of which proportional to the Reynolds number R. If instead of integrating over the whole sample we integrate over a small region Δ, like in the experiment of [CL98], we can expect to see a fluctuation relation for the entropy creation rate only if the fluctuation theorem *holds locally*, *i.e.* for the entropy creation in a small region.

This is certainly not implied by the proof in [GC95]: however *when the dissipation is homogeneous through the system*, as it is the case in the Rayleigh model there is hope that the fluctuation relation holds locally, again for the same reasons behind the equivalence conjecture (*i.e.* a small subsystem should be equivalent to a large one). The actual possibility of a local fluctuation theorem in systems with homogeneous dissipation has been shown in [Ga99a], after having been found through numerical simulations in [PG99], and therefore we can imagine that it might apply to the present situation as well.

If the contributions to the entropy creation due to the term $R\int_\Delta u_z\vartheta\, d\mathbf{x}$, where Δ is the region where the measurements of [CL98] are performed, dominate over the others we have an explanation of the remarkable experimental result. Unfortunately in the experiment [CL98] the contributions not explicitly proportional to R to the entropy creation rates have not been measured. But the Authors hint that they should indeed be smaller; in any event they might be measurable by improving the same apparatus, so that one can check whether the above attempt to an explanation of the experiment is correct, or try to find out more about the theory in case it is not right. If correct the above "theory" the experiment in [CL98] would be quite important for the status of the chaotic hypothesis.

VII. CONCLUSIONS.

We have tried to show how, still today, one can attribute to the motions of complex systems the character of periodic motions, as in the observation in [Bo66] which gave birth to modern statistical mechanics of equilibrium. Yet such periodic motions are motions of huge period and they cannot be confused with the epicyclical motions of Aristoteles which survived in mechanics until Boltzmann and Poincaré and in fluid mechanics until the early 1960's.

Thinking all motions as periodic allows us to unify the statistical mechanics of equilibrium and nonequilibrium and at the same time to unify them with the theory of (developed) turbulence. It also shows that the discrete viewpoint of Boltzmann which started with an attempt to save the Aristotelian view of motion (as in the quoted passage of [Bo66]), and which is *necessary* to avoid contradictions inherent in the dogmatic conception of space time as a continuum, is very powerful also to attack problems that seemed treatable *only* by very refined mathematical analysis.

And in nonequilibrium problems a theory of *statistical ensembles* might be possible that extends in a bold and surprising way the theory of the equilibrium ensembles: in this theory the phenomenological constants that appear in the equations of motion of thermostated systems can be replaced by fluctuating quantities *with appropriate averages* turning certain *other* fluctuating observables into *exact constants*. This is "as" in equilibrium where we can introduce a *constant* canonical temperature by imposing that it is equal to the *average* of the fluctuating microcanonical temperature (*i.e.* kinetic energy). In this way we are not forced to attribute a fundamental role to the phenomenological transport coefficients: they are just convenient Lagrange multipliers for the statistics of the stationary states. Like the temperature o the activity in equilibrium statistical mechanics of the canonical or grand canonical ensembles.

The fluctuation theorem seems to open the way to considerations over out-of-equilibrium systems that were unthinkable until recently: perhaps this is not the right approach but it has led to interesting experimental questions which might attract more interest in the future.

The above picture is lacking sufficient experimental confirmations to be considered established or even likely: it has to be regarded at the moment as one more attempt among many in this century to understand a difficult problem. We should not forget that the whole XX-th century failed to give us a theory of nonequilibrium phenomena and of turbulence which could be regarded as fundamental as the Boltzmann–Maxwell–Gibbs principles of equilibrium statistical mechanics: the problem is so fundamental that it will (almost) certainly attract the attention of the new generations of physicists and a solution of it is certainly awaited.

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Giovanni Gallavotti
Dipartimento di Fisica
Universitá di Roma "La Sapienza"
P.le Moro 2, 00185 Roma, Italy
email: giovanni@ipparco.roma1.infn.it
tel. +39-06-49914370, fax. +39-06-4957697

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