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Approximation of H-measures and beyond

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In memory of Roland Glowinski

This article suggests a few ways for approximating H-measures. Since an important use of H-measures is to show how some conserved quantities hide at mesoscopic levels, not dissipated but transported away, it proposes questions to address for correcting some defects of physical theories.

For a conference in Tours (France) in 1997, on the occasion of the 60th birthday of my good friend Roland Glowinski, I wrote an article *Approximation of H-measures*, and although I had finished it for the beginning of the conference, it was too late to be included in the proceedings of the conference.[1]

Roland suggested that I publish my contribution elsewhere, but I did not try, and I actualize it here.

1. Memories

While a student at "École Polytechnique" (located in Paris on the "Montagne Sainte Geneviève" in those days), which I had preferred to "École Normale Supérieure" because I wanted to become an engineer, I had finally changed my mind after hearing a talk by Laurent Schwartz on the role and responsibility of a scientist, and I had decided to do research in Mathematics. Laurent Schwartz was teaching the Analysis course, and Jacques-Louis Lions was teaching the Numerical Analysis course, and I chose to ask Jacques-Louis Lions to become my advisor; with their help I obtained a position of Stagiaire de Recherche at "CNRS" (Centre National de la Recherche Scientifique), starting in October 1968 after the completion of my three year contract with the Army (since École Polytechnique is also a military school).

[1] I had not noticed that the deadline was much earlier than the time of the conference. The 1997 text is available as Research Report 97-204 of the Department of Mathematical Sciences at Carnegie Mellon University.

Jacques-Louis LIONS later mentioned the possibility of a position at "IRIA" (Institut de Recherche en Informatique et Automatique, which became INRIA a few years later, adding a N for National), where he was going to lead a research group in Numerical Analysis, but the status of this newly created institute was not so clear at the time.

Thanks to a government effort towards research at that time (of which the creation of IRIA was an aspect), the third year of my contract with the Army was spent in an efficient way as my only duty during the academic year 1967–1968 was to obtain a "DEA" (Diplôme d'Études Approfondies) at the University (not yet cut into quite a few pieces), a mandatory requirement before I could be allowed to work on my thesis. For my DEA in Numerical Analysis, I had to take a few courses in the North of Paris at "Institut Blaise Pascal", and Jacques-Louis LIONS was giving a course and organizing a seminar there, but I was also following another course that he was teaching at "IHP" (Institut Henri Poincaré), behind the Panthéon in the "Quartier Latin"; the Lions-Schwartz seminar was also held there. In addition, Jacques-Louis LIONS had also asked me to follow a few lectures and seminars at IRIA, which was located quite a few kilometers West of Paris, in Rocquencourt, in buildings which had become vacant as a consequence of the decision of General DE GAULLE to take France out of the military part of NATO.

My only contact with Numerical Analysis had been through what Jacques-Louis LIONS had been teaching, and although I had learned Fortran during the preceding Summer as he had told me, I was surprised to discover that the official programming language taught for the DEA was Algol. Nevertheless, I was determined to use Fortran for the algorithm that I had been given to test (and which I had found not too efficient from a theoretical point of view), but when I finally decided to write my program (which meant punching cards in those early days), Institut Blaise Pascal had been closed: the general strike of May 1968 had begun! I quickly found a solution to that problem, and went to École Polytechnique, where I could enter easily; I went to the computer room, which I found open but with no one there, and I had time to punch my cards, put them in the feeder, run my program, get a few pages of results and leave without been asked anything! I have not written another program since that time.

Sometime during that year, I had first met Roland GLOWINSKI, but I am not entirely sure where and when it was, partly because I was quite shy at the time and I did not talk much with the people I met, and partly because of that busy schedule; I was going to IRIA with a fellow student who owned a car and he always wanted to leave immediately after the last talk. The following year was quite different, as I only had to go to IHP and IRIA, and I could spend more time at IRIA because I had my own car, and being at IRIA more often, I had more contacts with Roland GLOWINSKI, whom I found quite different from all the other persons whom I was seeing around Jacques-Louis LIONS.

In those days, as I mentioned to him some time after, I could not grasp the physical properties related to the various partial differential equations that we were studying, as my understanding of them was primarily through their mathematical properties, and I was thinking in terms of Functional Analysis and Sobolev spaces. A few years after, having learned from Jacques-Louis LIONS the state of the art concerning the mathematical methods for attacking linear and nonlinear partial differential equations, I finally had the way to understand what I had been told at École Polytechnique in Continuum Mechanics, but it was the discovery in my work with François MURAT about how to use *various weak convergences* for describing the relations between microscopic and macroscopic levels that gave me the possibility of starting to understand what I had been told in Physics. In those early years then, I was not even trying to understand more about Continuum Mechanics or Physics: I was just following a direction chosen by Jacques-Louis LIONS.

Roland GLOWINSKI had graduated from École Polytechnique seven years before me (and neither Laurent SCHWARTZ nor Jacques-Louis LIONS were teaching there at the time), and he had acquired experience as an engineer and as a numerical analyst before learning anything on Sobolev spaces, and the difference with the others, which I perceived more and more afterwards, was that he did not need to follow a direction chosen by Jacques-Louis LIONS, like that of translating into a framework of Functional Analysis and Sobolev spaces all the numerical schemes that engineers had used before with success, and proving that they indeed converge to the desired solution, according to the philosophy valid for linear problems and attributed to Peter LAX that a numerical scheme is convergent if and only if it is consistent and stable.

I understood easily everything that Jacques-Louis LIONS was teaching, but I was discovering that it was mainly the skeleton of Numerical Analysis, necessary to know but not really sufficient, as some of the spirit was missing: how to invent efficient algorithms was the crucial problem, almost impossible to learn by reading or by listening to lectures, because experience cannot be learned but has to be acquired by practice, and this was one ingredient which made Roland GLOWINSKI different.

Other things made Roland different, his personal qualities, that one measured by counting the number of friends he had, even within his own professional circle. It certainly took me many years to notice that, because for a long time my "life" was mostly concentrated upon Mathematics, until a crisis happened, and among a very small circle of friends Roland and his wife Angela were of immense help to me.

In the Fall of 1975, after a year spent in Wisconsin, I had left my first position at "Université Paris IX-Dauphine" to go to "Université Paris-Sud" in Orsay, and looking for buying a house in the area it was natural to check if anything was available in "Les Hauts de Chevreuse", where I had once visited Roland and Angela; in January 1976, I had moved then in the same "Allée Blaise Pascal" where they lived.

Academic life in Orsay appeared to be difficult, as political reasons outweighed scientific considerations almost all the time, and when in the Fall of 1979 my colleagues accepted to send falsified results of votes to the minister in charge of the universities, and pretended to give me reasons why I did not have the right to vote in the commission that I was supposed to be part of, a crisis began.

I had not in my youth lived events as traumatic as those Roland went through, but I had certainly suffered of being a son of a protestant minister isolated in a catholic majority, and nothing had hurt me as much as hearing teachers talk about an infamous massacre of protestants in French History, on Saint Barthelemew's day, without condemning it, producing then a wave of sinister comments directed at me from some of my fellow students, who seemed to enjoy that idea of killing protestants.

Facing the inadmissible behaviour of my colleagues in Orsay, I felt that my religious upbringing forced me to react, and show them the only way an honest person could behave, and I expected to revive their conscience so that they would understand that it was the duty of any citizen to denounce falsifications of administrative documents; I was hoping too that at least some would understand how racist a behaviour it was to deny me any of my rights. As my only "life" consisted in existing as a mathematician, it had a dramatic effect for me to put all my energy for saving it and obtain no answer but the smiles of the organizers of the falsifications, who sometime boasted of their political connections.

I shall be eternally grateful to Roland and Angela for their warm support at this critical point of my life, together with my other friends of Chevreuse and Saint Rémy lès Chevreuse; without their help I would not have found the way out of this abyss. I am thankful to Robert Dautray for having given me the way to resume my work in a safe environment at "Commissariat à l'Énergie Atomique" (for five years).

2. The ingredients of success

There were various reasons which had enabled Jacques-Louis Lions to develop a new and different school of Numerical Analysis in France, with many of his students continuing on the ideas that he had taught them, but Roland Glowinski had arrived with a slightly different background and it was him who took the lead for what concerned practical applications.

What can one learn from the conditions that made this creation and evolution possible, and could one recreate some of these optimal combinations for meeting the challenges of the 21st century?

One reason was that there had been some recent advances in the understanding of Partial Differential Equations, based on earlier improvements in Functional Analysis and the relatively new theory of Distributions developed by Laurent Schwartz (and also by Sergeï Sobolev, but he was not free in USSR to publish his results); as his student, Jacques-Louis Lions had mastered the essentials, and he had decided to push in the direction of some applications to continuum mechanics, probably influenced by Peter Lax.

Another reason was that there was a large pool of good students, due to the French system of “Grandes Écoles”, of which the two more prestigious were École Normale Supérieure and École Polytechnique. For two years after obtaining their “baccalauréat”, which gives automatic entrance to the universities, the best students in the scientific sections usually did not go study at university, but prepared for the difficult “concours” by studying in classes of “mathématiques supérieures” and “mathématiques spéciales”.

Roland Glowinski had studied at “Lycée Charlemagne”, where I went myself later, and I must admit that what I learned there was an excellent blend of Algebra, Analysis and Geometry. Unfortunately, due to what may be called the “Bourbaki sabotage”, many professors now teaching Mathematics in these classes have often been brainwashed at the university to consider Analysis as part of Physics, and can only transmit to their students a distorted view of Mathematics.

Another reason was that the students at École Polytechnique were taught a good set of courses, well adapted to start doing research in Applied Analysis, while the students at École Normale Supérieure did not, as the Bourbaki sabotage prevailed there. It is a tradition at École Polytechnique that promotions are alternately yellow or red; Laurent Schwartz had become Professor at École Polytechnique in 1959, teaching Analysis to the yellow promotions, so he was my teacher in 1965, while in 1958 Roland must have had courses by Favard, who was teaching Analysis to the red promotions until he died in 1964; since Jacques-Louis Lions only started teaching Numerical Analysis at École Polytechnique in 1964 (teaching both promotions), I do not know what Roland had been taught in his days.

In my days, the Analysis course of Laurent SCHWARTZ contained the essentials in Topology, basic Functional Analysis, Measure theory and the theory of Distributions, and it gave the necessary background for studying the partial differential equations of Continuum Mechanics and Physics.

The Numerical Analysis course of Jacques-Louis LIONS contained many basic algorithms, together with Finite Difference approximations for basic partial differential equations, but there were no Sobolev spaces which I only heard about in a seminar that he organized for interested students; of course, they appeared in his course a few years after when he included Finite Elements approximations, which were obviously not something he knew when I was a student, as I clearly remember that he invited Jean DESCLOUX (from EPFL, École Polytechnique Fédérale de Lausanne, Switzerland) to give a talk on finite elements at IRIA, and he asked me at the end of the talk if I saw the difference with the Galerkin method, which was one of his favorite constructive tool (as him, I did not see the difference at that time).

At École Polytechnique too, another type of sabotage has occurred since, spreading partly from Orsay but not only because I had failed to gather support against the experts in falsification of administrative documents there, as signs of it can easily be traced much earlier and at a wider scale, and I propose to call it the "Cold War sabotage".

Because of the talk that had convinced me to become a mathematician, I had asked some help from Laurent SCHWARTZ, expecting him to understand the similarity of my situation with that of the scientists fighting against oppression, whom he had chosen as an illustration in his talk; he had refused.

A few years after, I wrote many letters to Laurent SCHWARTZ for describing what had happened in Orsay, only to find that like my ex-colleagues he had killed his conscience many years before, and he supported himself the destruction that I was trying to avoid.

I never understood on what side Jacques-Louis LIONS was: he once told me that most of the military engineers, on which the French industry of military applications relied, had come out of École Polytechnique; obviously, the changes in the program, putting emphasis on Ordinary Differential Equations and Geometry and promoting Classical Mechanics (i.e. 18th century Mechanics), was the best way to form inept engineers, to the benefice of the other side in the Cold War.

I never understood on what side Ciprian FOIAS was either: he once told me that since its strong education system was one of the strength of France, it would be the first target of its ennemies, but Cold War had raged for a while already, and the education system was already quite crippled as a result.

How will the challenges of the 21st century be met with students who are no longer taught the adequate pieces of Mathematics, and who have been brainwashed by the mathematical and non mathematical media, in majority favourable to the Cold War sabotage?

Will there be enough students who can find their way through the fashions and wrongful advertisements like the theory of catastrophes which studies singularities of differentiable mappings and assumes its proponents to be brainless so that they can believe that the World is described by Ordinary Differential Equations; or slogans like "God is a geometer", obviously invented by atheists for having such a bad opinion of God?

Perhaps not, but there might be more students who will follow some unconventional path, maybe like Roland GLOWINSKI or myself who started our studies to become engineers and ended up being mathematicians working in a university environment.

3. What are H-measures?

In 1997, the only new item that I could add as a possible ingredient of my preceding list, was a relatively new piece of Mathematics, that I had developed a few years before, and which I called H-measures, because I first introduced these measures for questions of Homogenization.

In part because of my fight against the Cold War sabotage, many like to attribute my ideas to others, if not to themselves, and that process is not new.

Around 1930, Sergeï SOBOLEV was the first to invent weak derivatives for defining the functional spaces bearing now his name, and Jean LERAY also used this concept for weak solutions of the incompressible Navier–Stokes equations (which he wrongly thought related to turbulence), but the notion of distributions is now widely attributed to Laurent SCHWARTZ, who developed it only around 1945. As from what I have been told, the reasons of this misattribution look very similar to the political reasons which make many avoid mentioning my name, I want to explain what I was told on this question.

Like Roland GLOWINSKI's father, Jean LERAY was an officer in the French Army and was taken prisoner by the Germans in 1940; they both spent a few years in a camp, and I understand that officers were treated almost decently. Jean LERAY told me that a university was organized in his camp (and he was its rector), and that he stopped working on Navier–Stokes equation for fear that his results could be used by the Germans (that is when he switched to Algebraic Topology, I believe, and developed Sheaf theory).

Unlike Jean Leray, André Weil (a member of the Bourbaki group) avoided the draft and barely escaped being sentenced to death for that; he related his story in a book [22], where he failed to convince me as he forgot to mention that in France at that time only communists were against the war and proned desertion (which made me see a link between the Bourbaki sabotage and the Cold War sabotage).

His wartime behaviour made André Weil lose against Jean Leray for a position at "Collège de France" in 1948; as Jean Leray told me, one result was that another member of the Bourbaki group plagiarized his articles and got then all the references for himself. As Laurent Schwartz was also a member of the Bourbaki group, it explains then why many prefer to forget to mention Leray and Sobolev when talking about Distributions.

I first talked about H-measures in some conferences in 1988 [12], and gave a talk in the seminar of Jacques-Louis Lions at Collège de France in the beginning of 1989 [13], but my text was not included in the proceedings of the seminar, probably because it contained allusions to what had happened at Orsay, and although Jacques-Louis Lions had been aware of it for many years he may have thought that it was forbidden to write about it under a socialist government; it must be public knowledge by now. Due to my slowness, the article containing the detailed proofs only appeared in 1990 [14].

H-measures are quadratic microlocal objects which I first introduced for questions of small amplitude Homogenization, and then for another question of Homogenization where a lower order term could be computed explicitly using these H-measures, as this question was my first hint that such a formula could be written [15], and it was related to some of my ideas about turbulence; then I used them to improve my method based on the Compensated Compactness method for obtaining bounds on effective coefficients [15, 16].

If I did not have such huge difficulties for writing, I would have written these results as a first article, but I wanted to check my tool on another front: for many years I had explained that it was the propagation of oscillations and not the (wrongly called) "propagation of singularities" (since it is propagation of microlocal regularity) in the style of Lars Hörmander which was important for understanding Physics, but I was facing there another aspect of the Cold War sabotage, with a whole group of my ex-colleagues from Orsay involved in it, brainwashing the poor students (and the bad physicists) into believing that a ray of Light is the question that they are studying in their propagation of microlocal regularity (advertised as "propagation of singularities") which occurs along bi-characteristic rays; I wanted to check if my new objects could describe propagation of oscillations, and the results were beyond my expectations.

H-measures are indeed adapted to describing the propagation of oscillations, and concentration effects, for a class of systems endowed with a quadratic conserved quantity, definitively showing what is wrong in the point of view of Lars Hörmander, but they do more than that.

H-measures provide at last a rational and mathematical explanation of one of the more crucial question of contemporary Physics, explain why some particles may behave like waves: my interpretation relies on an idea proposed by Louis de Broglie in his thesis in 1924, that there are no particles out there, there are only waves, probably then described by some *semi-linear* system of *partial* differential equations, whose oscillating solutions *for linear cases* define some adapted H-measure and propagate so that the adapted H-measure satisfies a system of *ordinary* differential equations, and this is what one interprets as "particles".

There is still a lot of work ahead, as the theory does not say much about nonlinear effects and obviously there are coupling effects, but I have not been able to create a mathematical theory for explaining what physicists describe in Quantum Field theory (sometimes with Feynman diagrams, for example).

After my talk in Paris in January 1989, I learned about the work of Patrick Gérard [4, 5], who had independently introduced almost the same objects for a completely different purpose, the question of compactness by averaging (which I had not been able myself to put into my framework).

He called his objects "mesures microlocales de défaut" (microlocal defect measures), which is not a good name as it reminds too much of the wrong point of view of Lars Hörmander that microlocal regularity (which is propagated along bi-characteristic rays) is important, and can only encourage more brainwashing from the adepts of the Cold War sabotage, who of course refer now to Patrick Gérard for *my* results of propagation; they also refer to Gilles Francfort & François Murat [3] for *my* results of propagation, when they had only taken care of clarifying a question about initial data for the wave equation with constant coefficients, with the technical help of Patrick Gérard.

I hope that one day will come when the adepts of the Bourbaki sabotage and the Cold War sabotage would have lost some of their power of intimidation and that more honest references will become the norm, and that one will attribute the work of Patrick Gérard, Jean Leray, Laurent Schwartz, Sergeï Sobolev, or Luc Tartar to whomever would have done it.

H-measures are microlocal objects which do not use any characteristic length.

In the general work on Homogenization, done partly in collaboration with François Murat, we took great care of using no characteristic length, and it is a pity that those who have specialized in applying our methods only in the periodic case would rarely mention that our work had no such restriction (and they usually forget to mention too the early work of Évariste Sanchez-Palencia, which had been of great help to me for developing an intuition and create the new point of view of using *various weak convergences* for relating microscopic and macroscopic levels).

In applying my H-measures to the wave equation, I found that propagation of oscillations and concentration effects behave according to the laws of Geometrical Optics, but my statement is quite different from the formal asymptotic theory (with a phase satisfying an eikonal equation and an amplitude satisfying a transport equation where the gradient of the phase appears); in the limit of infinite frequency, I found an analog of Geometrical Optics, with no phase necessary, the dual variable ξ replacing the direction of the gradient of the phase in the transport equation for the amplitude, and I found that some H-measure satisfies a first order partial differential equation in (x, ξ), whose characteristic curves are the bi-characteristic rays.

The caustics do not play a primary role, since I obtained the equation directly in its weak formulation form and not by trying to obtain an equation for its possible density in (x, ξ) (the caustics do appear if one wants to study the regularity of the density).

One is far from the construction of Fourier Integral Operators that Lars Hörmander had developed for describing the solutions of waves operators, because the classical theory of pseudo-differential operators was not powerful enough, but anyway I could not even have thought of using that classical theory of pseudo-differential operators because of the inadmissible hypothesis of C^∞ coefficients that would rule out most of the applications, and I had to develop a class of operators adapted to my purpose. Since I assumed that the coefficients of my wave equation are of class C^1, there are still some improvements to be made for describing general refraction effects.

From the point of view of numerical approximation, one sees that H-measures may provide a way to avoid many details which are not necessarily of great importance, and instead of integrating a wave equation on a very fine mesh, or discretising some Fourier Integral Operators, I think that developing discrete approximations of H-measures together with an approximate transport equation for these discrete H-measures may be of some interest in the future.

For what concerns large but not infinite frequencies, the task of deciding how much of Joseph KELLER's Geometric Theory of Diffraction is right is still largely open (he himself had pointed out that it is wrong near the caustics), although Patrick GÉRARD has obtained some partial results using his semiclassical measures in his work with E. LEICHTNAM [7]. The adepts of the Cold War sabotage have been wrongly claiming that Gilles LEBEAU has explained Joseph KELLER's theory, but I believe that his work has not much to do with what Joseph KELLER has proposed: Gilles LEBEAU's work is in the wrong spirit of Lars HÖRMANDER and deals with microlocal regularity using the space G^3, and this particular Gevrey space appears because of properties of the Airy function, while in Joseph KELLER's theory one computes the integral of $|k|^{1/3}$ along grazing rays, and this particular power of the wave number k also appears because of properties of the Airy function, but the similarity stops there; my guess is that in order to explain Joseph KELLER's theory one should derive an equation for some kind of microlocal measure to be defined (and using at least one characteristic length), and this equation would confirm only a part of Joseph KELLER's theory, and should explain what happens near caustics.

Before describing variants of H-measures using one or more characteristic lengths, I want to give an intuitive description of what H-measures are.

Let us consider first a scalar sequence $u^{(n)}$ converging weakly to 0 in $L^2_{loc}(\Omega)$, where Ω is an open set of $\mathbb{R}^N$; for localizing in x one chooses a test function $\varphi \in C_c(\Omega)$ and one considers $\mathcal{F}(\varphi\, u^{(n)})$, where $\mathcal{F}$ denotes the Fourier transform (and since I was taught by Laurent SCHWARTZ, I use $\mathcal{F}f(\xi) = \int_{\mathbb{R}^N} f(x)e^{-2i\pi(x,\xi)}\,dx$ for $f \in L^1(\mathbb{R}^N)$, which extends as an isometry on $L^2(\mathbb{R}^N)$); as $\mathcal{F}(\varphi\, u^{(n)})$ tends to 0 in $L^2_{loc}(\mathbb{R}^N)$ strong but does not converge to 0 in $L^2(\mathbb{R}^N)$ strong if $\varphi\, u^{(n)}$ does not, one wants to study how $|\mathcal{F}(\varphi\, u^{(n)})|^2$ converges near infinity in any particular cone centered at 0, and the basic result is that after extracting a subsequence $u^{(m)}$, there is a nonnegative Radon measure μ in $(x,\xi) \in \Omega \times S^{N-1}$ which describes those limits, and more precisely for every $\psi \in C(S^{N-1})$ and every $\varphi \in C_c(\Omega)$ one has

$$\begin{aligned}\lim_{m\to\infty} \int_{\mathbb{R}^N} |\mathcal{F}(\varphi\, u^{(m)})|^2(\xi)\psi\Big(\frac{\xi}{|\xi|}\Big)\,d\xi &= \int_{\Omega\times S^{N-1}} |\varphi(x)|^2\psi(\xi)\,d\mu(x,\xi)\\ &= \langle \mu, |\varphi|^2 \otimes \psi\rangle.\end{aligned}$$

For a vector valued sequence $u^{(n)}$ converging weakly to 0 in $L^2_{loc}(\Omega;\mathbb{R}^p)$, after extracting a subsequence $u^{(m)}$, there is a Hermitian nonnegative $p\times p$ matrix of Radon measures $\mu = (\mu_{i,j}, i,j = 1,\ldots,p)$ in $(x,\xi) \in \Omega \times S^{N-1}$ such that for all $i, j = 1, \ldots, p$, for every $\varphi_1, \varphi_2 \in C_c(\Omega)$ and every $\psi \in C(S^{N-1})$ one has

$$\lim_{m\to\infty} \int_{\mathbb{R}^N} \mathcal{F}(\varphi_1 \, u_i^{(m)})(\xi)\overline{\mathcal{F}(\varphi_2 \, u_j^{(m)})(\xi)}\psi\Big(\frac{\xi}{|\xi|}\Big)\, d\xi = \\ \int_{\Omega\times S^{N-1}} \varphi_1(x)\overline{\varphi_2(x)}\psi(\xi)\, d\mu_{i,j}(x,\xi) = \langle \mu_{i,j}, \varphi_1\overline{\varphi_2}\otimes\psi\rangle.$$

If one uses real valued functions, as I have implicitely assumed (although there is no difficulty in dealing with complex valued functions), one finds that the corresponding H-measures are invariant by changing ξ into $-\xi$; a consequence of this remark is that one cannot send a beam of Light in one direction without sending the same amount of Light in the opposite direction if one uses real data (this is valid for scalar Light described by the wave equation as well as for the real polarized Light that we experience every day, described by the Maxwell–Heaviside system).

From the point of view of creating discrete approximations of H-measures, one could devise various ways, like a decomposition into spherical harmonics to deal with the variable ξ, as physicists often do, but that might not be a good idea, since H-measures often live on small sets, as a consequence of what I have called the Localization Principle, which transforms differential informations on $u^{(m)}$ into constraints for the support of μ: if the functions $A_{j,k}$ are continuous, and

$$\sum_{j=1}^{N}\sum_{k=1}^{p} \frac{\partial(A_{j,k}u_k^{(m)})}{\partial x_j} \to 0 \text{ in } H^{-1}_{loc}(\Omega) \text{ strong},$$

then one has

$$\sum_{j=1}^{N}\sum_{k=1}^{p} \xi_j A_{j,k}\mu_{k,i} = 0 \text{ in } \Omega\times S^{N-1} \text{ for every } i = 1,\ldots,p.$$

One may then encounter H-measures living on a union of smooth manifolds inside the sphere S^{N-1}, but one may also find cases where the support of μ is countable, as in the periodically modulated case: let Q be the unit cube $(0,1)^N$, and let $v \in L^2(Q)$ with average 0, extended to $\mathbb{R}^N$ into a function of period 1 in each x_i, $i = 1,\ldots,N$, and having Fourier expansion

$$v(x) = \sum_{q\in Z^N\setminus 0} v_q e^{2i\pi(q,x)},$$

then the sequence $u^{(n)}$ defined by $u^{(n)}(x) = v(n\,x)$, corresponds (without extraction of a subsequence) to the H-measure

$$\mu = \sum_{q \in Z^N \setminus 0} |v_q|^2 \delta\Big(\xi - \frac{q}{|q|}\Big), \quad \text{i.e.} \quad \langle \mu, \Phi \rangle = \sum_{q \in Z^N \setminus 0} \int_\Omega |v_q|^2 \Phi\Big(x, \frac{q}{|q|}\Big)\,dx$$
$$\text{for all } \Phi \in C_c(\Omega \times S^{N-1}).$$

It is also important to notice that $\xi \in S^{N-1}$ does not really correspond to a constraint $|\xi| = 1$: on $\mathbb{R}^N \setminus 0$ one says that x and y are equivalent if they are proportional with a positive factor, and the equivalence classes are rays through the origin, the unit sphere is only a convenient way of choosing a particular element in each equivalence class.

The preceding remarks suggest that it is not always a good idea to use spherical harmonics for approximating H-measures, but the case of some concentration effects might hint otherwise: for a given function $\psi \in L^2(\mathbb{R}^N)$, let $z \in \Omega$ and let $u^{(n)}$ be defined by

$$u^{(n)}(x) = n^{N/2} \psi\big(n(x - z)\big),$$

then $|u^{(n)}|^2$ converges weakly to $C^2 \delta_z$ where C is the L^2 norm of ψ, and this sequence corresponds (without extraction of a subsequence) to a H-measure of the forme $\delta_z \otimes g$ for some density g on S^{N-1} defined by the formula

$$\langle \mu, \Phi \rangle = \int_{\mathbb{R}^N} |\mathcal{F}\psi(\xi)|^2 \Phi\Big(z, \frac{\xi}{|\xi|}\Big)\,d\xi$$
$$\text{for all } \Phi \in C_c(\Omega \times S^{N-1}),$$

i.e. $g(\eta) = \int_0^\infty |\mathcal{F}\psi(t\eta)|^2 t^{N-1}\,dt$ for $\eta \in S^{N-1}$, and if g is smooth, it can be well approximated by spherical harmonics.

I knew that for some problems I was going to need a characteristic length, and I was thinking about diffusion equations with a small diffusion coefficient like

$$\frac{\partial u}{\partial t} - \varepsilon^2 \sum_{i,j=1}^N \frac{\partial}{\partial x_i}\Big(A_{i,j} \frac{\partial u}{\partial x_j}\Big) + \sum_{k=1}^N B_k \frac{\partial u}{\partial x_k} + C\,u = f,$$

with initial data generalizing $e^{-\varepsilon(D\,x.x)}$ for which I knew a more direct approach; I was certainly not thinking about the Schrödinger equation because many years before I had arrived at the conclusion that the Dirac equation (without mass term) contains all the right information and that everything useful obtained from the Schrödinger equation should be derived from the Dirac equation, while some of the paradoxes of Quantum Mechanics are due to the defects of the Schrödinger equation, where hyperbolicity has been lost.

I was not either thinking in terms of periodicity like in crystals, because I do not like the idea of considering a crystal as given, not only because one mostly observes poly-crystals with grain boundaries moving and getting stuck on defects, but because I am more interested in explaining why crystals are formed. As a prerequisite, one would have to derive a better version of Thermodynamics or Statistical Mechanics (i.e. correct their defects) using H-measures or other objects, which remains to be done.

I explained my idea for using one characteristic length in a talk that I gave in the seminar of Jacques-Louis Lions at Collège de France in the beginning of 1990; my text was ready at that time but three years after it was not published yet and I was asked to translate it into English (due to a change of owner of the publishing house) and it finally appeared in 1994 [17]. Again, a few months after my talk, I learned that Patrick Gérard had introduced a similar idea, a little more easy to handle than mine, and he had called his objects semiclassical measures, in relation with some methods used by physicists [6]. I had no reason to invent a name, as my idea (shown on an example) is to introduce another variable and consider H-measures with one more variable; more precisely, if $u^{(n)}$ was a given sequence converging weakly to 0 in $L^2_{loc}(\Omega)$, I introduced a new variable x_{N+1} and a new function $U^{(n)}$ defined by

$$U^{(n)}(x, x_{N+1}) = u^{(n)}(x) \cos\Big(\frac{x_{N+1}}{\varepsilon_n}\Big),$$

ε_n being the chosen characteristic length tending to 0; as $U^{(n)}$ converges weakly to 0 in $L^2_{loc}(\Omega \times \mathbb{R})$, one can extract a subsequence corresponding to a H-measure μ, living on $(\Omega \times \mathbb{R}) \times S^N$, but looking at the definition one sees easily that if one stays away from $\xi_{N+1} = 0$, then that measure is independent of x_{N+1}, and its projection on $\Omega \times (S^N \setminus S^{N-1})$ is essentially the same measure on $\Omega \times \mathbb{R}^N$ which Patrick Gérard had defined (considering the point $(\xi', 1)$ in each equivalence class instead of the point in S^N). However, Patrick Gérard's definition is more easy to handle, and he thought of more general situations than the ones I had in mind. He did not impose that $u^{(n)}$ converge weakly to 0, and he wanted to consider all the possible limit points because of some different situations that he had in mind, so for $\varphi \in C^\infty_c(\Omega)$ and $\psi \in S(\mathbb{R}^N)$ he considered a semiclassical measure for the characteristic length ε_n associated to a subsequence $u^{(m)}$ to be defined by the formula

$$\lim_{m\to\infty}\int_{\mathbb{R}^N} |\mathcal{F}(\varphi\, u^{(m)})(\xi)|^2\psi(\varepsilon_m\xi)\, d\xi = \int_{\Omega\times\mathbb{R}^N} |\varphi(x)|^2\psi(\xi)\, d\mu(x,\xi)$$
$$= \langle\mu, |\varphi|^2\otimes\psi\rangle.$$

His definition has two defects: as ψ is continuous at 0, the informations corresponding to wavelengths tending to 0 but much longer than ε_n are mixed for all directions, while as ψ is 0 at infinity, the informations corresponding to wavelengths much smaller than ε_n are lost, and Patrick GÉRARD therefore introduced two definitions to name those sequences where no information is lost, and without these precautions one cannot in general recover the H-measure from the semiclassical measure, contrary to what Pierre-Louis LIONS & Thierry PAUL wrongly wrote in their article [11], where they wanted to rename Wigner measures the same measures that Patrick GÉRARD had already correctly defined and named in a reasonable way (although it is questionable to give different names to various variants of H-measures), when they were not even able to understand what Patrick GÉRARD had done.

For sequences converging weakly to 0, the two defects of Patrick GÉRARD's definition can be easily fixed by considering ψ to be of the form $\psi_0(\xi/|\xi|)$ near 0 and either the same condition near infinity or a more general one like ψ bounded uniformly continuous at infinity; the first choice consists in compactifying $\mathbb{R}^N\setminus 0$ by a sphere at 0 and a sphere at infinity, while in the second case the compactification at infinity is more subtle; only after a compactification like one of these can one expect to recover the H-measure from the semiclassical measure (I do not want to go into the details of the proofs of all my statements, which are either in [14], or in the lecture notes [19], which I had announced in 1997, but mostly wrote in the Fall of 2007 during a sabbatical semester at Politecnico di Milano, in Milan, Italy), but the proof for H-measures is based on a commutation lemma saying that a commutator is compact, while the proof for semiclassical measures requires estimating the norm of a similar commutator and showing that it tends to 0.

From the approximation point of view of either H-measures or their variants, what we see here is that the choice of a characteristic length gives a little more precision on a portion of the information carried by oscillations and concentration effects, and in the case where there is only one characteristic length, the measure using the characteristic length does contain more information, but as realistic problems often contain more than one characteristic length, and some may contain an infinity of them, it is useful to see how the preceding approach fails, by looking at the following computation, done with Patrick Gérard.

One considers the following sequence

$$u^{(n)}(x) = \begin{cases} \sqrt{n} & \text{if } kn \le n^2 x < kn + 1 \text{ for } k = 0, \ldots, n-1, \\ 0 & \text{otherwise.} \end{cases}$$

One sees easily that $u^{(n)}$ is bounded in $L^2(0,1)$, converges weakly to 0 in $L^2(0,1)$, and that $|u^{(n)}|^2$ converges vaguely to 1 (i.e. for continuous test functions), but not weakly in $L^1(0,1)$ (i.e. for bounded measurable test functions); however this is not the point of interest here, but the fact that this sequence obviously contains two scales $\alpha_n = 1/n^2$ and $\beta_n = 1/n$, and the question is to guess what the semiclassical measures would be, depending upon the choice of ε_n. With the intuition behind the definition of semiclassical measures, we expected to observe the following five cases.

Case 1: ε_n very large compared to β_n; one expects that all the information will be lost at infinity.

Case 2: ε_n of the order of β_n; one expects to find a nonzero semiclassical measure and that some of the information will be lost at infinity.

Case 3: ε_n very small compared to β_n but very large compared to α_n; one expects that some of the information will be lost at zero and some of the information will be lost at infinity.

Case 4: ε_n of the order of α_n; one expects to find a nonzero semiclassical measure and that some of the information will be lost at zero.

Case 5: ε_n very small compared to α_n; one expects that all the information will be lost at zero.

However, when we computed the various semiclassical measures, we observed only the following three cases

Case 1&2&3: ε_n very large compared to α_n; all the information is lost at infinity.

Case 4: ε_n of the order of α_n; one finds a nonzero semiclassical measure, but no information is lost at zero or infinity.

Case 5: ε_n very small compared to α_n; all the information is lost at zero.

In consequence the information corresponding to the larger characteristic length β_n seems to have disappeared, a quite strange fact if one considers that $u^{(n)}$ is periodic with period $\beta_n = 1/n$ on the interval $(0,1)$. As we discovered on this example, our intuition was right that there would be a scale of $n = 1/\beta_n$ shown in the Fourier transform, but instead of showing up at a distance of order $1/\beta_n$ from the origin as we expected, it appeared at a distance of order $1/\alpha_n$ from the origin, inside the information that we had naively thought would only come from the scale α_n.

Of course, we could have thought of it, as it is but the classical phenomenon of beats, but from the mathematical point of view it tells us that one should devise a way to *discover which characteristic lengths appear in a given problem and how they interact*, and then *track a hierarchy of interacting oscillations*; in some way, it might be what physicists have been doing for quite a while, and why FEYNMAN invented his famous diagrams.

In 1997 I thought that soon one would have a mathematical understanding of many questions related to Physics, where different scales interact, but 25 years later it is not done yet.

It cannot be done by following blindly what physicists say, in order to avoid the mistake that Pierre-Louis LIONS & Thierry PAUL did, probably because they believed from the start that H-measures were but the same idea that WIGNER had developed; I shall certainly quote WIGNER in the future if I am shown any evidence of that, but Pierre-Louis LIONS & Thierry PAUL advocating WIGNER but showing a bad understanding of what they were talking about is a good hint that WIGNER had not been able to explain clearly what I have expressed in mathematical terms.

From the approximation point of view, I cannot guess what the best method will be for approaching these better equipped objects that I have hinted at here, but I still have another approach to explain, which I discovered with Patrick GÉRARD by trying to do simply what Pierre-Louis LIONS & Thierry PAUL were doing in a complicated way. Wigner transform consists in associating to a function $u \in L^2(\mathbb{R}^N)$ the function $W_u \in C_0(\mathbb{R}^N \times \mathbb{R}^N)$ by

$$W_u(x,\xi) = \int_{\mathbb{R}^N} u\Big(x+\frac{y}{2}\Big)\overline{u\Big(x-\frac{y}{2}\Big)}e^{-2i\pi(y,\xi)}\,dy,$$

and this transformation was shown to me in the early 80s by George PAPANICOLAOU when I had mentioned to him my idea of splitting Young measures in ξ (an idea which I had to abandon in order to introduce H-measures); he had stressed that the interest of Wigner transform was that it could see both u and its Fourier transform: indeed, allowing for a little more regularity for u, one has

$$\int_{\mathbb{R}^N} W_u(x,\xi)\,dx = |\mathcal{F}u(\xi)|^2 \text{ for } u \in L^2(\mathbb{R}^N)\cap L^1(\mathbb{R}^N),$$
$$\int_{\mathbb{R}^N} W_u(x,\xi)\,d\xi = |u(x)|^2 \text{ for } u \in L^2(\mathbb{R}^N)\cap \mathcal{F}L^1(\mathbb{R}^N).$$

I had not seen how George Papanicolaou's idea of using Wigner transform could help for my purpose, but he himself had pursued his idea, and in collaboration with Joseph Keller and their student Leonid Ryzhik, they obtained results for propagation of waves in random media [9, 10].

Pierre-Louis Lions & Thierry Paul, having a one characteristic length point of view of the World, had the idea of introducing the sequence

$$W^{(n)}(x,\xi) = \int_{\mathbb{R}^N} u^{(n)}\Big(x + \frac{\varepsilon_n y}{2}\Big)\overline{u^{(n)}\Big(x - \frac{\varepsilon_n y}{2}\Big)}e^{-2i\pi(y,\xi)}\,dy,$$

and showed that $W^{(n)}$ converges vaguely to the semiclassical measure of Patrick Gérard (so they should have entitled their article "another way of introducing semiclassical measures based on Wigner transform"); the main difficulty in their proof consisted in proving directly that the limit is a nonnegative measure.

It seems that Wigner discovered that if u solves a zero potential Schrödinger equation then his function W_u solves a free streaming equation where ξ plays the role of a velocity, and he would have liked to have $W_u \geq 0$ so that he could interpret it as a density of particles having velocity ξ; he noticed that a convolution in ξ by a suitable Gaussian gives a nonnegative result and this is the crucial observation used by Pierre-Louis Lions & Thierry Paul to show that the limit is a nonnegative measure, although they attribute this idea to someone else. What I found with Patrick Gérard is a simple way to explain what there is behind these formulas, and I wonder if this was what Wigner had in mind when he invented his transformation: once one has decided to use a characteristic length ε_n, the natural thing to do is to use it for defining correlations, and it is natural for two-point correlations to extract a subsequence such that

$$u^{(m)}(x+\varepsilon_m y)\overline{u^{(m)}(x+\varepsilon_m z)} \rightharpoonup G_2(x;y,z) \text{ vaguely in } \mathcal{M}(\Omega\times\mathbb{R}^N\times\mathbb{R}^N),$$

using of course continuous test functions with compact support (so that on the support $x+\varepsilon_n y, x+\varepsilon_n z \in \Omega$ for n large enough), and notice that, as ε_n tends to 0, the measure G_2 has the form

$$G_2(x;y,z) = \Gamma(x;y-z) \text{ on } \Omega\times\mathbb{R}^N\times\mathbb{R}^N;$$

then one observes that for every points $z_j \in \mathbb{R}^N$ and every scalar λ_j one has

$$\sum_{j,k}\Gamma(x;z_j-z_k)\lambda_j\overline{\lambda_k} = \sum_{j,k}G_2(x;z_j,z_k)\lambda_j\overline{\lambda_k}$$

$$= \lim_{m\to\infty} \Big| \sum_j \lambda_j u^{(m)}(x+\varepsilon_m z_j) \Big|^2 \geq 0,$$

and therefore by Bochner theorem (extended to tempered distributions by Laurent SCHWARTZ), there exists a nonnegative Radon measure $\mu(x,\cdot)$ such that $\Gamma(x,\cdot) = \mathcal{F}\mu(x,\cdot)$, and this measure is precisely the semiclassical measure defined by Patrick GÉRARD.

From the approximation point of view, it may be preferable to approach correlations, which are more classical objects to handle, and although one has not yet defined suitable microlocal objects that could describe trilinear or more general multilinear effects, one can always define correlations if the corresponding L^p bounds are available, and obtain equations that they satisfy, as in the following computations, done with Patrick GÉRARD. If the coefficients $b_k, k = 1, \ldots, N$, are of class C^1 and real and if $u^{(m)}$ satisfies the equation

$$\frac{\partial u^{(m)}}{\partial t} + \sum_{k=1}^N b_k \frac{\partial u^{(m)}}{\partial x_k} + c\, u^{(m)} = 0 \text{ in } \Omega \times (0,T),$$

and $u^{(m)}$ defines the semiclassical measure μ in $\Omega \times (0,T)$, one can deduce an equation satisfied by μ from the equation satisfied by the correlation function Γ: one considers the equation evaluated at $x+\varepsilon_m z$ and multiplied by $\overline{u^{(m)}}$ evaluated at x, and one adds the complex conjugate of the equation evaluated at x and multiplied by $u^{(m)}$ evaluated at $x+\varepsilon_m z$, and letting ε_m tend to 0, one finds that Γ satisfies the equation

$$\frac{\partial \Gamma}{\partial t} + \sum_{k=1}^N b_k \frac{\partial \Gamma}{\partial x_k} + \sum_{k,l=1}^N \frac{\partial b_k}{\partial x_l} z_l \frac{\partial \Gamma}{\partial z_k} + 2\Re(c)\Gamma = 0 \text{ in } \Omega \times (0,T) \times \mathbb{R}^N,$$

and therefore μ, its Fourier transform in the variable z, satisfies the equation

$$\frac{\partial \mu}{\partial t} + \sum_{k=1}^N b_k \frac{\partial \mu}{\partial x_k} - \sum_{k,l=1}^N \frac{\partial b_k}{\partial x_l} \frac{\partial (\xi_k \mu)}{\partial \xi_l} + 2\Re(c)\mu = 0 \text{ in } \Omega \times (0,T) \times \mathbb{R}^N.$$

If one denotes $P(x,\xi) = \sum_{k=1}^N b_k \xi_k$, and one identifies the Poisson bracket $\{P,\mu\}$ among the terms, one finds the same equation that I had derived for H-measures

$$\frac{\partial \mu}{\partial t} + \{P,\mu\} + (2\Re(c) - div\, b)\mu = 0 \text{ in } \Omega \times (0,T) \times \mathbb{R}^N.$$

At this point, I should warn of a treacherous trap: as Patrick Gérard noticed, although all the semiclassical measures satisfy the same equation independently of what sequence ε_n one chooses, it does not prove that the H-measures do satisfy that equation; indeed, there are situations where for every sequence ε_n the information is either lost at zero or at infinity, and therefore H-measures cannot even be deduced from the knowledge of all semiclassical measures, for all sequences tending to zero.

If in the equation for $u^{(m)}$ one then adds a term $-\varepsilon_m^2 \frac{\partial}{\partial x_i}\big(A_{i,j}\frac{\partial u^{(m)}}{\partial x_j}\big)$ with A Hermitian and continuous, the equation for Γ will contain a new term, which is $2\sum_{i,j=1}^N A_{i,j}\frac{\partial^2\Gamma}{\partial z_i \partial z_j}$, and the equation for the semiclassical measure μ will contain a new term, which is $8\pi^2\big(\sum_{i,j=1}^N A_{i,j}\xi_i\xi_j\big)\mu$. It seems more easy then to approximate the two-point correlation Γ, and obtain a discrete version of the equation that it satisfies, than approximate the semiclassical measure μ itself; actually for what concerns three-point correlations, one can define an analog of Γ but one does not know how to define an analog of μ: if $u^{(m)}$ tends to 0 weakly in $L^3_{loc}(\Omega)$, then one can extract a subsequence such that

$$
\begin{gathered}
u^{(m)}(x+\varepsilon_m z_1)u^{(m)}(x+\varepsilon_m z_2)u^{(m)}(x+\varepsilon_m z_3) \quad \rightharpoonup \quad G_3(x;z_1,z_2,z_3) \\
\text{vaguely in } \mathcal{M}(\Omega\times\mathbb{R}^N\times\mathbb{R}^N\times\mathbb{R}^N),
\end{gathered}
$$

and G_3 satisfies

$$
\sum_{j=1}^3 \frac{\partial G_3}{\partial z_j} = 0, \text{ i.e. } G_3(x;z_1+h,z_2+h,z_3+h) \text{ is independent of } h.
$$

If $u^{(m)}$ satisfies

$$
\frac{\partial u^{(m)}}{\partial t} - \varepsilon_m^2 \Delta\, u^{(m)} = 0 \text{ in } \Omega\times(0,T).
$$

then G_3 satisfies

$$
\frac{\partial G_3}{\partial t} - \sum_{i\neq j}\frac{\partial^2 G_3}{\partial z_i \partial z_j} = 0 \text{ in } \Omega\times\mathbb{R}^N\times\mathbb{R}^N\times\mathbb{R}^N.
$$

4. My hopes in 1997

There are other aspects of H-measures that may well be worth considering for questions of approximation, but I preferred to concentrate my attention on the use of H-measures (and of their variants with one or more characteristic lengths), for questions of propagation of oscillations and concentration effects.

The main reason was that I thought that many important developments would occur in the opening years of the 21st century in relation with using a better mathematical understanding of what it means for "particles" to be waves (and that is valid for atoms or molecules). As a consequence, one might have to switch from some classical models to new systems of partial differential equations or even to more general models, and the reason why it had not been possible to do that before was that without a precise mathematical definition of what one has to do, it is difficult to find one's way in the jungle of different models used by physicists. The situation may be clearer for some problems coming from Engineering, but the boundary is becoming fuzzy because some recent technological advances have forced to use phenomena occurring at a very small scale, not far from where "particles" appear not to be particles!

The transition to the new era might be difficult for many who may see their preferred equation lose part of its scientific interest, although one should remember that obsolete problems may still contain quite interesting Mathematics, but *one should not lure students into working on an obsolete problem without having explained to them what one is really looking for*.

A typical example will be those models from kinetic theory, like the Maxwell–Boltzmann equation, which were derived in the 19th century by very good scientists who were obviously *thinking in terms of classical particles interacting through an instantaneous force at distance*, a concept that we know now to be wrong (although it is still helpful to imagine things like Lennard-Jones potentials); moreover these particles only interacted by pairs and (in the process of determining the fluid limit) formal expansions implied an ideal gas behaviour which is not at all what one observes for real gases, so that *either the formal expansions are wrong, or they are right but the model is therefore irrelevant for describing the real World*.

Transport equations will remain as important as ever, but one will have to derive correct ones.

The theory of H-measures has opened a new way for understanding these questions, with a rational derivation from partial differential equations, and although it may take a few more years before one obtains a mathematical understanding about what to do for semi-linear hyperbolic systems, I have strong hopes that the theory will be improved and will accomplish a great unification.

I guess that these theoretical considerations, which were at the core of my research work for a long time now, will have interesting repercussions on the way some numerical solutions will be sought in the future.

5. What is the situation now?

My predictions of 1997 have not happened yet, I believe: more should be done about developing better adapted mathematical tools for *improving our understanding of how the real World functions at small scales*.

My former PhD student Nenad Antonić organized that I taught a few lectures in 2000 in Dubrovnik (Croatia) for a conference on *Multiscale problems in science and technology*, and my course [18], entitled *Mathematical tools for studying oscillations and concentrations: from Young measures to H-measures and their variants* describes the development of H-measures and variants from a slightly different angle, which inspired Nenad to introduce his H-distributions, and provide precise constructions as its possible realisations with his collaborators, in [1] and [2] for example.

However, I noticed a few other questions which should be addressed, for example about thermodynamics.

— Sometimes around 2000, I heard a talk by Kunbakonam Rajagopal, whom his friends call Raj, in Paris (and he noticed that we always met abroad although both of us were working in Pittsburgh at the time, since he worked at University of Pittsburgh then, and not yet at Texas A&M): he took some paste out of a jar, and he started malaxing it in order to make a ball, while telling us that the paste could be considered a (viscoelastic) liquid since if he put it in a bowl it would slowly fill the bottom with an horizontal interface, but then he showed that it bounced back like a rubber ball, so that one could consider it an elastic solid supporting high deformations, without dissipation of energy. Then, he threw it as hard as he could against the blackboard, so that everyone ducked, since we expected the ball to bounce back into the room where we sat, but something quite different happened: it splashed against the blackboard as if it was made of jelly!

Raj then mentioned that if he had not warmed it first and had hit it hard with a hammer, it would have exploded into many small pieces flying into the room, like for a brittle solid, and it would not have been wise since the paste is slightly corrosive, and he left the room to go wash his hands.

Raj then concluded his talk by saying that one does not know a modelling for such a material, which behaves so differently for slow motions and for fast motions.

My understanding is that he meant to say that *thermodynamics is wrongly named because it is adapted to slow motions but ill-suited for fast motions, for which a good description of the material at mesoscopic levels is necessary for hoping to predict what will happen next!*

— In the beginning of the Summer of 2000, I was invited for two weeks at University of Rennes, in Brittany, by Roger LEWANDOWSKI, and I gave him a book by Jerry ERICKSEN after reading it (so that I am not sure about its title anymore), and Jerry wrote in it something like "we do not have yet a theory of plasticity compatible with thermodynamics". It made me think that Jerry assumed thermodynamics to be right and hoped that one would soon find a theory of plasticity compatible with thermodynamics.

My personal interpretation now is that *thermodynamics is not adapted to fast processes*; elasticity is a slow process while *plasticity is a fast process*, hence *thermodynamics does not apply to plasticity*, but *new rules for "a new thermodynamics for fast processes" should be found, and applied to plasticity!*

— I have not read much on plasma physics, but enough to observe that there are plenty of instabilities, and it is then difficult to get a global picture. I noticed once the term "anomalous diffusion" (of heat inside a plasma), and it is the sign of a wrong attitude among some "physicists".

My definition of Science is about discovering how the World functions, while Engineering is about doing useful things for society, so that what Nature does is what scientists must understand, and calling a part of its behaviour anomalous is a sign that some "scientists" have transformed into a kind of religious fundamentalists: they prefer to believe some dogmas invented a long time ago for luring naive people, instead of using their brain for *thinking about these dogmas, in order to discard the silly ones*.

If plasmas violate some of the dogmas that "physicists" have taught to generations of students, is not the only possible explanation that the dogmas do not correspond to what Nature does, and if they want to continue claiming to be interested in real Physics, i.e. what Nature does (at atomic scales), should they not be looking for *correcting their early proposals which are in contradiction with experimental facts?*

One should observe that plasmas use fast processes (and the speed of light c appears more or less explicitly in the part of the equations describing electromagnetic effects), hence the rules which are taught for slow processes do not apply! However, since calling something anomalous suggests that one has no intention to elucidate this question for the benefit of future generations, it shows that some are not really interested in Science, hence they do not care about failing to train their students to understand real Science!

— There are difficulties of the same kind with fluids.

In the spring of 1985 I spent two months at IMA (Institute for Mathematics and its Applications) in Minneapolis, MN, and on the occasion of a conference there, Dan JOSEPH showed his laboratory to a group of people, and someone, seeing some strange shapes taken by very viscous fluids around rotating axes, started a question "does not this contradict ...", but Dan did not let him finish his question, and he said firmly "I do not care if it contradicts anything, since it is there!".

On another occasion, Dan described another kind of experiment, which showed a regime where a fluid reacts like an elastic body, and it is only now that I realize that it was in the situation of a fast process, and not in the situation of a slow process, which one explains by the so-called "Navier–Stokes" equation: in 1821, NAVIER postulated a dissipation for modelling viscosity (which I see now as an error!) and he obtained what should be known as *Navier's equation*, and in 1843 BARRÉ de Saint-Venant derived the same equation by using a constitutive relation giving the Cauchy stress tensor (which was unknown in 1821), and it was only 2 years after (in 1845) that STOKES made the same observation than SAINT-VENANT.

Later, Dan JOSEPH made experiments on suspensions, and he collaborated with Roland GLOWINSKI on this question. Roland once mentioned to me some questions about this subject, and it confirmed my former impression that the classical "Navier–Stokes" equation may not be a good model for real fluids near the boundary of the domain, and the discrepancy may become worse in the case of moving boundaries, like for various objects falling in a liquid when they squeeze boundary layers between them.

It was in a second subject proposed by Jean-Pierre GUIRAUD in a thesis defence in Paris around 1970 that I first heard about PRANDTL's idea concerning boundary layers (and the work of Olga OLEINIK about the equations he had proposed); for another thesis defence, Jean-Pierre GUIRAUD proposed a subject on hyperbolic conservation laws, and my memory is not clear about which of the two was for Roland's thesis.

In 1982, I stopped in Madison (Wisconsin) on my way back from LANL (Los Alamos National Laboratory, in Los Alamos, New Mexico) to Paris, and I asked Richard MEYER (whom I knew from the year 1974–75 that I spent in Madison) about what he thought would be important questions to solve in continuum mechanics: he gave me a report of his on the Stewartson triple deck structure for boundary layers.

Jean-Pierre Guiraud gave me some hints about the triple deck structure, and my good friend Edward Fraenkel told me that Stewartson had made a list of constraints that a good theory of boundary layers in hydrodynamics should satisfy (and obviously the goal was to correct the bad guess of Prandtl), but that his triple deck construction does not satisfy all of them! Olivier Pironneau told me that the triple deck is used for describing how a boundary layer detaches, and that it is what one wants in some cases: for example, golf balls are made with dimples so that the boundary layer detaches, and it reduces the drag.

When specialists of boundary layers in hydrodynamics explain the ideas of Prandtl or of Stewartson, they start by neglecting some terms in "Navier–Stokes" equation, and I first thought that they knew these terms to be small, but then I assumed that they felt that these terms should not be there, i.e. that *"Navier–Stokes" equation is probably not good near the boundary, and the equation should be corrected!*

In his 3-volumes lectures on Physics, Feynman has a chapter on Euler equation called "equation for dry water", a way to say that Euler equation is not a good model for Physics, and he has a chapter on "Navier–Stokes" equation called "equation for wet water", but Feynman forgot to mention an important point: wetting is about forces at molecular level, like for surface tension effects, so that viscosity is not at all about dissipation, as Navier thought when he derived his equation! Neither Barré de Saint-Venant in 1843 nor Stokes in 1845 understood this unphysical effect, and they just rediscovered Navier's equation as a consequence of a constitutive relation, but why did Feynman fail to mention this discrepancy?

In 1982, Jean-Pierre Guiraud mentioned to me a question of counter-flow in the triple deck. In 2006 (while at a conference in Paris honouring Évariste Sanchez-Palencia), he mentioned to me that the size of the various layers (in the triple deck) can be understood from an analysis of instabilities.

Concerning counter-flows, once that I was looking at the waves on a sandy beach, I thought that while the top of the wave goes up the beach there is already a counter-flow below going down, but after the water was gone I noticed that the sand was striated in two directions, and I imagined that there are two directions of counter-flows at the same time, i.e. a really 3-dimensional effect (which cannot exist in 2-dimensional situations); of course, the boundary conditions over wet sand are different than those for a solid boundary.

I then read that the skin of sharks has some kind of triangular denticles, which I thought adapted to the two counter-flows which I had imagined, hinting at a reason why sharks move so easily in water.

Later, I read about "biomimetism", which is the idea of engineers to mimic what Nature does efficiently, and some naval engineers use shark skins (or approximations of them) for diminishing the drag on boats, but I am more interested in the scientific aspect instead: I think one has wrong ideas about what happens in the boundary layers around a moving 3-dimensional body (together with wrong ideas outside the boundary layers since I said that "Navier–Stokes" equation should be corrected).

— When I read about Joe KELLER's GTD (Geometric Theory of Diffraction) in the mid 1980s, it was for what he called the acoustic case, i.e. the scalar wave equation, by opposition to the whole Maxwell–Heaviside system, which serves for real "light", or more precisely for general electromagnetic waves.

Around 1990, when I talked with Joe (in his office at Stanford) about his theory, he mentioned some defects near caustics, but he said "quantum mechanics says that light can go through an obstacle (with some probability), but my theory says that light does not go through an obstacle, it turns around it!".

In 2007, while I was writing [19], during a sabbatical semester at Politecnico di Milano, I found on the Internet a thesis corresponding to using GTD for antennas and radar waves, and it mentioned sizes of boundary layers, so that *GTD can be seen as being about boundary layers in electromagnetism!*

Actually, one of my Physics teachers mentioned a boundary layer effect in talking about a "skin effect" for high frequency waves, but it was not for wavelengths near visible light, and quite different from Joe KELLER's mentioning that his computations of *light creeping into the shadow reminds of the tunnelling effect in quantum mechanics, except that light does not go through the obstacle, but around the obstacle!*

It was only about ten years ago that Michel GONDRAN, who studied with me at École Polytechnique, mentioned something from his forthcoming book [8] with his son, about an interesting episode which had occurred in 1818. In England, one had developed a cult of personality towards NEWTON, so that his point of view of a particle-nature of light prevailed against the point of view of a wave-nature of light proposed by HUYGENS, until the 1802 two-slits experiment of (Thomas) YOUNG (in which one observes interferences) revived the wave-nature point of view about light.

In 1818, the Académie des Sciences in Paris proposed a prize on the subject of diffraction of light, the president of the jury (ARAGO) being a partisan of HUYGENS, while the rest of the jury (BIOT, GAY-LUSSAC, LAPLACE, and

POISSON) were partisans of NEWTON. FRESNEL presented a memoir in which he proposed that light is a transverse vibration of an hypothetic (elastic) medium, and this was before CAUCHY had written his equation for linear elasticity (completed by LAMÉ); this medium was later called æther, and was thought to exist until the 1887 experiment of MICHELSON and MORLEY about it, and even later.

POISSON looked for a flaw in FRESNEL's ideas, and he found that they imply that there would be a bright spot in the middle of the shadow of a solid opaque sphere illuminated by a point source, which he thought nonsense! However, ARAGO ordered to check this effect in a careful experiment, and the bright spot is there, which one now calls either the *Poisson spot* or the *Arago spot*; of course, FRESNEL got the prize.

I was not surprised, since I had read about GTD, but POISSON thought that the center of the shadow would be as illuminated as if the opaque sphere was not there, and it is not what GTD predicts: only the grazing rays are supposed to creep into the shadow, but losing their energy away exponentially fast with a coefficient proportional to the cubic root of the wavenumber, and using the radius of curvature, since an exponential must be applied to a real or complex number (or an element of a normed algebra).

However, I now have a doubt, since the source sends energy uniformly in directions, and the grazing rays correspond to a measure 0 solid angle: one must then hope that the behaviour described for grazing rays is valid inside a boundary layer of a precise thickness, so that it becomes important to prove how much of the intuition of Joe KELLER is right; actually, at the conference in Tours in 1997 for the 60th birthday of Roland GLOWINSKI, after Cathleen MORAWETZ told me that Joe had just received the Wolf Prize, she said that he knew about many computations which had been done before, for various geometries.

One reason why I find it important to derive rigorously the behaviour for the solutions of the Maxwell–Heaviside system in various boundary layers is that it should have been done a long time ago for stopping a silly fashion, consisting in believing the strange ideas of a "physicist" with no "physical intuition"!

When FRESNEL proposed transverse waves in 1818, the theory of elasticity dit not exist yet, and it grew out later from the work of CAUCHY, LAMÉ, PIOLA, and KIRCHHOFF. Once it existed, one observed that in an isotropic medium there are longitudinal waves (related to $div(u)$ satisfying a scalar wave equation) and transverse waves (related to $curl(u)$ satisfying a scalar wave equation), and in the end of the 19th century (after seismology started), the first ones were called P waves (primary of pressure waves) and the second ones were called S waves (secondary or shear waves); later, (Lord) Rayleigh studied a third kind of waves, surface waves, which are evanescent waves, whose amplitude decreases exponentially fast with depth.

Once the theory of elasticity existed, did someone ask a question like "if light is a transverse wave in an elastic medium called æther, what are the longitudinal waves in that medium?".

Clerk-Maxwell had the idea of unifying the laws about electricity and the laws about magnetism, and my good friend Robin Knops told me that he used some mechanistic ideas, so that he arrived at a very complicated system; it was through the simplifying work of Heaviside that one arrived at the system used nowadays, which I call the *Maxwell–Heaviside* system. At some point, probably before the simplification by Heaviside, Maxwell had computed the velocity of perturbations for his system and found it like the velocity of light c, so that he guessed that light is related to the electromagnetic medium he modelled.

In the beginning of the 20th century, one then knew that light is governed by the Maxwell–Heaviside equation, although it was restricted to what happens outside matter, where the components of the electric field E and the components of the magnetic field H satisfy a scalar wave equation (in an isotropic medium).

Partial differential equations were not understood (although it is the language for 19th century continuum mechanics, while ordinary differential equations is the language for 18th century classical mechanics), but one forgot to mention the coupling between E and H through the boundary conditions to be satisfied!

Why did one trust Einstein about new equations in 1915, since he did not understand known equations like the Maxwell–Heaviside equation? He convinced himself and others with words from elasticity, although the Cauchy–Lamé equation for linearized elasticity does not resemble the Maxwell–Heaviside equation!

Einstein thought that "rays of light" are bent by the mass of the Sun, although one already knew from the intuition of Fermat (in the middle of the 17th century) that *a "ray of light" is bent by the variations of the index of refraction, at the places where the ray goes through, and not what happens elsewhere!*

For a scalar wave equation in an isotropic medium, so that the velocity is $\frac{c}{n}$ and the index of refraction n may vary, asymptotic solutions using an amplitude and a phase (probably developed in the middle of the 19th century) suggest that (away from caustics) it is in the limit of a frequency ν tending to ∞ that "light" propagates along the curved lines of the intuition of Fermat. My claim is that Einstein's "physical intuition" was wrong, since he forgot about a difference between low frequencies and high frequencies!

— Actually, what is "physical intuition"? Sometimes it may come from accepting dubious dogmas, so that it may be wrong! "Physical intuition" is like conjectures in mathematics.

After glancing at a book by Courant & Friedrichs about shock waves, I noticed that there were articles mentioned in a paragraph about the genesis of the subject, and I tried to read them; later, Cathleen mentioned that Friedrichs was her PhD advisor and had asked her to proofread the book: the historical part was originally longer, but cuts were made since the publishing company had found the book too long.

In 1808, Poisson wrote an article about a "discrepancy" in the velocity of sound: the constitutive relation $p = A\,\rho$ (Boyle–Mariotte) between the pressure and the density of a gas (at constant temperature) leads to a theoretical velocity of sound much smaller than what is measured! Poisson chose an engineer's approach (following an idea of Laplace, I think), of using a constitutive relation $p = B\,\rho^\gamma$, and he took for γ the value that gives the observed velocity of sound! He then computed what one now calls a "centered rarefaction wave" (by opposition to a "centered compression wave", that leads to shock waves), an he left his solution in implicit form. In 1848, Challis found that this implicit form might be wrong.

Challis was the astronomer in Cambridge (England), and I do not know why he was interested in the quasi-linear wave equation considered by Poisson, but he noticed that for sinusoidal initial data, the implicit form found poses a problem, since local minima and local maxima could be at the same place.

Stokes explained in a subsequent article that for a compression wave the profile becomes steeper and steeper until one has to accept a discontinuous profile, and *using the conservation of mass and the conservation of linear momentum he derived the correct jump conditions at a shock*. Before the article by Stokes, Airy (who was the Royal Astronomer at the time, working at the observatory in Greenwich) had written an article mentioning a few questions, for which I did not see a connection with what had bothered Challis: one of his observations was that echo does not send back the letter s.

Once that I was visiting Joe Keller in Stanford, I told him about Airy's observations. Concerning that with echo, Joe told me "it must be that the letter s uses high frequencies, which are damped more quickly!". However, I realised many years after that this is a wrong “physical intuition”, related to the inadequacy of thermodynamics for fast motions: my theorem of propagation of H-measures (which is not restricted to a scalar wave equation, like the amplitude/phase asymptotical solutions of geometrical optics) says that they are transported “away”, so that *in the limit of an infinite frequency oscillations and concentration effects are not damped locally, they are transported elsewhere*, in particular, they may contribute to the internal energy but not by increasing the temperature at the point where energy goes into hiding at mesoscopic scales!

Of course, where my physical intuition is not good enough is to tell about what happens to high but not infinite frequencies, and it is for this reason that *one needs to prove in a mathematical way the sizes of the various boundary layers in a corrected version of Joe* K*ELLER's GTD theory*.

In April 2016, I sent an email message to Joe for asking him about a different question:
“For a few years, I have criticized Einstein's theory of general relativity, and written that your GTD theory is much better for explaining how light is bent near the Sun, but I am not sure about what kind of boundary condition is good to apply to the Maxwell equation (which I call the Maxwell-Heaviside equation) because of the presence of a hot plasma near the surface of the Sun.
However, I have a question even for GTD in the acoustic case of a scalar wave equation. In the grazing rays which you studied, you found an exponential decay with a coefficient proportional to the cubic root of the wave number, but it seems to me that there might also be a small change in wavelength: is it part of your results?”

I had no answer, but Joe died in September 2016 and he may have not read my message. My idea is that in the boundary layer corresponding to the “light creeping in the shadow”, the amplitude of the wave may be decreasing exponentially but there might also be a small change in frequency, so that the light coming from a star and encountering many dust particles on its way may be changing its frequency slowly, so that in the end the so-called red shift would not be caused by a Doppler effect (due to the star moving away).

In March 2016, I had mentioned my idea to Évariste SANCHEZ-PALENCIA, who told me that Jean Claude PECKER had worked on this subject, so that I sent him a message for asking about references: PECKER answered that (with Jean Pierre VIGIER) he had defended in the early 70s the thesis that the red-shift in the spectrum of far away galaxies may not be due to a

Doppler effect, and they talked about "tiredness of light", mimicking the term "tired light mechanisms" used for example by ZWICKY in the 1930s.

PECKER also mentioned that Max BORN had used the same idea in 1954 and 1955. The hypothesis of PECKER & VIGIER was that a photon could interact with light bosons (of a still unknown nature) and thus be deflected and reddened, and he said that Louis DE BROGLIE shared their idea.

One sees an important difference with the approach of a mathematician like me.

I only have conjectures about what photons are, since physicists are not clear about what the "particles" they talk about are! A remark by FEYNMAN helps: in a text of "vulgarisation", he used the term "spin 0 photon", for expressing the fact that he was not talking about a "real photon", which has a non-zero spin!

When dealing with the scalar wave equation or the Maxwell–Heaviside equation (which is the framework I used above), one cannot define what spin is, hence one cannot talk about a "real photon"! My guess is that spin has a meaning for the Dirac equation (coupled with the Maxwell–Heaviside equation) with no mass term (of course!), but even in this case I only have a guess about what photons are, as an interpretation of an idea of BOSTICK (for the shape of an electron, but he included a 2-dimensional guess for a photon).

— Actually, I am not so interested in this question of red-shift compared to distance, because the concept of "distance" used in cosmology is rather strange, as was pointed out to me decades ago by Manuel RICOU, whom his friends call Manely. I knew that a parsec is "the distance" at which one sees the orbit of Earth (about 300 million kilometers in diameter) under an angle of one second of arc (a parallax of one second), but I did not know what the accuracy of the telescopes was for deducing how many parsecs was the limit distance one could measure. However, Manely told me that for measuring the distance of stars which are too far, astronomers have noticed that for particular stars (called Cepheid variables) one sees a connexion between their distance (measured by parallax) and the period at which their brightness changes, so that they switch to a first "pseudo-distance" measured by the period for such stars! Then, they switch further away to other "pseudo-distances", but I forgot the detail. Hence, it becomes utterly non-scientific for "astronomers" to pretend that there is a relation between red-shift and distance, since they do not describe what is the kind of "pseudo-distance" they use!

The Big-Bang is a religious concept accepted by people who were brainwashed by the biblical myth of creation, since 5 centuries before our era one wondered in India if the universe had always existed or if it had a beginning, and a wise man suggested that it is a useless question: it is like the mathematical statement that it is undecidable (because there is an upper limit to the velocity at which information can propagate)!

Then, one brainwashed the partisans of Big-Bang by telling them that it is "proved" by a "fossil background microwave radiation", so that they cannot avoid the stupid mistake of confusing "A implies B" with "B implies A": actually, none of my Physics teachers mentioned to avoid that type of mistake, while my mathematics teacher in the class of "mathématiques supérieures" had reminded us (during the first week of class) of the important difference, and he showed that if a proposition P is false then the proposition "P implies Q" is true, whatever the proposition Q is!

Actually, Pecker told me that he had published (with Karlikar) a much simpler "explanation" of the "background microwave radiation": the stars of the Milky Way (the galaxy we belong to) heat the dust around, up to a few degrees Kelvin, and the dust then radiates this energy away according to the blackbody radiation at that temperature. In agreement with the suggestion of a 14th century philosopher (William of Ockham), I much prefer this simpler local idea than the complicated global idea based on a Big-Bang!

Moreover, the adepts of Big-Bang talk about temperatures of billions of degrees, without noticing that temperature is an equilibrium concept and the hypothetical situation they consider is quite far from equilibrium, or realizing that thermodynamics is not adapted to fast processes and their hypothetical situation is an extremely fast process, or that the so-called "scientific community" led astray government agencies for promising in the last 60 years to control thermonuclear fusion, and one of the many reasons why one spent billions of dollars without success is that the laws of Nature at a few million degrees are not yet known!

— In my message to Pecker, I also mentioned another question, related to the fact that physicists use linear equations for describing an interaction (of light with matter, whatever matter is), which I find silly. Although the correct nonlinear model is not clear, one should expect that once such a model will be known one will have to correct the absorption/emission "lines" by nonlinear effects, so that one should be careful.

In the mid 1980s, I found a mention that Voigt had noticed (possibly before 1900) that the measurements of absorption of a monochromatic light in a gas do not have the shape that one pretends (that only isolated frequencies play a role), and in some domains in frequency there is a density of absorption which is very well approximated by a sum of *Lorentzian profiles*, i.e. of the form $\frac{a}{(\nu-\nu_0)^2+b^2}$ with $a,b>0$.

It then looks that what Nature produces is the imaginary part (on the real axis) of a meromorphic function having single poles a little above the real axis, since $\frac{\gamma}{z-(\alpha+i\,\beta)}=\gamma\,\frac{z-\alpha+i\,\beta}{(z-\alpha)^2+\beta^2}$: each pole comes with 3 real parameters, which are the real part α of the pole, the imaginary part $\beta>0$ of the pole, and the residue $\gamma>0$ for this pole. By pretending that only α is important, "physicists" have destroyed reality.

If they had observed that *Nature plays a game with 3 parameters for each "absorption peak"*, they would have invented other games, and *quantum mechanics would have developed differently!*

Before (since I remember thinking about that while I was at ICM1978 in Helsinki), I thought that the rules of absorption/emission imagined by physicists might be the sign that an homogenization question results in an effective equation containing a memory effect. Memory effects had already appeared in effective equations, and Évariste SANCHEZ-PALENCIA had written about that, but always for dissipative equations, while I wanted to consider an hyperbolic context, and the methods used for dissipative cases do not apply.

The problem was (and still is) too difficult: one wants to understand a question of multiple scattering by plenty of objects which are not so well known; a mole of hydrogen (weighing 2 grams, since the atomic weight of H is 1, but hydrogen makes molecules H^2) occupies 22.4 liters under usual conditions and contains the Avogadro number of molecules (around 6 10^{23}), so that each molecule uses a volume approximately a cube of side 3.25 nanometer, but there are absorption frequencies in the visible light spectrum, corresponding to wavelengths between 400 and 800 nanometers, so that there are obviously many length-scales appearing in this problem! However, "physicists" used the engineering approach of curve-fitting, pretending that the lines of absorption are all the fault of "the electron", forgetting that there are of the order of 10^{23} of them, so that there may be a first length scale associated to all the electrons, a second length scale associated to all the protons, a third length scale associated to the size of molecules, and according to the philosophy that Jean Pierre GUIRAUD told me about, a few other length scales that would be related to some instabilities: the games invented by "physicists" may give good results, but do not explain anything at all!

— It was during my stay in Rennes in 2000 that Roger LEWANDOWSKI mentioned to me the new Millenium Prizes which some had convinced a philanthropist (Landon T. Clay) to endow, but after reading the part concerned with "Navier–Stokes" equation, I found it was appalling.

Some words (meteorology and turbulence) looked like propaganda, and one does not need to have learned much about mechanics to know that the hypothesis of incompressibility is unphysical, and replaces the real velocity of sound by infinity and that the "reduced pressure" appearing in the equation is only defined up to addition of a constant. Actually, I wonder why Mr. Clay did not make a comment like "how can you expect to say anything relevant for meteorology with a model which does not have a temperature?".

Moreover, the equation is proposed for the whole space $\mathbb{R}^3$ or for a flat torus (periodic conditions), i.e. open sets without boundary, while a crucial mathematical question is about *finding bounds for the vorticity*, and that a common guess (which may be wrong) is that *vorticity seems to be created at the boundary!*

— I have mentioned some of these problems in letters, with not so many answers. However, Roland often answered by repeating advices of diplomacy, with which I disagreed, but I never mentioned to him that I could not follow this type of advice.

I am not sure why no one dared to mention the defects of classical models: if one mentions them, the best students and researchers learn a way to show their ability, and improve our understanding of science.

It is not because almost every one believes in a model that it forces Nature to follow it! Is not the goal of science to understand the functioning of Nature, while the goal of engineers is to do useful things for society?

I only understand now that the difference between Roland's diplomacy and my willingness to testify that one taught wrong things to generations of students is related to a difference in religious background.

In his early childhood, Roland discovered that it was dangerous to be from a jewish family, and he lived a few years away from his parents, hidden in the french countryside, and he learned with a loving foster family that it was essential to avoid being noticed! As for myself, son of a protestant minister in a mostly catholic country, I observed the strange aggressive way one looked at me after the teacher in primary school had mentioned an infamous massacre of protestants in french history (a few centuries ago).

Only later did I learn that the term protestant was coined at a time when to protest meant to bear witness about what one believes, so although I stopped believing in "God" before turning thirteen years old, I definitely chose to use the old protestant approach to testify, and it was in the curious political situation of fighting against an incredible method of falsifying results of particular experiments, in the form of votes, and in a campus of a university supposed to be specialized in sciences! I thought that once someone had dared to react despite the political group behind the chief organizer of the falsification of results of votes sent to the minister in charge of the universities, my colleagues would understand how silly a choice they made to be remembered forever as forgers of administrative documents! It obviously meant that they thought that their mathematical level was so completely fake, that being expelled from public service was what they wanted.

Without the help of Roland (and Angela, and very few other friends) fourty years ago I would have collapsed, but it is sad that it took me that long to overcome my psychological problems and become able to talk about all the lies which one unfortunately repeats to more generations of students and researchers.

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