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Could the Interaction of Mathematics with Continuum Mechanics or Physics be Better?

1960- I was gifted for mathematics in high school, but I was not attracted by the job of a teacher, and since I wanted to use my mathematical ability in a useful way, I decided to become an engineer, although I had no idea about what engineers do, and there was no knowledge about mathematics or engineering in my family. By chance, I fell upon the program of the entrance exam at École Polytechnique, and I decided that I was going to study there.
1962- Because of political events, I moved to live with my (maternal) grandparents in Paris, and the nearest high school, lycée Charlemagne, had classes for preparing the exams for the "grandes écoles", another chance.
1963- In the first year of preparation, the physics/chemistry teacher seemed to hate me, and I wondered later if he had thought that some of my questions showed that I came from a group hostile to physicists, but I had no a priori knowledge of the physics which he was teaching us, and the problem probably was that my way of thinking was that of a mathematician: *if I was not given clear rules for a game to be played, I could not play it*, and my first difficulty of this kind was with *thermodynamics*.

So, my first difficulty with physics was that my physics teachers had lacked precision, but much later I realized that it is difficult to be precise on subjects for which the mathematical methods do not exist yet. However, it is difficult for a mathematician to work intuitively (on something not clearly defined). Of course, it would be more easy if the mathematicians from the previous generation explained what they have learned in their quest for a better understanding of continuum mechanics and physics, but so few followed this path.

1965- I succeeded both the entrance exams at École Normale Supérieure and at École Polytechnique, where I went to study, as I had decided more than four years before. Over the two years of study, I had many different professors who taught various parts of physics, and only one left me with a good impression, because of his style of teaching with many anecdotes (Louis Leprince-Ringuet, 1901–2000), but overall I got the impression that my physics teachers were not able to distinguish well what is mathematics from what is physics, and that some physicists invent strange games which they unfortunately transform into dogma, probably because they (quite naively) believe that nature must be playing the games which they invent.

I had a good teacher in mechanics, Jean Mandel (1907–1982), who taught us classical mechanics during the first year, and continuum mechanics during the second year: his course was well planned (although it did not include his own specialty of plasticity, or anything about turbulence), but his way of teaching was not as brilliant as that of my two teachers in analysis, Laurent Schwartz (1915–2002) and Jacques-Louis Lions (1928–2001).

The teacher in probability was problematic, but it is not the reason why I advocate avoiding probabilities in science (and not in engineering): the only actual models consistent with the principle of relativity of Poincaré (1854–1912) are semi-linear hyperbolic systems of partial differential equations having characteristic velocities (at most) equal to the speed of light, so that they should be used at microscopic level in place of the variants used by physicists in their quantum mechanical games, which are not hyperbolic systems, and at best are models corresponding to letting the speed of light c tend to ∞. At first sight, one might think that such approximations are reasonable, since macroscopic bodies hardly move at noticeable velocities compared to the speed of light, but we are not talking about macroscopic scales or even mesoscopic scales here: at microscopic scales, i.e. inside an atom or in the interaction between nearby atoms, it is the realm of the speed of light, i.e. electromagnetism on one side, and another part like Dirac's equation for describing matter, with the Planck constant h appearing in the coupling between these two parts (so that h appears in the cases where light and matter interact). One would then need to understand adapted limits *in the sense of homogenization* for expressing the consequences at mesoscopic levels, and since such mathematical questions are far from being understood yet, it is hoped that the actual laws which one uses are reasonable approximations of reality; however, the processes invented by probabilists are too crude since they depend upon too few parameters, hence they can only give a distorted view of reality, but probabilists also seem to believe that nature must follow the games which they invent. Consequently, it is better to be cautious when one replaces real phenomena by invented stochastic processes.

In the beginning of my second year of studies at École Polytechnique, I went to an evening talk by Laurent Schwartz on the "role and duty of a scientist": I must point out that in France, mathematics is considered as one of the sciences, so that mathematicians are particular kind of scientists. Among other things, he mentioned that his friends who had become engineers complained after a while of too heavy administrative duties, and it made me realize that with my difficulties in (oral and written) communication, I should not become an engineer: at the end of that evening, I chose to do research in mathematics, and with my intention of doing useful things with my mathematical ability I "naturally" chose Jacques-Louis LIONS as an advisor.

1967- I had come from a background with no information on scientific matters, and with luck I had found the best place for learning a good mixture of mathematics, mechanics, and physics, with teachers in mathematics including Laurent SCHWARTZ, the "father" of the theory of distributions, which is the modern language for the (linear) partial differential equations of mechanics and physics, and his former student Jacques-Louis LIONS, considered the star of the French applied way of doing mathematics (which is quite different from the British way, of course). I could not have started in a better way, since there was no mathematician in the previous generation who had developed the kind of specialization which I was slowly making mine.

1969- I was told by someone whom I knew from École Polytechnique (but was from the year after mine) that I should read a book entitled "Foundations of Mechanics", so that I bought it and read it: I was quite puzzled, since it was a useful book containing plenty of results about manifolds, but there was no mechanics in it! I noticed that many "pure mathematicians" seemed stuck with classical mechanics, the 18th century point of view about mechanics which is based on (small systems of) ordinary differential equations, and often ignore continuum mechanics, the 19th century point of view about mechanics which is based on partial differential equations.

1970- I was shown an article by two differential geometers, about the Euler equation for an inviscid and incompressible fluid, in a framework possibly due to Vladimir ARNOL'D (1937–2010) based on using affine connections and covariant derivatives, and I wondered if such a framework extended to real fluids, and what was the interest of translating truncated versions of the equations of continuum mechanics in a language which differential geometers preferred.

There is a famous quote by GOETHE (1749–1832) (*Die Mathematiker sind eine Art Franzosen; redet man zu ihnen, so übersetzen sie es in ihre Sprache, und dann ist es alsobald ganz etwas anders*), that mathematicians are a sort of Frenchmen, and if you say something to them, they translate it into their own language, and then it is immediately something quite different: could GOETHE have foreseen the behaviour of differential geometers?

FEYNMAN (1918–1988) recalled a lesson learned from his father, that one might know the name of a bird in many different languages without knowing anything about the bird itself. He also described the behaviour of some graduate students whom he taught to in Rio de Janeiro (where he was going for playing the bongo in the samba schools), and he said that they learned physics like a foreign language, speaking the words but not understanding that what he was teaching them was about describing the real world.

1974- In the beginning of 1974 I decided to understand more about the mechanics / physics content of the partial differential equations which were studied, in part because I had understood a framework which had appeared to be new, where I used various topologies of a weak type for explaining the relations between various levels, without the need of the probabilities which had been mentioned in my physics courses, and without the simplification of periodicity assumptions which Évariste SANCHEZ-PALENCIA was using for his asymptotic methods, and my insight had grown out of my joint work with François MURAT on the appearance of homogenization in questions in optimal design, which we had developed without realizing that we were actually talking about *effective properties of mixtures*.

With such a new insight, I started the long and arduous process of creating the mathematical tools for a better understanding of what I call the *20th century point of view in mechanics / physics*, which corresponds to *partial differential equations with small scales*, like (in alphabetic order) *atomic physics, meteorology, phase transitions, plasticity, turbulence.*

1982- I worked at Orsay from 1975 to 1982. Through my work on compensated compactness and homogenization, I was seeing the need for techniques from algebra and geometry, although not in the way differential geometers were working, and I could not find anyone serious able to explain the fashion about Yang–Mills equation around this time. Since there were discussions on various branches of mathematics, a famous differential geometer, Marcel BERGER, had been invited. He did not explain which questions geometers were interested in and why, but he mentioned the names of a few young geometers who could be hired. I asked him if among them there were some who were interested in understanding physics, and his answer was "it is already very difficult to become good in one discipline, and it is extremely harder to become good in one discipline and a half".

The situation inside mathematics is then similar to what FEYNMAN had observed, that it was quicker for him to develop the mathematics which he needed, since it was difficult to discover a mathematician able to understand what he needed, and who would also know if some mathematicians knew the answers to his questions.

Robert DAUTRAY offered me a position in his group at CEA (Commissariat à l'Énergie Atomique), and my job was both simple and impossible, read the physics books which he told me, and figure out a better mathematical approach, so that I easily found the problem which a few others must have noticed, even though no one says it explicitly: although we have heard claims since the mid 1950s that fusion is controllable, some recent reports mention that one may succeed only in the second part of this century, and that only with the help of a new generation of powerful lasers for assessing experimentally the properties of plasmas at millions of degrees, since one cannot trust the invented laws, despite popular games using what happens at billions of degrees in the next milli-second after the supposed big-bang!

Thermodynamics is not about dynamics, since it describes what happens at equilibrium, and one cannot expect to be doing good mechanics / physics if there is no time, but thermodynamics was developed in the 19th century before much of the important work on differential equations was done by POINCARÉ and by LYAPUNOV (1857–1918). Given an ordinary differential equation in $\mathbb{R}^N$ with N small, one can study its stationary points, and the stability of each stationary point (and more generally, one can study ω-limit sets which describe the asymptotic behaviour of a given solution and its stability properties); however, knowing the stationary solutions does not tell what the evolution equation is, and it is quite naive and not very good mechanics / physics to only think in terms of gradient flows, although it is so often done.

One cannot either expect to be doing good mechanics / physics if there is no space, and although both MAXWELL (1831–1879) and BOLTZMANN (1844–1906) had already thought of developing a kinetic theory of gases for correcting the defect of equations of state in continuum mechanics, their ideas are only reasonable for rarefied gases, if the only source of kinetic energy is translational; the framework is already inadequate for real gases, and certainly more so for liquids and for solids; besides, the Maxwell–Boltzmann equation is already flawed for having imposed probabilities, which have replaced a conservative framework by a dissipative one, hence one cannot explain the origin of irreversibility, since one has imposed it!

It is my opinion that why people have trouble imagining something different than the Maxwell–Boltzmann equation is that they think in terms of "particles": it has been proposed to correct the classical picture of binary "collisions" with three-particle collisions; although three particles being at the same point at the same time is a rare event, the interaction of three of more particles in a conservative model with attracting forces depending upon the distance can be considered, and the collective behaviour of a group of particles dancing together for a while before separating in smaller groups is certainly an important scenario to consider for denser gases, and probably a better way to understand fluids than using the limiting problem considered by HILBERT (1862–1943) for the Maxwell–Boltzmann equation (when the mean-free path during collisions tends to 0).

It is my opinion that talking about particles is the language of 18th century mechanics, and talking about waves is the language of 19th century mechanics, so that it is better to base one's intuition in terms of waves at the surface of the sea, and maybe imagine the "surface" of the sea in stormy weather, which it is no longer realistic to describe by an altitude $h(x, y)$ in two horizontal coordinates. In other words, the classical intuitive picture used in kinetic theory looks like ideal sailing on an unperturbed lake, encountering a single wave or a single gust of wind: actual sailing creates waves, and the pattern of wind and waves can be very intricate, and one cannot ignore the instabilities of the surface of the sea which a sustained strong wind automatically creates, as in the roaring 40s, the furious 50s, and the screaming 60s in the southern hemisphere.

The point of view of homogenization says that there are microstructures which are adapted to a given system of partial differential equations, and that one wants to deduce what they are, and how they evolve (and not impose what they must be, and how they must evolve), but it is not the detail of the microstructures which one wants to describe, but only their effective properties. Of course, since one already knows simple examples where effective equations show nonlocal dependence in time and space, one may have to go *beyond partial differential equations*, although the precise class to consider has to be ascertained.

If one wants to consider the discrete versions of groups of particles dancing together, then passing to the limit might be tricky, but since one starts from a conservative framework, the effective limiting equation should be conservative, and it has puzzled people before, since this fact is not consistent with using (a finite number of) partial differential equations.

What probably happens for the effective equations is that they contain nonlocal effects, and that they probably imply a better mechanics / physics than what has been used up to now.

Due to the complexity of the materials used, one will have to look precisely into what the quantum mechanical models say, and it is my opinion that the ideas of DIRAC (1902–1984) should become more useful than they are at the moment, because at the level of atoms one needs equations with a finite propagation speed, and the Schrödinger equation does not have this property. How many students are told that?

In conclusion, it may be very difficult for a mathematician to learn continuum mechanics and physics, but it is important that mathematicians learn these subjects, and that they also write books on the mathematical techniques and ideas which can be "digested" by engineers and physicists.

In my writing program which I plan to restart after retiring in a few months, I plan to write about what I have understood, and explain how the progress in mathematics during the 20th century permits to correct a few unnecessary dogmas. It is not an easy task, and I hope to be able to write some books which will induce more mathematicians to learn more on mechanics and on physics, and books which will explain to engineers and physicists what the mathematics of evolving mixtures is all about.