APPROXIMATION OF H-MEASURES

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Dedicated to Roland GLOWINSKI on the occasion of his sixtieth birthday

Memories

While a student at "Ecole Polytechnique" (located in Paris on the "Montagne Sainte Geneviève" in those days), which I had preferred to "Ecole 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. Between Laurent SCHWARTZ, who was teaching the Analysis course, and Jacques-Louis LIONS who was teaching the Numerical Analysis course, I had chosen to ask Jacques-Louis LIONS to become my advisor; with their help I had 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 (as Ecole Polytechnique is a military school). Jacques-Louis LIONS had later mentioned to me 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 institut 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'Etudes 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 Ecole 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 then (as a result of being married, not too happily unfortunately, I was living in an apartment and driving a car that I could certainly not have afforded with my minuscule CNRS salary). 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 Ecole Polytechnique in Continuum Mechanics, but it was the discovery in my work with François MURAT about how to use 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 Ecole Polytechnique seven years before me, 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 have 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 had been already used with success by engineers and proving that they indeed converged 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 could understand 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 can measure by counting the number of friends he has, 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 one year spent in Wisconsin, I had left my first position in "Université de Paris IX-Dauphine" to go to "Université de Paris-Sud" in Orsay, and looking for buying a house in the area it had come naturally 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 protestant 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 enjoyed that idea of killing protestants. Facing the inadmissible behaviour of my colleagues in Orsay, I felt that my special religious upbringing gave me the mission 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 boasted of their political connections. As a matter of bad timing, my wife had decided to dissolve our marriage, but instead of constructing her new life, she enjoyed in threats of destructing herself and others, being all the more agressive that I was approaching a nervous breakdown. I will 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'Energie Atomique" for five years.

The ingredients of success

There were various reasons which had enabled Jacques-Louis LIONS to develop a new 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 we learn from the conditions that had made this creation and evolution possible, and could we recreate some of these optimal combinations for meeting the challenges of the twenty first 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; as his student, Jacques-Louis LIONS had mastered the essentials, but he had decided to push in the direction of some applications, probably influenced by Peter LAX.

Another reason was that there was a large pool of good students, due to the French system of "Grandes Ecoles", of which the two more prestigious were Ecole Normale Supérieure and Ecole Polytechnique. For two years after obtaining their "baccalauréat", which gives automatic entrance in the universities, the best students in the scientific sections usually do not go to study at the university, but prepare 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 Ecole Polytechnique were taught a good set of courses, well adapted to start doing research in Applied Analysis, while the students at Ecole Normale Superieure did not, as the BOURBAKI sabotage prevailed there. It was a tradition at Ecole Polytechnique that promotions were alternatingly yellow and red; Laurent SCHWARTZ had become Professor at Ecole 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; as Jacques-Louis LIONS only started teaching Numerical Analysis at Ecole 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. I remember that 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 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 Ecole 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 to 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 had written to him many letters to describe 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, but he had once mentioned to me that most of the military engineers, on which the French industry of military applications relied, had come out of Ecole Polytechnique; obviously, the changes in the program, putting emphasis on Ordinary Differential Equations and Geometry and promoting Classical Mechanics (i.e. eighteenth century Mechanics), was the best way to form inept engineers, to the benefice of only one side in the Cold War. I never understood on what side Ciprian FOIAS was either, but he had once commented that its strong education system was one of the strength of France, and that it would be the first target of its ennemies, but Cold War had raged for quite a while at that time already, and the education system was already badly crippled as a result.

How will the challenges of the twenty first 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 favorable 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 consists in studying 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? May be 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.

What are H-measures?

The only new item that I can put on the table as a possible ingredient of my preceding list, is a relatively new piece of Mathematics, that I have developed a few years ago, and which I have 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, Sergei 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 thought related to turbulence), but the notion of distributions is now widely attributed to Laurent SCHWARTZ, who developped 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 know of this question.

Jean LERAY, as Roland GLOWINSKI's father, was an officer in the French Army who 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 developped Sheaf theory). Quite 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 himself his story in a book [W], where he failed to convince me as he forgot to mention that in France at that time only communists were against the war and proning 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 his friend SOBOLEV when talking about Distributions.

I first talked about H-measures in some conferences in 1988 [T1], and gave a talk in the seminar of Jacques-Louis LIONS at Collège de France in the beginning of 1989 [T2], 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 is public knowledge now. Due to my slowness, the article containing the detailed proofs only appeared in 1990 [T3]. 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 had been my first hint that such a formula could be written [T4], 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 [T4,T5], method which is now known after the name coined for it by Graeme MILTON, the translation method. 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 "propagation of singularities" 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 was the question that they were studying in their "propagation of singularities" which they claimed occurred along bicharacteristic 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 is that there are no particles out there, there are only waves, probably described by some system of *partial* differential equations whose oscillating solutions define some adapted H-measure and propagate so that the adapted H-measure satisfies a system of *ordinary* differential equations, and this is what we interpret 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, corresponding to these curious questions of interaction that physicists describe with the FEYNMAN diagrams in Quantum Field theory, but I have not been able to create a mathematical theory for explaining what they do.

After my talk in Paris in January 1989, I learned about the work of Patrick GÉRARD [G1,G2], who had independently introduced 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 indeed propagated along bicharacteristic 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 [F&M] 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 where the adept 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 correctly attribute the work of Patrick GÉRARD, Jean LERAY, Laurent SCHWARTZ, Sergei SOBOLEV, or me 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 Enrique SANCHEZ-PALENCIA, which had been of great help to me for developing an intuition and create the new point of view of using weak convergence for relating microscopic and macroscopic levels). In applying my general framework of 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 was quite different from the formal asymptotic theory which has 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 Geometrical Optics in its geometric form, with no phase necessary, and the dual variable ξ replacing the gradient of the phase in the transport equation for the amplitude, and I found that some H-measure satisfied a first order partial differential equation in (x, ξ), whose characteristic curves were the bicharacteristic rays. The caustics did not play a primary role, as 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 when 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, as 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. As I assumed that the coefficients of my wave equation were 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 [G&L]. 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 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 R^N; for localizing in x one chooses then a test function $\varphi \in C_c(\Omega)$ and one considers $\mathcal{F}(\varphi u^{(n)})$, where $\mathcal{F}$ denotes the FOURIER transform (as I was taught by Laurent SCHWARTZ, I use $\mathcal{F}f(\xi) = \int_{R^N} f(x)e^{-2i\pi(x.\xi)}\,dx$ for $f \in L^1(R^N)$, which extends as an isometry on $L^2(R^N)$); as $\mathcal{F}(\varphi u^{(n)})$ tends to 0 in $L^2_{loc}(R^N)$ strong but does not converge to 0 in $L^2(R^N)$ 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

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

For a vector valued sequence $u^{(n)}$ converging weakly to 0 in $L^2_{loc}(\Omega; R^p)$, after extracting a subsequence $u^{(m)}$, there is a Hermitian nonnegative $p \times p$ matrix of RADON measures $\mu = (\mu_{ij}, i,j = 1,\ldots,p)$ 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

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

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 realistic polarized Light that we experience every day, described by MAXWELL's 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 as 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_{jk} are continuous, and

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

then one has

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

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 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

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

It is also important to notice that $\xi \in S^{N-1}$ does not really correspond to a constraint $|\xi| = 1$: on $R^N \setminus 0$ 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 being 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 $\psi \in L^2(R^N)$, 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

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

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 indeed 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

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

with initial data generalizing $e^{-\varepsilon(D\,x.x)}$ for which I knew a more direct approach; I was certainly not thinking about SCHRÖDINGER equation because many years before I had arrived at the conclusion that DIRAC equation contained all the right information and that everything useful obtained from SCHRÖDINGER equation should be derived from DIRAC equation, while the paradoxes of Quantum Mechanics were only due to the defects of SCHRÖDINGER equation, where hyperbolicity had been lost. I was not either thinking in terms of periodicity like in crystals, because I did not like the idea of considering a crystal as given, not only because one mostly observes polycrystals with grain boundaries moving and getting stuck on defects, but because I was more interested in explaining why crystals were formed and a prerequisite was to be able to derive a version of Thermodynamics or Statistical Mechanics 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 [T6]. 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 [G3]. I had no reason to invent a name, as my idea (shown on an example) was 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 $L^2_{loc}(\Omega \times R)$, one can extract a subsequence corresponding to a H-measure μ, living on $(\Omega \times R) \times S^N$, 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 $\Omega \times R^N$ which Patrick GÉRARD had defined (considering the point $(x, 1)$ 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 $\psi \in S(R^N)$ he considered a semiclassical measure for the characteristic length ε_n associated to a subsequence $u^{(m)}$ to be defined by the formula

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

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 was 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 have wrongly written in their article [L&P], 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 $R^N \setminus 0$ 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 [T3] or will be in the lecture notes [T7] if I ever finishes to write them, 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, where 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.

I predict then that soon we will have a mathematical understanding of many questions related to Physics, where different scales interact, but that should certainly not be done by following blindly what physicists say in order to avoid the kind of stupid 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; that could have been true actually, and I will certainly quote WIGNER in the future if I am shown any evidence of that, but the main result of Pierre-Louis LIONS & Thierry PAUL deciding to become the advocates of WIGNER and showing that they did not understand 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 $u \in L^2(R^N)$ the function $W_u \in C_0(R^N \times R^N)$ by

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

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

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

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, so that recently, in collaboration with Joseph KELLER and their student Leonid RYZHIK, they were able to obtain results for propagation of waves in random media [K&P&R1],

[K&P&R2]. Pierre-Louis LIONS & Thierry PAUL, having a one characteristic length point of view of the World, had the idea of introducing the sequence

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

and showed that $W^{(n)}$ converges vaguely to the precise semiclassical measure introduced by 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 was a nonnegative measure. It seems that WIGNER had discovered that if u was solving a zero potential SCHRÖDINGER equation then his function W_u was solving a free streaming equation where ξ played 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 had noticed then that a convolution in ξ by a suitable Gaussian gave 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 was a simple way to explain what there is behind all these formulas, and I wondered 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

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

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

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

then one observes that for every points $z_j \in R^N$ 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's 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 that had been 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

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

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

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

and 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

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

At this point, I should warn of a dangerous trap: as Patrick GÉRARD has noticed, although all the semi-classical measures satisfy the same equation independently of what sequence ε_n has been chosen, 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 $-\varepsilon_m^2 \frac{\partial}{\partial x_i}\Big(A_{ij}\frac{\partial u^{(m)}}{\partial x_j}\Big)$ with A Hermitian and continuous, the equation for Γ will contain a new term $2\sum_{i,j=1}^{N} A_{ij}\frac{\partial^2\Gamma}{\partial z_i \partial z_j}$, and the equation for the semiclassical measure μ will contain a new term $8\pi^2\Big(\sum_{i,j=1}^{N} A_{ij}\xi_i\xi_j\Big)\mu$. 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

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

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

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

Conclusion

There are other aspects of H-measures that may well be worth considering for questions of approximation, but I have preferred to concentrate my attention here 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 is that I think that many important developments will occur in the opening years of the twenty first 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 had to do, it was difficult to find one's way in the jungle of different models used by physicists. The situation was much clearer for many problems coming from Engineering, but the boundary is becoming fuzzy because so many recent technological advances have forced to use phenomena occuring 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 BOLTZMANN equation, which were derived in the nineteenth century by very good scientists who were obviously thinking in terms of classical particles interacting through a 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 deduced 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 the right 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 semilinear hyperbolic systems, confirming or infirming what physicists have been doing in Quantum Field theory, it is clearly one of the few promising ideas which have arisen in the last ten years; I have strong hopes that the theory will be improved and will accomplish a great unification.

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

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