Science of Chaos or Chaos in Science?*

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Abstract

I try to clarify several confusions in the popular literature concerning chaos, determinism, the arrow of time, entropy and the role of probability in physics. Classical ideas going back to Laplace and Boltzmann are explained and defended while some recent views on irreversibility, due to Prigogine, are criticized.

1 Introduction

We might characterize today's breakdown of industrial or "Second Wave"
society as a civilizational "bifurcation", and the rise of a more differentiated,
"Third Wave" society as a leap to new "dissipative structures" on a world scale.
And, if we accept this analogy, might we not look upon the leap from Newtonianism to
Prigoginianism in the same way? Mere analogy, no doubt. But illuminating, nevertheless.
Alvin Toffler (preface to [96]).

Popularization of science seems to be doing very well: the Big Bang, the theory of elementary particles or of black holes are explained in countless books for the general public. The same is true for chaos theory, irreversibility or self-organization. However, it seems also that a lot of confusion exists concerning these latter notions, and that at least some of the popular books are spreading misconceptions. The goal of this article is to examine some of them, and to try to clarify the situation.

In particular, I will make a critical evaluation of the various claims concerning chaos and irreversibility made by Prigogine and by Stengers, since "La Nouvelle Alliance". Several of those claims, especially the most recent ones, are rather radical: "the notion of chaos leads us to rethink the notion of "law of nature"." ([98], p.15)[1] For chaotic systems, "*trajectories are eliminated from the probabilistic description* ... The statistical description is *irreducible*." ([98], p.59) The existence of chaotic dynamical systems supposedly marks a radical departure from a fundamentally deterministic world-view, makes the notion of trajectory obsolete, and offers a new understanding of irreversibility. Prigogine and Stengers claim that the classical conception was unable to incorporate time

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[1]Here and below, I have translated the texts that were available only in French.

in our view of the world ([95], chap.1) or to account for the irreversibility of macroscopic phenomena. Boltzmann's attempt to explain irreversibility on the basis of reversible laws failed ([98], p.41).

On the basis of these theories, a number of speculations are put forward on the notion of "event", on the place of human beings in Nature, or even on overcoming Cartesian dualism (see [98], chap.9, [99], p.106, and [101]). These writings have been indeed quite influential, mostly among non-experts. They are frequently quoted in philosophical or cultural circles, as an indication that chaos, nonlinear phenomena or the "arrow of time" have led to a profound revolution in our way of thinking.

I want to develop quite different views on most of these issues. In my opinion, chaos does not invalidate in the least the classical deterministic world-view; the existence of chaotic dynamical systems actually strengthens that view (Sect. 2). Besides, the relationship between chaos and irreversibility is quite different from what is claimed e.g. in "Les lois du chaos" [98] . Finally, when they are correctly presented, the classical views of Boltzmann perfectly account for macroscopic irreversibility on the basis of deterministic, reversible, microscopic laws (Sect. 3). Part of the difficulty in understanding those views comes from some confusions about the use of the words "objective" and "subjective", associated with probability or entropy. I will try to be careful with these notions (Sect. 4 and 5). In section 6, I will discuss the applications of probabilistic reasoning to complex phenomena and biology. I shall also argue that most of the speculation on the "new alliance" between the human sciences and the natural ones is misguided and that the people working in sociology or psychology have very little to learn from the alleged "leap from Newtonianism to Prigoginianism" (Sect. 7).

On the other hand, I believe that the ideas of Laplace and of Boltzmann are worth defending against various misrepresentations and misunderstandings. Quite independently of the work of Prigogine, there are serious confusions that are found in the literature on irreversibility, chaos or time (some of which go back to philosophers such as Popper, Feyerabend or Bergson). Besides, many textbooks or popular books on statistical mechanics are rather obscure, at least in the part concerning the foundations of the field (e.g., on the role played by ergodic theorems). I will try to clarify these questions too (Sect. 4).

I wrote this paper in a not too technical language, relegating formulas to footnotes and remarks. Nothing of what I say is new[2]. In fact, everything is quite standard and old, and it is a sad fact that those ideas that were so nicely explained by Boltzmann a century ago [11] have to be reexplained over and over again.

Finally, I have to emphasize that this is in no way a criticism of Prigogine's work in general, and even less of the Brussels' school. I shall only discuss the radical claims made in the popular books and, in particular, the idea that fundamental flaws have been found in the scientific world-view and that one has to rethink the *notion* of law of nature. I believe that a lot of interesting scientific ideas have been developed around Prigogine and that he has had an exceptional taste for discovering new directions in physics, whether in irreversible thermodynamics or in chaotic phenomena. But this

[2]On the issue of irreversibility, see Feynman [38], Jaynes [56], Lebowitz [68, 69], Penrose [87]. For a similar and less technical critique of various confusions about chaos, see Maes [75].

does not put his views on foundational questions beyond criticism[3].

2 Chaos and determinism: Defending Laplace.

The concept of dog does not bark.
B. Spinoza

2.1. Determinism and predictability.

A major scientific development in recent decades has been popularized under the name of "chaos". It is widely believed that this implies a fundamental philosophical or conceptual revolution. In particular, it is thought that the classical world-view brilliantly expressed by Laplace in his "Philosophical Essay on Probabilities" has to be rejected[4]. Determinism is no longer defensible. I think this is based on a serious confusion between *determinism* and *predictability.* I will start by underlining the difference between the two concepts. Then, it will be clear that what goes under the name of "chaos" is a major scientific progress but does not have the radical philosophical implications that are sometimes attributed to it.

In a nutshell, determinism has to do with how Nature behaves, and predictability is related to what we, human beings, are able to observe, analyse and compute. It is easy to illustrate the necessity for such a distinction. Suppose we consider a perfectly regular, deterministic *and* predictable mechanism, like a clock, but put it on the top of a mountain, or in a locked drawer, so that its state (its initial conditions) become inaccessible to us. This renders the system trivially unpredictable, yet it seems difficult to claim that it becomes non-deterministic[5]. Or consider a pendulum: when there is no external force, it is deterministic and predictable. If one applies to it a periodic forcing, it may become unpredictable. Does it cease to be deterministic?

In other words, anybody who admits that *some* physical phenomena obey deterministic laws must also admit that some physical phenomena, although deterministic, are not predictable, possibly for "accidental" reasons. So, a distinction must be made[6]. But,

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[3] I must add that I have defended, in the past, some of the ideas criticized below. Needless to say, I am interested in the critique of ideas and not of individuals.

[4] For rather negative comments on Laplace, see e.g. Ekeland ([34], p.31), and Gleick ([45], p.21 in the French edition).

[5] Likewise, only the most radical social constructivist might object to the idea that Neptune or Pluto were following their (deterministic) trajectories before they were discovered.

[6] In an often quoted lecture to the Royal Society, on the three hundredth anniversary of Newton's Principia, Sir James Lighthill gave an inadvertently perfect example of how to slip from inpredictability to indeterminism: "We are all deeply conscious today that the enthusiasm of our forebears for the marvellous achievements of Newtonian mechanics led them to make generalizations in this area of *predictability* which, indeed, we may have generally tended to believe before 1960, but which we now recognize were false. We collectively wish to apologize for having misled the general educated public by spreading ideas about *determinism* of systems satisfying Newton's laws of motion that, after 1960, were to be proved incorrect..." [72] (Italics are mine; quoted e.g. by Reichl [103], p.3, and by Prigogine and Stengers, [95], p.93, and [98], p.41). See also, [119] p.7 where, after describing a chaotic system, one concludes that "the deterministic approach fails" .

once this is admitted, how does one show that *any* unpredictable system is *truly* non-deterministic, and that the lack of predictability is not merely due to some limitation of our abilities? We can never infer indeterminism from our ignorance alone.

Now, what does one mean exactly by determinism? Maybe the best way to explain it is to go back to Laplace :" Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it- an intelligence sufficiently vast to submit these data to analysis- it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present before its eyes." [67] The idea expressed by Laplace is that determinism depends on what the laws of nature are. Given the state of the system at some time, we have a formula (a differential equation, or a map) that gives in principle the state of the system at a later time. To obtain predictability, one has to be able to measure the present state of the system with enough precision, and to compute with the given formula (to solve the equations of motion). Note that there exist alternatives to determinism: there could be no law at all; or the laws could be stochastic: the state at a given time (even if it is known in every conceivable detail) would determine only a probability distribution for the state at a later time.

How do we know whether determinism is true, i.e. whether nature obeys deterministic laws? This is a very complicated issue. Any serious discussion of it must be based on an analysis of the fundamental laws, hence of quantum mechanics, and I do not want to enter this debate here[7]. Let me just say that it is conceivable that we shall obtain, some day, a complete set of fundamental physical laws (like the law of universal gravitation in the time of Laplace), and then, we shall see whether these laws are deterministic or not[8]. Any discussion of determinism outside of the framework of the fundamental laws is useless[9]. All I want to stress here is that the existence of chaotic dynamical systems does not affect *in any way* this discussion. What are chaotic systems? The simplest way to define them is through sensitivity to initial conditions. This means that, for any initial condition of the system, there is some other initial condition, arbitrarily close to

[7] I have expressed my point of view on the foundations of quantum mechanics in [16]; for related views, see Albert [1, 2], Bell [7], Dürr et al. [31], Maudlin [76]. For a precise discussion of determinism in various physical theories, see Earman [32].

[8] Most of the laws that are discussed in the literature on chaos (e.g. on the weather) are actually macroscopic laws, and not fundamental, or microscopic ones. This distinction will be discussed in Section 3.

[9] Opponents of determinism are quick to point out that determinism cannot be proven. While it is of course true that no statement about the world can literally be *proven*, these opponents do not always see how vacuous are their own arguments in favour of indeterminism, arguments that rely ultimately on our ignorance. Popper, in [93], gives a long series of such arguments. In a review of this book, the biologist Maynard Smith shows a rather typical misunderstanding of Laplace: first, he agrees with Popper's criticism of Laplace, because the latter's computations are, in practice, impossible to do. But, then he disagrees with Popper about free will and gives, as far as I can see, a perfectly causal and Laplacian account of human actions, which of course, are not practically computable either ([79], p.244). To avoid misunderstandings, I am not trying to say that determinism is or must be true. All I say is that various arguments against determinism miss the point.

the first one so that, if we wait long enough, the two systems will be markedly different[10]. In other words, an arbitrarily small error on the initial conditions makes itself felt after a long enough time. Chaotic dynamical systems are of course unpredictable in practice, at least for long enough times[11], since there will always be some error in our measurement of the initial conditions. But this does not have any impact on our discussion of determinism, since we are assuming from the beginning that the system obeys some deterministic law. It is only by analysing this deterministic system that one shows that a small error in the initial conditions may lead to a large error after some time. If the system did not obey any law, or if it followed a stochastic law, then the situation would be very different. For a stochastic law, two systems with the *same* initial condition could be in two very different states after a short time[12].

It is interesting to note that the notion that small causes can have big effects (in a perfectly deterministic universe) is not new at all. Maxwell wrote: "There is a maxim which is often quoted, that "The same causes will always produce the same effects"". After discussing the meaning of this principle, he adds: "There is another maxim which must not be confounded with that quoted at the beginning of this article, which asserts "That like cause produce like effects." This is only true when small variations in the initial circumstances produce only small variations in the final state of the system" ([77], p.13)[13]. One should not conclude from these quotations[14] that there is nothing new under the sun. A lot more is known about dynamical systems than in the time of Poincaré.

[10] Here is a simple example. Consider the "phase space" to be simply the interval $I = [0, 1[$. And take as (discrete time) dynamics the map $f : x \to 10x \; mod \; 1$. This means, we take a number between 0 and 1, multiply it by 10, write the result as an integer plus a number between 0 and 1 and take the latter as the result (i.e. $f(x)$). This gives again a number between 0 and 1, and we can repeat the operation. Upon iteration, we obtain the *orbit* of x; x itself is the initial condition. To describe concretely the latter, one uses the decimal expansion. Any number in I can be written as $x = 0.a_1 a_2 a_3 \ldots$, where a_i equals $0, 1, 2, \ldots, 9$. It is easy to see that $f(x) = 0.a_2 a_3 \ldots$. This is a perfect example of a *deterministic* but *unpredictable* system. Given the state x at some initial time, one has a rule giving the state of the system for arbitrary times. Moreover, for any fixed time, one can, in principle, find the state after that time, with any desired accuracy, given a sufficiently precise characterization of the initial state. This expresses the deterministic aspect. Unpredictability comes from the fact that, if we take two initial conditions at a distance less than 10^{-n}, then the corresponding orbits could differ by, say, $1/2$, after n steps, because the difference will be determined by the nth decimal. One of the relatively recent discoveries in dynamical systems is that simple physical examples, like a forced pendulum, may behave more or less like this map.

[11] How long a time this is depends on the details of the system.

[12] It is worth observing that Turing machines, or the Game of Life, provide examples of deterministic automata whose evolution is more unpredictable (in a precise technical sense) than the one of the usual chaotic dynamical systems.

[13] As for applications to the weather, Poincaré ([88], p.69) already noticed that the rainfalls or the storms seem to occur at random, so that people are more likely to pray for rain than for an eclipse (for an exception to this rule, but based on prior knowledge, see [52], p.59). We are not able to predict the storms, because the atmosphere may be in a state of "unstable equilibrium". It may all depend on a tenth of a degree. And he adds: "If we had known this tenth of a degree, one could have made predictions", but since our observations are not sufficiently precise, it all appears to be due to randomness.

[14] Hadamard, [49], Duhem [29] and Borel [13] made similar observations. See Ruelle, [104] for a discussion of that history from a modern perspective, and a good popular exposition of chaos.

But, the general idea that not everything is predictable, even in a deterministic universe, has been known for centuries. Even Laplace emphasized this point: after formulating universal determinism, he stresses that we shall always remain "infinitely distant" from the intelligence that he just introduced. After all, why is this determinism stated in a book on *probabilities*? The reason is obvious: for Laplace, probabilities lead to rational inferences in situations of incomplete knowledge (I'll come back below to this view of probabilities). So he is assuming from the beginning that our knowledge is incomplete, and that we shall never be able to *predict* everything. It is a complete mistake to attribute to some "Laplacian dream" the idea of perfect predictability[15]. But Laplace does not commit what E. T. Jaynes calls the "Mind Projection Fallacy": "We are all under an ego-driven temptation to project our private thoughts out onto the real world, by supposing that the creations of one's own imagination are real properties of Nature, or that one's own ignorance signifies some kind of indecision on the part of Nature" [16]([57], p.7). As we shall see, this is a most common error. But, whether we like it or not, the concept of dog does not bark, and we have to carefully distinguish between our representation of the world and the world itself.

Let us now see why the existence of chaotic dynamical systems in fact supports universal determinism rather than contradicts it[17]. Suppose for a moment that no classical mechanical system can behave chaotically. That is, suppose we have a theorem saying that any such system must eventually behave in a periodic fashion[18]. It is not completely obvious what the conclusion would be, but certainly *that* would be an embarassment for the classical world-view. Indeed, so many physical systems seem to behave in a non-periodic fashion that one would be tempted to conclude that classical mechanics cannot adequately describe those systems. One might suggest that there must be an inherent indeterminism in the basic laws of nature. Of course, other replies would be possible: for example, the period of those classical motions might be enormously long. But it is useless to speculate on this fiction since we know that chaotic behaviour is compatible with a deterministic dynamics. The only point of this story is to stress that determin-

[15]It is interesting to read the rest of the text of Laplace. First of all, he expresses the belief that there are indeed fundamental, universally valid laws of nature which can be discovered through scientific investigation (the only examplethat Laplace had of a fundamental law was that of universal gravitation). In that respect, nothing has changed today. One of the goals of physics is still to discover those fundamental laws. His basic idea could be called universal reductionism rather than universal determinism. Since reductionism is remarkably well defended in Weinberg's book "Dreams of a Final Theory" [120], I shall not pursue this point. Reading a little further, we see that Laplace's goal is to use science against superstition. He mentions the fears caused by Halley's comet in the Middle Ages (where it was taken as a sign of the divine wrath) and how our discovery of the laws of the "system of the world" "dissipated those childish fears due to our ignorance of the true relations between Man and the Universe". Laplace expresses also a deep optimism about the progress of science. Again nothing of that has been refuted by the evolution of natural sciences over the last two centuries. But one will not find any claim about the computability, by us humans, of *all* the consequences of the laws of physics.

[16]Jaynes' criticisms were mostly directed at the way quantum theory is presented, but they also apply to some discussions of chaos theory or of statistical mechanics.

[17]Of course, since classical mechanics is not really fundamental (quantum mechanics is), this issue is rather academic. We nevertheless want to discuss it, because there seems to be a lot of confusion in the literature.

[18]Imagine, for example, that the Poncaré-Bendixson theorem held in all dimensions.

istic chaos increases the explanatory power of deterministic assumptions, and therefore, according to normal scientific practice, *strengthens* those assumptions. And, if we did not know about quantum mechanics, the recent discoveries about chaos would not force us to change a single word of what Laplace wrote[19].

2.2. Trajectories and probabilities.

Now, I will turn to the main thesis of Prigogine and his collaborators on chaotic dynamical systems: the notion of trajectory should be abandoned, and replaced by probabilities. What does this mean? Let me quote Prigogine: "Our leitmotiv is that the formulation of the dynamics for chaotic systems must be done at the probabilistic level" ([98], p.60). Or: " We must therefore eliminate the notion of trajectory from our microscopic description. This actually corresponds to a realistic description: no measurement, no computation lead strictly to a point, to the consideration of a *unique* trajectory. We shall always face a *set* of trajectories" ([98], p.60)[20].

Let us first see how reasonable it is to "eliminate the notion of trajectory" for chaotic systems by considering a concrete example[21]. Take a billiard ball on a sufficiently smooth table, so that we can neglect friction (for some time), and assume that there are suitable obstacles and boundaries so that the system is chaotic. Now suppose that we use an "irreducible" probabilistic description, that is, instead of assigning a position to the ball, we assign to it a probability distribution[22]. Consider next the evolution of that probability distribution. Since we are dealing with a chaotic system, that distribution will spread out all over the billiard table. This means that after a rather short time, there will be an almost uniform probability of finding the ball in any given region of the table. Indeed, even if our initial probability distribution is well peaked around the initial position of the ball, there will be lots of nearby initial conditions that will give rise to very different trajectories (that is exactly what it means to say that the system is chaotic). But now we can hardly take the probability distribution after some time seriously as an "irreducible" *description* of the system. Indeed, whenever we look at the system, we find the ball somewhere, at a rather well defined position on the table. It is certainly not completely described by its probability distribution. The latter describes adequately our knowledge (or rather our ignorance) of the system, obtained on the basis of our

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[19] And, concerning quantum mechanics, see the references in note 7.

[20] See also [95], p.28: "As we shall see, there exists, for sufficiently unstable systems, a "temporal horizon" beyond which no determined trajectory can be attributed to them". To be fair, I should add that these radical statements are combined with more reasonable, but more technical ones, e.g.: "This formulation implies that one must study the eigenfunctions and the eigenvalues of the evolution operator." ([98], p.60) The question is of course which statements will strike most the non-specialized reader.

[21] See Batterman [3] for a related, but different, critique. Batterman says that the replacement of trajectories by probabilities is "very much akin to the claim in quantum mechanics that the probabilistic state description given by the Ψ-function is complete, that is, that underlying exact states cannot exist." (p.259) But he notes that here, unlike in quantum mechanics, no no-hidden variable argument is given to support that claim (for the exact status of no-hidden variable arguments in quantum mechanics, see Bell, [7] and Maudlin, [76]).

[22] An absolutely continuous one, i.e., given by a density. If one considers probabilities given by delta functions, it is equivalent to considering trajectories.

initial information. But it would be difficult to commit the Mind Projection Fallacy more radically than to confuse the objective position of the ball and our best bet for it . In fact, chaotic systems illustrate this difference: if all nearby initial conditions followed nearby trajectories, the distinction between probabilities and trajectories would not matter too much. But chaotic system show exactly how unreasonable is the assignment of "irreducible" probabilities, since the latter quickly spread out over the space in which the system evolves.

Of course, nobody will deny that the ball is always somewhere. But this example raises the following the question: what does it mean exactly to "eliminate trajectories"[23]. Either the dynamics is expressed directly at the level of probability distributions, and we run into the difficulties mentioned in the previous paragraph, or the dynamics is *fundamentally* expressed in terms of trajectories (remembering that the discussion takes place in a classical framework), probabilities are a very useful tool, whose properties are *derived* mathematically from those of the trajectories, and nothing radically new has been done. In [98][24], Prigogine emphasizes the "irreducible" spectral decompositions of the so-called Perron-Frobenius operator. This is a rather technical notion, which I will discuss in Appendix 2. It suffices to say here that this will not solve the dilemma raised above. If one reformulates the laws of physics, or understands them differently, or whatever, there is still presumably something that evolves, in some fashion. The question is: what evolves, and how?

What the example of the billiard ball also shows is that we must distinguish different levels of analysis. First of all, we may describe the system in a certain way: we may assign to the ball at least an approximate position at each time, hence an approximate trajectory[25]. Certainly the ball is not *everywhere*, as the "irreducible" probabilistic description would suggest. The next thing we can do is to try to find exact or approximate laws of motion for the ball. The laws of elastic reflection against obstacles, for example. Finally, we may try to solve the equations of motion. We may not be able to perform the last step. But this does not mean that one should give up the previous ones. We may even realize that our laws are only approximate (because of friction, of external perturbations, etc...). But why give up the notion of (approximate) trajectories? Of course, since we are not able to predict the evolution of trajectories one may *choose* to study instead the evolution of probability distributions. This is perfectly reasonable, as long as one does not forget that, in doing so, we are not only studying the physical system but also our ability or inability to analyse it in more detail. This will be very important in the next Section.

[23] And somewhat more importantly, what does the general educated public, which reads the popular books, understand from such sentences? For example, in a review in "Le Monde" of Prigogine's most recent book [101], Roger-Pol Droit notes that: "The main discovery explained in "La fin des certitudes" is the possibility to consider trajectories as probabilistic quantities and to express the laws of dynamics in terms of ensembles." ([28]). If one understand probabilities in the classical sense, this is perfectly acceptable, but it is not exactly a discovery (statistical mechanics is more than a century old). And if it is a major discovery, what are these new laws of dynamics?

[24] And in a private communication.

[25] I say "approximate", because I describe the system as it is seen. I do not yet consider any theory (classical or quantum).

At this point, I want to briefly discuss the classical status of probability in physics, i.e. of probability as "ignorance". This will also be very important in the next Section. To quote Laplace again: "The curve described by a molecule of air or of vapour is following a rule as certainly as the orbits of the planets: the only difference between the two is due to our ignorance. Probabilility is related, in part to this ignorance, in part to our knowledge."[67] Let us consider the usual coin-throwing experiment. We assign a probability 1/2 to heads and 1/2 to tails. What is the logic of the argument? We examine the coin, and we find out that it is fair. We also know the person who throws the coin and we know that he does not cheat. But we are unable to control or to know exactly the initial conditions for each throw. We can however determine the average result of a large number of throws. This is simply because, if one considers as a single "experiment" N consecutive throws of a coin, the overwhelming majority (for N large) of the possible results will have an approximately equal number of heads and of tails. It is as simple as that, and there will be nothing conceptually more subtle in the way we shall use probabilities below. The part "due to our ignorance" is simply that we *use* probabilistic reasoning. If we were omniscient, it would not be needed (but the averages would remain what they are, of course). The part "due to our knowledge" is what makes the reasoning work. We could make a mistake: the coin could be biased, and we did not notice it. Or we could have a "record of bad luck" and have many more heads than tails. But that is the way things are: our knowledge *is* incomplete and we have to live with that. Nevertheless, probabilistic reasoning is extraordinarily successful in practice, but, when it works, this is due to our (partial) knowledge. It would be wrong to attribute any constructive role to our ignorance. And it is also erroneous to assume that the system must be somehow indeterminate, when we apply probabilistic reasoning to it. Finally, one could rephrase Laplace's statement more carefully as follows: "Even if the curve described by a molecule of air follows a rule as certainly as the orbits of the planets, our ignorance would force us to use probabilistic reasonings".

3 Irreversibility and the arrow of time

Since in the differential equations of mechanics
themselves there is absolutely nothing analogous to the
Second Law of thermodynamics the latter can be mechanically
represented only by means of assumptions regarding initial conditions.
L. Boltzmann ([11], p.170)

3.1. The problem.

What is the problem of irreversibility? The basic physical laws are reversible, which simply means that, if we consider an isolated system of particles, let it evolve for a time t, then reverse exactly the velocities of all the particles, and let the system again evolve for a time t, we get the original system at the initial time with all velocities reversed[26].

[26] Mathematically, the microscopic state of the system is represented by a point in its "phase space" Ω. Each point in that space represents the positions and the velocities of *all* the particles of the system

Now, there are lots of motions that we see, without ever observing their associated "time-reversed" motion: we go from life to death but not vice versa, coffee does not jump out of the cup, mixtures of liquids do not spontaneously unmix themselves. Some of these examples taken from everyday life involve non-isolated systems, but that is not relevant[27]. I shall center the discussion below on the canonical physical example (and argue that the other situations can be treated similarly): consider a gas that is initially compressed by a piston in the left half of a box; the piston is then released so that the gas expands into the whole container. We do not expect to see the particles to go back to the left half of the box, although such a motion would be as compatible with the laws of physics as the motion that does take place. So, the question, roughly speaking, is this: if the basic laws are reversible, why do we see some motions but never their time-reversed ones?

The first point to clarify is that this irreversibility does not lead to a *contradiction* with the basic physical laws[28]. Indeed, the laws of physics are always of the form: given some initial conditions, here is the result after some time. But they never tell us how the world *is or evolves*. In order to account for that, one always needs to assume something about the initial conditions. The laws of physics are compatible with lots of possible worlds: there could be no earth, no life, no humans. Nothing of that would contradict the fundamental physical laws. So, it is hard to see what kind of argument would imply a contradiction between the reversibility of the laws and the existence of irreversible phenomena. But no argument at all is given, beyond a vague appeal to intuition, as for example: "No speculation, no body of knowledge ever claimed the equivalence between doing and undoing, between a plant that grows, has flowers and dies, and a plant that resuscitates, becomes younger and goes back to its primitive seed, between a man who learns and becomes mature and a man who becomes progressively a child, then an embryo , and finally a cell. Yet, since its origins, dynamics, the physical theory that

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under consideration. So the phase space is $\mathbf{R}^{6 \cdot N}$ where N is the number of particles (of the order of 10^{23} for a macroscopic system), since one needs three coordinates for each position and three coordinates for each velocity. Hamilton's equations of motion determine, for each time t, a map T^t that associates to each initial condition $\mathbf{x} \in \Omega$, at time zero, the corresponding solution $T^t\mathbf{x}$ of the equations of motion at that time. Reversibility of the equations of motion means that there is a transformation (an involution) I acting on Ω that satisfies the following relation:

$$T^t I T^t \mathbf{x} = I\mathbf{x}, \tag{1}$$

or $IT^t = T^{-t}I$. In classical mechanics, I reverses velocities, without changing the positions. (In quantum mechanics, Ω is replaced by a Hilbert space, and I associates to a wave function its complex conjugate. For the role of weak interactions, see Feynman [38].)

[27] We shall discuss in Section 4.3 a frequent confusion that assigns the *source* of irreversibility to the (true but irrelevant) fact that no system is ever perfectly isolated. But let us point out here that it is easy to produce non-isolated systems that behave approximately in "time reversed" fashion: a refrigerator, for example. Living beings also seem to violate the Second Law of thermodynamics. But put a cat in a well-sealed box for a sufficiently long time and it will evolve towards equilibrium.

[28] Such a contradiction is suggested by the following statement of Prigogine and Stengers: "Irreversibility is either true on all levels or on none: it cannot emerge as if out of nothing, on going from one level to another"([96], quoted by Coveney, [20], p.412.) Also, "Irreversibility is conceivable only if the notion of point or of trajectory loose their meaning" [100], p.166.

identifies itself with the triumph of science, implied this radical negation of time." ([95], p.25. The first of these sentences is quoted again in [101], p.178). But nobody says that there is an "equivalence" between the two motions, only that both are compatible with the laws of physics. Which one, if any, occurs depends on the initial conditions. And, if the laws are deterministic, assumptions about the initial conditions are ultimately assumptions about the initial state of the Universe.

Once one has remarked that, a priori, there is no contradiction between irreversibility and the fundamental laws, one could stop the discussion. It all depends on the initial conditions, period. But this is rather unsatisfactory, because, if one thinks about it, one realizes that too many things could be "explained" by simply appealing to initial conditions. Luckily, much more can be said. It is perfectly possible to give a natural account of irreversible phenomena on the basis of reversible fundamental laws, and of suitable assumptions about initial conditions. This was essentially done a century ago by Boltzmann, and despite numerous misunderstandings and misguided objections (some of them coming from famous scientists, such as Zermelo or Poincaré), his explanation still holds today. Yet, Prigogine writes ([98], p.41): "He (Boltzmann) was forced to conclude that the irreversibility postulated by thermodynamics was incompatible with the reversible laws of dynamics"[29]. This is in rather sharp contrastwith Boltzmann's own words: "From the fact that the differential equations of mechanics are left unchanged by reversing the sign of time without anything else, Herr Ostwald concludes that the mechanical view of the world cannot explain why natural processes run preferentially in a definite direction. But such a view appears to me to *overlook that mechanical events are determined not only by differential equations, but also by initial conditions.* In direct contrast to Herr Ostwald I have called it one of the most brilliant confirmations of the mechanical view of Nature that it provides an extraordinarily good picture of the dissipation of energy, as long as one assumes that the world began in an initial state satisfying certain initial conditions" (italics are mine; quoted in [69], replies, p.115). I will now explain this "brilliant confirmation of the mechanical view of Nature", and show that all the alleged contradictions are illusory[30].

3.2. The classical solution.[31]

First of all, I should say that Boltzmann gives a *framework* in which to account for irreversible phenomena on the basis of reversible microscopic laws. He does not explain in detail every concrete irreversible phenomenon (like diffusion, or the growth of a plant). For that, more work is needed and, while the general framework that I shall discuss uses very little of the properties of the microscopic dynamics, the latter may be important in

[29] I. Stengers goes even further: "The reduction of the thermodynamic entropy to a dynamical interpretation can hardly be viewed otherwise than as an "ideological claim"..." ([113], p.192). We shall see below in what precise sense this "reduction" is actually a "scientific claim".

[30] This is of course not new at all. Good references, apart from Boltzmann himself, [11], include Feynman [38], Jaynes [56], Lebowitz [68, 69], Penrose [87], and Schrödinger [107].

[31] By classical, I mean "'standard". However, all the discussion will take place in the context of classical physics. I will leave out quantum mechanics entirely. Although the quantum picture may be more complicated, I do not believe that it renders obsolete the basic ideas explained here.

the explanation of specific irreversible phenomena[32].

Let us now see which systems do behave irreversibly. A good test is to record the behaviour of the system in a movie, and then to run the movie backwards. If it looks funny (e.g. people jump out of their graves), then we are facing irreversible behaviour. It is easy to convince oneself that all the familiar examples of irreversible behaviour involve systems with a large number of particles (or degrees of freedom). If one were to make a movie of the motion of one molecule, the backward movie would look completely natural. The same is true for a billiard ball on a frictionless billiard table[33]. If, however, friction is present, then we are dealing with many degrees of freedom (the atoms in the billiard table, those in the surrounding air etc...).

There are two fundamental ingredients in the classical explanation of irreversibility, in addition to the microscopic laws. The first has already been introduced: initial conditions. The second is suggested by the remark that we deal with systems with many degrees of freedom: we *have* to distinguish between microscopic and macroscopic variables. Let us consider the phase space Ω (see note 26) of the system, so that the system is represented by a point $\mathbf{x}$ in that space and its evolution is represented by a curve $\mathbf{x}(t) = T^t(\mathbf{x})$. Various quantities of physical interest, for example the density, or the average energy, or the average velocity in a given cubic millimeter, can be expressed as functions on Ω[34]. These functions (call them F) tend to be many-to-one, i.e. there are typically a huge number of configurations giving rise to a given value of F[35]. For example, if F is the total energy, then it takes a constant value on a surface in phase space. But if F is the number of particles in a cubic millimeter, there are also lots of microscopic configurations corresponding to a given value of F. Now, let me make two statements, the first of which is trivial and the second not. Given a microscopic initial configuration $\mathbf{x}_0$, giving rise to a trajectory $\mathbf{x}(t)$, any function on phase space follows an induced evolution $F_0 \to F_t$, where $F_0 = F(\mathbf{x}_0)$, and $F_t = F(\mathbf{x}(t))$ (here and below, I shall assume that t is positive). That is the trivial part. The non-trivial observation is that, in many situations, one can find a suitable family of functions (I'll still denote by F such a family) so that this induced evolution is actually (approximately) *autonomous*. That is, one can determine F_t given F_0 alone, without having to know the microscopic configuration from which it comes[36]. This means that the different microscopic configurations on which F takes the value F_0, will induce the same evolution on F_t. A very trivial example is given by the globally conserved quantities (like the total energy): for all microscopic

[32] This is analogous to the theory of natural selection. The latter provides a scheme of explanation for the appearance of complex organs, but more detailed arguments are needed to account for concrete properties of living beings. See Section 6 for further discussion of this analogy.

[33] Of course, the billiard ball itself contains many molecules. But the rigidity of the ball allows us to concentrate on the motion of its center of mass.

[34] By "functions", I mean also families of functions indexed by space or time, i.e. fields, such as the local energy density, or the velocity field.

[35] I am a little vague on how to "count" configurations. If I consider discrete (finite) systems, then it is just counting. Otherwise, I use of course the Lebesgue measure on phase space. All statements about probabilities made later will be based on such "counting".

[36] Although not trivial, this expresses the fact that reproducible macroscopic experiments exist and that a deterministic macroscopic description of the world is possible.

configurations, $F_t = F_0$, for all times. But that is not interesting. It is more interesting to observe that the solutions of all the familiar macroscopic equations (Navier-Stokes, Boltzmann, diffusion, ...) can be considered as defining such an induced evolution $F_0 \to F_t$. Actually, there are several provisos to be made here: first of all, it is not true that *all* microscopic configurations giving rise to F_0 lead to the same evolution for F_t. In general, only the (vast) majority of microscopic configurations do that[37]. Moreover, if we want that evolution to hold for all times, then this set of microscopic configurations may become empty[38]. Finally, the laws used in practice may contain some further approximations.

So, the precise justification of a macroscopic law should be given along the following lines: given F_0, and given a (not too large) time T[39], there exists a large subset of the set of $\mathbf{x}$'s giving rise to F_0 (i.e. of the preimage in Ω, under the map F, of F_0) such that the induced evolution of F_t is approximately described by the relevant macroscopic equations up to time T. It should be obvious that it is not easy to prove such a statement. One has to deal with dynamical systems with a large number of degrees of freedom, about which very little is known, and in addition one has to identify limits in which one can make sense of the approximations mentioned above (a large subset, a not too large time T ...). Nevertheless, this can be done in some circumstances, the best known being probably the derivation of Boltzmann's equation by Lanford [64, 65, 66]. In Appendix 1, I discuss a model due to Mark Kac which, while artificially simple, can be easily analysed and shows exactly what one would like to do in more complicated situations[40].

Let us come back to the problem of irreversibility: should we expect those macroscopic laws to be reversible? A priori, not at all. Indeed, I have emphasized in the abstract description above the role of initial conditions in their derivation[41]. The macro-

[37] To make the micro/macro distinction sharp, one has to consider some kind of limit (hydrodynamic, kinetic, etc...), where the number of particles (and other quantities) tend to infinity. That is a convenient mathematical setting to prove precise statements. But one should not confuse this limit, which is an approximation to the real world, with the physical basis of irreversibility. See Lebowitz [68] and Spohn [112] for a discussion of those limits.

[38] This is due, for example, to the Poincaré recurrences, see Section 4.1 and Appendix 1.

[39] I mean shorter than the Poincaré recurrence time.

[40] It is also important to clarify the role of "ensembles" here. What we have to explain is the fact that, when a system satifies certain macroscopic initial conditions (F_0) it *always* (in practice) obeys certain macroscopic laws. The same macroscopic initial conditions will correspond to many different microscopic initial conditions. We may introduce, for mathematical convenience, a probability distribution (an "ensemble") on the microscopic initial conditions. But one should remember that we are physically interested in "probability one statements", namely statements that hold for (almost) all microscopic configurations, as opposed to statements about averages, for example. Otherwise, one would not explain why all individual macroscopic systems satisfy a given law. In practice, those "probability one statements" will only hold in some limit. Physically, they should be interpreted as "very close to one" for finite systems with a large number of particles. To see how close to one this is, consider all the bets and games of chance that have ever taken place in human history. One would certainly expect the laws of large numbers to apply to such a sample. But this number is minuscule compared to the typical number (10^{23}) of molecules in a cubic centimeter.

[41] This remark is of some interest for the issue of *reductionism*: higher level laws, such as the macroscopic laws, are reduced to the microscopic ones *plus* assumptions on the initial conditions. If this is the case in statistical mechanics, where it is usually granted that reductionism works, it should clarify the situation in other fields, like biology, where reductionism is sometimes questioned. In particular,

scopic equations may be reversible or not, depending on the situation. But since *initial* conditions enter their derivation, there is no *logical* reason to expect them to be reversible[42].

3.3. The reversibility objection.

Let me illustrate this explanation of irreversibility in a concrete physical example (see also Appendix 1 for a simple mathematical model). Consider the gas introduced in Section 3.1 that is initially compressed by a piston in the left half of a box, and that expands into the whole box. Let F be the density of the gas. Initially, it is one (say) in one half of the box and zero in the other half. After some time t, it is (approximately) one half everywhere. The explanation of the irreversible evolution of F is that the overwhelming majority of the microscopic configurations corresponding to the gas in the left half, will evolve deterministically so as to induce the observed evolution of F. There may of course be some exceptional configurations, for which all the particles stay in the left half. All one is saying is that those configurations are extraordinarily rare, and that we do not expect to see even one of them appearing when we repeat the experiment many times, not even once "in a million years", to put it mildly [38] (see the end of note (40)).

This example also illustrates the answer to the reversibility objection. Call "good" the microscopic configurations that lead to the expected macroscopic behaviour. Take all the good microscopic configurations in the left half of the box, and let them evolve until the density is approximately uniform. Now, reverse all the velocities. We get a set of configurations that still determines a density one half in the box. However, they are not good. Indeed, from now on, if the system remains isolated, the density just remains uniform according to the macroscopic laws. But for the configurations just described, the gas will move back to the left half, leading to a gross violation of the macroscopic law. What is the solution? Simply that those "reversed-velocities" configurations form a very tiny subset of all the microscopic configurations giving rise to a uniform density. And, of course, the original set of configurations, those coming from the left half of the box, also form such a small subset. Most configurations corresponding to a uniform

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the fact that some assumptions must be made on the initial conditions in going from the microscopic to the macroscopic should not be forgotten, nor should it be held as an argument against reductionism. Another frequent confusion about reductionism is to remark that the macroscopic laws do not uniquely determine the microscopic ones. For example, many of the macroscopic laws can be derived from stochastic microscopic laws or from deterministic ones. That is true, but does not invalidate reductionism. What is true on the microscopic level has to be discovered independently of the reductionist programme. Finally, what is considered microscopic or macroscopic is a question of scale. The classical description considered here at the "microscopic" level is an approximation to the quantum description and neglects the molecular and atomic structure. And the "macroscopic" level may in turn be considered microscopic if one studies large-scale motions of the atmosphere. But, despite frequent claims to the contrary, reductionists are quite happy not to explain carburetors directly in terms of quarks (see Weinberg [120] for a good discussion of reductionism).

[42] Note that I am not discussing irreversibility in terms of the increase of entropy, but rather in terms of the macroscopic laws. After all, when we observe the mixing of different fluids, we see a phenomenon described by the diffusion equation, but we do not see entropy flowing. The connection with entropy will be made in Section 5.

density do not go to the left half of the box, neither in the future nor in the past (at least for reasonable periods of time, see Sect. 4.1). So that, if we prepare the system with a uniform density, we do not expect to "hit" even once one of those bad configurations[43].

Now comes a real problem. We are explaining that we never expect to get a microscopic configuration that will lead all the gas to the left of the box. *But we started from such a configuration.* How did we get there in the first place? The real problem is not to explain why one goes to equilibrium, but why there are systems out of equilibrium to start with. For the gas, obviously the system was not isolated: an experimentalist pushed the piston. But why was there an experimentalist? Human beings are also systems out of equilibrium, and they remain so (for some time) thanks to the food they eat, which itself depends on the sun, through the plants and their photosynthesis. Of course, in order to be able to take advantage of their food, humans also need their genetic program, which itself results from the long history of natural selection.

As discussed e.g. in Penrose [87], the earth does not gain energy from the sun (that energy is re-radiated by the earth), but low entropy (likewise, we seek low entropy rather than energy in our food); the sun sends (relatively) few high energy photons and the earth re-radiates more low energy photons (in such a way that the total energy is conserved). Expressed in terms of "phase space", the numerous low energy photons occupy a much bigger volume than the incoming high energy ones. So, the solar system, as a whole, moves towards a larger part of its phase space while the sun burns its fuel. That evolution accounts, by far, for what we observe in living beings or in other "self-organized" structures[44]. I shall come back to this point in Section 6. Of course, for the sun to play this role, it has to be itself out of equilibrium, and to have been even more

[43] To put it in formulas, let $\overline{\Omega}_t$ be the configurations giving to F its value at time t. If we denote by F_t that value, $\overline{\Omega}_t$ is simply the preimage of F_t under the map F. Let Ω_t be the set of good configurations, at time t, that lead to a behaviour of F, for later times (again, not for *too* long, because of Poincaré recurrences), which is described by the macroscopic laws. In general, Ω_t is a very large subset of $\overline{\Omega}_t$, but is not identical to $\overline{\Omega}_t$. Thus, $\overline{\Omega}_0$ are all the configurations in the left half of the box at time zero, and Ω_0 is the subset consisting of those configurations whose evolution lead to a uniform density. Microscopic reversibility says that $T^t(I(T^t(\Omega_0))) = I(\Omega_0)$ (this is just (1) in note (26) applied to Ω_0). A reversibility paradox would follow from $T^t(I(\Omega_t)) = I(\Omega_0)$ (one takes all the good configurations at time t, reverses their velocities, lets them evolve for a time t and thereby gets the original set of initial conditions, with velocities reversed). But Ω_t is *not equal*, in general, to $T^t(\Omega_0)$ (and this is the source of much confusion). In our example, $T^t(\Omega_0)$ is a tiny subset of Ω_t, because most configurations in Ω_t were not in the left half of the box at time zero. Actually, $I(T^t(\Omega_0))$ provides an example of configurations that belong to $\overline{\Omega}_t$ but not to Ω_t. These configurations correspond to a uniform density at time t, but not at time $2t$.

[44] Failure to realize this leads to strange statements, as for example in Cohen and Stewart ([19], p.259): speaking of the evolution since the Big Bang, the authors write: "For systems such as these, the thermodynamic model of independent subsystems whose interactions switch on but not off is simply irrelevant. The features of thermodynamics either don't apply or are so long term that they don't model anything interesting. Take Cairns-Smith's scenario of clay as scaffolding for life. The system consisting of clay alone is *less* ordered than that of clay plus organic molecules: Order is increasing with time. Why?" The explanation given afterwards ignores both the action of the sun, and the original "improbable state" discussed here. As Ruelle wrote in a review of this book "if life violates the Second Law, why can't one build a power plant (with some suitable life forms in it) producing ice cubes and water currents from the waters of Loch Ness?" ([105]). A similar confusion can be found in Popper: "This law of the increase of disorder, interpreted as a cosmic principle, made the evolution of life incomprehensible, apparently even paradoxical." ([93], p.172)

so in the past. We end up with an egg and hen problem and we have ultimately to assume that the Universe started in a state far from equilibrium, an "improbable state" as Boltzmann called it. To make the analogy with the gas in the box, it is as if the Universe had started in a very little corner of a huge box[45].

To account in a natural way for such a state is of course a major open problem, on which I have nothing to say (see Penrose [87] for further discussion, and Figure 7.19 there for an illustration), except that one cannot avoid it by "alternative" explanations of irreversibility. Given the laws of physics, as they are formulated now, the world could have started in equilibrium, and then we would not be around to discuss the problem[46]. To summarize: the only real problem with irreversibility is not to explain irreversible behaviour in the future, but to account for the "exceptional" conditions of the Universe in the past.

3.4. Chaos and irreversibility.

Now, I come to my basic criticism of the views of Prigogine and his collaborators, who argue that dynamical systems with very good chaotic properties, such as the baker's map, are "intrinsically irreversible". Let me quote from a letter of a collaborator of Prigogine, D. Driebe [27], criticizing an article of Lebowitz [69] explaining Boltzmann's ideas. This letter is remarkably clear and summarizes well the main points of disagreement. "If the scale-separation argument were the whole story, then irreversibility would be due to our approximate observation or limited knowledge of the system. This is difficult to reconcile with the constructive role of irreversible processes... Irreversibility is not to be found on the level of trajectories or wavefunctions but is instead manifest on the level of probability distributions... Irreversible processes are well observed in systems with few degrees of freedom, such as the baker and the multibaker transformations... The arrow of time is not due to some phenomenological approximations but is an intrinsic property of classes of unstable dynamical systems"[47].

Let us discuss these claims one by one. First of all, as I emphasized above, the scale-separation (i.e. the micro/macro distinction) is not "the whole story". Initial conditions have to enter into the explanation (and also the dynamics, of course). Next, what does it mean that "irreversible processes are observed in systems such as the baker transformation"? This transformation describes a chaotic system with few degrees of freedom, somewhat like the billiard ball on a frictionless table[48]. For those systems, there is no sense of a micro/macro distinction: how could one define the macroscopic variables? To put it otherwise, we can make a movie of the motion of a point in the plane evolving under the baker's map, or of a billiard ball, or of any isolated chaotic

[45] I neglect here the effect of gravity: see Penrose [87].

[46] As Feynman says: "Therefore I think it necessary to add to the physical laws the hypothesis that in thepast the universe was more ordered, in the technical sense, than it is today - I think this is the additional statement that is needed to make sense, and to make an understanding of the irreversibility." ([38], p.116)

[47] In a recent textbook one reads, after a discussion of the baker's map: "Irreversibility appears only because the instantaneous state of the system cannot be known with an infinite precision" ([119], p.198).

[48] The baker map is quite similar to the map discussed in note 10, and has the same chaotic properties as the latter, but is invertible.

system with few degrees of freedom, and run it backwards, we shall not be able to tell the difference. There is nothing funny or implausible going on, unlike the backward movie of any real irreversible macroscopic phenomenon. So, the first critique of this alleged connection between unstable dynamical systems (i.e. what I call here chaotic systems) and irreversibility is that one "explains" irreversibility in systems in which nothing irreversible happens, and where therefore there is nothing to be explained.

It is true that probability distributions for those systems evolve "irreversibly", meaning that any (absolutely continuous, see note 22) probability distribution will spread out all over the phase space and will quickly tend to a uniform distribution. This just reflects the fact that different points in the support of the initial distribution, even if they are close to each other initially, will be separated by the chaotic dynamics. So, it is true, in a narrow sense, that "irreversibility is manifest on the level of probability distributions". But what is the physical meaning of this statement? A physical system, chaotic or not, is described by a trajectory in phase space, and is certainly not described adequately by the corresponding probability distributions. As I discussed in Section 2.2, the latter reflects, in part, our ignorance of that trajectory. Their "irreversible" behaviour in this sense is therefore not a genuine physical property of the system. We can, if we want, focus our attention on probabilities rather than on trajectories, but that "choice" cannot have a basic role in our explanations.

One cannot stress strongly enough the difference between the role played by probabilities here and in the classical solution. In the latter, we use probabilities as in the coin-throwing experiment. We have some macroscopic constraint on a system (the coin is fair; the particles are in the left half of the box), corresponding to a variety of microscopic configurations. We predict that the behaviour of certain macroscopic variables (the average number of heads; the average density) will be the one induced by the vast majority of microscopic configurations, compatible with the initial constraints. That's all. But it works only because a large number of variables are involved, *in each single physical system*. However, each such system is described by a point in phase space (likewise, the result of many coin throwings is a particular sequence of heads and tails). In the "intrinsic irreversibility" approach, a probability distribution is assigned to *each single physical system*, as an "irreducible" description. The only way I can make sense of that approach is to consider a *large number* of billiard balls or of copies of the baker's map, all of them starting with nearby initial conditions. Then, it would be like the particles in the box, the average density would tend to become uniform, and we are back to the standard picture. But this does not force us to "rethink the notion of law of nature".

3.5. Is irreversibility subjective?

I will now discuss the alleged "subjectivity" of this account of irreversibility (i.e., that it is due to our approximate observation or limited knowledge of the system). I shall consider in Section 6 the "constructive role" of irreversible processes, mentioned in Driebe's letter [27]. Branding Boltzmann's ideas as "subjective" is rather common. For example, Prigogine writes: "In the classical picture, irreversibility was due to our approximations, to our ignorance." ([98], p.37) But, thanks to the existence of unstable dynamical systems, "the notion of probability that Boltzmann had introduced in order to express the arrow of time does not correspond to our ignorance and acquires an objective meaning" ([98], p.42)[49]. To use Popper's image: "Hiroshima is not an illusion" (I shall come back to Popper's confusions in Section 4.4.). This is only a dramatization of the fact that irreversible events are not subjective, or so it seems. The objection is that, if the microscopic variables behave reversibly and if irreversibility only follows when we *"choose"* to concentrate our attention on macroscopic variables, then our explanation of irreversibility is unavoidably tainted by subjectivism. I think that this charge is completely unfair, and reflects some misunderstanding of what irreversible phenomena really are. The point is that, upon reflection, one sees that all irreversible phenomena deal with these macroscopic variables. There is no subjectivism here: the evolution of the macroscopic variables is objectively determined by the microscopic ones, and they behave as they do whether we look at them or not. In that sense they are completely objective. But it is true that, if we look at a single molecule, or at a collection of molecules represented by a point in phase space, there is no sense in which they evolve "irreversibly", if we are not willing toconsider some of the macroscopic variables that they determine.

However, the apparently "subjective" aspect of irreversibility has been sometimes overemphasized, at least as a way to speak. Heisenberg wrote: "Gibbs was the first to introduce a physical concept which can only be applied to an object when our knowledge of the object is incomplete. If for instance the motion and the position of each molecule in a gas were known, then it would be pointless to continue speaking of the temperature of the gas."([50], p.38)[50]. And Max Born said: "Irreversibility is therefore a consequence of the explicit introduction of ignorance into the fundamental laws." ([14], p.72). These formulations, although correct if they are properly interpreted, lead to unnecessary confusions. For example, Popper wrote: "It is clearly absurd to believe that pennies fall or molecules collide in a random fashion *because we do not know* the initial conditions, and that they would do otherwise if some demon were to give their secret away to us: it is not only impossible, it is absurd to explain objective statistical frequencies by subjective ignorance." ([92], p.106)[51]. However, just after saying this, Popper gives what he calls "an objective probabilistic explanation of irreversible processes" ([92], p.107), attributed to Planck, which, as far as I can tell, is not very different from what I call the classical solution. The source of the confusion comes from two uses of the word "knowledge". Obviously, the world does what it does, whether we know about it or not. So, indeed,

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[49]See e.g. Coveney ([20], p.412): "Another quite popular approach has been to relegate the whole question of irreversibility as illusory." See also Lestienne ([71], p.176) and Prigogine and Stengers ([94] p.284) for similar remarks.

[50]Pauli made a similar remark, see [84], quoted in Popper, [92], p.109.

[51]In his textbook on Statistical Mechanics; S.-K. Ma shows similar concerns: "In one point of view, probability expresses the knowledge of the observer. If he knows more about the system, the probability is more concentrated. This is obviously incorrect. The motion of the system is independent of the psychological condition of the observer." ([74], p.448) And H. Bondi wrote: "It is somewhat offensive to our thought to suggest that if we know a system in detail then we cannot tell which way time is going, but if we take a blurred view, a statistical view of it, that is to say throw away some information, then we can..." ([12], quoted in [63], p.135. T. Gold expressed similar views, see [63]).

if "some demon" were to provide us with a detailed knowledge of the microscopic state of the gas in the left half of the box, nothing would change to the future evolution of that gas. But we may imagine situations where one can *control* more variables, hence to "know" more about the system. When the piston forces the gas to be in the left half of the box, the set of available microscopic states is different than when the piston is not there, and obviously we have to take that "knowledge" into account. But there is nothing mysterious here.

I believe that statistical mechanics would become easier to understand by students if it were presented without using an anthropomorphic language and subjective sounding notions such as information, observation or knowledge. Or, at least, one should explain precisely why these notions are introduced and why they do not contradict an objectivist view of natural phenomena (see the writings of Jaynes on this point [56, 59]). But I also believe that the charge of subjectivity should be completely reversed: to "explain" irreversibility through the behaviour of probability distributions (which *are* describing our ignorance), as Prigogine does, is to proceed as if the limitations of human knowledge played a fundamental physical role.

4 Some misconceptions about irreversibility

The Second Law can never be proved mathematically
by means of the equations of dynamics alone.
L. Boltzmann ([11], p.204).

4.1. The Poincaré recurrence theorem.

According to Prigogine ([98], p.23) Poincaré did not recommend reading Boltzmann, because his conclusions were in contradiction with his premises. Discussing our example of a gas expanding in a container, Prigogine observes that "if irreversibility was only that, it would indeed be an illusion, because, if we wait even longer, then it may happen that the particles go back to the same half of the container. In this view, irreversibility would simply be due to the limits of our patience." ([98], p.24) This is basically the argument derived from the Poincaré recurrence theorem (and used by Zermelo against Boltzmann [122]), which says that, if the container remains isolated long enough, then indeed the particles will return to the half of the box from which they started. Replying to that argument, Boltzmann supposedly said "You should live that long". For any realistic macroscopic system, the Poincaré recurrence times (i.e. the time needed for the particles to return to the left half of the box) are much much larger than the age of the universe. So that again no contradiction can be derived, from a physical point of view, between Boltzmann's explanations and Poincaré's theorem. However, there is still a mathematical problem (and this may be what Poincaré had in mind): if one tries to rigorously derive an irreversible macroscopic equation from the microscopic dynamics and suitable assumptions on initial conditions, the Poincaré recurrence time will put a limit on the length of the time interval over which these statements can be proven. That is one of the reasons why one discusses these derivations in suitable limits (e.g. when the number of particles goes to infinity) where the Poincaré recurrence time becomes infinite. But one should not confuse the fact that one takes a limit for mathematical convenience and the source of irreversibility. In the Kac model discussed in Appendix 1, one sees clearly that there are very different time scales: one over which convergence to equilibrium occurs, and a much larger one, where the Poincaré recurrence takes place. But the first time scale is not an "illusion". In fact, it is on that time scale that all phenomena that we can possibly observe do take place.

4.2. Ergodicity and mixing.

One often hears that, for a system to reach "equilibrium", it must be ergodic, or mixing. The fact is that those properties, like the "intrinsic irreversibility" discussed above, *are neither necessary nor sufficient* for a system to approach equilibrium. Let me start with ergodicity. A dynamical system is *ergodic* if the average time spent by a trajectory in any region of the phase space is proportional to the volume of that region. To be more precise: average means in the limit of infinite time and this property has to hold for all trajectories, except (possibly) those lying in a subset of zero volume. One says that it holds for "almost all" trajectories. This property implies that, for any reasonable function on phase space, the average along almost all trajectories will equal the average over the phase space[52]. Then, the argument goes, the measurement of any physical quantity will take some time. This time is long compared to the "relaxation time" of molecular processes. Hence, we can approximately regard it as infinite. Therefore, the measured quantity, a time average, will approximately equal the average over phase space of the physical quantity under consideration. But this latter average is exactly what one calls the equilibrium value of the physical quantity. So, according to the usual story, if a dynamical system is ergodic, it converges towards equilibrium. This appeal to ergodicity in order to justify statistical mechanics is rather widespread[53] even though it has been properly criticized for a long time by, e.g., Tolman [117], p.65, Jaynes [56],

[52]In formulas, let Ω be the "phase space" on which the motion is ergodic (i.e. a constant energy surface, which is a subset of the space considered in note 26, on which is defined the measure induced by the Lebesgue measure, normalised to one, and denoted $d\mathbf{x}$). Then, ergodicity means that for F integrable,

$$\lim_{T\to\infty} \frac{1}{T}\int_0^T F(T^t\mathbf{x})dt = \int_\Omega F(\mathbf{x})d\mathbf{x}, \tag{2}$$

for almost all initial conditions $\mathbf{x} \in \Omega$. The LHS is the time average and the RHS the space average. If we take for F the characteristic function of a (measurable) set $A \subset \Omega$, the time average equals the fraction of time spent by the trajectory in A, and the space average is the volume of A.

[53]For a history of the concept of ergodicity, and some very interesting modern developments, see Gallavotti [39]. It seems that the (misleading) emphasis on the modern notion of ergodicity goes back to the Ehrenfests' paper [33], more than to Boltzmann. A careful, but nevertheless exaggerated interest in ergodicity and mixing is found in the work of Khinchin [61] and Krylov [62]; it is also found e.g. in Chandler [18], p.57, Hill [53], p.16, S. K. Ma [74] Chap.26, Thompson [116], App.B, and Dunford and Schwartz [30], p.657 (but see Schwartz [109] for a self-criticism of [30]); in the recent textbook of Vauclair [119], one reads: "One considers that during the time δt of the measurement, the system has gone through all the possibly accessible states, and that it spent in each state a time proportional to its probability." (p.11) And: "Only the systems having this property (mixing) tend to an equilibrium state, when they are initially in a state out of equilibrium." (p.197).

p.106, and Schwartz [109].

Let us see the problems with this argument: a well-known, but relatively minor, problem is that it is very hard to give a mathematical proof that a realistic mechanical system is ergodic. But let us take such a proof for granted, for the sake of the discussion. Here is a more serious problem. Assume that the argument given above is true: how would it then be possible to observe or measure *any non-equilibrium* phenomenon? In the experiment with the box divided in two halves, we should not be able to see any intermediate stage, when the empty half gets filled, since the time for our measurements is supposed to be approximately infinite. So, where is the problem? We implicitly identified the "relaxation time" with what one might call the "ergodic time", i.e. the time taken by the system to visit all regions of phase space sufficiently often so that the replacement of time averages by spatial averages is approximately true. But, whatever the exact meaning of the word "relaxation time" (for a few molecules) is, the ergodic time is certainly enormously longer. Just consider how large is the volume in phase space that has to be "sampled" by the trajectory. For example, all the particles could be in the right half of the box, and ergodicity says that they will spend some time there (note that this is not implied by Poincaré's theorem; the latter only guarantees that the particles will return to the part of the box from which they started, i.e. the left half here). To be more precise, let us partition the phase space into a certain number of cells, of a given volume, and consider the time it takes for a given trajectory to visit each cell, even once, let us say[54]. That, obviously, will depend on the size (hence, on the number) of the cells. By taking finer and finer partitions, we can make that time as large as one wishes. So, if one were to take the argument outlined above literally, the "ergodic time" is infinite, and speaking loosely about a relaxation time is simply misleading.

At this point of the discussion, one often says that we do not need the time and space average to be (almost) equal for all functions, but only for those of physical relevance (like the energy or particle densities). This is correct, but the criticism of the "ergodic" approach then changes: instead of not being *sufficient* to account for irreversibility, we observe that it is not *necessary*. To see this, consider another partition of phase space: fix a set of macroscopic variables, and partition the phase space according to the values taken by these variables (see e.g. figures 7.3 and 7.5 in Penrose [87] for an illustration, and Appendix 1 here for an example). Each element of the partition consists of a set of microscopic states that give the same value to the chosen macroscopic variables. Now, these elements of the partition have very different volumes. This is similar to the law of large numbers. There are (for N large) vastly more results of N throws of a coin where the number of heads is approximately one half than throws where it is approximately one quarter (the ratio of these two numbers varies exponentially with N). By far the largest volumes correspond to the *equilibrium values* of the macroscopic variables (and that is how "equilibrium" should be defined). So, we need a much weaker notion than ergodicity. All we need is that the microscopic configuration evolves in phase space towards those regions where the relevant macroscopic variables take their equilibrium values. The Kac

[54] If there is some cell which has not been visited even once, there will be a function on phase space for which the space average and the time average, computed up to that time, differ a lot: just take the function which takes value one on that cell and is zero elsewhere.

model (see Appendix 1) perfectly illustrates this point: it is not ergodic in any sense, yet, on proper time scales, the macroscopic variables evolve towards equilibrium.

There is a hierarchy of "ergodic" properties that are stronger than ergodicity: mixing, K-system, Bernoulli, see Lebowitz and Penrose [70]. But none of these will help us to understand, in principle, irreversible behaviour any more than ergodicity.

The problem with all those approaches is that they try to give a purely mechanical criterion for "irreversible behaviour". Here is the basic dilemna: either we are willing to introduce a macro/micro distinction and to give a basic role to initial conditions in our explanation of irreversibility or we are not. If we make the first choice, then, as explained in Section 3, there is no deep problem with irreversibility, and subtle properties of the dynamics (like ergodic properties) play basically no role. On the other hand, nobody has ever given a consistent alternative, namely an explanation of irreversibility that would hold for *all* initial conditions or apply to *all* functions on configuration space (therefore avoiding the micro/macro distinction). So, we have to make the first choice. But then, everything is clear and nothing else is needed.

Another critique of the "ergodic" approach is that systems with one or few degrees of freedom may very well be ergodic, or mixing, or Bernoulli (like the baker's transformation). And, as we discussed in Section 3.4, it makes no sense to speak about irreversibility for those systems. So, this is another sense in which the notion of ergodicity is not sufficient (see e.g. Vauclair ([119] p.197), where the approach to equilibrium is illustrated by the baker's transformation).

To avoid any misunderstandings, I emphasize that the study of ergodic properties of dynamical systems gives us a lot of interesting information on those systems, especially for chaotic systems. Besides, ergodic properties, like other concrete dynamical properties of a system, may play a role in the form of the macroscopic equations obeyed by the system, in the value of some transport coefficients or in the speed of convergence to equilibrium. But, and this is the only point I wanted to make, the usual story linking ergodicity (or mixing) and "approach to equilibrium" is highly unsatisfactory.

4.3. Real systems are never isolated.

Sometimes it is alleged that, for some reason (the Poincaré recurrences, for example) a truly isolated system will never reach equilibrium. But it does not matter, since true isolation never occurs and external ("random") disturbances will always drive the system towards equilibrium [55]. This is true but irrelevant[56].

[55] One can even invoke a theorem to that effect: the ergodic theorem for Markov chains. But this is again highly misleading. This theorem says that probability distributions will converge to an "equilibrium"'distribution (for suitable chains). This is similar, and related, to what happens with strongly chaotic systems. But it does not explain what happens to a single system, unless we are willing to distinguish between microscopic and macroscopic variables, in which case the ergodic theorem is not necessary.

[56] Borel [13] tried to answer the reversibility objection, using the lack of isolation and the instability of the trajectories. As we saw in Section 3.3, this objection is not relevant, once one introduces the micro/macro distinction. And Fred Hoyle wrote: "The thermodynamic arrow of time does not come from the physical system itself...it comes from the connection of the system with the outside world" [54], quoted in [63]. See also Cohen and Stewart ([19], p. 260) and [51] for similar ideas.

In order to understand this problem of non-isolation, we have to see how to deal with idealizations in physics. Boltzmann compares this with Galilean invariance (see [11], p.170). Because of non-isolation, Galilean (or Lorentz) invariance can never be applied strictly speaking (except to the entire universe, which is not very useful). Yet, there are many phenomena whose explanation involve Galilean (or Lorentz) invariance. We simply do as if the invariance was exact and we then argue that the fact that it is only approximate does not spoil the argument. One uses a similar reasoning in statistical mechanics. If we can explain what we want to explain (e.g. irreversibility) by making the assumption that the system is perfectly isolated, then we do not have to introduce the lack of isolation in our explanations. We have only to make sure that this lack of isolation does not conflict with our explanation. And how could it? The lack of isolation should, in general, speed up the convergence towards equilibrium[57]. Also, if we want to explain why a steamboat cannot use the kinetic energy of the water to move, we apply irreversibility arguments to the system boat+water, even though the whole system is not really isolated.

Another way to see that lack of isolation is true but irrelevant is to imagine a system being more and more isolated. Is irreversibility going to disappear at some point? That is, will different fluids not mix themselves, or will they spontaneously unmix? I cannot think of any example where this could be argued. And I cannot tell with a straight face to a student that (part of) our explanation for irreversible phenomena on earth depends on the *existence* of Sirius.

4.4. Bergson, Popper, Feyerabend (and others).

Here, I will discuss various confusions that have been spread by some philosophers. Bergson was a rather unscientific thinker, and many readers may wonder why he belongs here. I have myself been very surprised to see how much sympathy Prigogine and Stengers seem to have for Bergson (see the references to Bergson in [94, 95]). But Bergson has been extremely influential, at least in the French culture, and, I am afraid, still is[58].

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[57] One has to be careful here. If we shake a mixture of fluids, it should become homogeneous faster. But of course, there are external influences that prevent the system from going to equilibrium, as with a refrigerator. Also, the time scale on which approach to equilibrium takes place may vary enormously, depending on the physical situation. This is what is overlooked in [51].

[58] I remember that when I first heard, as a teenager, about the special theory of relativity, it was through Bergson's alleged refutation of that theory! He thought, probably rightly so, that there was aconflict between his intuitive views on duration and the absence of absolute simultaneity in Relativity. So he simply decided that there was a "time of consciousness", as absolute as Newtonian time, and that the Lorentz transformations were merely some kind of coordinates "attributed" by one observer to the other. Running into trouble with the twin paradox, he decided that acceleration is relative, like uniform motion, and that, when both twins meet again, they have the same age! (see [9]). At least, Bergson had the good sense, after his lengthy polemic with Einstein, to stop the republication of his book. But, and this is a remarkable aspect of our "intellectual" culture, the *very same mistake* is repeated by some of his admirers, Jankelevitch ([55], Chap. 2), Merleau-Ponty ([81], p. 319) and Deleuze ([23], p.79; see also his later writings). Of course, all this is explained by telling to the physicists that they should stick to their "mathematical expressions and language" (Merleau-Ponty, [81], p. 320), while leaving the deep problems of the "time of consciousness" to philosophers. For a modern attempt to make some sense of Bergson's universal time, see the first Appendix of "Entre le Temps et l'Eternité" ([95]).

In particular, he is one source of the widespread confusion that there is contradiction between life and the Second Law of thermodynamics. Roughly speaking, Bergson saw a great opposition between "matter" and "life", and a related one between intellect and intuition. The intellect can understand matter, but intuition is needed to apprehend life[59]. Bergson was not a precursor of the discovery of DNA, to put it mildly[60]. The Second Law of thermodynamics, which he called the "most metaphysical of the laws of physics" ([8], p.264), was very important for him[61]. It reinforced his "vision of the material world as that of a falling weight." ([8], p.266), hence, that "all our analyses show indeed in life an effort to climb the slope that matter has descended." ([8], p.267) "The truth is that life is possible wherever energy goes down the slope of Carnot's law, and where a cause, acting in the opposite direction, can slow down the descent."([8], p.278) It's all metaphorical, of course, but Bergson's philosophy *is* entirely a "metaphorical dialectics devoid of logic, but not of poetry", as Monod calls it ([82]). In any case, life is perfectly compatible with the Second Law (see Section 3.3).

Turning to Popper, we have already seen that he had lots of problems with statistical mechanics. Since Popper is generally considered positively by scientists[62], it is worth looking more closely at his objections. He took too literally the claims of Heisenberg, Born and Pauli on irreversibility as "subjective" (see Section 3.5), which he thought (maybe rightly so) were precursors of the subjectivism of the Copenhagen interpretation of quantum mechanics (see [92]). Besides, he was strongly opposed to determinism and he was convinced that "the strangely law-like behaviour of the statistical sequences remain, for the determinist, *ultimately irreducible and inexplicable*." ([93], p.102). As I discussed in Section 2.2, there is no problem in using probabilities, even in a deterministic universe. He then invented a rather obscure "propensity" interpretation of probabilities. He also felt that one should define "objectively" what a random sequence is. A sequence (of zeros and ones) will be random if there are (almost) as many zeroes and ones, as many pairs 00, 01, 10, 11, etc... (see e.g. [92], p.112). He did not seem to realize that this is like saying that a "microscopic configuration" (a sequence) gives to certain "macroscopic variables" (the average number of occurences of finite subsequences) the values which are given to them by the overwhelming majority of sequences. So that the difference with what he calls the "subjective" viewpoint is not so great.

Finally, Popper was very critical of Boltzmann. Although he admires Boltzmann's realist philosophy, he calls Boltzmann's interpretation of time's arrow "idealist" and

[59] See Monod [82] and B. Russell [106] for a critique of his philosophy and [83] for the relation between Bergson and Prigogine. The main problem with Bergson's lasting influence is well expressed by Bertrand Russell: "One of the bad effects of an anti-intellectual philosophy such as that of Bergson, is that it thrives upon the errors and confusions of the intellect. Hence it is led to prefer bad thinking to good, to declare every momentary difficulty insoluble, and to regard every foolish mistake as revealing the bankruptcy of intellect and the triumph of intuition." ([106], p.831)

[60] This remark is not as anachronistic as it may seem. Think of the work of Weismann, at the turn of the century, on the continuity of the germ-plasm.

[61] As we shall see in Section 5, it is probably the least metaphysical of those laws (although I do not like this terminology), since it is not a purely dynamical law.

[62] See, e.g. the introduction by Monod to the French edition of "The Logic of Scientific Discovery" [91] and also Prigogine and Stengers, e.g. ([95], p.173). For philosophical critiques of Popper, see Putnam [102], and Stove [114]. For a critique of his views on the arrow of time, see Ghins [44].

claims that it was a failure. As we saw, any explanation of irreversibility ultimately forces us to say that the universe started in an "improbable" state. Boltzmann tried to explain it as follows: in an eternal and infinite universe globally in equilibrium, all kinds of fluctuations will occur. What we call our universe is just the result of one such gigantic fluctuation, on its way back to equilibrium. But this explanation does not really work. Indeed, the most probable assumption, if a fluctuation theory is to hold, is simply that my brain is a fluctuation out of equilibrium, just at this moment and in this small region of space, while none of the familiar objects of the universe (stars, planets, other human beings) exist and all my (illusory) perceptions and memories are simply encoded in the states of my neurons (a "scientific" version of solipsism). However improbable such a fluctuation is, it is still far more probable than a fluctuation giving rise to the observed universe, of which my brain is a part. Hence, according to the fluctuation theory, that "solipsist" fluctuation must actually have occurred many more times than the big fluctuation in which we live, and therefore no explanation is given for the fact that we happen to live in the latter (see Feynman [38] and Lebowitz [68] for a discussion of that fluctuation theory).

Boltzmann's cosmology does not work. So, what? When Popper wrote (1974), no one took Boltzmann's cosmology seriously anyway: it had long since been superseded by cosmologies based on general relativity. Besides, Popper does not raise the objection I just made. His criticism is, rather, that this view would render time's arrow "subjective" and make Hiroshima an "illusion". This is complete gibberish. Boltzmann gives a complete and straightforward explanation of irreversible processes in which Hiroshima is as objective as it unfortunately is (when it is described at the macroscopic level, which is what we mean by "Hiroshima"). Of course, questions remain concerning the initial state of the universe. In the days of Boltzmann, very little was known about cosmology. What the failure of Boltzmann's hypothesis on the origin of the initial state shows is that cosmology, like the rest of science, cannot be based on pure thought alone[63].

Popper was also too much impressed with Zermelo's objections to Boltzmann, based

[63] There are indications that Boltzmann did not take his fluctuation theory too seriously. For example, he wrote "that the world began from a very unlikely initial state, this much can be counted amongst the fundamental hypotheses of the whole theory and we can say that the reason for it is as little known as that for why the world is as it is and not otherwise."([11], p.172; compare with note 46). In general, Boltzmann is quite opposed to unscientific speculations. In his criticism of Schopenhauer, he takes a very Darwinian (and surprisingly modern) view of mankind. He starts by observing that drinking fermented fruit juices can be very good for your health: "if I were an anti-alcoholic I might not have come back alive from America, so severe was the dyssentry that I caught as a result of bad water...it was only through alcololic beverages that I was saved." ([11], p.194) But, with alcohol, one can easily overshoot the mark. It is the same thing with moral ideas. "We are in the habit of assessing everything as to its value; according to whether it helps or hinders the conditions of life, it is valuable or valueless. This becomes so habitual that we imagine we must ask ourselves whether life itself has a value. This is one of those questions utterly devoid of sense." ([11], p.197) Finally, for theoretical ideas, he observes that our thoughts should correspond to experience and that overshooting the mark should be kept within proper bounds: "Even if this ideal will presumably never be completely realized, we can nevertheless come nearer to it, and this would ensure cessation of the disquiet and the embarassing feeling that it is a riddle that we are here, that the world is at all and is as it is, that it is incomprehensible what is the cause of this regular connection between cause and effect, and so on. Men would be freed from the spiritual migraine that is called metaphysics."([11], p.198).

on the Poincaré recurrence theorem, and discussed above (see [89]). But he has even stranger criticisms: in [90], he argues that Brownian motion (where fluctuations may pull the particle against gravity) is a serious problem for the Second Law. Maxwell had already observed that "The Second Law is constantly being violated... in any sufficiently small group of molecule... As the number ... is increased ... the probability of a measurable variation ... may be regarded as practically an impossibility." ([78], quoted in [69]) Going from bad to worse, Feyerabend invents a "perpetuum mobile of the second kind" (i.e. one respecting the first law but not the second) using *a single molecule* [37]. He adds that he assumes "frictionless devices" (he had better do so!). Those claims are then repeated in his popular book "Against Method" [36], where it is explained that Brownian motion refutes the Second Law[64]. This is how the general educated public is misled into believing that there are deep open problems which are deliberately ignored by the "official science"!

Unfortunately, this is not the end of it. Contemporary (or post-modern) French "philosophy" is an endless source of confusions on chaos and irreversibility. Here are just a few examples. The well-known philosopher Michel Serres says, in an interview with the sociologist of science Bruno Latour, entitled paradoxically "Eclaircissements": "Le temps ne coule pas toujours selon une ligne (la première intuition se trouve dans un chapitre de mon livre sur Leibniz, pp. 284–286) ni selon un plan, mais selon une variété extraordinairement complexe, comme s'il montrait des points d'arrêt, des ruptures, des puits, des cheminées d'accélération foudroyante, des déchirures, des lacunes, le tout ensemencé aléatoirement, au moins dans un désordre visible. Ainsi le développement de l'histoire ressemble vraiment à ce que décrit la théorie du chaos ..."[65] ([110]). Another philosopher, Jean-François Lyotard writes: "L'idée que l'on tire de ces recherches (et de bien d'autres) est que la prééminence de la fonction continue à derivée comme paradigme de la connaissance et de la prévision est en train de disparaître. En s'intéressant aux indécidables, aux limites de la précision du contrôle, aux quanta, aux conflits à l'information non complète, aux "*fracta*", aux catastrophes, aux paradoxes pragmatiques, la science postmoderne fait la théorie de sa propre évolution comme discontinue, catastrophique, non rectifiable, paradoxale. Elle change le sens du mot savoir, et elle dit comment ce changement peut avoir lieu. Elle produit non pas du connu, mais de l'inconnu. Et elle suggère un modèle de légitimation qui n'est nullement celui de la meilleure performance, mais celui de la différence comprise comme paralogie."[66] ([73]).

[64] This error is repeated, with many others, in [121], p.177.

[65] I will leave these texts in the original language, and provide only a rough translation, because nonsense is hard to translate: the book is called "Clarifications" and the quotation is: "Time does not always flow along a line, nor along a plane, but along an extraordinarily complex manifold, as if it showed stopping points, ruptures, sinks, chimneys of striking acceleration, rips, lacunas, everything being randomly sowned, at least in a visible disorder. So, the development of history really ressembles what is described by chaos theory."

[66] "The idea derived from those researches (and from many others) is that the pre-eminence of the continuous function with a derivative as paradigm of knowledge and forecast is disappearing. By being interested in undecidables, in limits of precision of control, in quanta, in conflicts with incomplete information , in "fracta", in catastrophes, in pragmatical paradoxes, postmodern science makes the theory of its own evolution as discontinuous, catastrophic, not rectifiable, paradoxical. It changes the meaning of the word knowledge, and it says how this change can occur. It produces not the known,

A sociologist, Jean Baudrillard observes that “Il faut peut-être considérer l’histoire elle-même comme une formation chaotique où l’accélération met fin à la linéarité, et où les turbulences créées par l’accélération éloignent définitivement l’histoire de sa fin, comme elles éloignent les effets de leurs causes. La destination, même si c’est le Jugement dernier, nous ne l’atteindrons pas, nous en sommes désormais séparés par un hyperespace à réfraction variable. La rétroversion de l’histoire pourrait fort bien s’interpréter comme une turbulence de ce genre, due à la précipitation des événements qui en inverse le cours et en ravale la trajectoire.”[67] ([5]). Finally, Gilles Deleuze and Félix Guattari understood chaos as follows: “ On définit le chaos moins par son désordre que par la vitesse infinie avec laquelle se dissipe toute forme qui s’y ébauche. C’est un vide qui n’est pas un néant, mais un *virtuel*, contenant toutes les particules possibles et tirant toutes les formes possibles qui surgissent pour disparaître aussitôt, sans consistance ni référence, sans conséquence (Ilya Prigogine et Isabelle Stengers, *Entre le temps et l’éternité*, pp. 162–163).”[68] ([24]) Of course, Prigogine and Stengers are not responsible for *these* confusions (in that reference, they discuss the origin of the universe). But this illustrates the difficulties and the dangers of the popularization of science. Besides, Guattari wrote a whole book on “Chaosmose” ([48]), which is full of references to non-existent concepts such as “nonlinear irreversibility thresholds” and “fractal machines”[69].

5 Entropies

Holy Entropy! It’s boiling!
Mr Tompkins (G. Gamow) ([40], p.111).

There is some kind of mystique about entropy. According to [25], [118], von Neumann suggested to Shannon to use the word “entropy” adding that “it will give you a great edge in debates because nobody really knows what entropy is anyway”. But there is a very simple way to understand the notion of entropy. Just consider any set of macroscopic variables (at a given time) and consider the volume of the subset of phase space (of the microscopic variables) on which these macroscopic variables take a given value. The *Boltzmann entropy* (defined as a function of the values taken by the macroscopic variables) equals the logarithm of that volume. Defined this way, it looks quite

but the unknown. And it suggests a model of legitimation which is not at all the one of the best performance, but rather the one of difference understood as paralogism.”

[67] “One must, maybe, consider history itself as a chaotic formation where acceleration puts an end to linearity, and where turbulence created by acceleration separates definitively history from its end, as it separates effects from their causes. The destination, even if it is the Last Judgment, we shall not reach it, we are separated from it by a hyperspace with variable refraction. The retroversion of history could very well be interpreted as such a turbulence, due to the precipitancy of events which inverts its path and swallows its trajectory.”

[68] “Chaos is defined not so much by its disorder than by the infinite speed with which every form being sketched is dissipated. It is a vacuum which is not a nothingness, but a *virtual*, containing all possible particles and extracting all possible forms, which appear and disappear immediately, without consistence, nor reference, nor consequence.”

[69] I recommand to people interested in *tensors* (applied to psychology, sociology, etc . . .) Guattari’s contribution to [100].

arbitrary. We may define as many entropies as we can find sets of macroscopic variables. Furthermore, since the micro/macro distinction is not sharp, we can always take finer grained entropies, until we reach the microscopic variables (the positions and the momenta of the particles), in which case the entropy is constant and equals zero (giving a volume equal to one to a single microstate, which is rather a quantum-mechanical way to count).

But one should make several remarks:

1) These entropies are not necessarily "subjective". They are as objective as the corresponding macroscopic variables. Jaynes, following Wigner, calls these entropies "anthropomorphic" ([56], p.85). A better word might be "contextual", i.e. they depend on the physical situation and on its level of description.

2) The "usual" entropy of Clausius, the one which is most useful in practice, corresponds to a particular choice of macroscopic variables (e.g. energy and number of particles per unit volume for a monoatomic gas without external forces). The derivative with respect to the energy of *that* entropy, restricted to equilibrium values, defines the inverse temperature. One should not confuse the "flexible" notion of entropy introduced above with the more specific one used in thermodynamics[70].

3) The Second Law seems now a bit difficult to state precisely. "Entropy increases"; yes, but which one? One can take several attitudes. The most conservative one is to restrict oneselve to the evolution of a given isolated system between two equilibrium states and then the increasing entropy is the one discussed in point (2) above. The Second law is then a rather immediate consequence of the irreversible evolution of the macroscopic variables: the microscopic motion will go from small regions of phase space to larger ones (in the sense of the partitions discussed in Section 4.2). The gas in the box goes from an equilibrium state in the left half of the box to another equilibrium state in the whole box. There are many more microscopic configurations corresponding to a uniform density than there are configurations corresponding to the gas being entirely in one half of the box. But this version of the Second Law is rather restrictive, since most natural phenomena to which we apply "Second Law" arguments are not in equilibrium. When used properly in non-equilibrium situations, reasonings based on the Second Law give an extremely reliable way to predict how a system will evolve. We simply assume that a system will never go spontaneously towards a very small subset of its phase space (as defined by the macroscopic variables). Hence, if we observe such an evolution, we expect that some hidden external influence is forcing the system to do so, and we

[70] This is again the source of much confusion, just like the "subjectivity of irreversibility" discussed in Section 3.5, see for example Popper [92], p.111, Denbigh [25], S.K. Ma [74]. In the popular book, "The Quark and The Jaguar", we read: "Entropy and information are closely related. In fact, entropy can be regarded as a measure of ignorance."([42], p.219) And further: " Indeed, it is mathematically correct that the entropy of a system described in perfect detail would not increase; it would remain constant."([42], p.225). This is correct, if properly understood, but it might be useful to emphasize that one does not refer to the "usual" thermodynamic entropy.

try to discover it (see also Jaynes [59] for a nice discussion of apparent violations of the Second Law)[71].

4) In most non-equilibrium situations, most of these entropies are very hard to compute or even to estimate. However, Boltzmann was able to find an approximate expression of his entropy (minus his H function), valid for dilute gases (e.g. for the gas in the box initially divided in two of Section 3) and to write down an equation for the evolution of that approximate entropy. A lot of confusion is due to the identification between the "general" Boltzmann entropy defined above, and the approximation to it given by (minus) the H-function (as emphasized by Lebowitz in [68]). Another frequent confusion about Boltzmann's equation is to mix two conceptually different ingredients entering in its derivation[72]: one is an assumption about *initial conditions* and the other is to make a particular approximation (i.e. one consider the Boltzmann-Grad limit, see Spohn [112], in which the equation becomes exact; in the Kac model in Appendix 1, this limit reduces simply to letting n go to infinity for fixed t). To account for irreversible behaviour, one has always, as we saw, to assume something on initial conditions, and the justification of that assumption is statistical. But that part does not require, in principle, any approximation. To write down a concrete (and reasonably simple) equation, as Boltzmann did, one uses this approximation. Failure to distinguish these two steps leads one to believe that there is some deep problem with irreversibility outside the range of validity of that approximation[73].

5) Liouville's theorem[74] is sometimes invoked against such ideas. For instance, we read in Prigogine and Stengers ([95], p.104): "All attempts to construct an entropy function, describing the evolution of a set of trajectories in phase space, came up

[71] See e.g. the constraints on the plausible mechanisms for the origin of life, due to the Second Law, discussed by Elitzur ([35], Sect.11). There is some similarity between this use of the Second Law and the way biologists use the law of natural selection. The biologists do not believe that complex organs appear "spontaneously". Hence, when they occur, they look for an adaptative explanation (see [21] for an introduction to the theory of evolution). Both attitudes are of course similar to elementary probabilistic reasoning: if we throw a coin a million times and find a significant deviation from one-half heads one-half tails, we shall conclude that the coin is biased (rather than assuming that we observe a miracle).

[72] As for example in: "This so-called hypothesis of "molecular chaos" admits the absence of correlations between the velocities of the molecules in the initial state of the gas, although, obviously, correlations exists between the molecules after the collisions. The hypothesis of molecular chaos amounts to introduce, in a subtle way, the irreversibility that one tries demonstrate." (Lestienne [71], p.172) Boltzmann never said that he would demonstrate irreversibility without assuming something about initial conditions. Another, more radical, confusion is due to Bergmann: "It is quite obvious that the Boltzmann equation, far from being a consequence of the laws of classical mechanics, is inconsistent with them." (in [47], p. 191)

[73] To make a *vague* analogy, in equilibrium statistical mechanics, one has the concept of phase transition. Mean field theory (or the van der Waals theory, Curie-Weiss or molecular field approximation) gives an approximate description of the phase transition. But the concept of phase transition is much wider than the range of validity of that approximation.

[74] This theorem says that, if A is a subset of the phase space Ω, then $Vol(T^t(A)) = Vol(A)$, where $Vol(A) = \int_A d\mathbf{x}$.

against Liouville's theorem, since the evolution of such a set cannot be described by a function that increases with time"[75] (see [51], p.8 for a similar statement). What is the solution of that "paradox"? Here I consider *a single system* evolving in time and associate to it a certain set of macroscopic variables to which in turn an entropy is attached. But, since the values of the macroscopic variables change with time, the corresponding set of microstates changes too. For the gas in the box, the initial set of microstates are all those where the particles are in the left half, while the final set consists of the microstates giving rise to a uniform density. In other words, I "embed" my microscopic state into different sets of microscopic states as time changes, and the evolution of that set should not be confused with a set of *trajectories*, whose volume is indeed forced to remain constant (by Liouville's theorem)[76].

6) A related source of confusion comes from the fact that Gibbs' entropy, $-\int \rho \log \rho d\mathbf{x}$, which is sometimes viewed as more "fundamental" (because it is expressed via a distribution function ρ on phase space), is indeed constant in time (by Liouville's theorem again). But why should one use this Gibbs entropy out of equilibrium? In equilibrium, it agrees with Boltzmann and Clausius entropies (up to terms that are negligible when the number of particles is large) and everything is fine[77]. When we compare two different equilibrium states all these entropies change, and the direction of change agrees with the Second Law[78]. The reason being that the values taken by the macroscopic variables are different for different equilibrium states. Actually, trying to "force" the Gibbs entropy to increase by various coarse-graining techniques, gives then the impression that irreversibility is only due to this coarse-graining and is therefore arbitrary or subjective (see e.g. Coveney ([20], p.412).: "Irreversibility is admitted into the description by asserting that we only observe a coarse-grained probability;").

7) Finally, why should one worry so much about entropy for non-equilibrium states? A distinction has to be made between two aspects of irreversibility: one is that macroscopic variables tend to obey irreversible laws and the other is that when an isolated system can go from one equilibrium state to another, the corresponding thermodynamic entropies are related by an inequality. Both aspects are connected, of course, and they can be both explained by similar ideas. But this does not mean

[75] See also [100], p.160: "According to the mechanical view of the world, the entropy of the universe is today identical to what it was at the origin of time." Or as Coveney says ([20], p.411): "As long as the dynamical evolution is unitary, irreversibility cannot arise. This is the fundamental problem of non-equilibrium statistical mechanics."

[76] Let me use the notations of note 43. By Liouville's Theorem, indeed $Vol(T^t(\Omega_0)) = Vol(\Omega_0)$. But $T^t(\Omega_0)$ is a very small subset of Ω_t. Confusing the two sets leads to the (wrong) idea that $Vol(T^t(\Omega_0)) = Vol(\Omega_t)$. The evolution of Ω_t does not coincide with a set of trajectories.

[77] Note, that these entropies agree with (minus) Boltzmann's H function only when the interparticle forces are negligeable (as in a very dilute gas). This is rather obvious since the H function is an approximation to the Boltzmann entropy, see Jaynes ([56], p.81).

[78] Amusingly enough, this conclusion can be reached using only Liouville's theorem (see Jaynes [56] p.83) which is blamed as the source of all the troubles!

that, in order to account for the irreversible behaviour of macroscopic variables, we have to introduce an entropy function that evolves monotonically in time. It may be useful or interesting to do so, but it is not required to account for irreversibility. All we really *need* is to define suitably the entropy for equilibrium states, and that was done a long time ago.

8) Jaynes rightly says that he does not know what is the entropy of a cat ([56] p.86). The same thing could be said for a painting, an eye or a brain. The problem is that there is no well-defined set of macroscopic variables that is specified by the expression “a cat”.

6 Order out of Chaos?

In my view all salvation for philosophy may be expected
to come from Darwin's theory. As long as people believe in a special spirit
that can cognize objects without mechanical means, or in a special will
that likewise is apt to will that which is beneficial to us, the simplest
psychological phenomena defy explanation.
L. Boltzmann ([11], p.193)

In this section, I will discuss the “constructive role” of irreversible processes[79]. But I also want to discuss the impact of scientific discoveries on the cultural environment. At least since the Enlightenment and the Encyclopaedia, scientists have communicated their discoveries to society, and, through the popular books and the educational system, have profoundly influenced the rest of culture. But one has to be very careful. In his recent book on Darwin, the philosopher D. Dennett makes a list of popular misconceptions about the theory of evolution ([26], p.392). One of them is that one no longer needs the theory of natural selection, since we have chaos theory! He does not indicate the precise source of this strange idea, but this illustrates how easily people can be confused by loose talk, analogies and metaphors.

I think that one should clearly reaffirm certain principles: first of all, no macroscopic system has ever jumped out of equilibrium spontaneously. Moreover, isolated macroscopic systems always evolve towards equilibrium. These are general qualitative statements that one can make about macroscopic mechanical systems. No violations of them have ever been found. Of course, nobody explicitly denies those principles, but I am nevertheless afraid that many people are confused about this point[80].

Of course, it has always been known that very complicated and interesting phenomena occur out of equilibrium, human beings for example. But this raises two completely

[79]Note that the word “chaos” in the title is used in a somewhat ambiguous way: sometimes it has the technical meaning of Section 2, sometimes it means “disordered” or “random”.

[80]Here are some examples; in Cohen and Stewart, one reads: “The tendency for systems to segregate into subsystems is just as common as the tendency for different systems to get mixed together.” ([19], p. 259). Or in Meessen: “by watching some phenomena, one is led to say that time has an arrow pointing towards a greater disorder, but by considering other phenomena, it seems that time has an arrow pointing, on the contrary, towards a greater order. Then, what does this arrow mean? If we can orient it in opposite directions, it is better not to talk about it any more.” ([80], p.119).

different problems. Oneis to explain those phenomena on the basis of the microscopic laws and of suitable assumptions on initial conditions. Much progress in this direction have been made, but we are far from understanding everything, and, of course, to account for the existence of human beings, Darwin's theory *is* needed.

The other question, a much easier one, is to understand why there is no *contradiction* between the general tendency towards equilibrium and the appearance of self-organization, of complex structures or of living beings. *That* is not difficult to explain qualitatively, see Section 3.3 and Penrose [87].

Going back to Popper (again), he wanted to solve the alleged contradiction between life and the Second Law (see note 44) by turning to Prigogine [97] and saying that "*open systems in a state far from equilibrium* show no tendency towards increasing disorder, even though they produce entropy. But they can export this entropy into their environment, and can increase rather than decrease their internal order. They can develop structural properties, and thereby do the very opposite of turning into an equilibrium state in which nothing exciting can happen any longer." ([93], p. 173). This is correct, provided that part of the environment *is more ordered than the system*, where "order" is taken in a technical sense: the system plus its environment (considered as approximately isolated) is in a state of low entropy, or is in a small subset of its *total* phase space and moves towards a larger subset of that space[81] (where the subsets are elements of a partition like the one discussed in Section 4.2). But it is misleading to suggest that order is created out of nothing, by rejecting "entropy" in an unspecified environment[82]. It is not enough to be an "open system"; the environment must be in a state of low entropy. While it is correct to say that the Second Law "applies only to isolated systems", it should not be forgotten that most systems can be considered, at least approximately, as subsystems of isolated ones, and that, therefore, the Second Law does imply some constraints even for open systems.

Here are some examples which *may* create this confusion[83]: In ([100], p.157) Prigogine wants to give an example of one of the "many phenomena'" that cannot be understood through the "general interpretation of the growth of entropy" due to Boltzmann. He considers a system of particles (on a line), which start in a disordered configuration. Then "the strong interactions between those particles" will push them to form an ordered crystal. It looks like a "passage from a disordered situation to an ordered one". But

[81] One should always distinguish this precise but technical sense of order, from our intuitive idea of order. In particular, when one identifies increase of entropy and increase of "disorder", one should realize that this is correct only if it is a tautology (i.e. if "disorder" is defined through the more precise notion of entropy). Otherwise, it can be misleading. A similar problem occurs with intuitive words like "complexity" (or "information" in the past). If we speak of the "complexity of the brain", it has of course evolved from a less "complex" structure, except that those words do not have a precise meaning. Note that precise definitions of complexity, like algorithmic complexity, do not at all capture the intuitive meaning of the word, since "random" sequences are algorithmically complex, and, whatever "complexity of the brain" means, and whatever "random" means, they do not mean the same thing; see Gell-mann [42], Chap. 3 for a good discussion of this issue.

[82] Besides, as we saw in Section 5, entropy is not really a "substance" to be "exported". This is a somewhat strange terminology for a philosopher like Popper, so critical of "essentialism".

[83] To avoid misunderstandings, let me stress that my main criticism here is that one may, through ambiguous statements, unwillingly mislead the non-specialized reader.

is this an isolated system? This is not clear if one considers the pictures. The final configuration looks like a perfect crystal. But if there are interactions between the particles favoring an ordered crystal, the disordered initial configuration must have been one of high potential energy, hence the "ordered" configuration will have a high kinetic energy, and oscillations will occur. Of course, if the total initial energy is sufficiently small, the oscillations will be small and the equilibrium state will be crystalline. But that is not incompatible with the "general interpretation of the growth of entropy". Equilibrium states maximize entropy, for a given energy, but may be crystalline (at least for higher dimensional lattices). This is one example where maximum entropy is not necessarily the same as maximal disorder (in the intuitive sense of the word). On the other hand, if dissipation takes place, the "passage from a disordered situation to an ordered one" is possible, even starting from a configuration of high potential energy. But this means that some environment absorbs the energy of the system, in the form of heat, hence it increases *its* entropy. And the environment must have been more "ordered" to start with. Again, this is in agreement with the "general interpretation of the growth of entropy".

To give another example, Prigogine and Stengers emphasize in ([94], p.427) that, for the Bénard instability[84] to occur *one must provide more heat to the system*. As noticed by Meessen ([80], p.118) "It is remarkable that the creation of a structure is initiated by a source of heat, which is usually a source of disorder". This quotation shows clearly what is confusing: heating suggests an increase of disorder, while the result is the appearance of a self-organized structure. But what is needed, of course, is a temperature *difference* between the two plates. So, if one heats up from below, one must have some cooling from above. The cooling acts like a refrigerator, so it requires some "ordered" source of energy. The more one heats, the more efficient must be the cooling.

These are fairly trivial remarks, but which, I believe, have to be made, at least for the general public, if one wants to avoid giving the impression that processes violating the Second Law can occur: all the emergence of complex structures, of whatever one sees, is perfectly compatible with the universal validity of the "convergence to equilibrium", provided one remembers that our universe started (and still is) in a low entropy state[85].

Besides, one should be careful with the issue of determinism, at the level of macroscopic laws, for example when bifurcations occur. In many places, Prigogine and Stengers seem to attach a deep meaning to the notion of *event*: "By definition, an event cannot be deduced from a deterministic law: it implies, one way or another, that what hap-

[84] A fluid is maintained between two horizontal plates, the lower one being hotter than the higher one. If the temperature difference is large enough, rolls will appear, see e.g. [94, 95] for a discussion of the Bénard instability.

[85] Somewhat to my surprise, I found the following *theological* commentary on the "constructive role" of irreversible phenomena: "Each time that a new order of things appear, it is marked by the dissipation of a chaotic behaviour, by a broken form of movement, that is, by a "fractal", by non-linearity, by variations or "fluctuations", by instability and randomness. In this way the dynamics of self-organization of matter, which reaches the great complexity of consciousness, manifests itself"(Ganoczy, [41], p.79). This is a bit of an extrapolation, starting from the Bénard cells. The quotation appears in a section of a chapter on "God in the language of the physicists", where the author refers mostly to Prigogine and Stengers, [94, 97].

pened "could" not have happened." ([95], p.46)[86] Let us consider Buridan's ass. One can describe it as being "in between" two packs of food. It could choose either. But that is a macroscopic description. Maybe one of the eyes of the ass is tilted in one direction, or some of its neurons are in a certain state favoring one direction. This is an example where the macroscopic description does not lead to an autonomous macroscopic law. At the macroscopic level, things are indeterminate, and the scheme of Section 3 does not apply: the microscopic configurations may fall into different classes, corresponding to different future evolutions for the macroscopic variables, and no single class constitutes an overwhelming majority. Thus, when we repeat the experiment (meaning that we control the same *macroscopic* variables) different outcomes will occur, because different experiments will correspond to microscopic variables that belong to different classes.

The same thing may happen in a variety of phenomena, e.g. which way a roll in a Bénard cell will turn. But that (true) remark has nothing to do with the issue of determinism, which is meaningful only at the microscopic level: in a perfectly deterministic universe (at that level) there will always be lots of situations where no simple autonomous macroscopic laws can be found, hence we shall have the illusion of "indeterminism" if we consider only the macroscopic level[87].

One should avoid (once more) the Mind Projection Fallacy. The macroscopic description may be all that is accessible to us, hence the future becomes unpredictable, but, again, it does not mean that Nature is indeterminate[88].

I will conclude with some remarks on Boltzmann and Darwin, which may also clarify the relation between "subjective" evaluations of probabilities and what we call an "explanation". As we saw, Boltzmann had a great admiration for Darwin. While preparing this article, I read in "La Recherche" that "the couple random mutations-selection has some descriptive value, but not at all an explanatory one" ([108]). That attitude is rather common (outside biology), but it goes a bit too far. Actually, there is an analogy between the kind of explanation given by Darwin and the one given by Boltzman, and they are both sometimes similarly misunderstood[89] (of course, Darwin's discovery,

[86]And even more misleading: "In a deterministic world, irreversibility would be meaningless, since the world of tomorrow would already be contained in the world of today, there would be no need to speak of time's arrow."([100] p.166)

[87]It is also a bit too fast to say, as Prigogine and Stengers do, that this kind of mechanism allows us to go beyond the "very old conflict between reductionists and antireductionists." ([94], p.234, quoted in [15], p.274) Any reductionist is perfectly happy to admit that some situations do not have simple, deterministic, macroscopic descriptions, while I doubt that antireductionists such as Popper and Bergson would be satisfied with such a simple admission.

[88]Here is another theological commentary: "Irreversibility means that things happen in time *and thanks to time*, that it could be that they did not happen, or did happen otherwise and that an infinite number of possibilities are always open." ""Inventive" disorder is part of the definition of things... Impredictability which is not due to our inability to control the nature of things, but to their nature itself, whose future simply does not yet exist, and could not yet be forecasted, even by "Maxwell's demon" put on Sirius." (Gesché, [43], p. 121) The author claims to find his inspiration on the "new scientific understanding of the Cosmos" from, among others, "La nouvelle alliance" ([43], p.120).

[89]In a critique of several "almost mystical views of life", which deny "an evolutionary role to Darwinian selection", the biologist Elitzur observes that "such a misleading discussion of evolution is based on a complete distortion of thermodynamics" ([35], p.450). Besides, I disagree, needless to say, with the comment of Prigogine and Stengers ([95], p.23-24), that there is an "antithesis" between Boltzmann

although less quantitative than statistical mechanics, had a much deeper impact on our culture). What does it mean to explain some fact, like evolution or irreversibility? As we saw, we claim to understand some macroscopically observed behaviour when, given some macroscopic constraint on a system, the overwhelming majority of the microscopic configurations compatible with those constraints (and evolving according to the microscopic laws) drive the macroscopic variables in agreement with that observed behaviour.

Turning to Darwin, his problem was to explain the diversity of species and, more importantly, the *complexity* of living beings, "those organs of extreme perfection and complication", like eyes or brains, as Darwin called them[90]. The fact is that we do not know, and we shall never know every microscopic detail about the world, especially about the past (such as every single mutation, how every animal died etc ...). Besides, the initial conditions of the world could be just so that complex organs are put together in one stroke. To use a common image, it would be like "hurling scrap metal around at random and happening to assemble an airliner" (Dawkins,[21], p.8). This does not violate any known law of physics. But it would be similar to various "exceptional" initial conditions that we encountered before (e.g. the particles going back to the left half of the box). And we would not consider an explanation valid if it appealed to such "improbable" initial conditions. But to say that such a scenario is "improbable" simply means that, given our (macroscopic) description of the world, there are very few microscopic configurations compatible with that description and giving rise to this scenario. And, indeed, if the world was four thousand years old, the existence of those complex organs would amount to a miracle.

To understand the Darwinian explanation, one must take into account four elements, at the level of the macroscopic description: natural selection (very few animals have offspring), variation (small differences between parents and offsprings occur, at least in the long run), heritability and time (the earth is much older than used to be thought). Then, the claim is that the overwhelming majority of microscopic events (which mutations occur, which animals die whithout children) compatible with such a macroscopic description leads to the appearance of those " organs of extreme perfection and complication" [91]. Note that we do not need to assume that mutations are genuinely "random".

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and Darwin and that the theories of Darwin were a success while those of Boltzmann failed.

[90] Here, I mean complexity in an intuitive sense. Of course, it is somewhat related to entropy, because, if we consider the set of molecules in an eye, say, there are very few ways to arrange them so as to produce an eye compared to the number of arrangement that cannot be used for vision. But as Jaynes says (see Remark 8 in Section 5), as long as we do not have a well-defined set of macroscopic variables that define precisely what an eye is, we cannot give a precise characterization of this "complexity" in terms of entropy (and, probably, such an entropy would not be the right concept anyway).

[91] Not being a biologist, I do not want to enter into any debate about the origin of life, the speed of evolution, or how far the Darwinian explanation goes. I only want to underline the similarity with the type of (probabilistic) explanation used in statistical physics. As I learned from V. Bauchau [4], this analogy was made already in 1877 by C. S. Peirce :" Mr. Darwin proposed to apply the statistical method to biology. The same thing has been done in a widely different branch of science, the theory of gases. Though unable to say what the movements of any particular molecule of gas would be on a certain hypothesis regarding the constitution of this class of bodies, Clausius and Maxwell were yet able, eight years before the publicationof Darwin's immortal work, by the application of the doctrine of probabilities, to predict that in the long run such and such a proportion of the molecules would, under

They may obey perfectly deterministic laws, and the randomness may reflect only our ignorance of the details.

A final point which is common to Boltzmann and to Darwin (and his successors) is that they have provided "brilliant confirmations of the mechanical view of Nature"[92]. Many people simply cannot swallow mechanical and reductionist explanations. They need some vital spirit, some teleological principle or some other animist view. Their philosophies "thrive upon the errors and confusions of the intellect". And this is probably why the theories of Boltzmann and of Darwin have been constantly attacked and misrepresented. Putting philosophical considerations aside, I believe that what we understand well, we understand in mechanical and reductionist terms. There is no such thing as a holist explanation in science. And thanks to people like Boltzmann and Darwin the "mechanical view of Nature" is alive and well, and is here to stay.

7 Conclusion: What makes poets happy?

I do not think we should embrace scientific theories because they are more hopeful, or more exhilarating ... I feel sensitive on this matter because, as an evolutionary biologist, I know that people who adopt theories because they are hopeful finish up embracing Lamarckism, which is false, although perhaps not obviously so, or Creationism, which explains nothing, and suggests no questions at all. If non-equilibrium thermodynamics makes poets happier, so be it. But we must accept or reject it on other grounds." ([79], p.257, in a review of [96]).

This paper has been written mainly for scientists. However, many references to Prigogine are found in the literature of the human sciences and philosophy. But why should anybody in those fields worry about what happens in physics or chemistry? In his most recent book [101], Prigogine starts by opposing the objective scientific view of the world with the subjective view (our feeling of time or of "free will") which some philosophers take as their starting point. His goal is to reconcile both approaches through his new understanding of physics.

Of course, it would be nice if one could fulfill that goal. But there are again basic confusions[93]. Take the issue of free will. It is true that if the fundamental laws of physics are deterministic, and if one rejects dualism, then free will is, in some sense, an illusion. But it is not clear that an element of "intrinsic randomness" in the fundamental physical laws would make it less an illusion. The only thing which is clear is that our inability

given circumstances, acquire such and such velocities; that there would take place, every second, such and such a relative number of collisions, etc.; and from these propositions were able to deduce certain properties of gases, especially in regard to their heat-relations. In like manner, Darwin, while unable to say what the operation of variation and natural selection in any individual case will be, demonstrates that in the long run they will, or would, adapt animals to their circumstances."[85]

[92]Speaking about DNA as the solution to the enigma of "life", the biologist Dawkins writes: "Even those philosophers who had been predisposed to a mechanistic view of life would not have dared hope for such a total fulfillment of their wildest dreams." ([22], p.17) Not surprisingly, Popper said that molecular biology became "almost an ideology" ([93], p.172). As for Bergson, he must be turning over in his grave.

[93]See also Maes [75] for a discussion of these problems.

to predict the future is not very relevant for this discussion. So that the fact that I am unable to predict which way a Bénard cell will rotate is not going to make me feel "free". Ignorance does not explain anything. And there is no precise sense in which a "narrow path" has been found between "blind laws" and "arbitrary events" ([101], p.224).

Another confusion concerns the relationship between the natural and the social sciences. In our discussion of the macroscopic level versus the microscopic one, we should locate the problems that psychology or the social sciences deal with at a very macroscopic level. Humans or societies are so many scales above molecules that modifications in the basic physical laws is (probably) almost irrelevant for the understanding of human actions[94]. The main problem of the social sciences is to exist as sciences, i.e. to discover theories that are well tested and that explain some non-trivial aspect of human affairs. The only thing that people working in those fields might learn from the natural sciences is a general scientific attitude, what one might call the epistemology of the Enlightenment: a critical mind, not to rely on authorities, to compare theory with experiment, etc.... But there is no need to ape what happens in the exact sciences. So that, even if there was really a shift of paradigm (whatever that means) from Newtonianism to Prigoginianism in physics, that would be no reason at all for the social scientists to rush towards theories where randomness is important[95]. And, of course, probabilistic models may be relevant in the social sciences, even if the fundamental laws are determistic.

The final confusion concerns the "end of certainties" [101] or the "desillusion with science" [10]. The plain fact is that we know much more about the world than we did three centuries ago, or fifty, or twenty years ago. Even the discovery that one cannot predict the weather (for more than a few weeks) means that our understanding of the laws governing the weather has improved. The general feeling that there is a "crisis in science" in turn fuels various anti-scientific attitudes that combine an extreme skepticism towards science with an equally unreasonable openness towards pseudo-sciences and superstitions[96]. In intellectual circles, this attitude is found in cultural and philosophical relativism or in some parts of the "sociology of science"[97]. Of course, science is in a perpetual "crisis", because it is not a dogma, and is subject to revision. But what is

[94]In saying this, I do not want to contradict a strongly reductionist viewpoint. But higher level laws, even though they are, in principle, reducible to the lower level ones, are not necessarily modified if the latter change. For example, Navier-Stokes equations were not much affected by the advent of quantum mechanics. Besides, so little is known scientifically about human actions that establishing a link between what is known there and molecules is not an urgent problem, to put it mildly. On the other hand, the word "almost" is important here. Our knowledge of physics does rule out many irrational beliefs at the human level (see e.g. Weinberg, [120], p.49 for further discussion of this point).

[95]For an extreme example of this confusion, see [6] where quantum theory is "applied" to politics.

[96]See, for example, the discussion of parapsychology by Stengers in [113], p.105. She claims that Rhine, the founder of parapsychology, has "devoted all his efforts to invent increasingly rigorous experimental protocols, but meets "non"-interlocutors, ready to admit any hypothesis provided it implies that there are no facts". For a scientific discussion of parapsychology, see e.g. Broch [17].

[97]I should emphasize that Prigogine himself does not have an explicit anti-scientific attitude. But, as Gross and Levitt point out in their analysis of superstitions in academic circles, "his name keeps coming up in postmodern discourses with depressing frequency" ([46], p. 96). That is exactly what one would expect when a famous scientist tells to the general educated public, a large part of which believe in New Age, in alternative medicines, or in some such nonsense, that one must "rethink the notion of law of nature".

not revisable is what I called the epistemology of the Enlightenment, and I have more than a suspicion that this epistemology is really what is being attacked by people who insist that there is a deep "crisis in science". It is interesting to note (but another article would be needed to develop that point) that skepticism with respect to science is based on two very different lines of thought: the first one is based on traditional philosophical arguments going back to Berkeley, Hume or Kant. While some of these arguments are clever and interesting, the progress of science is such that these a priori skeptical arguments leave many people cold. Another, conceptually different, line of thought is to try to show that science itself has reached some kind of limit, or "has to admit" that one cannot go further. Quantum mechanics, Chaos, the Big Bang or Gödel's theorem are usually cited as evidence for those claims. But this is basically pure confusion and misunderstanding, as I tried to show in this paper, at least for one of those examples. When all is said and done, science and reason is all we have. Outside of them, there is no hope.

APPENDIX 1. The Kac ring model.

Let me analyse a simple model, due to Mark Kac ([60] p.99, see also Thompson ([116] p.23)), which nicely illustrates Boltzmann's solution to the problem of irreversibility, and shows how to avoid various misunderstandings and paradoxes.

I shall describe a slightly modified version of the model and state the relevant results, referring to [60] for the proofs (the quotations below come from [60]).

"On a circle we consider n equidistant points"; m of the intervals between the points are marked and form a set called S. The complementary set (of $n-m$ intervals) will be called $\bar{S}$.

"Each of the n points is a site of a ball which can be either white (w) or black (b). During an elementary time interval each ball moves counterclockwise to the nearest site with the following proviso".

If the ball crosses an interval in S, it changes color upon completing the move but if it crosses an interval in $\bar{S}$, it performs the move without changing color.

"Suppose that we start with all white balls; the question is what happens after a large number of moves". Below (after eq. 3), we shall also consider other initial conditions.

Let us emphasize the analogy with mechanical laws. The balls are described by their positions and their (discrete) "velocity", namely their color. One of the simplifying features of the model is that the "velocity" does not affect the motion. The only reason I call it a "velocity" is that it changes when the ball collides with a fixed "scatterer", i.e. an interval in S. Scattering with fixed objects tends to be easier to analyse than collisions between particles. The "equations of motion" are given by the counterclockwise motion, plus the changing of colors (see eqs (5,6) below). These equations are obviously deterministic and reversible: if after a time t, we change the orientation of the motion from counterclockwise to clockwise, we return after t steps to the original state[98].

[98] There is a small abuse here, because I seem to change the laws of motion by changing the orientation. But I can attach another discrete "velocity" parameter to the particles, having the same value for all of them, and indicating the orientation, clockwise or counterclockwise, of their motion. Then, the motion

Moreover, the motion is strictly periodic: after $2n$ steps each interval has been crossed twice by each ball, hence they all come back to their original color. This is analogous to the Poincaré cycles, with the provision that, here, the length of the cycle is the same for all configurations (there is no reason for this feature to hold in general mechanical systems). Moreover, it is easy to find special configurations which obviously do not tend to equilibrium: start with all white balls and let every other interval belong to S (with $m = \frac{n}{2}$). Then, after two steps, all balls are black, after four steps they are all white again, etc... The motion is periodic with period 4. Turning to the solution, one can start by analysing the approach to equilibrium in this model à la Boltzmann:

Analog of the Classical Solution of Boltzmann. Let $N_w(t)(N_b(t))$ denote the total number of white (black) balls at time t (i.e., after t moves; t being an integer) and $N_w(S;t)(N_b(S;t))$ the number of white (black) balls which are going to cross an interval in S at time t.

"We have the immediate conservation relations:

$$\begin{aligned} N_w(t+1) &= N_w(t) - N_w(S;t) + N_b(S;t) \\ N_b(t+1) &= N_b(t) - N_b(S;t) + N_w(S;t) \end{aligned} \tag{1}$$

Now to follow Boltzmann, we introduce the assumption ("Stosszahlansatz" or "hypothesis of molecular chaos"[99])

$$\begin{aligned} N_w(S;t) &= mn^{-1}N_w(t) \\ N_b(S;t) &= mn^{-1}N_b(t)\text{''} \end{aligned} \tag{2}$$

Of course, if we want to solve (1) in a simple way we have to make some assumption about $N_w(S;t), N_b(S;t)$. Otherwise, one has to write equations for $N_w(S;t), N_b(S;t)$ that will involve new variables and lead to a potentially infinite regress.

The intuitive justification for this assumption is that each ball is "uncorrelated" with the event "the interval ahead of the ball belongs to S", so we write $N_w(S;t)$ as equal to $N_w(t)$, the total number of white balls, times the density $\frac{n}{m}$ of intervals in S. This assumption looks completely reasonable. However, upon reflection, it may lead to some puzzlement (just as the hypothesis of "molecular chaos" does): what does "uncorrelated" exactly mean? Why do we introduce a statistical assumption in a mechanical model? Fortunately here, these questions can be answered precisely and we shall answer them later by solving the model exactly. But let us return to the Boltzmannian story.

"One obtains

$$N_w(t+1) - N_b(t+1) = (1 - 2mn^{-1})(N_w(t) - N_b(t))$$

Thus

$$\begin{aligned} n^{-1}[N_wt) - N_b(t)] &= (1 - 2mn^{-1})^t n^{-1}[N_w(0) - N_b(0)] \\ &= (1 - 2mn^{-1})^t \end{aligned} \tag{3}$$

is truly reversible, and the operation I of note 26 simply changes that velocity parameter.

[99] The word "chaos" here has nothing to do with "chaos theory", and, of course, Boltzmann's hypothesis is much older than that theory.

and hence if

$$2m < n \tag{4}$$

(as we shall assume in the sequel) we obtain a *monotonic* approach to equipartition of white and black balls." Note that we get a monotonic approach for *all* initial conditions $(N_w(0) - N_b(0))$ of the balls.

The variables $N_w(t), N_b(t)$ play the role of macroscopic variables. We can associate to them a Boltzmann entropy[100] $S_b = ln \begin{pmatrix} n \\ N_w(t) \end{pmatrix}$, i.e. the logarithm of the number of (microscopic) configurations whose number of white balls is $N_w(t)$. Since

$$\begin{pmatrix} n \\ N_w(t) \end{pmatrix} = \frac{n!}{N_w(t)!(n - N_w(t))!}$$

reaches its maximum value for $N_w = \frac{n}{2} = N_b$, we see that (3) predicts a monotone increase of S with time. We can also introduce a partition of the "phase space" according to the different values of N_w, N_b. And what the above formula shows is that different elements of the partition have very different number of elements, the vast majority corresponding to "equilibrium", i.e. to those near $N_w = \frac{n}{2} = N_b$.

We can see here in what sense Boltzmann's solution is an approximation. The assumption (2) cannot hold for all times and for all configurations, because it would contradict the reversibility and the periodicity of the motion. However, we can also see why the fact that it is an approximation does not invalidate Boltzmann's ideas about irreversibility.

Let us reexamine the model at the microscopic level, first mechanically and then statistically. For each $i = 1, \cdots, n$, we introduce the variable

$$\epsilon_i = \begin{cases} +1 \text{ if the interval in front of } i \in \bar{S} \\ -1 \text{ if the interval in front of } i \in S \end{cases}$$

and we let

$$\eta_i(t) = \begin{cases} +1 \text{ if the ball at site } i \text{ at time } t \text{ is white} \\ -1 \text{ if the ball at site } i \text{ at time } t \text{ is black} \end{cases}$$

Then, we get the "equations of motion"

$$\eta_i(t) = \eta_{i-1}(t-1)\epsilon_{i-1} \tag{5}$$

whose solution is

$$\eta_i(t) = \eta_{i-t}(0)\epsilon_{i-1}\epsilon_{i-2}\cdots\epsilon_{i-t} \tag{6}$$

(where the subtractions are done modulo n). So we have an explicit solution of the equations of motion at the microscopic level.

[100] The simplifying features of the model (the balls do not interact) have the unpleasant consequence that the "full" Boltzmann entropy introduced here and defined in Section 5 actually coincides with (minus) the Boltzmann H-function. But, in general, the latter should only be an approximation to the former.

We can express the macroscopic variables in terms of that solution:

$$N_w(t) - N_b(t) = \sum_{i=1}^{n} \eta_i(t) = \sum_{i=1}^{n} \eta_{i-t}(0)\epsilon_{i-1}\epsilon_{i-2}\cdots\epsilon_{i-t} \tag{7}$$

and we want to compute $n^{-1}(N_w(t) - N_b(t))$ for large n, for various choices of initial conditions ($\{\eta_i(0)\}$) and various sets S (determining the ϵ_i's). It is here that "statistical" assumptions enter. Namely, we fix an arbitrary initial condition ($\{\eta_i(0)\}$) and consider all possible sets S with $m = \mu n$ fixed (one can of course think of the choice of S as being part of the choice of initial conditions). Then, for each set S, one computes the "curve" $n^{-1}(N_w(t) - N_b(t))$ as a function of time. The result of the computation, done in [60], is that, for any given t and for n large, the overwhelming majority of these curves will approach $(1-2\frac{m}{n})^t = (1-2\mu)^t$, i.e. what is predicted by (3). (to fix ideas, Kac suggests to think of n as being of the order 10^{23} and t of order 10^6). The fraction of all curves that will deviate significantly from $(1-2\mu)^t$, for fixed t, goes to zero as $n^{-\frac{1}{2}}$, when $n \to \infty$.

Of course when I say "compute" I should rather say that one makes an estimate of the fraction of "exceptional" curves deviating from $(1-2\mu)^t$ at a fixed t. This estimate is similar to the law of large number and (7) is indeed of the form of a sum of (almost independent) variables.

Remarks

1. The Poincaré recurrence and the reversibility "paradoxes" are easily solved: each curve studied is periodic of period $2n$. So that, if we did not fix t and let $n \to \infty$, we would not observe "irreversible" behaviour. But this limit is physically correct. The recurrence time (n) is enormous compared to any physically accessible time. As for the reversibility objection, let us consider as initial condition a reversed configuration after time t. Then we know that, for that configuration and *that set* S, $n^{-1}(N_w(t) - N_b(t))$ will not be close to $(1-2\mu)^t$ at time t (since it will be back to its initial value 1). But all we are saying is that, for the vast majority of S's this limiting behaviour will be seen. For the reversed configuration, the original set S happens to be exceptional. The same remark holds for the configuration with period 4 mentioned in the beginning.

   Note also that, if we consider the set of configurations for which $n^{-1}(N_w(t)-N_b(t))$ is close to $(1-2\mu)^t$ for *all times*, then this set is empty, because of the periodicity.

2 We could consider other macroscopic variables, such as the number of white and black balls in each half of the circle ($1 \le i \le \frac{n}{2}$ and $\frac{n}{2}+1 \le i \le n$), and define the corresponding entropies. We could go on, with each quarter of the circle etc..., until we reach a microscopic configuration (number of white or black ball at each site) in which case the entropy is trivially equal to zero (and therefore constant).

3 This model, although perfectly "irreversible", is not ergodic! Indeed since it is periodic, no trajectory can "visit" more than $2n$ microscopic configurations. But the "phase space" contains 2^n configurations (two possibilities -black or white- at each site). So, only a very small fraction of the phase space is visited by a trajectory.

> This nicely illustrates the fact that ergodicity is not necessary for irreversibility. What is used here is only the fact that the vast majority of configurations give to the macroscopic variables a value close to their equilibrium one.

Conclusion

I do not want to overemphasize the interest of this model. It has many simplifying features (for example, there is no conservation of momentum; the scatterers here are "fixed", as in the Lorentz gas). However, it has *all* the properties that have been invoked to show that mechanical systems cannot behave irreversibly, and therefore it is a perfect counterexample that allows us to refute all those arguments (and to understand exactly what is wrong with them): it is isolated (the balls plus the scatterers), deterministic, reversible, has Poincaré cycles and is not ergodic.

This result, obtained in the Kac model, is exactly what one would like to show for general mechanical systems, in order to establish irreversibility. It is obvious why this is very hard. In general, one does not have an explicit solution (for an n-body system !) such as (5,6), in terms of which the macroscopic variables can be expressed, see (7). It is also clear in this example what is exactly the status of our "ignorance". If we prepare the system many times and if the only variables that we can control are n and m, then we indeed expect to see the irreversible behaviour obtained above, simply because this is what happens *deterministically* for the vast majority of microscopic initial conditions corresponding to the macroscopic variables that we are able to control. We may, if we wish, say that we "ignore" the initial conditions, but there is nothing "subjective" here. Finally, I shall refer to Kac [60], for a more detailed discussion, in this model, of the status of various approximations used in statistical mechanics (e.g. the Master equation).

APPENDIX 2. On Spectral Representations.

I will briefly discuss the mathematical basis of the claim that "trajectories are eliminated from the probabilistic description." The relevant mathematics are nicely summarized in the Appendix of [98] and I shall therefore refer to that Appendix. Let T be an invertible transformation on a space X and let μ be a measure invariant under T. In [98], X is the unit square, T the baker's map and μ is the Lebesgue measure. We can associate to T a unitary operator U in $L^2\ (X, \mu)$ (or an isometry in any $L^p(X, \mu)$):

$$Uf(x) = f(T^{-1}x) \tag{1}$$

and $U^\dagger = U^{-1}$ is defined[101] by

$$U^\dagger f(x) = f(Tx) \tag{2}$$

[101] I follow here the conventions of [98] for the definition of $U, U^\dagger$. In [98] non-invertible transformations such as the Bernoulli map are also considered. U describes here the evolution of probability distributions and is called the Perron-Frobenius operator.

Since the operators U and $U^\dagger$ are *entirely defined* in terms of T, it seems bizarre, to put it middly, to claim that *any* property of U, for example its spectral properties, are "irreducible" to trajectories (i.e. to the action of T). "Irreducible" is a semi-philosophical notion so that one has some freedom in the way one uses this word, but I do not think that the meaning of the word in this context is close to what the general educated public has in mind[102].

Anyway, putting this issue aside, one should note that the operator U can be defined more generally. For example, U acts on distributions. In particular, writing $\delta_{x_0}(x) = \delta(x - x_0)$, we have, for a volume preserving map, like the baker's map,

$$U\delta_{x_0}(x) = \delta_{Tx_0}(x) \tag{3}$$

and the action of U on such delta functions just reflects the evolution of trajectories.

Another observation is that it has been known for some time that properties of the dynamics (of the map T), are reflected in spectral properties of U: for example, T is ergodic if and only if 1 is a non-degenerate eigenvalue of U.

The new feature discussed in [98] is that for chaotic systems such as the bakers's map one can write down a spectral representation of the form:

$$U = \Sigma |F_n(x,y)\rangle 2^{-n} \langle \widetilde{F_n}(x,y)| \tag{4}$$

where F_n (resp. $\widetilde{F_n}$) is a product of a polynomial in x (resp. in y) times a distribution in y (resp. in x).

This fact is very interesting mathematically but it is difficult to see why it implies radical consequences on "the laws of nature". The argument given in [98] is that the representation (4) cannot be applied to delta functions in x and y, which would represent a point, or a trajectory, because $\widetilde{F_n}$ involves a distribution in x and one cannot multiply distributions.

Let us see the force of this argument. Here is an analogy. Consider the operator $\frac{d}{dx}$. In $L^2(\mathbf{R}, dx)$ its spectrum is the imaginary axis and one can write

$$\frac{d}{dx} f(x) = \frac{1}{\sqrt{2\pi}} \int ik e^{ikx} \hat{f}(k) dk \tag{5}$$

where $\hat{f}(k)$ is the Fourier transform of f. Now obviously for this formula to hold, it is necessary that $\hat{f}(k)$ exists. But it is easy to find functions that are differentiable but whose Fourier transform does not exist. It would be strange to say that, for those functions, derivatives are eliminated from the irreducible representation (5). And, of course, as we have seen in (3) the operator U can perfectly be defined on delta functions. Simply the formula (4) does not apply in that case.

It is also claimed that formula (4) includes the "approach to equilibrium". But, as we discussed in Section 3, the notion of equilibrium does not make sense for a system with a single degree of freedom, so that this would be one more argument, if any more were needed, if favour of a dynamics expressed *fundamentally* in terms of trajectories.

[102] Compare with statements such as: "The mind is irreducible to the body" or "The behaviour of a crowd is irreducible to the psychology of individuals".

All this illustrates the remark made by J.T. Schwartz in his severe critique of the "pernicious influence of mathematics on science": "The intellectual attractiveness of a mathematical argument, as well as the considerable mental labor involved in following it, makes mathematics a powerful tool of intellectual prestidigitation - a glittering deception in which some are entrapped, and some, alas, entrappers." [109].

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