http://www.aimspress.com/journal/mine

Mathematics in Engineering, 3(6): 1–24.
DOI:10.3934/mine.2021045
Received: 06 March 2020
Accepted: 28 August 2020
Published: 09 November 2020

Research article

Foundations of physics in Milan, Padua and Paris. Newtonian trajectories from celestial mechanics to atomic physics†

L. Galgani*

Department of Mathematics "F.Enriques", University of Milan, via Saldini 50, 20133 Milan, Italy

† **This contribution is part of the Special Issue:** Modern methods in Hamiltonian perturbation theory
Guest Editors: Marco Sansottera; Ugo Locatelli
Link: www.aimspress.com/mine/article/5514/special-articles

* **Correspondence:** Email: luigi.galgani@unimi.it.

Abstract: This paper is written, in a very informal and colloquial style, on the occasion of the seventieth birthday 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 enlighten 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, the attention being mainly addressed to those relevant for the original foundational program.

Keywords: KAM; Nekhoroshev; FPU; Wheeler-Feynman; atomic-physics

1. Introduction

This paper is written on the occasion of the seventieth birthday of Antonio Giorgilli, to whom the present collection of papers is dedicated. Since Antonio is mostly known for his contributions to dynamical systems theory and its application to Celestial Mechanics, I thought of contributing by illustrating, in an informal and very colloquial style, how such results actually originated within a group of people involved in a very ambitious program of a foundational character, trying to reconstruct the peculiar atmosphere in which this happened. The program was to understand whether the progress in dynamical systems theory that had become available in the years seventies might allow one to enlighten in a new way the relations between quantum mechanics and classical physics. Or, even, whether it be altogether possible to explain Quantum Mechanics in classical terms, thus implementing what Einstein used to call his "Classical Program" (see [75], page 666 and following ones, and [30]).

This required to clarify what impact dynamical systems theory may have, on the one hand on the foundations of Classical Statistical Mechanics (with special attention to the Fermi-Pasta-Ulam, or FPU, problem), and on the other hand on the classical description of matter-radiation interaction.

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 will give a quick review of the results obtained since those times, of interest for the original foundational program. However, a mention will also be given of the contributions of Antonio Giorgilli, Giancarlo Benettin and their groups to dynamical systems theory and to Celestial Mechanics.

The way the original group was formed is recalled in section 2, and then the review of the results is given. First of all, section 3, the early mathematical results on KAM and Nekhoroshev's theories. Then are recalled the results of interest for the foundational problem at hand starting, in section 4, from those concerning 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 and to pair creation and annihilation are given in section 6. The conclusions then follow in section 7. Finally, in an Appendix is given an excursus on two further results, namely, faraway galaxies as a possible substitute for dark matter, and disruptions in plasma physics.

2. The foundational problem raised, and a research group established between Milan, Padua and Paris

Everything started in the year 1971 within the group of theoretical physicists in Milan. Angelo Loinger had come to know (through a work of Chirikov) of the Fermi-Pasta-Ulam (FPU) 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 statistical 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 [19], 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 (Planck 1900 and Einstein 1907);
2). falling of the electrons on the nuclei by radiation emission due to accelerated motions (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 as 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 Milanese 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 Milan was unanimously considered by far the more gifted one, soon joined us. This story I already told elsewhere [44].

So, together with Tonino Scotti and Carlo Cercignani, we started studying some of the many problems involved [38, 45, 46], 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 the problem should 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, the harmonic oscillators corresponding to the normal modes of the system – 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" usually shows 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 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.

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 Milan. Thus he started a thesis on the construction of the integrals of motion by perturbation methods, studying in particular the works of Contopoulos [39]. His results were described in his first two papers [53] and [41] (see also [50]), 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, in my role of 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 having computed Poincaré surfaces of section for a mapping. Not much later he produced a program for implementing his very method for constructing integrals of motion, and a paper was soon published in the journal Computer Physics Communications [49]. Soon, he came to be unanimously reputed among the best experts in Milan, in the field of numerical computations.

With Giancarlo Benettin the cooptation occurred through a copy of an ideological paper 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 Padua, in a period in which, after having graduated, he was involved in his military service. He thus visited me in Milan, 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, [14] that is still considered a reference paper on the subject, in addition to the subsequent paper to which Antonio too took part [11].

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 for the overflow 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 occurred because of the choice of the initial data, which would make the particle escape to infinity, just in virtue of the dynamics, rather than 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.

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, Massimo Bertini, Simone Paleari, Massimo Miari (with the great Marco Brunella), Lia Forti, . . . , and eventually Sergio Cacciatori.

Then, little by little, the group started somehow separating. Antonio, after some relevant contributions to the FPU problem, looked upon in the spirit of the original program (see [5, 33]), started to be mostly interested in Celestial Mechanics and in more theoretical arguments about quasi-integrable Hamiltonian systems, on which he worked with Alessandro Morbidelli, Ugo Locatelli, Simone Paleari, Marco Sansottera and Tiziano Penati. In Padua, Giancarlo continued to work in the FPU problem, in perturbation theory and on other features of dynamical systems, with Francesco Fassò, Massimiliano Guzzo, Antonio Ponno and Helen Christodoulidi. In Milan, Dario Bambusi worked on several mathematical aspects of perturbation theory, looking in particular at its extension to Partial Differential Equations (PDEs). 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, Alberto Maiocchi and me in Milan, the brothers Fabrizio and Roberto Gangemi in Brescia, and Antonio Ponno in Padua. 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.

3. Mathematical results in perturbation theory

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

3.1. 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 [34]). 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.

[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 [73].

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 subset of invariant tori becomes of 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 Shlomo Sternberg. 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 Milan) that he really doubted that the Kolmogorov proof be correct. More precisely, in the journal Mathematical Reviews he wrote: "*The proof of this theorem was published in Doklady Nauk SSSr **98** (1954) 527-30, but the convergence discussion does not seem convincing to the reviewer*" [71]. 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 Padua to Milan 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 crowded with many many people and we were standing up tight as sardines among them), and I saw that the paper was just a few pages long (i.e., was 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.[2] 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 [12]. 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".

Further contributions were then given by Antonio Giorgilli and his group. Indeed, some years later he started to reconsider the problem, having in mind to design a new proof in terms of the classical methods based on series expansions in a small parameter. His new proof scheme was so successful that it allowed to remove the so-called super-convergent procedure, that is reminiscent of the Newton

[2]The only nontrivial point, that we imagine may have been the critical point for Moser, was that, in an analytic frame, one has to introduce suitable domains in performing canonical transformations. This we had learned from an appendix to a paper of Arnold.

method and, up to a few years before that time, was considered to be essential in order to complete the demonstration [54]. In this context, the key element introduced by Antonio was a reformulation of the constructive algorithm implementing a sequence of canonical transformations; this was made in a so efficient way that the corresponding scheme of estimates has been shown to be convergent also when the Bruno non-resonance condition is assumed on the frequencies, that is considered to be optimal and more relaxed with respect to the usual Diophantine inequality [55, 58]. Moreover, this new approach was shown to be convenient in other slightly different contexts, e.g., the construction of lower-dimensional elliptic tori [57].

3.2. 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 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 read 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 [72] 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 the original 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 [13].

Only later, at the beginning of the 90's, it occurred to me to meet Kolya (i.e., Nikolay Nekhoroshev), in Kiev, where I was attending a conference together with Pierre Lochack, that had previously been my guest for perhaps two months in Milan, 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 Milan 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 Milan), 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 Milan 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 nonrussian person that even knew of his name, before our paper did appear.

Concerning Nekhoroshev's theorem, once again Antonio improved the theoretical results available.

In close collaboration with a former disciple of his, Alessandro Morbidelli, they succeeded in combining the KAM theorem with that due to Nekhoroshev, in such a way to prove that the eventual diffusion in phase space is super-exponentially slow with respect to the inverse of the distance from an invariant torus [69]. This result marked the birth of the concept of super-exponential stability.

3.3. The applicability of the new perturbation theories to dynamical systems of a relatively low number of degrees of freedom, in particular to Celestial Mechanics

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 mathematical ideas and implements them, having to wait, in our case, for something between fifty years and a century since the problem had been posed.

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 theorem too. A breakthrough was eventually attained with the work [52], performed by Antonio Giorgilli in collaboration with me and the group of our friends from Barcelona around Simó, in which realistic estimates in a Nekhoroshev's frame were eventually obtained in Celestial Mechanics, in connection with the Lagrangian L_4 equilibrium point for the motions of the asteroids.

Analogous results were later obtained, within the group around Antonio, also for KAM tori in the Sun-Jupiter-Saturn system [65, 66]. The feasibility of a proof of super-exponential stability for such a planetary model was first given in [56]. For a general review, see the lectures given in [51]. A similar approach based on the construction of suitable normal forms was successfully used also to implement control theory for symplectic maps of interest for possible applications to betatronic motions in accelerators [74]. Finally, other remarkable applications were obtained in the field of Astronomy by the next generation of people making part of the research group initially established between Milan, Padua and Paris. In particular, Nekhoroshev's theorem was applied in the Asteroid Belt dynamics and, more recently, also for KAM theorem in the study of extrasolar planetary systems [63, 70, 76].

Without any doubt, these interesting results can be seen as representative fruits of the research project independently started by Antonio, which is in turn a byproduct of the initial one of a foundational character, oriented to theoretical physics. The same can be said for what occurred with Giancarlo and his group in Padua. See for example the works [4, 7–10, 15, 43, 61, 62].

3.4. Extension of perturbation methods to infinite systems, i.e., PDEs and Statistical Mechanics. The Carati theorem

Quite naturally the problem presents itself of extending KAM and Nerhoroshev's theorems to the case of infinitely many particles, namely, either the case of continuous bodies, described by PDEs, or that of Statistical Mechanics. In the latter case one deals with discrete systems constituted by N particles, in the so-called thermodynamic limit, i.e., $N \to \infty$, with finite values of specific energy

$\epsilon = E/N$ and of specific volume $v = V/N$.

For what concerns PDEs, "since always" [48] 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 PDEs. In our group the interest for this kind of problems was arisen by Antonio Ponno, and was particularly tackled by Dario Bambusi [3]. The applicability of perturbation methods was proved, first by Sergej Kuksin for what concerns finite dimensional KAM tori, and eventually by Dario Bambusi [1, 2] (and later by others) for PDEs in the case of space dimension 1.

In the case of the thermodynamic limit, which is the one of interest for the foundational problem discussed in this paper, a breakthrough was obtained by Andrea Carati [23]. In our group, since a long time we knew that Nekhoroshev's theorem cannot be extended to the thermodynamic limit in a naive pointwise 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, which introduces some smoothing effect. 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. Applications are presently being performed to FPU-like systems of interest for statistical mechanics, even within the group of Antonio [36, 40, 59, 60, 67, 68].

This is a quite relevant result, since 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, thus proving the failure of classical physics. On this point I will come back in the next section.

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

Almost seventy 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 the vanishing of the specific heat as $T \to 0$, for solids (T denoting absolute temperature). They considered as a model a discretization of a string, i.e., a system of N particles on a line with nearest neighbor interactions, which is equivalent to a system of weakly coupled oscillators. By performing numerical simulations on a system of 32 or 64 particles[3] FPU 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 – see [45]). What happened in the more than sixty elapsed years?

[3]This perhaps constitutes the first case in which numerical simulations were used in a scientific research.

I dare to summarize the present situation in the following way. The FPU "final" state actually turned out to be a state of apparent equilibrium. As time goes on, equipartition is eventually attained.[4] This was usually called the metastability perspective. Moreover, the times needed for such a relaxation to take place in the thermodynamic limit depend on specific energy $\epsilon \equiv E/N$), and in particular diverge as $\epsilon \to 0$.[5] A similar 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, during the approach to equipartition, the fluctuations of the mode-energies are eventually in agreement with the Maxwell-Boltzmann distribution.[6]

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, Antonio Ponno 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,

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

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). In other terms, the particles are distinguishable.*[7] *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.* So 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 nonergodic 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 implementation of statistical thermodynamics in terms of time averages apparently exist (at least to my knowledge), apart from the works [21, 22] of Andrea Carati.

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

[4] Within our group, this was first seen in the work [18], performed by following an idea of Antonio Giorgilli.

[5] The check of the dependence on specific energy required a hard labour, in particular from the part of Giancarlo Benettin and his collaborators [6, 16, 17], who emphasized the role played, for the study of FPU models, by the "tangent dynamics" of the corresponding integrable Toda model. I'm skipping here quoting a mass of works which took a huge labour from many persons - especially in Italy - with whom we were having strong discussions during many, many years. I'm thinking of Giovanni Gallavotti, Francesco Guerra, Giorgio Parisi, Stefano Ruffo, Roberto Livi, Marco Pettini, Angelo Vulpiani, Mario Casartelli, Jayme de Luca, Alan Lichtenberg, Bob Rink, Thomas Kappeler ... *Meminisse iuvabit* (it will be beautiful to recall).

[6] Moreover, the relaxation times were estimated in physical terms, i.e., in picoseconds.

[7] This is also explicitly stated in one of Fermi's papers.

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

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

In other terms, in classical physics too there should exist a nonvanishing 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[8]) 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, in classical models of solids, the state equation, i.e., the relation $E = E(T)$, should possess the property (2).

In any case, a big problem remains open, which may hinder the physical significance of all the FPU or 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 rather detailed review of this subject on the occasion of a conference which should be organized in Milan in commemoration of Carlo Cercignani, here I will just give a draft of what we plan to present there.

5.1. 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 kinematic character: the dynamical variables have become operators and so on, and the stability of the atom is just reduced to the kinematic 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 quantum electrodynamics (QED), but such a problem is not even mentioned in the handbooks, and is not dealt with (at least, to our knowledge) in the available literature.

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

[8]Namely, the ions keep a nonvanishing kinetic energy, i.e. continue to move, also at zero temperature.

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 familiar classical point-particles.

5.2. 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, and several works were performed, to which Francesco Guerra too participated. However, at a certain moment an essential progress was obtained, in an unexpected subitaneous 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 acted upon by the field, without influencing it.

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 [77] of the year 1945 (see also [78]), 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 forces. 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 [27] is just a system of harmonic oscillators (the Planck resonators) of the same frequency, moving on a line, that are attracted each towards a site of a regular lattice located on that line. Each of them is subject, in addition to the linear restoring force towards a site, also to its own radiation reaction force (taken, as Wheeler and Feynman did, in the form of Planck, Abraham and Lorentz and Dirac, i.e., proportional to the time derivative of acceleration), and moreover to the retarded electric fields "created" by all the other oscillators. Linearization is introduced in the standard way, i.e., by evaluating retardation with respect to equilibrium positions of the oscillators. 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 turns out to be complex, with its real part and its imaginary parts, 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 real 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 before WF, and in our opinion remained essentially not understood also after their work. 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. A procedure which is correct, but unjustified.

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, which turns out to be cancelled (this is indeed the physical meaning of the mentioned identity). 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 (in an Appendix) that this fact was pointed out to us by Matteo Zuin, a plasma physicist of Padua, who also indicated a possible explanation, analogous to the one occurring in the Carati model, actually, in our opinion, a very probable one.

5.3. 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 happens "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 cancelled by the retarded fields created by all the other ones; more precisely, by the semidifference 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 semisum 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 a corresponding formulation of electrodynamics of point particles in relativistic form, which is known as *the Wheeler-Feynman theory*. The idea of just considering semisums 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, semisum 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 occurred 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 (involving however 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 [29], but is due to Andrea.

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 and to pair creation and annihilation

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 due to accelerated motions, it is then possible to deal with atomic physics in a classical frame exactly as it is done in a quantum frame, namely, as if one were dealing with purely mechanical systems, and the electromagnetic field did not exist.

6.1.1. 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 crystals 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 (quantum electrodynamics) 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 on quantum mechanics.

6.1.2. 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 (Lithium Fluoride), the paradigm of ionic crystals, proceeding in a standard way [64]. Namely, the ions are dealt with as point-particles with mutual retarded electric interactions (cared as usual through the Ewald summation procedure), and standard linearization (i.e., with retardation evaluated for the equilibrium points). Following the works of the Born school, the degrees of freedom of the electrons were neglected and were implicitly taken into account through 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.

6.1.3. Existence of infrared spectral lines (in a classical frame)

Once, during a lesson for a course on the foundations 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 it 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, as was eventually understood, in a quantum frame provides a procedure having a theoretical basis more motivated than the familiar one. Such a new 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 quantum mechanics (QM) the positions are operators. However, the Green-Kubo 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.[9] 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 [31, 35, 47]. In other words, one can neglect not only energy levels, but also, within the Green-Kubo approach, the commutation problems that occur when products are met. 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 $E = E(T)$, at fixed pressure, which requires the existence of a nonvanishing zero-point energy.

[9]Instead, a concrete dynamical treatment for nonlinear systems is essentially non implementable in a purely quantum approach. The reason is that, at variance with the case of the dispersion curves, in the case of spectral lines nonlinearity plays an essential role in the dynamics, not being however an obstacle to numerical computations, in a classical frame. Instead, in a quantum frame computations are implementable only within a perturbation approach. However, even in the classical approach a difficulty remains, since for nonlinear systems retardation cannot be taken into account in a simple way. This is the reason why, in our model of LIF, the electric forces were dealt with in the instantaneous approximation.

6.1.4. Eventually, Newtonian trajectories for electrons (in the case of just one electron). 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 in the realistic LIF model, following Born, we introduced such a potential in a phenomenological way, in MD simulations the effective potential due to the electrons can be 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 involving more than one electron. However such difficulties do not occur 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 indeed one electron. Computing the trajectories of the corresponding relativistic three-body Coulomb model, we could show that a stable ion exists, with an effective potential that is qualitatively correct [32]. Actually, the potential reproduces in a surprisingly good way the quantum one computed in the Born-Oppenheimer approximation, at least in a rather extended interval about the minimum. So this is a particular counterexample to the common statement that the chemical bond would constitute a phenomenon inconceivable in terms of classical trajectories. Quite interesting is also the fact that the binding occurs only for initial data which give rise to motions that are sufficiently ordered (i.e., are not too much chaotic, in the familiar sense of dynamical systems theory). In fact, regular enough motions are found to occur only if the angular momentum of the electron (along the straight line through the protons) is sufficiently large, indeed of the order of Planck constant h. And this, in a purely mechanical relativistic three-body Coulomb model, involving no adjustable parameter at all.

We are confident that a further relevant physical feature may be found, which might allow one not only to obtain better results for the H_2^+ ion, but also to implement a classical description of the chemical bond for systems involving more than one electron.

6.2. High energy physics: pair creation and annihilation in a classical frame

According to many theoretical physicists of the previous generation (in Milan, 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 annihilation, 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 at the price of introducing a modified form of electrodynamics. Some days later Andrea Carati came up with his solution [20]. 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.[10]

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 indeed 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 mentioned paper, one finds in the Mathematical Reviews a very complimentary comment by T. Erber [42], to which I completely subscribe.

The comment goes as follows. "…*Under these circumstances it might seem foolhardly 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 compehensive 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 (the Cercignani shortcut).

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 a dynamically consistent formulation of classical statistical mechanics. In particular, one should find an effective way for distinguishing mechanical energy from thermal energy, as it occurs typically in the favourite Boltzmann example, that of perfectly smooth spheres, and in the Debye-Waller effect. This would imply that zero-point energy, i.e., the third principle of thermodynamics, has a consistent place in classical physics [11]

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 proving the Wheeler-Feynman identity, a relation

[10] A strange thing indeed: the creator of QED goes back, ten years later, to classical physics.

[11] **Note added during the refereeing process.** New results exhibiting quantum-like pheneomena, and in particular the existence of zero-point energy, in the realistic Lithium Fluoride model discussed above, are illustrated in the preprint A. Carati. L. Galgani. F. Gangemi, R. Gangemi, *Tsallis distributions, their relaxations and the relation $\Delta t \cdot \Delta E \simeq h$ in the dynamical fluctuations of a classical model of a crystal*.

between retarded electric forces and the radiation reaction force (in its correct Dirac's form). This allowed us to explain, within a classical frame involving Newtonian trajectories, several phenomena usually considered to be typical quantum ones, such as the existence of polaritons, of infrared spectral lines and of chemical bond (in the simplest case involving just one electron). Among them, the most striking one is perhaps that of pair production.

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.

In the course of such a long way 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. Especially dear to me are my masters Tonino Scotti and the late Carlo Cercignani, together with my three musketeers, Antonio Giorgilli, Giancarlo Benettin and Jean-Marie Strelcyn, and the younger fourth one, Andrea Carati. All of us, on heaven and earth, together with all the other friends mentioned in this paper, are glad to greet Antonio for his birthday, still continuing to wait for the new scientific results he's going to put out of his hat.

Conflict of interest

The author declares no conflict of interest.

References

1. D. Bambusi, Galerkin averaging method and Poincaré normal form for some quasilinesr PDEs, *Ann. Scuola Norm. Sup. Pisa Cl. Sci.*, **IV** (2005), 669–702.
2. D. Bambusi, B. Langella, A C^∞ Nekhoroshev theorem, *Mathematics in Engineering*, **3** (2020), 1–17.
3. D. Bambusi, A. Ponno, On metastability in FPU, *Commun. Math. Phys.*, **264** (2006), 539–561.
4. G. Benettin, The elements of Hamiltonian perturbation theory, In: D. Benest, C. Froesché, E. Lega, *Hamiltonian systems and frequency analysis*, Cambridge Sci. Pub., 2004.
5. G. Benettin, A. Carati, L. Galgani, A. Giorgilli, *The Fermi-Pasta-Ulam problem and the metastability perspective*, Berlin: Springer, 2007.
6. G. Benettin, H. Christodoulidi, A. Ponno, The Fermi-Pasta-Ulam problem and its underlying integrable dynamics, *J. Stat. Phys.*, **152** (2013), 195–212.
7. G. Benettin, F. Fassò, Fast rotations of the symmetric rigid body: A general study by Hamiltonian perturbation theory. Part I, *Nonlinearity*, **9** (1996), 137–186.
8. G. Benettin, F. Fassò, M. Guzzo, Fast rotations of the symmetric rigid body: A study by Hamiltonian perturbation theory. Part II, Gyroscopic rotations, *Nonlinearity*, **10** (1997), 1695–1717.
9. G. Benettin, F. Fassò, M. Guzzo, Nekhoroshev-stability of L4 and L5 in the spatial restricted three-body problem, *Regul. Chaotic Dyn.*, **3** (1998), 56–72.

10. G. Benettin, F. Fassò , M. Guzzo, Long term stability of proper rotations of the perturbed Euler rigid body, *Commun. Math. Phys.*, **250** (2004), 133–160.

11. G. Benettin, L. Galgani, A. Giorgilli, J. M. Strelcyn, Lyapunov characteristic exponents for smooth dynamical systems and for hamiltonian systems; a method for computing all of them. Part 1: Theory — Part 2: Numerical application, *Meccanica*, **15** (1980), 9–30.

12. G. Benettin, L. Galgani, A. Giorgilli, J. M. Strelcyn, A proof of Kolmogorov's theorem on invariant tori using canonical transformations defined by the Lie method, *Il Nuovo Cimento B*, **79** (1984), 201–223.

13. G. Benettin, L. Galgani, A. Giorgilli, A proof of Nekhoroshev's theorem for the stability times in nearly integrable Hamiltonian systems, *Cel. Mech.*, **37** (1985), 1–25.

14. G. Benettin, L. Galgani, J. M. Strelcyn, Kolmogorov entropy and numerical experiments, *Phys. Rev. A*, **14** (1976), 2338–2345.

15. G. Benettin, M. Guzzo, V. Marini, Adiabatic chaos in the spin orbit problem, *Celest. Mech. Dyn. Astr.*, **101** (2008), 203.

16. G. Benettin, A. Ponno, Time-scales to equipartition in the Fermi-Pasta-Ulam problem: Finite-size effects and thermodynamic limit, *J. Stat. Phys.*, **144** (2011), 793.

17. G. Benettin, A. Ponno, Understanding the FPU state in FPU-like models, *Mathematics in Engineering*, **3** (2020), 1–22.

18. L. Berchialla, L. Galgani, A. Giorgilli, Localization of energy in FPU chains, *Discrete Cont. Dyn. A*, **11** (2004), 855–866.

19. P. Bocchieri, A. Scotti, B. Bearzi, A. Loinger, Anharmonic chain with Lennard-Jones interaction, *Phys. Rev. A*, 1970, 2013–2019.

20. A. Carati, Pair production in classical electrodynamics, *Found. Phys.*, **28** (1998), 843–853.

21. A. Carati, Thermodynamics and time averages, *Physica A*, **348** (2005), 110–120.

22. A. Carati, On the definition of temperature using time-averages, *Physica A*, **369** (2006), 417–431.

23. A. Carati, An averaging theorem for Hamiltonian dynamical systems in the thermodynamic limit, *J. Stat. Phys.*, **128** (2007), 1057–1077.

24. A. Carati, F. Benfenati, A. Maiocchi, M. Zuin, L. Galgani, Chaoticity threshold in magnetized plasmas: Numerical results in the weak coupling regime, *Chaos*, **24** (2014), 013118.

25. A. Carati, S. Cacciatori, L. Galgani, Discrete matter, far fields and dark matter, *EPL*, **83** (2008), 59002.

26. A. Carati, S. Cacciatori, L. Galgani, Far fields, from electrodynamics to gravitation, and the dark matter problem, In: *Chaos in Astronomy. Astrophysics and Space Science Proceedings*, Berlin, Heidelberg: Springer, 2008, 325–335.

27. A. Carati, L. Galgani, Nonradiating normal modes in a classical many-body model of matter-radiation interaction, *Il Nuovo Cimento B*, **8** (2003), 839–851.

28. A. Carati, L. Galgani, Far fields as a possible substitute for dark matter, In: *Chaos, diffusion and nonintegrability in Hamiltonian systems*, La Plata, 2012.

29. A. Carati, L. Galgani, Classical microscopic theory of dispersion, emission and absorption of light in dielectrics, *Eur. Phys. J. D*, **68** (2014), 307.
30. A. Carati, L. Galgani, Progress along the lines of the Einstein Classical Program: An enquiry on the necessity of quantization in light of the modern theory of dynamical systems. Available from: `http://www.mat.unimi.it/users/galgani`.
31. A. Carati, L. Galgani, F. Gangemi, R. Gangemi, Relaxation times and ergodic properties in a realistic ionic-crystal model, and the modern form of the FPU problem, *Physica A*, **532** (2019), 121911.
32. A. Carati, L. Galgani, F. Gangemi, R. Gamgemi, Electronic trajectories in atomic physics: The chemical bond in the H_2^+ ion, *Chaos*, **30** (2020), 063109.
33. A. Carati, L. Galgani, A. Giorgilli, The Fermi-Pasta-Ulam problem as a challenge for the foundations of physics, *Chaos*, **15** (2005), 015105.
34. A. Carati, L. Galgani, A. Maiocchi, F. Gangemi, R. Gangemi, The FPU problem as a statistical-mechanical counterpart of the KAM problem, and its relevance for the foundations of physics, *Regul. Chaotic Dyn.*, **23** (2018), 704–719.
35. A. Carati, L. Galgani, A. Maiocchi, F. Gangemi, R. Gangemi, Classical infrared spectra of ionic crystals and their relevance for statistical mechanics, *Physica A*, **506** (2018), 1–10.
36. A. Carati, A. Maiocchi, Exponentially long stability times for a nonlinear lattice in the thermodynamic limit, *Commun. Math. Phys.*, **314** (2012), 129–161.
37. A. Carati, M. Zuin, A. Maiocchi, M. Marino, E. Martines, L. Galgani, Transition from order to chaos, and density limit, in magnetized plasmas, *Chaos*, **22** (2012), 033124.
38. C. Cercignani, L. Galgani, A. Scotti, Zero-point energy in classical non-linear mechanics, *Phys. Lett. A*, **38** (1972), 403–404.
39. G. Contopoulos, A review of the "Third" integral, *Mathematics in Engineering*, **2** (2020), 472–511.
40. W. De Roeck, F. Huveneers, Asymptotic localization of energy in nondisordered oscillator chains, *Commun. Pure Appl. Math.*, **68** (2015), 1532–1568.
41. E. Diana, L. Galgani, A. Giorgilli, A. Scotti, On the direct construction of integrals of Hamiltonian systems near an equilibrium point, *Boll. U. M. I.*, **11** (1975), 84–89.
42. T. Erber, *Mathematical Reviews MR1652395*, 2000a:78006.
43. F. Fassò, M. Guzzo, G. Benettin, Nekhoroshev stability of elliptic equilibria of Hamiltonian systems, *Commun. Math. Phys.*, **197** (1998), 347–360.
44. L. Galgani, Carlo Cercignani's interests for the foundations of physics, *Meccanica*, **47** (2012), 1723–1735.
45. L. Galgani, A. Scotti, Planck-like distribution in classical nonlinear mechanics, *Phys. Rev. Lett.*, **28** (1972), 1173–1176.
46. L. Galgani, A. Scotti, Recent progress in classical nolinear dynamics, *La Rivista del Nuovo Cimento*, **2** (1972), 189–209.

47. F. Gangemi, A. Carati, L. Galgani, R. Gangemi, A. Maiocchi, Agreement of classical Kubo theory with the infrared dispersion curves $n(\omega)$ of ionic crystals, *EPL*, **110** (2015), 47003.

48. C. S. Gardner, J. M. Green, M. D. Kruskal, R. M. Miura, Korteweg-devries equation and generalizations. VI. Methods for exact solutions, *Commun. Pure Appl. Math.*, **27** (1974), 97–133.

49. A. Giorgilli, A computer program for integrals of motion, *Comput. Phys. Commun.*, **16** (1979), 331–343.

50. A. Giorgilli, Rigorous results on the power expansions for the integrals of a hamiltonian system near an elliptic equilibrium point, *Ann. Inst. H. Poincaré*, **48** (1988), 423–439.

51. A. Giorgilli, Perturbation methods in celestial mechanics, In: *Satellite dynamics and space missions*, Springer INDAM Series, 2019, 51–114.

52. A. Giorgilli, A. Delshams, E. Fontich, L. Galgani, C. Simó, Effective stability for a hamiltonian system near an elliptic equilibrium point, with an applicatiion to the restricted three body problem, *J. Differ. Equations*, **77** (1989), 167–198.

53. A. Giorgilli, L. Galgani, Formal integrals of motions for an autonomous Hamiltonian system near an equilibrium point, *Cel. Mech.*, **17** (1978), 267–280.

54. A. Giorgilli, U. Locatelli, Kolmogorov theorem and classical perturbation theory, *Z. Angew. Math. Phys.*, **48** (1997), 220–261.

55. A. Giorgilli, U. Locatelli, On classical series expansion for quasi-periodic motions, *Math. Phys. Electron. J.*, **3** (1997), 1–25.

56. A. Giorgilli, U. Locatelli, M. Sansottera, Kolmogorov and Nekhoroshev theory for the problem of three bodies, *Discrete Cont. Dyn. A*, **104** (2009), 159–173.

57. A. Giorgilli, U. Locatelli, M. Sansottera, On the convergence of an algorithm constructing of the normal form for lower dimesionality elliptic tori in planetary systems, *Discrete Cont. Dyn. A*, **119** (2014), 397–424.

58. A. Giorgilli, S. Marmi, Convergence radius in the Poincaré-Siegel problem, *Discrete Cont. Dyn. S*, **3** (2010), 601–621.

59. A. Giorgilli, S. Paleari, T. Penati, Extensive adiabatic invariants for nonlinear chains, *J. Stat. Phys.*, **148** (2012), 1106–1134.

60. A. Giorgilli, S. Paleari, T. Penati, An extensive adiabatic invariant for the Klein-Gordon model in the thermodynamic limit, *Ann. I. H. Poincaré PR*, **16** (2015), 897–959.

61. M. Guzzo, L. Chierchia, G. Benettin, The steep Nekhoroshev's theorem, *Commun. Math. Phys.*, **342** (2016), 569–601.

62. M. Guzzo, F. Fassò, G. Benettin, On the stability of elliptic equilibria, *Math. Phys. Electron. J.*, **4** (1998), 1–16.

63. M. Guzzo, A. Morbidelli, Construction of a Nekhoroshev–like result for the asteroid belt dynamical system, *Discrete Cont. Dyn. A*, **66** (1996), 255–292.

64. A. Lerose, A. Sanzeni, A. Carati, L. Galgani, Classical microscopic theory of polaritons in ionic crystals, *Eur. Phys. J. D*, **68** (2014), 35.

65. U. Locatelli, A. Giorgilli, Invariant tori in the Sun-Jupiter-Saturn system, *Discrete Cont. Dyn. B*, **7** (2007), 377–398.

66. U. Locatelli, A. Giorgilli, Invariant tori in the secular motions of the three-body planetary systems, *Discrete Cont. Dyn. A*, **78** (2000), 47–74.

67. A. Maiocchi, Freezing of the optical-branch energy in a diatomic FPU chain, *Commun. Math. Phys.*, **372** (2019), 91–117.

68. A. Maiocchi, D. Bambusi, A. Carati, An averaging theorem for FPU in the thermodynamic limit, *J. Stat. Phys.*, **155** (2014), 300–322.

69. A. Morbidelli, A. Giorgilli, Superexponential stability of KAM tori, *J. Stat. Phys.*, **78** (1995), 1607–1617.

70. A. Morbidelli, M. Guzzo, The Nekhoroshev thorem and the asteroid belt dynamical system, *Discrete Cont. Dyn. A*, **65** (1996), 107–136.

71. J. Moser, *Mathematical Reviews MR0097508*, 20 n. 4066.

72. N. N. Nekhoroshev, An exponential estimate of the time of stability of nearly-integrable Hamiltonian systems, *Russ. Math. Surv.*, **32** (1977), 1–65.

73. A. Ponno, L. Galgani, F. Guerra, Analytical estimate of stochasticity thresholds in Fermi-Pasta-Ulam and ϕ^4 models, *Phys. Rev. E*, **61** (2000), 7081–7086.

74. M. Sansottera, A. Giorgilli, T. Carletti, High-order control for symplectic maps, *Physica D*, **316** (2016), 1–15.

75. P. A. Schilpp, *Albert Einstein, Philosopher-scientist*, Library of Living Philosophers, Volume VII, Northwestern University, 1949.

76. M. Volpi, U. Locatelli, M. Sansottera, A reverse KAM method to estimate unknown mutual inclinations in exoplanetary systems, *Discrete Cont. Dyn. A*, **130** (2018), 36.

77. J. A. Wheeler, R. P. Feynman, Interaction with the absorber as the mechanism of radiation, *Rev. Mod. Phys.*, **17** (1945), 157–181.

78. J. A. Wheeler, R. P. Feynman, Classical electrodynamics in terms of direct interparticle action, *Rev. Mod. Phys.*, **21** (1949), 425–433.

Appendix: An excursus into dark matter and plasma physics

An excursus into cosmology: far-away galaxies versus dark matter

The results we had obtained on the WF identity in the Carati model gave origin to an excursion into stellar dynamics, in connection with the problem of dark matter, through the work [25] (see also [26]), by Andrea Carati, Sergio Cacciatori and myself.

Far-away galaxies versus dark matter

Things went as follows. Once, at a conference devoted to celestial mechanics, held in Todi (a medieval town of central Italy), I was giving a talk in which I had the opportunity to describe, somehow as a curiosity, the results we had recently obtained on the Carati model. The conference was attended also by George Contopoulos, who "since always" (actually since the year 1971, when we first met in Cagliari) was following all works of our group, not only for his personal interest in the integrals of motion of Hamiltonian systems (in particular in connection with the results of Antonio Giorgilli), but also for his general endorsement to our approach to quantum mechanics, that he had explicitly expressed in several occasions. So, when I described the role of the far-away charges in the WF cancellation, he asked me whether some similar phenomenon may occur also in astrophysics, in view of the strict analogy between electric and gravitational forces, especially for their long-range action.

So, when I came back to Milan, I passed the question to Andrea Carati. A little later he came up with a possible answer: he had made a computation which showed that, taking into account Hubble's law, the retarded forces due to the far-away galaxies give an effect of the order of magnitude of that due to the gravitational force which is needed to "save the phenomena", and is usually attributed to some unobserved matter (or dark matter).

Then I met Sergio Cacciatori, who in Milan was very well known as a specialist in essentially all domains of physics and geometry, in particular in general relativity and cosmology. He took the thing very seriously, thinking of it during the whole summer. His answer was eventually that, while Andrea had considered one of the several terms that produce the force, and estimated correctly its order of magnitude, he instead had taken all of them into consideration, finding out that eventually there is an exact cancellation among them. A fact that, by the way, is predicted by a general theorem of Birkhoff.

However, the ability of Andrea Carati as an inventor is almost inexhaustible: the cancellation no more occurs if the far-away galaxies' spatial distribution is assumed to be fractal. What vanishes in that case is the mean value, whereas the typical value (the standard deviation) does not, and its estimate in a typical case still is of the correct order of magnitude. So, the prediction about the force generated by far-away galaxies works, in the hypothesis that the galaxies' distribution is fractal. Notice that one deals here with a "physical" hypothesis, that may be in agreement with observations or not. Strangely enough, it seems that on the basis of the same sets of experimental data, two groups of physicists are expressing opposite answers: according to one of them the distribution is fractal, according to the other one it is not.[12]

A comment on the possibility of using Newtonian trajectories for electrons

It was discussed, in the last part of the present paper, how Newtonian trajectories are normally used for ions, by people implementing Molecular Dynamics simulations, while the electrons are dealt with in quantum mechanical terms. The questions of principle raised by Heisenberg against trajectories in atomic physics, would thus hold only for electrons, inasmuch as the ions are "heavy". This is indeed quite strange, since not only photons and electrons, but also neutrons, for example, present diffraction effects, which are usually considered to be typical quantum phenomena.[13] So, where does one have to situate the boundary between quantum and classical? Perhaps, many things are still to be understood.

Thinking of mass as the distinctive feature, sounds very strange. We know that Newtonian trajectories make sense, not only for stones, considered by Galileo, and for the Planets (considered by Newton), but also for Galaxies, as in the example discussed a few lines above. So trajectories work

[12]Andrea Carati also made an application of his idea of far-away galaxies as a possible substitute for dark matter, in connection with the rotation curves of galaxies. He obtains curves fitting extremely well the phenomenological ones, some of which are reported in a joint paper we wrote for a series of lectures we gave in Argentina. See [28].

[13]We are confident that diffraction effects may be obtained in a classical frame through the mechanism which allowed us to reproduce the tunnel effect. See [30].

for masses from 1 kg up to 10^{42} kg. If we admit that they work also for a proton, then trajectories would work in a range of masses from 10^{42} down to 10^{-27} kg, i.e., for about 70 orders of magnitude. Then, suddenly, going down to electrons, just three more orders of magnitude, in a quite abrupt way trajectories would no more make sense, and quantum features would play an essential role. Here the Einstein's idea, if fully implemented, would do the job in a frankly more satisfactory way.

An excursus into Plasma physics: the "little discovery" of Matteo Zuin about "disruptions"

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 Padua, 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 years ago there was held in Padua a conference on the occasion of my seventieth birthday, 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 relations that had been obtained numerically. The figure contained several curves $\omega = \omega(k)$, which depended parametrically 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 produced 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, towards the axis of the abscissas. Then, above a critical density, they intersect the axis, and turn back. In other words, there exists a critical density, above which some frequencies 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 physicists 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 [24, 37], and quantitatively not so badly. Now, it is well known that other physical parameters in addition to the magnetic field are relevant in controlling the disruptions, so that a general understanding of them is still lacking. Here, however, an unexpected feature was pointed out, which is based on some "first principles property".

Analogous disruptions. or rather explosions, were observed by Andrea Carati and me, together with the brothers Gangemi in Brescia, in the realistic ionic models illustrated above. Perhaps one is 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.