Foundations of physics in Milano, Padova and Paris. Newtonian Trajectories from Celestial Mechanics to Atomic Physics

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Abstract

This paper is written, in a very informal and colloquial style, on the occasion of the seventeenth anniversary of Antonio Giorgilli. The aim is to describe how his first scientific works were actually conceived within a group that happened to be formed in the years seventies with an ambitious program on the foundations of physics. Namely, to understand whether the recent (at those times) progress in Dynamical Systems Theory might allow one to enlight in some new way the relations between quantum mechanics and classical physics. This required to understand what impact dynamical systems theory may have on the foundations of classical statistical mechanics (with particular attention to the Fermi Pasta Ulam problem), and on matter-radiation interaction. In such a frame Celestial Mechanics too started to be addressed. Particularly by Antonio, initially just as a kind of a byproduct. Here a recollection is given of how the group was formed. Then a quick review is given of the results, obtained since those times, which are relevant for the original foundational program.

1 Introduction

So I will recall how a group, involving Antonio Giorgilli, Giancarlo Benettin, Jean-Marie Strelcyn and me, with the supervision of Tonino Scotti and Carlo Cercignani, happened to be formed in the years seventies, in a quite peculiar atmosphere, around the foundational problem mentioned. Then I take this opportunity to give a quick review of the results obtained since those times, restricting the attention to those relevant for the original foundational program.

The way the original group was formed is recalled in section 2, and then the review of the results is given. First of all, the early mathematical results obtained in perturbation theory (with just a mention of celestial mechanics) in section 3. Then, are recalled the results of interest for the foundational problem

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at hand starting, in section 4, from those of interest for the FPU problem and the dynamical foundations of Statistical Mechanics. For what concerns matter-radiation interaction. the results of a general type are given in section 5, whereas the applications to atomic physics, plasma physics and high-energy physics are given in section 6. The conclusions then follow in section 7.

2 The foundational problem raised, and a research group established between Milano, Padova and Paris

Everything started in the year 1971 within the group of theoretical physicists in Milano. Angelo Loinger had come to know (through a work of Chirikov) of the Fermi-Pasta-Ulam problem (perhaps the last one of Fermi), that appeared to challenge the common wisdom about the failure of classical physics which had given origin to quantum mechanics. The problem concerns first of all the principle of energy equipartition, a pillar of classical mechanics that had been replaced by Planck's law, first in the black body by Planck in the year 1900, and then in the specific heat of solids by Einstein in the year 1907. Now, the FPU work appeared to show that energy equipartition fails in Classical Mechanics too, and this might appear to put in doubt the common wisdom about the relations between Classical and Quantum Mechanics. So, a numerical work on a variant of the FPU model was performed by Loinger together with Bocchieri and Scotti [3], and the result seemed to support the indications of the FPU work.

Thus Loinger gave a talk that still is vividly impressed in my mind. Two are the points, he said, which gave origin to quantum mechanics, namely:

1. Black–body and specific heat of solids (1900 and 1907),
2. Falling of the electrons on the nuclei by radiation emission (Rutherford 1911 and Bohr 1913).

According to Loinger, the statement that classical physics fails in such points was unjustified. What was certainly true is that the concrete predictions of classical physics were unknown, since the mathematics needed to settle the question was still lacking. Such a way of looking at the problem was in touch with what Einstein used to call, as I came to know later, his "Classical Program". In the words of Carlo Cercignani such a program can be summarized in the idea of "the shortcut" (in italian, "la scorciatoia"). Namely: quantum mechanics is, no doubt, the correct theory, but the possibility is still open that it may be recovered, as a kind of theorem, in a classical approach to atomic physics (an approach which, by the way, requires a mutual involvement of mechanics and electromagnetism).

The reaction of the Milan's theoreticians to Loinger's talk was very skeptical I instead was fascinated, and started working on the problem, together with

Tonino Scotti, who soon became my master. Also Carlo Cercignani, that among the young researchers in Milano was unanimously considered by far the more gifted one, soon joined us. This story I already told elsewhere [4].

So, together with Tonino Scotti and Carlo Cercignani, we started studying some of the many problems involved [5, 6, 7], which are both of a mathematical and of a physical character (or perhaps of one and the same character, if one agrees with Arnold's statement, that mathematics is just a chapter of physics). Moreover, one is confronted here with the great difficulty of even having to grasp the way itself in which should the problem be framed.

The first clear point was that we had to become acquainted with the recent progress in dynamical systems theory, because in the FPU problem one meets with a quite paradoxical situation. Namely, one deals with a perturbation of an integrable system, i.e., a system of N independent subsystems – actually harmonic oscillators – thus presenting N independent integrals of motion. On the other hand, the common wisdom on the applicability of the standard methods of statistical mechanics seems to require that the perturbed system should become ergodic, i.e. should have just one integral of motion – the total energy – no matter how small the perturbation is. Which is indeed paradoxical.

It then happened to Tonino Scotti and me to meet at a conference Joe Ford, who kindly indicated to us the relevant papers on KAM theory. We also started understanding the contributions of George Contopoulos and of Michel Hénon, namely, the strange different ways in which an integral of motion can exist, and how "chaos" and those things usually show up when a perturbation is added to an integrable system. Apparently no one in Italy was aware of such things at those times, not even among the pure mathematicians. I probably still have in one of my drawers some handwritten pages that illustrated such facts, and were sent to Rosenfeld, the pupil of Niels Bohr.

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In the meantime, the fascination I had received from Loinger was literally transmitted to Antonio Giorgilli and Giancarlo Benettin. With Antonio this occurred through a kind of seminar for students I had given in Milano. Thus he started a thesis on the construction of the integrals of motion by perturbation methods, studying in particular the works of Contopoulos [8]. His results were described in his first two papers [9][10] (see also [11]), in which he expounded the beginnings of what later became his original way of performing a direct construction of the integrals, i.e., one not defined in terms of canonical changes of variables.

Shortly afterwards Antonio happened to become involved in numerical computations. This occurred in a way of which I'm very proud, as a master. Indeed, I had previously been involved in numerical computations on the FPU problem by Tonino Scotti, from whom I had learned in the simple and natural way a baby starts playing piano by just imitating his father or mother. So I started telling Antonio that he too should learn how to perform numerical computations. But he repeatedly told me he surely was not gifted for that, because he had already made an attempt, after reading some book, and he decided he was unable, or even he definitely disliked the thing. However I insisted and, going to the blackboard, I showed him, in perhaps three minutes, the four elementary rules that are necessary and sufficient to do essentially everything on a computer (apart from some trivial rules on how to insert or read data). Then he went home, and a few days later came back with a program he had produced for implementing his very method for constructing integrals of motion, and a paper was soon published in the journal Computer Physics Communications [12]. Not much later he came to be unanimously considered among the best experts in Milano, in the field of numerical computations.

With Giancarlo the cooptation occurred through a copy of an ideological paper of I had just written down (by the title *Classical Mechanics and Quantum Mechanics*), that in a very fortuitous way had arrived on his desk at the Physics Department of Padova, in a period in which, after having graduated, he was involved in his military service. He thus visited me in Milano, and the decision was taken that we would work together, as soon as he would come back from his military engagement.

In the meantime Jean–Marie Strelcyn had already entered the game in some strange way. I had found in the library a book on Dynamical Systems Theory, containing lectures given at Warsaw by Sinai and edited by Jean-Marie. Now, in the preface there was written that a second volume would follow. Thus I wrote a letter to him, asking when would the second volume be published. The answer came much later from England. He told me that “after the facts occurred in the year 1968” he had to leave Poland, was at the moment in England, and would permanently live in Paris. So one day, being by chance in Geneva and knowing he was already in Paris, I called him on the phone, took a train and went to meet him there. So started a collaboration and a friendship for a life. His competence on Dynamical Systems proved to have a fundamental role for us. For example, never could I have imagined that something as the “shadowing lemma” for chaotic motions may exist. More concretely his contribution had a strong impact on our works concerned with Lyapunov exponents. In that connection I had already produced a work, together with Mario Casartelli, Emilio Diana and Tonino Scotti, implementing a method that had been suggested to Tonino and me by Arnold, during our one–month visit to Moscow and Dubna in the year 1974 (we had in fact a discussion of about six hours with Arnold and two discussions of about one hour with Kolmogorov). However, now with Jean-Marie everything became much clearer, and so we decided to write a paper collecting the relevant notions about Lyapunov exponents.

That was indeed the time Giancarlo was coming back. I thought he might contribute to the paper by exhibiting a numerical example, just in the celebrated Hénon-Heiles model. In such a way had origin the work Benettin–Galgani–Strelcyn on Lyapunov exponents,[13] that is still considered a reference paper on the subject, in addition to the subsequent paper to which Antonio too took part [14]. What impressed me about Giancarlo in such a first concrete collaboration is that, when we were performing our first run of the computation, the answer was an overflow, which concretely means that some quantity had become infinite, a fact often indicating that some error was made in writing the program.

Now, I had just explained Giancarlo how to implement the standard Verlet (or leap–frog) integration method (actually, the method employed by Newton himself in his first proof of a theorem - conservation of angular momentum in a central field, using his first and second laws). So I started looking for the mistake in the program. Instead he immediately pointed out to me that the reason might have been a different one. Namely, since we were concerned with the motion of a particle in a cubic potential, perhaps the program was correct, and the overflow had shown up because of the choice of the initial data, which would make the particle escape to infinity, just in virtue of the dynamics, instead of remaining confined in the potential well. Which was indeed the case. So his attention had been put already on the study of the model, rather than on the implementation of the integration method, which was new for him.

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In such a way the original group of the four of us, Antonio (Giorgilli), Giancarlo (Benettin), Jean-Marie (Strelcyn) and me, with the supervision of Tonino Scotti and Carlo Cercignani, was formed, and lasted for many, beautiful years. In the meantime Dario Bambusi, Andrea Carati and Antonio Ponno[1] had come in, essentially in the same enlarged group, together with several others such as Diego Noja, Andrea Posilicano and Ugo Bessi, Simone Paleari, Massimo Miari (with the great Marco Brunella), Lia Forti, ...

Then, little by little, the group started somehow separating. Antonio, after some relevant contributions to the FPU problem, started to be mostly interested in the mathematics of Small Denominators and in Celestial Mechanics, on which he worked with Alessandro Morbidelli, Ugo Locatelli, Marco Sansottera and Tiziano Penati. In Padova, Giancarlo continued to work in the FPU problem, in Perturbation Theory and on other features of Dynamical Systems, with Francesco Fassó, Massimiliano Guzzo, Antonio Ponno and Helen Christodoulidi. In Milano, Dario Bambusi worked on several mathematical aspects of perturbation theory, looking in particular at its extension to Partial Differential Equations (PDE's). Jean-Marie remained aside in Paris.

The original foundational project, concerning the Einstein classical program of the shortcut, continued to remain the center of interest for a remnant of the group, i.e., Andrea Carati and me, with the later joining of Alberto Maiocchi, and of the brothers Fabrizio and Roberto Gangemi in Brescia. In the meantime the role of the leader of such a "Classical Program (or Shortcut) group" had actually passed, especially since the year 2003, to Andrea Carati, to whom are due the opening and the implementation of relevant innovative perspectives, both in the frame of statistical mechanics and of matter-radiation interaction.

[1] The way Antonio Ponno entered the group is very peculiar. Once I was invited by Giovanni Gallavotti to give a talk in Roma on the FPU problem. After a few days I received a mail from Antonio Ponno, who told me he was an undergraduate student in Roma who happened to attend my talk. And he was able, he told me, to solve one of the open problems I had mentioned. Which was indeed the case, and led us to a joint paper with Francesco Guerra and me [15].

3 Mathematical results in perturbation theory

I already mentioned how we had started some studies of a mathematical character on dynamical systems, with our works on **Lyapunov exponents**.

KAM theorem

Great attention was then given to Perturbation Theory, because of the interest it presents for the foundations of classical statistical mechanics (a recent review can be found in [16]). The main point was the paradoxical theorem of Poincaré, according to which a perturbed integrable Hamiltonian system in general has just one integral of motion (the energy), no matter how small the perturbation is, whereas the unperturbed system has N integrals, which apparently is against the intuitive conception that some sort of continuity should occur . Some outstanding contribution was needed here, which actually took more than fifty years to be invented, by Kolmogorov. Indeed, continuity in measure is guaranteed by the Kolmogorov-Arnold-Moser (or KAM) theorem (1954, 1961): in general there exist invariant surfaces, actually tori in the relevant cases, (with chaotic motions "between them"), Moreover, the measure of the invariant tori becomes the full measure as perturbation tends to zero. In our group the first to understand KAM theorem was Carlo Cercignani, who wrote down some unpublished notes inspired by a version of the proof available in a book by Salomon. However, this was not enough to make us comfortable with the theorem.

So we started reading the version of the theorem given in the celebrated paper of Arnold, and some similar versions. Actually, in the paper of Arnold one finds some sentences which may give the impression that the original Kolmogorov theorem (of the year 1954) might not really provide a proof. Moser too told me on two occasions (in Princeton and in Milano) that he really doubted that the Kolmogorov proof be correct. He also told me he had written some comments about this point, in the account of the Kolmogorov paper he had given in the journal Mathematical Reviews, but actually we never checked this. So we started studying the Arnold version. However, I was particularly unsatisfied, because I felt everything was too complicated for me. One evening Antonio and me were coming back from Padova to Milano by train, after having discussed the proof with Giancarlo for many hours, and in the train it occurred to me to remember that I had recently found a copy of the original Kolmogorov paper. So I took it out of my bag (which had been a very difficult operation, since the train was filled up with many many people and we were standing up tight as sardines among them), and I saw that the paper was perhaps just five pages long (i.e., very very short) and the statements of the several parts composing the full theorem were extremely clear. In addition, the proof too seemed to be rather simple, and in particular the series entering the perturbation method were even convergent (at variance with those met in the Arnold method). This I immediately told to Antonio, lending to him the paper. In a moment he understood it, even concerning a passage that I had skipped (related to a translation of the actions, characteristic of Kolmogorov's method). The only point that was not explained in detail in Kolmogorov's paper was the check of the convergence of the sequence of successive perturbation steps that had to be performed, since he just was making reference to some known procedure. The convergence was later easily proved by Antonio making use of his beloved direct construction. However, the next time the three of us met, Giancarlo suggested that, in publishing an exposition of the Kolmogorov version, a more standard procedure in proving convergence should be used, in order that the attention be put on the theorem, which was our main objective. The more standard procedure was easily implemented. Jean-Marie was consulted, and after he could overcome some difficulties he had found, the paper was finished [17]. This I consider a real contribution offered to the scientific community, because otherwise the original Kolmogorov approach, which is also the simpler one, might have remained unknown for a long time. A new slightly different version of the theorem, along the Kolmogorov lines and inspired by our paper, was subsequently given within the group around the "Arnold seminar".

Nekhoroshev's theorem

The Nekhoroshev affair too was a very beautiful experience, touching even our life. Once it had occurred to me to read about his theorem (and about Neishtadt's one) in the fundamental book of Arnold by the original Russian title "Supplementary Chapters ..." (in Chapter 3, I seem to remember). The frame is the same as with KAM theory, but now one addresses the problem of controlling (i.e., finding upper bounds for) the time derivative of the actions, uniformly in an open domain of phase space, somehow dealing globally both with the putative invariant surfaces, and with the variation of the actions in the complementary "resonant zones". Actually, the distinction between such two complementary domains actually occurs only in the course of the proof, and not in the statement of the theorem, which says that, in general, in a slightly perturbed integrable Hamiltonian system the actions are quasi integrals of motion up to times which are exponentially long as perturbation tends to zero. In fact, when I first red about the theorem, it came to my mind that Arnold himself had explained the main idea to Tonino Scotti and me during our visit to him. I remember him drawing on the blackboard the now very well known figure displaying two intersecting resonant zones.

Now, the proof given by Nekhoroshev [18] was not at all easy: the analytic part was available only in Russian, and the statement of the corresponding lemma, involving a very large number of suitable constants, took, for what I remember, something as a full page, or a little more. The geometric part too, which constitutes the actual original Nekhoroshev's contribution, was rather complicated, and Giancarlo was the first among us to really capture it. In fact, it is the whole theorem that is complicated, and it is thus quite natural that people as us, having available a concrete version of a proof obtained with great difficulty, might produce some simpler version. This is what we did, with a paper published in the journal Celestial Mechanics [19].

Then I once met Kolya (i.e. Nikolay Nekhoroshev) in Kiev, where I was attending a conference together with Pierre Lochack, that had previously been my guest for one or two months in Milano, and had thus been introduced to the subject, to which he also gave a very interesting contribution. So Kolya became a great friend of ours, and lived in Milano for perhaps five years, a large part of which at my apartment. He was very shy, timid and extremely honest, and we passed many evenings (together with two nephews of mine who were studying Engineering in Milano), looking on TV at football (i.e., soccer) games, of which he was very fond. I also organized for him to become a Professor at the Milano University. Many times he told me he was very grateful to our italian group, for having made his scientific contribution to perturbation theory become known outside Russia. Actually I never met a non russian person that even knew of his name, before our paper did appear.

Carati's theorem

KAM theorem and Nekhoroshev's theorem were stated and proved in the style of existence theorems; for slightly perturbed integrable Hamiltonian systems with N degrees of freedom: there exist invariant tori whose measure tends to the full one (KAM); in an open domain the actions are quasi integrals of motion up to exponentially long times (Nekhoroshev). This is what matters when one invents new ideas and implements them, having to wait, in our case, for something between fifty years and a century since the problem had been opened.

Then comes the problem whether theorems are useful, in the elevated sense of being apt for describing nature. It seems that the first who conceived to check the applicability of KAM theorem to celestial mechanics was Hénon who showed that, using the estimates available from (perhaps) Arnold's proof, the theorem could be applied if Jupiter's mass were smaller than that of a proton. Something like that occurred for Nekhoroshev.s theorems too. However, many works were later performed about such problems, and it was shown, particularly around Antonio and with the relevant contribution of the group of our friends from Barcelona, that realistic estimates can be obtained in Celestial Mechanics, both in KAM and in Nekhoroshev frame. This will perhaps be illustrated in several contribution to the present volume.

Analogous problems occur for the extension of KAM and Nekhoroshev's theorems to the field of continuous bodies, described by PDE's, or of discrete systems constituted by N particles, in the limit $N \to \infty$. Actually, in the latter case the problem of interest for statistical mechanics is the so-called thermodynamic limit, in which one considers finite values of specific energy $\epsilon = E/N$ and of specific volume $v = V/N$.

In the case of the thermodynamic limit, which is the one of interest for our foundational problem, a positive result was obtained by Andrea Carati [20]. In our group, since a long time we knew that Nekhoroshev's theorem cannot be extended to the thermodynamic limit in a naive point wise way, i.e., uniformly for all points of an open domain of phase space, because there exist peculiar points which pose hard difficulties. So, some new idea was needed.

Such an idea was found, and in a quite natural way, indeed. The fact is that in statistical mechanics one is concerned with mean values with respect to a given invariant measure and, on the other hand, taking mean values is, so to say, a smoothing operation. Following such an idea, and overcoming serious technical difficulties, Andrea Carati was able to exhibit on a significant example that perturbation theory can be implemented at the thermodynamic limit in such a weak, statistical sense [20]. Applications are presently being performed to FPU-like systems of interest for statistical mechanics [21][22][23][24][25].

This is a quite interesting result because in the "FPU community" for a long time the dominant conjecture had been that in the thermodynamic limit perturbation theory cannot be implemented, so that, incredibly enough, only chaotic motions could exist. This would imply that the standard procedures of classical statistical mechanics, based on Gibbs' ensemble, can be applied at all temperatures. On this point I will come back in the next section.

For what concerns PDE's, "since always" [26] many examples of integrable systems were known, such as typically the one described by the Korteweg-deVries equation, On the other hand the works of Kruskal and Zabusky of the years around 1966 (perhaps the first ones at all making reference to the FPU work) strongly supported the idea that perturbation results for perturbed integrable systems may hold also for PDE's. This was proved, first by Sergej Kuksin for what concerns finite dimensional KAM tori, and eventually by Dario Bambusi (and later by others) for PDE's in the case of space dimension 1 [27].

4 Back to foundations: the "mechanical" FPU problem and the dynamical justification of classical statistical mechanics

More than sixty years did elapse, presently, since the FPU problem was raised. The problem was to check whether dynamics confirms that classical mechanics really predicts energy equipartition, say for oscillators, against quantum mechanics that predicts Planck's law, and in particular predicts, typically for solids, the vanishing of the specific heat for $T \to 0$ (where T is absolute temperature). FPU had made the "little discovery" that, starting from standard FPU–type initial data (i.e., with energy given just to a small number of modes of very low frequencies), pretty soon a state of apparent equilibrium is attained (the so–called formation of the packet), with energy concentrated on low-frequency modes and with an exponential decay at the higher frequencies (a state reminding of a Planck-like distribution). What happened in the more than sixty elapsed years?

I dare to summarize the present situation in the following way. The FPU "final" state actually turns out to be a state of apparent equilibrium. As time goes on, equipartition is eventually attained.[2] This was usually called the metasta-

[2] Apparently, this was first seen in the work [28], performed by following an idea of Antonio Giorgilli.

bility perspective. Moreover, the times needed for such a relaxation occur in the sense of the thermodynamic limit, i,e,, depend on specific energy $\epsilon \equiv E/N$, and in particular diverge as $\epsilon \to 0$.[3] A similar and even stronger result was obtained quite recently for a realistic FPU-like model describing ionic crystals, that will be illustrated in section 6. In such a case it was even checked that the fluctuations of the mode-energies are eventually in agreement with the Maxwell–Boltzmann distribution.

So, at first sight such results seem to invalidate the original FPU "little discovery", and to confirm the failure of classical mechanics. In the opinion of our "remnant group" (Andrea Carati, Alberto Maiocchi, Fabrizio and Roberto Gangemi, and me), however, the situation is more complicated. The point we make concerns essentially the identification of absolute temperature T in terms of mechanical energy. Usually one gives for granted that in classical statistical mechanics one should have, even in the case of a crystal, the identification of temperature with specific energy which is normally employed in the case of dilute gases, namely,

$$\epsilon = \frac{E}{N} = \alpha T \tag{1}$$

with a certain constant α. If this were justified, the specific heat would be independent of temperature, instead of vanishing at zero temperature, thus confirming the failure of classical physics.

Now, the familiar interpretation (1) certainly holds when one makes use of the Gibbs ensemble, which is dynamically justified for ergodic systems, as typically a dilute gas is supposed to be. However, we point out that any dynamical model of a crystal is certainly not ergodic. *Indeed, at the initial time the particles composing the crystal have a certain definite ordering, which remains unmodified until the dynamics continues to describe a crystal (say, millions of years). Thus, up to such times the system does not attain any of the other* $N! - 1$ *points of phase space (all having, in typical cases, the same energy) that describe the crystal with different permutations of the particles.* In other terms, it occurs that any putative model of a crystal, until it describes a crystal, does not behave as an ergodic system, inasmuch as it explores only an extremely restricted region of an energy surface. So the use of the Gibbs ensemble, and the familiar identification of temperature, is not justified. In such non ergodic situations, the quantities of interest should be defined in terms of time averages, as amply discussed after the Einstein talk at the first Solvay conference. However, no systematic studies on the definition of statistical thermodynamics in terms of time averages apparently exist (at least to my knowledge), apart from the works [31, 32] of Andrea Carati.

This is the reason why, in our opinion, the possible failure of classical statis-

[3] The check the dependence on specific energy required a hard labour, in particular from the part of Giancarlo Benettin and his collaborators [29, 30]. I'm skipping here quoting a mass of works which took a huge labour from many people - especially in Italy - with whom we were having strong discussions during many, many years. I'm thinking of Giorgio Parisi, Stefano Ruffo, Roberto Livi, Marco Pettini, Mario Casartelli, Jayme de Luca, Alan Lichtenberg, Bob Rink, Thomas Kappeler ... *Meminisse iuvabit.*

tical mechanics in connection with the vanishing of the specific heat of solids, i.e., of the third principle of thermodynamics, is still an open problem. We point out, however, that some recent studies on realistic FPU-type models already mentioned, that will be illustrated in section 6, give indications that the correct relation for the state equation (i.e., the relation $E = E(T)$ for example at fixed pressure) should be of the form

$$\frac{E}{N} \to \frac{E_0}{N} \quad \text{for } T \to 0 \ , \quad \text{with } E_0 > 0 \ , \tag{2}$$

i.e., that in classical physics too there should exist a non vanishing zero-point energy, which means that Nernst's third principle would be satisfied. More in general, the point we are making actually concerns a distinction between mechanical energy (the one that manifests itself in the Debye-Waller phenomenon) and thermal energy (a name that indeed exists "since always" in phenomenological thermodynamics, for example in all books of Nernst). And our last results on the realistic FPU-like models seem to indicate that the state equation, i.e., the relation $E = E(T)$, (for example at fixed specific volume) in classical models of solids should possess the property (2).

In any case, a big problem remains open, which may hinder the physical significance of all the FPU of FPU-like models discussed so far, even the ones that were here denoted as realistic. I mean the fact that all of them are of a purely mechanical character, i.e., do not take into consideration the radiant electromagnetic field with which the considered body should be at equilibrium. As strongly stressed in the first pages of the Einstein's paper on specific heats, from such a point of view a black body and a crystal are the same thing: i.e., dynamical systems constituted of matter and field (with all typical properties of the latter, first of all retardation). No result was yet obtained in such a direction, but the results of a general character illustrated in the next section seem to be promising.

5 Back to foundations: Matter–Radiation interaction. Progress along the lines of the Einstein Classical Program

I come now to matter-radiation interaction. Actually, since Andrea Carati and me plan to give a review of this subject on the occasion of a meeting that should be held in Milano the next month of may in commemoration of Carlo Cercignani, here I will just give a draft of what we plan to present there.

Matter-Radiation Interaction, and the Einstein's Classical Program

The origins of Quantum Mechanics are fully immersed in the domain of Matter-Radiation-interaction (black body, specific heats, but especially instability of the atom, i.e., falling of the electron on the nucleus by energy radiation), a domain where classical physics appeared to meet with inextricable, insurmountable difficulties. Paradoxically enough, however, in the solution invented by Heisenberg in the month of July 1925, all such problems seem to have disappeared, inasmuch as the solution seems to be of a purely kinematical character: the dynamical variables have become operators and so on, and the stability of the atom is just reduced to the kinematical fact that the ground state has a finite energy. The stability problem might perhaps show up at a more fundamental level involving both atoms and QED, but such a problem is not even mentioned in the handbooks.

So, with Heisenberg, particles' positions and trajectories lost their intuitive classical meaning or, as Einstein says, their "realistic character". The dream of Einstein was that a larger theory having a realistic character may be found, from which Quantum Mechanics, definitely the correct theory, should be recovered as a kind of corollary. This is what he called his "Classical Program". The first realistic theory he had in mind was obviously classical physics, with particles interacting with the electromagnetic field. But this appeared not to be implementable, due to the difficulties of dealing with matter radiation interaction in the case of point particles, well known since the times of Lorentz and Abraham. So he started thinking of the possible existence of a classical field theory admitting solitonic solutions (as we would say today), which would play the role of trajectories of the old classical particles.

The turning point

Within the "foundational group" mentioned above, the possibility of implementing the Einstein's Classical Program, in its original form involving Newtonian trajectories, was always taken into consideration. However, at a certain moment, an essential progress was obtained, in an unexpected sudden way, when, in a moment, new perspectives were disclosed. Andrea Carati and me were studying the papers of Planck about his microscopic black body model, which he had published in the year 1900, a few months before the two papers (October 19 and December 14) in which he introduced his formula. We were actually criticizing his model, since he was thinking of matter as constituted of resonators interacting with the field, but without any mutual interaction.

Guided by the idea that the field should be the one created by the resonators, Andrea Carati thought of a different model, somehow complementary to that of Planck, inasmuch as the field does not even show up as a part of the dynamical system. Perhaps he had some remembrance of the Wheeler-Feynman paper [33] of the year 1945 (see also [34]), in which too the field does not show up and only the particles are taken into account, with their mutual retarded electric interactions, in addition to their own radiation reaction force. But while the WF model is completely general, and thus very hard to be dealt with analytically, the Carati model is instead quite simple, and amenable to an analytic investigation. This is the reason, we believe, why some progress could be done with respect to Wheeler and Feynman. The Carati model [35] is just a system of harmonic oscillators of the same frequency, attracted towards the sites of a 1-dimensional infinite lattice. Each of them is subjected to its own radiation-reaction force (taken, as Wheeler and Feynman did, in the form of Planck, Abraham and Lorentz, i.e., proportional to the time derivative of acceleration), and moreover to the retarded electric fields due to all the other oscillators. Linearization is introduced in the standard way, by evaluating retardation relative to equilibrium positions. So we started looking for normal modes (which, by the way, excludes the possibility of the well known runaway solutions), in the standard way that leads to a "secular equation" depending on frequency ω and wave number k as parameters. Something, apparently, trivial at all.

Now comes, however, the crucial point, so relevant that we still are incredulous that it might not have been observed before. The point is that, with retardation taken into account, the secular equation is complex, with its real part and its imaginary part, which means two real equations in two parameters ω and k. If ω and k are dealt with as unknowns, one might have as solution a discrete set of pairs (ω_j, k_j). But we are looking for dispersion curves, which means functions $\omega = \omega(k)$, which thus cannot exist. However, a series entering the equation for the imaginary part, can be summed, and it turns out, in some miraculous way, that such an equation actually is an identity. So one remains with only a single equation, which implicitly defines the function $\omega = \omega(k)$. i.e. the dispersion relation.

This is the way in which dispersion relations come to exist for systems involving retardation. Incredibly enough, this fact (both in general, and in single models) was apparently unknown. For example, in all his books and papers, Born just does not even mention the equation for the imaginary part, behaving as if it did not exist, and computes the dispersion relation defined by the remaining equation.

Moreover, it occurs that in general the dispersion relation thus found produces real frequencies, and thus there is no dissipation, and the oscillators never come to rest, notwithstanding the presence of the radiation reaction acting on each of them. Thus, the main objection of principle to classical theory in atomic physics is overcome in the Carati model.

However, it also occurs that for high enough densities the solution becomes complex, a fact that manifests itself as a kind of explosion of the system. A phenomenon of such a type is well known in plasma physics, under the name of a *disruption*, but is apparently unexplained. I will recall later that this fact was pointed out to us by Matteo Zuin, a plasma physicist of Padova, who also indicated the explanation.

The Wheeler-Feynman identity proven. The electrons don't fall on the nuclei and the ions don't come to rest

So the existence of dispersion relations and the stability of matter (for not too high densities) were proven in a simple model. Let's now consider what occurs "in general", in the spirit of Wheeler and Feynman.

The paper of Wheeler and Feynman was known to us, but never could we really understand it. In a moment we now understood, and we can summarize things as follows. They were considering a very general model, i.e., a macroscopic system of point particles with standard radiation reaction forces and retarded electric interactions. They gave four qualitative semi-phenomenological arguments indicating that, if optical dispersion exists, then the radiation-reaction force acting on any particle has to be exactly deleted by the retarded fields created by all the other ones; more precisely, by the semi difference of the retarded and the advanced ones. Such a cancellation is what we call *the Wheeler-Feynman identity*. If this occurs, there is no dissipation, electrons don't fall on nuclei and ions don't come to rest. Moreover, the electromagnetic interaction among the particles just reduces to the semi sum of the retarded and the advanced ones, and thus the system presents time-reversal invariance.

So Wheeler and Feynman introduced in an explicit way the *conjecture* that the WF identity holds, giving it the name *conjecture of the existence of an universal absorber*. Later. in the year 1949 they proposed the correspondent formulation of electrodynamics in relativistic form, which is known as *the Wheeler-Feynman theory*. The idea of just considering semi sums of retarded and advanced fields (completely neglecting radiation reaction forces) was also transported by Feynman to Quantum Field Theory, where it shows up through the Feynman propagator, semi sum of the retarded and the advanced ones.

However, the WF identity in its general form had not been actually proven. In our case instead it occured that, by dealing with a simple, concrete model, the cancellation came out as a miracle through the "simple" summation of a series. Later, Andrea Carati was able to find a general proof, on the basis of an assumption of causality, expressed in a form resembling the familiar one of Quantum Field Theory (with correlations in place of commutators). In particular, it also becomes clear why the cancellation doesn't occur in the case of a macroscopic antenna. This is published in a joint paper [36], but is due to him.

As an aside comment, it should be pointed out that the WF identity holds only if the radiation-reaction force is taken in the standard form involving the time derivative of acceleration (possibly in its relativistic Dirac's variant). Other forms which are often taken into consideration, don't do the job.

6 Applications to Atomic Physics, Plasma Physics and High-Energy Physics

6.1 Atomic Physics

Having understood how it happens that in classical physics electrons don't fall on nuclei and ions don't come to rest, by radiation, it is then possible to deal with atomic physics in a classical frame exactly as is done in a quantum frame, namely, as if one were dealing with purely mechanical systems, and the electromagnetic field did not exist.

How did we start studying realistic models: the difficulties of QED with matter in bulk

The passage to studying realistic models of atomic physics occurred in the following way. One day I was illustrating, to Nicola Manini, a colleague working in Solid State Physics, the results of a quite general character we had just obtained on the Carati model, and he suggested we should address Giuseppe Grosso and Giuseppe Pastori Parravicini, who had just published a ponderous book. As Giuseppe Pastori Parravicini had been my university classmate, and I remembered him as a very kind person, I wrote to him. He immediately understood our result, which is not an obvious fact at all, and commented that, in light of our result, we might perhaps be able to provide a microscopic explanation of the existence of polaritons (a phenomenon concerning ionic crystal that I will recall in the next subsection). An explanation, he told me, that they (the persons working professionally on that subject) were not able to provide. As far as Andrea and me understand, the problem seems to be of a quite general character. Indeed, from a macroscopic treatment of the problem through Maxwell's equations, it is clear that the phenomenon is due to retardation, whereas the available formulation of QED is well known to be suited for dealing with scattering processes (which involve incoming and outcoming states), but not with phenomena involving bound states or matter in bulk. Which, by the way, is also the opinion expressed by Dirac himself, the father of QED, in the last page of his fundamental book.

The realistic ionic crystal model. The WF identity checked. Existence of macroscopic optics, and of polaritons, proven

Stimulated by the comment of Giuseppe Pastori Parravicini, we started implementing, together with two undergraduate students of physics, Alessio Lerose and Alessandro Sanzeni, a realistic model of LiF, the paradigm of ionic crystals, proceeding in a standard way [37]. The ions are dealt with as point particles with mutual retarded electric interactions (cared as usual through the Ewald summation procedure), and standard linearization. Following the works of the Born school, the degrees of freedom of the electrons were neglected and were implicitly taken into account trough empirical potentials acting among the ions, and suitable "effective charges" for the ions.

In such a model the WF identity was checked, as was also the existence of macroscopic optics, i.e., the propagation of light with a macroscopic speed different from the vacuum speed c. Perhaps such a result had already been obtained, but, as far as we know, it may be new.

The phenomenon of *polaritons* consists in a splitting of a dispersion curve "of optical type", that occurs where the latter would intersect the dispersion curve of light in vacuum $\omega = ck$. It is an example of a microscopic matter-radiation interaction corresponding to an actual macroscopic phenomenon, the proof of which in a microscopic model is still lacking, in a quantum frame. However, the existence of polaritons is exhibited, by a numerical computation, in our purely classical model.

Existence of infrared spectral lines (in a classical frame)

Once, during a lesson for a course on the foundation of physics, after having explained why in a classical frame the electrons don't fall on nuclei, quite naturally the problem was raised how can it be conceivable that spectral lines may indeed occur in a classical frame, without any possibility at all of invoking the existence of energy levels or quantum jumps, as is done in the familiar procedures of Bohr and Schrödinger. At the subsequent lesson Andrea came on with the solution.

Very simply, one has to make recourse to the standard linear response theory introduced by Green and Kubo in the late years fifties, which is normally applied in a quantum frame. Such a procedure makes reference to the time-dependent electric polarization $P(t)$, which is defined in terms of the positions $x_j(t)$ of the ions (of charge e_i) by $P(t) = \sum e_i x_i(t)$. Now, In QM the positions are operators. However, the formula makes perfect sense even in a classical frame, so that a naive classical approach is implementable, and even in a quite simple way, since Newtonian trajectories of the ions are easily determined by standard computer simulations.[4] By applying the standard Green-Kubo type formulas one can finally determine the spectral curves (as functions of frequency), which turn out (for example at room temperature) to reproduce in an impressively good way the phenomenological ones [38, 39, 40]. In other words, one can neglect not only energy levels, but also, within the Green-Kubo approach, the commutation problems that occur with products. Our impression is that the classically computed curves are even better than the analogous ones determined in a quantum frame.

A final comment of interest for the FPU problem, is that we also investigated the temperature dependence of the spectral curves. In so doing, we found out that agreement with experiment is obtained provided temperature is not identified as being proportional to specific energy. In some empirical way, by requiring agreement with experiment we obtain a state equation $E = E(T)$ which seems to require the existence of a non vanishing zero-point energy.

Eventually, Newtonian trajectories for electrons. The case of the chemical bond in the H_2^+ ion

In the microscopic models of ionic crystals used in Molecular Dynamics simulations, one deals with the ions as classical particles, and the role of the electrons is taken into account through a suitable effective potential acting among the ions, and suitable "effective charges for the ions. However while, following

[4] Notice by the way that, instead, a concrete dynamical treatment for nonlinear systems is essentially impossible in a purely quantum approach. In the case of spectral lines, at variance with the case of the dispersion curves, non linearity in the dynamics plays an essential role, so that a full quantum approach is practically non implementable. However, even in the classical approach a difficulty occurs, because retardation cannot be taken into account in a simple way, so that the electric forces were taken in the instantaneous approximation.

Born, we introduced such a potential in a phenomenological way, in MD simulations the effective potential due to the electrons is determined microscopically in quantum terms, through the Born-Oppenheimer method, which consists in identifying such a potential (as a function of the positions of the nuclei) with the energy of the ground electronic state determined for fixed nuclei.

The problem is then whether such a potential can also be obtained in a classical frame. At the moment we are meeting with apparently insurmountable difficulties in the general case that involves more than one electron. However, in the simplest possible case where just one electron is involved, i.e., the ion H_2^* of the Hydrogen molecule H_2, which consists of two protons and just one electron, we could show that a stable ion exists, with an effective potential which is qualitatively correct [41]. Moreover, a potential exists which reproduces in a surprisingly good way the quantum one computed in the Born-Oppenheimer approximation. But this occurs only for very particular states, and an instability is met under generic changes of the initial data. We hope the latter difficulty may be overcome, together with the more relevant one which is met in the case of more than one electron.

6.2 Plasma physics. The “little discovery” of Matteo Zuin: the plasma “disruptions” explained

The application that I'm going to illustrate now, is one that I like very much, for the peculiar way in which it was invented. It is due to Matteo Zuin, a young plasma physicist of Padova, to whom I'm personally acquainted since a long time, together with his family. I also suggested to him plasma physics as a possible research field. So, when ten year ago there was held in Padova a conference on the occasion of my seventieth anniversary, he attended the talks with special attention. One day he was attending the talk in which Andrea Carati was illustrating the results obtained on his model. In particular, having explained our understanding of the WF identity, he was showing the figure which reports the dispersion relation that had been obtained numerically. The figure contained several curves $\omega = \omega(k)$, which depended on the size (or step) a of the lattice at which the oscillators were located. For a of the order of say some Amstrongs, the curve was just a straight line corresponding to a constant value, $\omega(k) = \omega_0$, where ω_0 is the common mechanical frequency of the oscillators; so, the interactions had no effect. But when the step is decreased (i.e. - as Matteo immediately understood the thing - when matter density is increased - the curves start bending, tending towards the axis of the abscissas. Then, above a critical density, they intersect the axis, and come back. In other words, there exists a critical density, above which some frequencies did become complex. This is a point that Andrea and me, captured by the opportunity of understanding the WF conjecture. had completely overlooked.

Instead Matteo knew very well, as all plasma physicist do, that fusion machines are plagued by the problem of the “density limit”. Electrons are usually confined by the Lorentz force created by a huge magnetic field, but when, for a given field, matter density is increased, above a certain density limit an instability (*a disruption*, as they say) occurs, and the machine stops working or might even break down. We then checked in several ways that the idea of Matteo Zuin can be implemented in some suitable model, and the result fits the experiments qualitatively well [42, 43], and quantitatively not so badly. Now, some theories exist for explaining such disruptions, and I'm not entitled to discuss them. Here, however, there is a new one, which is related to some fundamental "first principles reason".

Analogous disruptions. or rather explosions, were observed by us, together with the brothers Gangemi in Brescia, in the realistic ionic models illustrated above. Perhaps we are meeting here with a phenomenon known as *single-crystal explosion*, a kind of phase transition that might have the same general origin previously discussed, when describing the WF identity.

6.3 High energy physics: pair creation and annihilation in classical physics

According to many theoretical physicists of the previous generation (in Milano, I recall the late Piero Caldirola), the relevant difference between classical physics and the quantum one, doesn't consist in the replacement of Newton equation by the Heisenberg or Schrödinger equation, but rather in the fact that the number N of particles constituting a system is fixed in classical physics, whereas in a quantum system it can change, due to the possibility of pair creation and annihilations, which is offered by Quantum Field Theory. In this connection, a result of Andrea Carati seems to be of interest. Things went as follows.

Many years ago, within our group we were discussing an unusual paper in which Feynman was trying to find a classical implementation (through Newtonian trajectories, indeed) of pair creation and annihilation. However, he was able to implement such an idea only through the introduction of a modified form of electrodynamics. Some days later Andrea Carati came up with his solution [44]. In his procedure, nothing is changed in the classical theory, if not for choosing a precise form for the radiation-reaction force, namely, the relativistic generalization of the old form (empirically found by Planck, and then studied by Lorentz and Abraham), introduced by Dirac in his paper of the year 1938, ten years after his formulation of QED.[5]

Andrea considers the case of a particle on a line, under the action of an external potential presenting a singularity at a point of the line. He finds that in a finite time the particle falls on the singularity. Then he performs an analytic continuation, and the particle is found to come out of the singularity as if it were going back in time. This is however equivalent to an antiparticle going forward, the original particle and the antiparticle annihilating at the singularity. This is very near to implementing pair creation, or annihilation. Actually, a full implementation would require to eliminate the external potential, its role being taken by the mutual Coulomb potential acting between particle and antiparticle.

In any case, the above result seems to be very promising. Concerning the

[5]A strange thing indeed, the creator of QED who goes back to classical physics.

mentioned paper, one finds in the Mathematical Reviews a very complimentary comment by T. Erber [45], to which I completely subscribe.

The comment goes as follows. "*... Under these circumstances it might seem foothardly and redundant to reach back to classical electrodynamics to locate precursors to pair production. But historical experience suggests that the prolonged stasis of the currently accepted theoretical framework can be broken only by the discovery of new phenomena (at still higher energies?); the shift to more comprehensive theoretical schemes (strings, branes, and "M"); or the renewed exploration of paths "not taken". Carati's paper is one of the rare efforts of this last kind.*"

7 Conclusions

So, I told a fifty-years long story about a group of people aiming at implementing the Einstein Classical Program, i.e., at proving that Quantum Mechanics is just a chapter of classical physics, with its realistic character, even in its extreme form involving Newtonian trajectories of point particles.

Two points were involved, related to different aspects of the problem. The first one is centered about the alternative of energy equipartition versus Planck's law. Here, the progress achieved was not yet sufficient to settle the problem. Further features remain to be clarified for an appropriate formulation of classical statistical mechanics. In particular, one should find an effective way for distinguishing mechanical energy from the thermal one, as occurs typically in the favourite Boltzmann example, that of perfectly smooth spheres, and in the Debye-Waller effect.

The second point concerns the idea that, within classical physics, electrons should fall on nuclei and ions come to rest, due to radiation emission by accelerated particles. Here a fundamental progress was obtained, inasmuch as such an idea was proven to be an unfounded prejudice, so that the main objection to the use of classical physics in the atomic domain is completely eliminated. The relevant progress, performed by Andrea Carati, consisted in bringing to completion the relation between retardation of the forces and existence of a radiation reaction force (in its correct Dirac's form), that had been formulated as a conjecture by Wheeler and Feynman. Such a jump having been performed, the path was opened to the explanation, within a classical frame involving Newtonian trajectories, of several phenomena usually considered to be typical quantum ones, such as polaritons, infrared spectral lines and chemical bond (at least in the simplest case involving just one electron). Among them, the most striking one is perhaps that of pair production.

In the course of such a long path towards a possible implementation of the Einstein Classical Program, I happened to have the fortunate chance of entering in strict relation with many persons, who actually became part of my life. For what concerns science, whether it will be possible to follow the Einstein's path up to the end is not yet clear. But I'm sure that my dear late friends Ed Nelson, Carlo Cercignani and Martin Gutzwiller would be gratified by the present state of the problem.

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