Stellar Dynamics and Molecular Dynamics: Possible Analogies

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Abstract. In stellar dynamics one is accostumed to deal with the Lynden–Bell distribution, which presents two peculiar characteristics: a) it resembles the quantum Fermi–Dirac distribution, and b) describes a state of metaequilibrium, that is expected to evolve on much longer time scales to a standard Maxwell–Boltzmann equilibrium. Here it is illustrated how an analogous situation seems to occur in molecular dynamics, described within the context of classical mechanics. The problem concerns the contribution of the internal degrees of freedom, typically the vibrations, to the specific heat; correspondingly, the metaequilibrium state leads to a Bose–Einstein–like rather than to a Fermi–Dirac–like distribution. We also point out that in molecular dynamics the "nonclassical" features seem to be related to the fact that the evolution of the energy is dominated by the presence of rare but conspicuous jumps, as in processes of Lévy type; this too has some analogies with stellar dynamics.

1 Introduction

About thirty years ago it occurred to one of the present authors, in collaboration with A. Scotti and C. Cercignani [1][2], to observe a quantum–like feature in the problem of the specific heats studied in the context of classical mechanics. The model considered was that of Fermi–Pasta–Ulam [3], describing a one–dimensional crystal with nonlinear interactions between adjacent atoms. The observation was that, if the specific energy was small enough and the energy was given initially to the lowest–frequency mode, the distribution of energy among the modes, estimated by numerical solutions of the equations of motion, turned out to have a Planck–like form. Moreover, to a great astonishment, even the action entering such a distribution turned out to be of the order of magnitude of Planck's constant. It took some months to become convinced that the latter fact was neither a mistake nor an accident, as we briefly recall now. The relevant point is that, in the model, realistic molecular parameters had been introduced; for example, for a model of crystal Argon, one introduces the mass m of Argon and a realistic Lennard–Jones interatomic potential $V(r) = 4\epsilon[(\sigma/r)^{12} - (\sigma/r)^6]$, with certain parameters ϵ and σ given in the literature. Now, one immediately checks that the action A naturally built up from the given parameters m, ϵ, σ is just $A = \sqrt{m\epsilon}\,\sigma$; on the other hand it turns out that for realistic parameters there exists the relation $A = 2Z\hbar$ where $\hbar$ is the (rationalized) Planck's constant and Z the atomic number. This remark explains how in classical models of molecular dynamics Planck's constant is introduced, so to say by hands, through the values of the molecular parameters.

In such a way an adventure was started, which consisted in trying to prove that, at least in the problem of the specific heats (see however [4], where the electrodynamics of point particles is also discussed), classical mechanics might not be inconsistent with quantum mechanics, or at least in understanding how something like this might make sense. Having explained in which way Planck's constant had been introduced, somehow by hands, through the molecular parameters, the main problem was then to understand how the dynamics itself, described by Newton's equations with given potentials, might make the job. This is the reason why intense contacts were started with scientists working in the field of dynamical systems, from mathematics to celestial mechanics. Perhaps the first among them was G. Contopoulos, with whom an everlasting friendship and scientific collaboration was initiated. The adventure had since then several phases, with several incursions into purely mathematical aspects of the theory of dynamical systems; a long collaboration with G. Benettin and A. Giorgilli took place, and finally the first of the present authors joined the party.

The present phase seems to be characterized by the realization that there should exist two (or several) well distinct relaxation times in the problem, as is now familiar in the physics of glasses or spin glasses. The point we want to stress here is that the existence of two relaxation times is a familiar feature in stellar dynamics too. Indeed, one there refers to a first "violent" relaxation to a metaequilibrium state of the type of Lynden–Bell [5] (during which the collisions can be neglected), that should then be followed by an extremely slow relaxation to a standard equilibrium state governed by the collisions. Moreover, such a phenomenon of the existence of a metaequilibrium state turns out to occur, in stellar dynamics, just in conjunction with the appearence of a quantum–like feature, namely the Fermi–like distribution of Lynden–Bell. These two features, namely the existence of a metaequilibrium state and its actual quantum–like aspect, constitute the analogies of stellar dynamics with the problem of the specific heats in classical mechanics which motivated the present talk, mainly addressed to people working in the field of stellar dynamics.

2 Planck's law, its interpretation by Einstein, and the points of view of Boltzmann and of Nernst

Planck's law is concerned with the mean energy U of a system of N harmonic oscillators of angular frequency ω at absolute temperature T. In terms of inverse temperature $\beta = 1/k_B T$ and of the quantum of energy $\hbar\omega$, where k_B is the Boltzmann's constant, it asserts that the mean energy U has the form

$$U = N \frac{\hbar\omega}{e^{\beta\hbar\omega} - 1} \ . \tag{1}$$

The relevant feature of this law is that in the limit of high temperatures or low frequencies, i.e. for $\beta\hbar\omega \ll 1$, it leads to the "classical" value $U = Nk_B T$, independent of frequency (this realizes the so–called equipartition of energy),

while it gives degeneration, i.e. the vanishing of U (in a frequency–dependent way), for $\beta\hbar\omega \gg 1$.

Everyone knows how Planck's law is usually deduced, by the standard equilibrium argument using the Maxwell–Boltzmann principle, if energy is assumed to be quantized, i.e. if one admits that the allowed values for the energy E are $E_n = n\hbar\omega$, $n = 1, 2, \cdots$. This standard argument in fact constitutes the second deduction given by Planck himself; a variant of it, which takes into account the zero–point energy $N\hbar\omega/2$, comes about if the energy levels are assumed to be given by $E_n = (n + 1/2)\hbar\omega$.

So, Planck's law turns out to be a consequence of quantization. Conversely, it was shown by Poincaré [6] (in one of his last papers, that he wrote under the stimulus of the discussions at the first Solvay Conference [7]) that quantization is necessary if one has to recover Planck's law at all. Actually, an accurate analysis of the paper of Poincaré shows that the situation is not so clear, and for example Einstein never was convinced of this necessity of quantization. This is witnessed by some remarks he made in his scientific autobiography [8]. Indeed, after having recalled how he himself had "*showed in a definitive and direct way that it is necessary to attribute a certain immediate concreteness to Planck's quanta and that, under the energetic aspect, radiation possesses a sort of molecular structure*", after a few lines he adds: "*This interpretation, that almost all contemporary physicists consider as essentially definitive, to me appears instead as a simple provisional way out*".

What did Einstein have in mind in saying these words? In our opinion the answer is found in his contribution to the Solvay Conference, where he showed how Planck's law can be obtained by arguments which make no reference to quantization at all. The key point is a physical interpretation of the procedure that had been followed by Planck in the first deduction of his law, on October 19, 1900. Let us recall that in that paper Planck had obtained his formula as a solution of the ordinary differential equation (we are using here a contamination of the notations of Planck and of Einstein)

$$\frac{\mathrm{d}U}{\mathrm{d}\beta} = -\big(\hbar\omega\, U + \frac{U^2}{N}\big)\,, \tag{2}$$

to which he had arrived, with no real physical interpretation, by a purely formal interpolation between two limit equations, well adapted to the cases of high frequencies and low frequencies respectively. What Einstein did, was to split such an equation into a system of two equations, namely

$$\frac{\mathrm{d}U}{\mathrm{d}\beta} = -\sigma_E^2 \tag{3a}$$

$$\sigma_E^2 = \hbar\omega\, U + U^2/N\,, \tag{3b}$$

where there appears a further quantity σ_E^2 having a well definite physical meaning, namely the variance of energy. Indeed the former equation (3a), relating specific heat to variance of energy, had been discovered in the year 1903 by Einstein himself in one of his first papers, as an identity in the canonical ensemble,

and was conceived by him as a kind of a general thermodynamic relation that should have some more general validity. In his mind, the second relation (3b) should instead have a dynamical character, and might in principle be deducible from a microscopic dynamics. In his very words [7]: these two relations "*exhaust the thermodynamic content of Planck's*" formula; and "*a mechanics compatible with the energy fluctuation* $\sigma_E^2 = \hbar\omega\, U + U^2/N$ *must then necessarily lead to Planck's*" formula. So the main idea is that the energy exchanged with a reservoir, i.e. the specific heat $\frac{\mathrm{d}U}{\mathrm{d}\beta}$, should be related to the energy fluctuations. In turn, the functional dependence of the energy fluctuations on the mean energy should fix the functional form of the mean energy in terms of temperature and of frequency. Clearly, no reference to quantization is made here.

Another key point enters now, which goes back to Boltzmann; we refer to the role of nonequilibrium. Indeed, being confronted with the phenomenological lack of equipartition of energy in crystals and in polyatomic molecules, Boltzmann conceived the idea that in such cases one was actually dealing with situations in which equilibium had not yet been reached. In his very words: [9] "*The constituents of the molecule are by no means connected together as absolutely undeformable bodies, but rather this connection is so intimate that during the time of observation these constituents do not move noticeably with respect to each other, and later on their thermal equilibrium with the progressive motion is established so slowly that this process is not accessible to observation*". In modern terms, the situations of nonequipartition of energy should be understood as analogous to the metaequilibrium situations occurring in glasses and spin glasses.

Let us recall that the idea of Boltzmann, according to which the situations of nonequipartition would correspond to states of metaequilibrium, was pursued for several years by Jeans [10], with the explicit aim of avoiding a recourse to quantization. But the work of Poincaré on the necessity of quantization made so strong an impression on him that he found himself forced to make a public retractation [11] and to abandon any further attempt in that direction. We will recall below how the problem was reopened much later, in the year 1954, by the work of Fermi, Pasta and Ulam. We would also like to mention that the idea of Boltzmann that there should exist a "time–dependent specific heat" is today accepted as a trivial fact, with no mention to Boltzmann at all (see for example [12]).

We add now here some further comments. The first remark is that there seems to be a strong relation between the point of view of Boltzmann and that of Einstein: the key point is the role of the dynamics in the problem of the specific heats. According to Boltzmann, what is relevant for the specific heat of a body is not the energy it possesses, but rather the energy it can exchange through the dynamical interaction with a heat reservoir within a given observation time. In turn, the latter energy, the exchangeable one, is related to the dynamical fluctuations of the energy of the body. This is in fact essentially the statement of the well known Fluctuation Dissipation Relation (see for example [13], of which the relation (3a) of Einstein seems to be a precursor. In fact, the Fluctuation Dissipation Relation has a form very similar to (3a), the main difference being

that it involves quantities having a dynamical character. Consider a system at inverse temperature β, and denote by $E(t)$ its energy at time t, and by $U(t)$ the mean energy exchanged up to time t with a reservoir at inverse temperature $\beta + \mathrm{d}\beta$. Then, the Fluctuation Dissipation Relation reads

$$\frac{\mathrm{d}U}{\mathrm{d}\beta} = -\frac{1}{2} < \big(E(t) - E(0)\big)^2 > , \tag{4}$$

the mean $< \cdot >$ being taken with respect to initial data with a Maxwell–Boltzmann distribution at inverse temperature β. From this, the static relation (3a) of Einstein is recovered at times so large that the autocorrelation of the energy vanishes, so that $E(t)$ and $E(0)$ become independent and thus the quantity $1/2 < \big(E(t) - E(0)\big)^2 >$ reduces to the static canonical variance σ_E^2.

A final comment concerns the role of the Maxwell–Boltzmann distribution as a statistical measure for the initial data, irrespective of the dynamics. The fact is that, for a system of independent harmonic oscillators distributed according to Maxwell–Boltzmann, one has for the mean energy U the value Nk_BT, i.e. formally equipartition of energy. But such an equipartition, as Boltzmann would say, concerns the mechanical energy possessed by the system just in virtue of the choice of the initial data, and has a priori nothing to do with the thermodynamic energy, which should be defined as the exchangeable one within the observation time. The latter, i.e. the thermodynamic or exchangeable energy, is instead measured by the dynamical fluctuations of energy. The fact that the initial distribution of energy presents a nonvanishing variance σ_E^2 is of no relevance for the specific heat, which depends on the exchangeable energy, i.e. on the dynamics.

This remark explains how one can have a situation in which there is both equipartition of energy in relation to the initial data, and Planck's law in relation to the exchangeable energy, as was first conceived by Nernst [14] in an extremely deep, almost unknown, work (see also [15]). In particular, Nernst also introduced a deep conception of the energy which, in the sense of Boltzmann, does not contribute to the specific heat: on the one hand it should be characterized as being, from the dynamical point of view, of ordered type (*geordnete*); on the other hand it would constitute a classical analog of the quantum zero-point energy. It is worth recalling the argument by Nernst. He assumes that the quantum of energy $\hbar\omega$ plays the dynamical role of a stochasticity threshold for the harmonic oscillators; the motions would be of ordered type below threshold and of disordered type (*ungeordnete*) above it. Furthermore, he assumes that the oscillators are distributed according to Maxwell–Boltzmann, so that one has equipartition for their mechanical energy. Now, one immediately computes the mean disordered energy (per oscillator) E_1, namely the mean energy conditioned by $E > \hbar\omega$ and the mean ordered energy (per oscillator) E_0, namely the mean energy conditioned by $E < \hbar\omega$, and one finds

$$E_1 = k_BT + \hbar\omega \quad , \quad E_0 = k_BT - \frac{\hbar\omega}{e^{\hbar\omega/k_BT} - 1} \quad . \tag{5}$$

Similarly one finds that the fraction n_1 of oscillators above threshold is $n_1 = \exp(-\hbar\omega/k_B T)$. Then the exchangeable energy U can be assumed to be defined by $U = Nn_1 (E_1 - E_0)$, which coincides with Planck's law.

3 The Fermi–Pasta–Ulam problem

The problem of a dynamical foundation for the principle of equipartition of energy in classical mechanics was reopened in the year 1954 by Fermi, Pasta and Ulam [3]. The interest of Fermi for this problem was indeed a rather old one, since it goes back to his work [16] of the year 1923 (which is sometimes misunderstood in the literature; see however [17]), where he improved the theorem of Poincaré on the integrals of motion of a Hamiltonian system. So Fermi came back to the problem when for the first time he had the facilities of a computer for the numerical integration of the equations of motion of a rather large system of particles.

As mentioned above, Fermi, Pasta and Ulam considered a system of N points (atoms) on a line, with a nonlinear interaction between adjacent atoms and certain boundary conditions; typically, the positions of the extreme atoms were kept fixed and the number N of moving atoms was 64. The interaction potential energy had the form $V(r) = r^2/2 + \alpha r^3/3 + \beta r^4/4$, with given constants α, β. For $\alpha = \beta = 0$ one has a linear system which, by a familiar argument, is equivalent to a system of N uncoupled harmonic oscillators (normal modes) having certain frequencies. The problem is then how many normal modes take part in the energy sharing, which should occur in virtue of the nonlinear interaction. The authors considered initial conditions in which the energy was given just to the lowest normal mode (i.e. to the mode of lowest frequency), and the aim was to observe, by numerical solutions of the equations of motion, the rate of the flow of energy towards the modes of higher frequency, which was expected to occur in order to establish the equipartition of energy among all the modes. They found the unexpected result that, up to the times considered, the energy appeared to be distributed just among a packet of normal modes of low frequency without flowing to the high frequency modes. They also gave a figure reporting the mean (in time average) energy versus frequency, which exhibited an exponential decay.

After this original work two more works had, in our opinion, a particularly relevant role, namely that of Izrailev and Chirikov [18] of the year 1966, and that of Bocchieri, Scotti and Loinger [19] of the year 1970. F.M. Izrailev and B. Chirikov understood that there existed the problem of an energy threshold. By analogy with the situations occurring in perturbation theory, in connection with the existence of ordered motions in the sense of Kolmogorov, Arnold and Moser, they conjectured that equipartition would be obtained if the initial energy E was larger than a certain threshold energy E^c. Thus the result of Fermi, Pasta and Ulam was explained as being due to the fact that only small energies, with $E < E^c$, had been considered. The crucial point is then to understand how does the critical energy E^c depend on the number N of degrees of freedom, because in situations of physical interest N should be of the order of the Avogadro

number. So one has to look at the specific critical energy $\epsilon^c = E^c/N$ (we are using here the symbol ϵ for the specific energy, with no relation to the parameter ϵ of the Lennard–Jones potential mentioned in the Introduction). The authors had clearly in mind the idea that one might prove that $\epsilon^c \to 0$ as $N \to \infty$ (at least for initial excitations of the high frequency modes). Indeed, if this were true, then in any physically meaningful system one would always have equipartition of energy. Then everybody would be happy, because this would prove that classical mechanics predicts a wrong result, as everyone has learned at school.

P. Bocchieri, A. Scotti and A. Loinger (working with a Lennard–Jones interaction potential) gave a strong indication in the sense that, on the contrary, the specific energy threshold ϵ^c should tend to a finite nonvanishing value in the limit $N \to \infty$. Shortly after such a work, in the paper [1] it was shown that, just in situations in which according to Bocchieri, Scotti and Loinger one does not have equipartition, the distribution of energy among the normal modes has a Planck-like form, and even with an action of the order of magnitude of Planck's constant. Thus the adventure was started of looking for a deeper understanding of the relations between classical and quantum mechanics.

After such "old" works, many other works followed (see for example [20] and [21]), mostly with the intent of establishing whether $\epsilon^c \to 0$ as $N \to \infty$, or not. The theoretical framework also changed a lot. Indeed, initially reference was made to KAM theory, while later the point of view of N.N. Nekhoroshev entered the game [22]. The attention was thus shifted towards the idea that one might always have equipartition as $t \to \infty$, but with the possibility that the relaxation time might increase exponentially fast as the specific energy decreases.

From this point of view, a recent relevant result was given, in our opinion, in the paper [23]. Here it is confirmed that the results depend on the specific energy ϵ, and actually in the following way. There exists a specific energy threshold ϵ^c such that, if the energy is initially given to a small packet of modes of very low frequency, the relaxation time to equilibrium (i.e. to equipartition) increases as a power of $1/\epsilon$ if $\epsilon > \epsilon^c$. Instead, if $\epsilon < \epsilon^c$, one has a first rapid (violent) relaxation to a natural packet extending up to a maximal frequency $\overline{\omega}(\epsilon) \simeq \epsilon^{1/4}$, while only on much larger time scales one would get equipartition. Moreover one starts now having analytical results confirming such a scenario, exactly in terms of the specific energy ϵ in the limit $N \to \infty$; such analytical results also give a precise analytical form for the spectrum corresponding to the natural packet mentioned above.

In conclusion, it seems that below a certain critical specific energy ϵ^c one deals with a metaequilibrium state, which is only later followed, on much longer time scales, by a real Maxwell–Boltzmann equilibrium. Such a scenario has many similarities with the one that is familiar for glasses and spin glasses, as was first pointed out in the paper [24].

4 The Landau–Teller model of molecular collisions

We finally give a short review on the contribution of the internal degrees of freedom to the specific heat of polyatomic molecules.

It was recalled above that we owe to Boltzmann the fundamental idea that the contribution of the internal degrees of freedom to the specific heat would manifest itself only on long time scales, in contrast to the degrees of freedom of the center of mass (also called external degrees of freedom), which are known to relax to equilibrium after a few collisions. We also recalled how Jeans gave support to the point of view of Boltzmann, but later made a public retractation after the work of Poincaré on the necessity of quantization. It is also of interest to mention that the problem of the existence of long relaxation times was discussed from the experimental point of view at the first Solvay Conference, by Nernst and others. The opinion was there expressed that there was no evidence at all for such longer relaxation times.

Actually such longer relaxation times were observed experimentally in the year 1925, in studies of the dispersion (and anomalous diffusion) of sound in diatomic gases [25], and it turned out that the times were even 6 orders of magnitude larger than the mean collision time. In looking for an explanation of such longer relaxation times, in a period in which the discussions of Boltzmann and Jeans had been completely forgotten, a most relevant contribution was given by Landau and Teller [26]. They considered an extremely simplified model capturing the essence of the problem, namely the exchange of energy in a collision between an atom and a linear spring, with an exponential interatomic potential. For the energy exchange δE they actually found an expression of a form already indicated by Jeans, namely $\delta E \simeq \exp(-\omega a/v)$, where ω is the frequency of the spring, a the range of the potential and v the velocity of the impinging atom. A relaxation time was then extracted from such a formula of the exchanged energy, and the common opinion was formed that the theory fits rather well the experimental data [27]. Serious doubts were however raised concerning the goodness of the agreement, as is witnessed for example by the following quotation [28]: "*It is impossible to determine whether the choice of the potential parameters is physically significant, because all errors in the theory are compensated by adjustable potential parameters*".

In any case, the common opinion is that the approach to equilibrium should be controlled by a single relaxation time, say τ_L. Correspondingly, the law of temporal approach should be exponential [29], of the type $\exp(-t/\tau_L)$, as is familiar in the Onsager theory. We are of the opinion that the situation is here, however much more delicate. Indeed the Onsager theory is well suited for the approach to equilibrium of systems presenting a completely chaotic dynamics, while we are here confronted with the opposite situation, namely with systems that are nearly integrable, for which no one was able up to now to produce a statistical mechanics compatible with the dynamics (see however the papers [30], where indications are given that the statistics might be given according to the ideas of Einstein recalled above).

Further results were recently given in the paper [31]. There it is shown that the statistics induced by the dynamics in the Landau–Teller model of molecular collisions is a rather complex one, because over very long times the processes of the energy exchanges have many similarities to the well known Lévy processes, which are dominated by the presence of rare, but highly conspicuous, jumps. For an analogous situation in stellar dynamics see [32]. We are still working on the Landau–Teller model, and we hope to be able to show that here too one meets with two relaxation times, the shorter one leading to a state of metaequilibrium characterized by a "nonclassical" statistics, and the second one leading to the final Mazwell–Boltzmann equilibrium. This fact might be of interest for the phenomenology of sound dispersion.

5 Conclusions

We have recalled above how strong was the impact of the theorem of Poincaré concerning the necessity of quantization if the phenomenological law of Planck is to be recovered. In fact, after that work essentially all attempts at providing a classical understanding of Planck's law along the lines indicated by Boltzmann were abandoned. Peculiar exceptions were Einstein, who never proved convinced, and Nernst, who introduced the dynamical interpretation for the zero–point energy illustrated above.

Now, if one looks at the proof of Poincaré's theorem, it seems evident that the fundamental hypothesisis there made is that one should be dealing with a real equilibrium (i.e., almost by definition, with the Maxwell–Boltzmann distribution). And in fact on several occasions Poincaré had stressed that, if one recedes from equilibrium, one cannot have any thermodynamics at all: everything would become fuzzy. It seems difficult to disagree. Let us quote Poincaré himself [33]: "*Jeans tried to reconcile things, by supposing that what we observe is not a statistical equilibrium, but a kind of provisional equilibrium. It is difficult to take this point of view; his theory, being unable to foresee anything, is not contradicted by experience, but leaves without explanation all known laws*".

But in fact it seems to us that there is a possibility of getting a thermodynamics without a full equilibrium. The possibility is that one would be actually dealing, not just with a nonequilibrium, but rather with a situation of metaequilibrium, as for example in the case of glasses. In such a case one can have situations which apparently are indistinguishable from situations of a true equilibrium, so that the critique of Poincaré could be overcome. This is exactly what we are proposing, and is the reason why in this review we insisted in a particular strong way on the relevance of being able to findi at least two time scales in the problem of diatomic molecules (the existence ot two time scales in the Fermi–Pasta–Ulam problem being, by now, almost granted): only on an extremely long time scale would one get a true equilibrium, while on a first, short, time scale one would reach a metastable state (as in stellar dynamics). In turn, the termodynamics of the metastable state could not be described by the usual procedure of the type of Onsager, beacuse the latter makes reference to a chaotic dynamics,

as stressed by Bowen, Ruelle, Sinai and by Gallavotti. In the state of metastability one is instead dealing, from a dynamical point of view, with the other extreme situation, namely with nearly integrable systems, and the suitable thermodynamics could perhaps be recovered along the lines suggested by Einstein and by Nernst. So, in conclusion, our suggestion is that, according to classical mechanics, Planck's law would describe a metaequilibrium state, at variance with quantum mechanics which interprets it as referring to a true equilibrium. It would be of a certain interest to ascertain whether the phenomenology might prove to be consistent with the scenario of metaequilibrium.

Finally we add a comment about Poincaré. It would appear that, if we are right in suggesting that the metaequilibrium scenario is a priori theoretically consistent, then Poincaré would be wrong. Thus we were very glad in discovering that Poincaré himself had some doubts about his attitude, essentially because he had to admit that, after all, there are more things in heaven and earth than his philosophy could imagine. Indeed, just a few lines after the destructive sentence concernig Jeans quoted above, in connection with the quantization of energy he added [33]: "*Will discontinuity reign over the physical universe and will its triumph be definitive? Or rather will it be recognised that such a discontinuity is only an appearence and that it dissimulates a series of continuous processes? The first person that saw a collision believed to be observing a discontinuous phenomenon, although we know today that the person was actually seeing the effect of very rapid changes of velocity, yet continuous ones*". It is true that Poincaré also adds the skeptical conclusion: "*To try to express today an opinion about these problems would mean to be wasting one's ink*". But at least we are comforted in learning that he admitted that other scenarios (such as that of Boltzmann, Einstein and Nernst, we would say) might be consistent.

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