Conference for the 65th birthday of Luísa MASCARENHAS, December 17–19, 2015, Caparica

Developing mathematical tools for real continuum mechanics and physics

Luc TARTAR, University Professor of Mathematics emeritus
Carnegie Mellon University, Pittsburgh, PA, USA

The first week in *mathématiques supérieures* (lycée Charlemagne, Paris, Fall of 1963), the mathematics teacher reviewed questions of logic, and observed that *if P is false, then 'P implies Q' is true, whatever Q is*: how then can one confuse 'A implies B' and 'B implies A', as my physics teachers often did?
If in a theory a proposition is both true and false, then all the propositions in the theory are both true and false: the theory is *contradictory* and should be thrown away. The opposite of contradictory is *consistent*.
I only learned later about a 1905 "paradox" by RUSSEL (1872–1970, 1950 Nobel Prize in Literature), which was resolved by inventing a definition for a *set*, and the "paradox" became a theorem: *the collection of all sets is not itself a set*.

One forgot to tell me that one does not know if set theory is consistent, and about the possibility that a proposition may be *undecidable*, i.e. *neither true nor false*: a theorem of GÖDEL (1906–1978) is that in any consistent theory which contains $\mathbb{N}$ (used for coding) there exists an undecidable proposition.
I was surprised by the lack of precision of thermodynamics, and puzzled when my physics teacher "deduced" from it that the logarithm is a concave function!
At *École Polytechnique* (Paris, 1965–1967), I was taught analysis by Laurent SCHWARTZ (1915–2002, 1950 Fields Medal), "numerical analysis" by Jacques-Louis LIONS (1928–2001), classical mechanics (the 18th century point of view) the first year, and continuum mechanics (the 19th century point of view) the second year, by Jean MANDEL (1907–1982), and the various aspects of physics, by less impressive teachers. I was puzzled by the dogmatic rules of *quantum mechanics*, and the assertions of my teacher in astrophysics about what happens in stars: *some physicists behave like religious fundamentalists*!

Since I had (oral and written) communication problems I abandoned the idea of becoming an engineer, and chose to do research in mathematics, with J.-L. LIONS, since he was supposed to be more "applied", but it was not the case!
I learned from him the technical tools for solving partial differential equations, like the spaces named after Sergei SOBOLEV (1908–1989), but in the early 1970s I had no intuition about the mechanics and physics behind the equations.
The first time I mentioned the continuum mechanics behind the PDE which I discussed was in January 1974, in Lisboa, invited by João-Paulo CARVALHO DIAS and Hugo BEIRÃO DA VEIGA, whom I had met in Paris during my studies.
I talked about the so-called *Navier–Stokes* equation. I did not know that it was written by NAVIER in 1821, using an argument of energy. SAINT-VENANT deduced it in 1843, by an argument of stress, introduced in the mean time by CAUCHY (1789–1857), and STOKES rediscovered his argument in 1845.
The equation is dissipative: does it contradict *conservation of total energy*?

After inventing *internal energy*, the *first principle of thermodynamics* is precisely the *conservation of total energy*: it does not help giving a name to the "energy disappearing from macroscopic level" without explaining how it could "disappear"! How good are postulated dissipative equations?
Thermodynamics is not about dynamics, but about situations near equilibrium: the study of equilibria gives *equations of state*, which may not be true far from equilibrium, so that the *second principle of thermodynamics is often flawed*.
The basic physical laws at microscopic level seem to be conservative: how does irreversibility appear at our macroscopic level, or at a few mesoscopic levels?
EINSTEIN (1879–1955, 1921 Nobel Prize in Physics) is famous for "God does not play dice", showing a poor scientific level: he could not give mathematical or physical arguments for rejecting some equations used in quantum mechanics. Probabilities are used precisely for badly understood phenomena: using probabilities is as bad as saying "God gave the equations".

The equations for diffusion of heat or mass were postulated by FOURIER (1768–1830) and by FICK (1829–1901), and they may be considered natural if one only knows PDE, but MAXWELL (1831–1879) and

HEAVISIDE (1850–1925) already understood more general laws, nonlocal in time. Convolution (in t) appears naturally in Heaviside calculus, which was given sound mathematical foundations by Laurent SCHWARTZ, and one of his important remarks are that derivation is a also convolution (with a distribution) and that *linearity* and *invariance by translation* (plus minor continuity assumptions) imply convolution.
Maybe under the influence of Clifford TRUESDELL (1919–2000), my colleagues Walter NOLL, Dick MACCAMY (1925–2011), Bernard COLEMAN, and Victor MIZEL (1931–2005) studied viscoelastic laws, but as another colleague, Mort GURTIN, pointed out, such laws are not *Galilean invariant* as they should!
One should identify a *good class of nonlinear nonlocal laws*, which are Galilean invariant; moreover, the class should be *closed under homogenization*.

In the Spring of 1975, I met Joe KELLER (1996/97 Wolf Prize) in Madison: he said that he had not come to my talk at NYU (New York University) the month before since the title of my talk looked uninteresting to him. It was a joint work with François MURAT, which we called "control in the coefficients of PDE", wrongly using the term control (as our common advisor was doing); it is a question of optimization, which may be labelled calculus of variations since the unknown is a domain, and the non-existence of an optimal domain results from an *homogenization* effect, which François had first discovered in 1970.
When I told Joe that the result of homogenization in the elliptic case (the G-convergence introduced by Sergio SPAGNOLO in the late 1960s) extends to the "hyperbolic" case, I was puzzled because he said that it is not true.
We were both right, but it took me some time to understand him, since his argument only applies to the periodically modulated case which I knew from the (formal) work of Évariste SANCHEZ-PALENCIA in the early 1970s.

In the general case without any (small) characteristic length which we use, one cannot do the analysis of BLOCH (1905–1983, 1952 Nobel Prize in Physics), adapted to the periodic case: one sends a wave with a wavelength of the order of the period, creating resonance effects. My statement was about a framework $u_n'' + A_n u_n = f$ with $u_n(0) = a, u_n'(0) = b$, but a, b independent of n.
In the late 1970s, I guessed that the spontaneous absorption and emission rules imagined by physicists should just be their way to describe nonlocal effects induced by homogenization, in a different way than what Évariste SANCHEZ-PALENCIA had done, since one has to work in an hyperbolic setting.
In a spectroscopy experiment, one sends a monochromatic wave in a gas: light without matter is described by the Maxwell–Heaviside equation (and not by a scalar wave equation), and matter forces to uses a system of PDE describing both light and matter, and their interaction, so that the system cannot be linear! This 20th century subject is far from being understood.

Interaction of light and matter *cannot be described by a linear system*, hence the rules of quantum mechanics are just a first approximation: *one should force the good students to criticize the dogmas* for helping the progress of science!
Using Fourier transform, a general wave can be decomposed into monochromatic waves, but measuring the (scattering) effects occurring for monochromatic waves does not help for nonlinear effects: if monochromatic waves of measured wavelengths are absorbed by a first gas, and one uses a mixture of gases (including the first), some slightly different wavelengths should be absorbed, not necessarily with an interpretation of Doppler effect!

In courses in quantum mechanics one studies an equation by SCHRÖDINGER (1887–1961, 1933 Nobel Prize in Physics) with a potential V independent of t. The example of a particle in a box has a discontinuous V, implying Dirac masses in the electric field E, which should be locally square integrable (since the density of electromagnetic energy is quadratic in E and H).

With V chosen smooth, it should evolve according to the Maxwell–Heaviside equation, so why is it chosen independent of t? There is a time scale for the evolution of V, but if V creates a barrier of potential for an electron (for example), the time scale for the moving around of the electron in the "box" is much smaller, hence it is reasonable to freeze the evolution of V.

DIRAC (1902–1984, 1933 Nobel Prize in Physics) had written earlier an equation for a "relativistic electron". Letting c tend to ∞ in Dirac's equation makes the Schrödinger equation appear, so that it is a model with $c = \infty$.
Inside an atom, electromagnetic (and other) effects move at the speed of light c, so that the Schrödinger equation should not be used for such questions, or generally for realistic interaction of light and matter.
FEYNMAN (1918–1988, 1965 Nobel Prize in Physics) invented a method with diagrams for solving a Schrödinger equation, and nevertheless obtained good predictions for interaction of particles by selecting particular diagrams. Why?

Probably the sum of all diagrams is divergent, and different ways of summing might actually give the solutions of other equations, possibly better.
A probably "classical" exercice: if $a_n \in \mathbb{R}$ satisfies $a_n \to 0$, $\sum_n (a_n)_+ = +\infty$, and $\sum_n (a_n)_- = +\infty$, then for every $b \in \mathbb{R}$ there exists a bijection f from $\mathbb{N}$ onto itself such that $\sum_n a_{f(n)} = b$: in other words on may rearrange the terms of the sequence to obtain a converging sum, but the sum can be any number and showing a reordering giving a measured value proves absolutely nothing!
In his 1924 thesis, L. DE BROGLIE (1892–1987, 1929 Nobel Prize in Physics) proposed that "particles" are waves, and have a wavelength. One should then work with hyperbolic systems of PDE like Dirac's equation, but not Schrödinger's equation (corresponding to $c = +\infty$). However, Dirac's equation has a defect, since DIRAC added a "mass term". I consider that one should not put it, but leave a possibly different term appear by homogenization, and it would correspond to the electromagnetic energy stored inside the waves.

In an electromagnetic field, a "particle" of electric charge q follows a force $q\,(E + v \times B)$ (where v is its velocity), which one calls after H.H. LORENTZ (1853–1928, 1902 Nobel Prize in Physics), although he only rediscovered the formula 30 years after MAXWELL had written it. POINCARÉ (1854–1912) observed that the Lorentz force is an action which should come with a reaction, i.e. there is *conservation of linear momentum*, so that if a particle receives linear momentum from the electromagnetic field, some waves must take away an opposite amount of linear momentum, and he concluded in 1900 that *the density of electromagnetic energy is equivalent to a density of mass*, with a formula which we now write $e = m\,c^2$, in a way EINSTEIN wrote it 5 years after, for a different reason (and he seems to have quoted POINCARÉ on this occasion), but DE PRETTO (1857–1921) had also arrived at the formula in 1903.
In 1983, when I read about the way DIRAC had written his equation and added a mass term, I thought that the mass term should not be included.

In analogy with what Doina CIORANESCU and François MURAT did in an elliptic setting (calling "strange term" something related to concentrations of "energy"), I expect that a similar effect may exist for the (semi-linear) hyperbolic system of Dirac equation (without mass term) coupled with the Maxwell–Heaviside equation: DIRAC had expressed the density of charge ρ and the density of current j as sesqui-linear quantities in $\psi \in \mathbb{C}^4$, which describes matter, and the coupling makes h appear, which PLANCK (1858–1947, 1918 Nobel Prize in Physics) had introduced for his *light quanta*, now called *photons*.
One difficulty concerns the existence of solutions for such a system, generalizing ideas which I heard in the early 1980s in a talk of Yvonne BRUHAT, and I hope to extend my ideas about *compensated integrability* for that.
With a good grasp on existence of solutions permitting to prepare sequences converging weakly, the next step would be to prove (or disprove) some assertions of physicists concerning "elementary particles" and their interactions.

Another part of my program, of a more algebraic/geometric nature, is to exhibit classes of explicit solutions along the line of the guesses of BOSTICK (1916–1991) concerning electrons or photons. Such a question about the *shape of elementary particles* could help understand what kind of concentration effects to expect.
An important property of the coupled system Dirac/Maxwell–Heaviside is to be *conformally invariant*, so that it may provide interesting effects at plenty of scales, and it is then of utmost importance to develop mathematical tools for analyzing solutions of systems of PDE at many scales.
There are interesting conjectures about the size of boundary layers in hydrodynamics, like the triple-deck of STEWARTSON (1925–1983), and in electromagnetism, more precisely for GTD, the geometric theory of

diffraction developed by Joe Keller in the 1950s, which I have in mind, and it would be a good test for new tools to be able to cast some light on these questions.
It is not clear yet if my *multi-scales H-measures* are adapted to this study.

There are many questions of continuum mechanics or physics for which I do not guess what kind of new mathematical tools must be developed, but in the late 1970s, I was thinking about the 1975 remark of Joe Keller and I guessed that the rules of absorption and spontaneous emission (used in spectroscopy) mean that homogenization makes "memory effects" appear, and that I had to look at something different from what Évariste Sanchez-Palencia had done in the early 1970s, since I had to work in an hyperbolic framework.
However, I now find the experimental data puzzling. In 1862, for a gas of hydrogen, Ångström (1814–1874) observed 4 lines in the visible spectrum: with unit an Ångström (a tenth of a nanometer) the wavelengths are 6563, 4861, 4340, 4102. Since a mole of hydrogen (about 2 grams) occupies 22.4 liters and the number of molecules, named after Avogadro (1776–1856), is about 6.023 10^{23}, the characteristic distance between molecules is about 30 Ångströms, much smaller than the wavelengths creating resonance effects.

Moreover, a molecule of hydrogen is H^2 and each H atom contains a core of one proton with an electron "around". Why do physicists say that the electron is responsible for the observed absorption and emission of light in the gas?
There are various ways to fit a curve through 4 points, but in 1885 Balmer (1825–1898) proposed $\frac{1}{\lambda} = R_H\big(\frac{1}{4} - \frac{1}{n^2}\big)$ for $n = 3, 4, 5, 6$, and R_H is now called the Rydberg constant (10967760 m^{-1}). A generalization of Balmer's formula, the Rydberg–Ritz formula, was proposed in 1888 by Rydberg (1854–1919) and Ritz (1878–1909), as $\frac{1}{\lambda} = R_H\big(\frac{1}{m^2} - \frac{1}{n^2}\big)$ for $m < n$.
The case $m = 1$ gives rays in the UV (ultra violet), observed between 1906 and 1914 by Lyman (1874–1954). The cases $m \geq 3$ give rays in the IR (infra red): the case $m = 3$ was observed in 1908 by Paschen (1865–1947), the case $m = 4$ was observed in 1922 by Brackett (1896–1988), the case $m = 5$ was observed in 1924 by Pfund (1879–1949), and the case $m = 6$ was observed in 1953 by Humphreys (1898–1986).

Since a wavelength λ corresponds to a frequency $\nu = \frac{2\pi\,c}{\lambda}$, it was said that photons of energy $2\pi\,h\,c\,R_H\big(\frac{1}{m^2} - \frac{1}{n^2}\big)$ are observed, corresponding to jumps between levels of energy $\frac{2\pi\,h\,c\,R_H}{n^2}$ for $n \geq 1$. After a computation of the spectrum of an operator gave eigenvalues proportional to $\frac{1}{n^2}$, a rule of quantum mechanics was invented. What else than a spectrum gives a list of numbers?
In 1896, Zeeman (1865–1943, 1902 Nobel Prize in Physics) found that a magnetic field splits spectral lines into several components; if it is strong enough (or for non-zero spin) the Zeeman effect is called the Paschen–Back effect, after Back (1881–1959) who studied the effect in his 1913 PhD thesis.
In 1913, Stark (1874–1957, 1919 Nobel Prize in Physics) found that an electric field splits spectral lines into several components, and the "Stark" effect was also found independently by Lo Surdo (1880–1949), so that in Italy one sometimes call it the Stark–Lo Surdo effect.

The splitting of the "lines" is attributed to the spin, an intrinsic angular momentum for "particles", but I am not sure what it means.
After technical improvements, a density of absorption was observed, looking like a Lorentzian (rescaling of $\frac{1}{1+x^2}$), as for the restriction to $\mathbb{R}$ of the real value of a meromorphic function with single poles near $\mathbb{R}$.
In 1983, I heard Jean Leray (1906–1998, 1979 Wolf Prize) say that physicists never mention the spectrum corresponding to a gas of helium He, suggesting that the measured values do not follow the rules of quantum mechanics!
Since physicists confused 'A implies B' with 'B implies A', nature has no reason to follow the strange rules of quantum mechanics: worse, physicists transformed them into dogma, so they look like religious fundamentalists.
Since 1924 (L. de Broglie' thesis), one knows that *"particles" are waves.* The 19th century point of view is that waves require PDE, of hyperbolic nature: mixing ODE (the 18th century point of view) with PDE is a backward move.

One must go *beyond PDE*: hyperbolic PDE with small scales produce a larger (not yet characterized) class of equations by homogenization.
Since the homogenization of first order (hyperbolic) equations $\frac{\partial}{\partial t} + \sum_j a_j^n \frac{\partial}{\partial x_j}$ (with $div(a^n)$ nice) would probably explain turbulence ("the most important unsolved problem of classical physics", FEYNMAN said), I looked at the simpler case $\frac{\partial}{\partial t} + a_n$, with a_n independent of t: it is far from the physical problem which I wanted to tackle, but it gave some interesting results.
Since the limit produces a memory effect, it is a simple example where a (weak) limit of semigroups is not a semigroup: I expected that the weak limit u_∞ of the solutions of $\frac{du_n}{dt} + a_n u_n = f(\cdot, t)$, with $u_n(0) = v$ would satisfy $\frac{du_\infty}{dt} + a_\infty u_\infty - \int_0^t K(t-s, \cdot) u_\infty\, ds = f$, with $u_\infty(0) = v$; since $f \geq 0, v \geq 0$ implies $u_n \geq 0$, hence $u_\infty \geq 0$, I hope that the (sufficient) condition $K \geq 0$ would hold.
Since Laplace transform gives $(p + a_n)\, \mathcal{L} u_n = \mathcal{L} f + v$ and $(p + a_\infty - \mathcal{L}K)\, \mathcal{L} u_\infty = \mathcal{L} f + v$, $\mathcal{L}K$ would be given explicitly using the Young measure ν of a_n.

I had heard in a 1977 talk by a physicist (Daniel BESSIS) about a theorem of S. BERNSTEIN (1880–1968) characterizing the Laplace transform of a non-negative measure: ψ should be C^∞ on $(0, \infty)$ and satisfy $(-1)^m \frac{d^m \psi}{dp^m} \geq 0$ on $(0, \infty)$ for all integers $m \geq 0$. I found a proof that $K \geq 0$ using properties of Bernstein functions, which I checked with my former colleague Yves MEYER, who showed me a simpler argument using convolution, and it was the one I mentioned to Luísa when I asked her to generalize my argument to the case where a_n depends upon x and t : she used an argument of semi-discretization in t for solving the question, and it was only in the late 1980s that I found an argument (using power expansions) needing less regularity in t, which I wrote for the 60th birthday of my former colleague Bernard COLEMAN.
Explicit solutions involve $e^{s\, a_n}$, so that I used 1-point statistics for a_n, called parametrized measures by GHOUILA-HOURI (1939–1966), but better called *Young measures* now, after Laurence YOUNG (1905–2000).

In the early 1970s, I worked at a direct approach (without using control theory) to Riccati equation, which has order preserving property, and I read about a theorem of LÖWNER (1893–1968): he looked at the continuous functions ϕ on $[a, b]$ which are monotone for symmetric operators, i.e. $a\, I \leq P \leq Q \leq b\, I$ implies $\phi(P) \leq \phi(Q)$ for all such symmetric operators on a Hilbert space H; the characterization depends upon the dimension of H, but for infinite dimension it is a class named after his advisor, PICK (1859–1942), those functions which extends to the upper half complex space $\Im(z) > 0$ into a holomorphic function satisfying $\Im\big(\psi(z)\big) > 0$, of which a representation formula was mentioned.
After the method with Bernstein functions or convolution, I thought of using the characterization of Pick functions, also named after NEVANLINNA (1895–1980) or STIELTJES (1856–1894), and it shows that K is actually a Bernstein function. I showed the approach to Luísa, who used it afterwards; I asked Ciprian FOIAS for a reference, but the one he gave me was not so early.

The characterization of Pick functions also played a role in questions of "homogenization" seen by physicists, David BERGMAN and Graeme MILTON, mixed with approximants named after PADÉ (1863–1953). Pick functions are adapted to the isotropic case, but effective tensors are not always scalar for real questions of homogenization, so that one needs to find an adapted class of functions of matrices, unknown as far as I know, but I do not read much.
For a conference at IMA in Minneapolis in 1984, I described a few remarks on homogenization, and one was an application with François MURAT to a slightly academic problem, $-a_n(y)\, \frac{\partial^2 u_n}{\partial x^2} + b_n(y)\, u_n = f(x, y)$ in $\mathbb{R} \times (y_-, y_+)$: one starts by applying a partial Fourier transform $\mathcal{F}_x$, one uses the Young measure of (a_n, b_n) for taking the limit, one applies the same identity for a characterization of a Pick function, and one finally invert explicitly $\mathcal{F}_x$, and an additional term appears, corresponding to a nonlocal effect in x (a convolution by an even function, giving the same weight to right or left in x).

Since Ennio DE GIORGI (1928–1996, 1990 Wolf Prize) had once mentioned to me a particular case of an equation $\frac{\partial u_n}{\partial t} + a_n(y)\, \frac{\partial u_n}{\partial x} = f(x, y, t)$ with $u_n(x, y, 0) = v(x, y)$, I wrote an article about memory effects for his 60th birthday, and I included this equation, attacked in the same way: one starts by applying a Laplace transform $\mathcal{L}$ in t and a partial Fourier transform $\mathcal{F}_x$, one uses the Young measure of a_n for taking the limit, one applies the same identity for a characterization of a Pick function but now outside the real axis (for p

purely imaginary), and one finally invert explicitly $\mathcal{L}$ and $\mathcal{F}_x$, and an additional term appears, corresponding to a nonlocal effect in x and t.
However, before my article was finished I had received an article by my former student Kamel HAMDACHE, with his usual collaborators at the time, Youcef AMIRAT and Hamid ZIANI (1949–2004), where they had made exactly these computations, so that they have priority. They also studied the new type of equation obtained directly for showing the finite propagation speed property.

One has $\frac{\partial u_\infty}{\partial t} + a_\infty(y)\,\frac{\partial u_\infty}{\partial x} - \int_0^t \frac{\partial^2 u_\infty}{\partial x^2}(x-\lambda\,(t-s),y,s)\,d\mu_y(\lambda)\,ds = f(x,y,t)$ with $u_\infty(x,y,0) = v(x,y)$, and $d\mu_y$ is a non-negative measure which is a nonlinear transform of the Young measure of a_n.
In a subsequent article, they wrote the equation as a system from kinetic theory, $\frac{\partial u(x,y,t)}{\partial t} + a_\infty(y)\,\frac{\partial u(x,y,t)}{\partial x} = \frac{\partial}{\partial x}\big(\int F(x,y,t;\lambda)\,d\mu_y(\lambda)\big)$, with $u_\infty(x,y,0) = v(x,y)$, and $\frac{\partial F(x,y,t;\lambda)}{\partial t} + \lambda\,\frac{\partial F(x,y,t;\lambda)}{\partial x} = \frac{\partial u(x,y,t)}{\partial x}$, with $F(x,y,0;\lambda) = 0$, or explicitly $F(x,y,t;\lambda) = \int_0^t \frac{\partial u}{\partial x}(x-\lambda\,(t-s),y,s)\,ds$.
If the sequence a_n takes m different values, the Young measures $d\nu$ are combinations of m Dirac masses, but the measures $d\mu$ are combinations of $m-1$ Dirac masses, at intermediate values (roots of a polynomial of degree $m-1$).
It is important to notice that one does not postulate a model from kinetic theory, but one deduces it in a mathematical way for describing the effective equation to use (as a consequence of homogenization).

A next step would be to consider more general oscillating sequences (with divergence free coefficients), but the Young measures will probably not be sufficient for describing the limiting effective equation, as for the homogenization of elliptic equations once one generalizes the one-dimensional situations.

It is important to notice that Navier–Stokes equation has been postulated, and if some people consider it natural it is because they cannot think in a new way.
18th century mechanics requires ODE, 19th century mechanics requires PDE, but 20th century mechanics/physics, like atomic physics, meteorology, phase transitions, plasticity, turbulence, requires going beyond PDE, because of the existence of small scales, which should not be averaged in a naive way.
For questions of small amplitude (elliptic) homogenization, I introduced a new tool in the late 1980s, which I called H-measures, and since (elliptic) homogenization is a “nonlinear microlocal theory”, and H-measures are “quadratic microlocal objects”, it is natural that they help.

However, there is no understanding yet of how to continue the expansion.
One should probably not go to cubic terms, which are difficult to define outside the periodic case (but nature with only one scale is some kind of a joke!).
If one avoids Taylor expansions, should one mimic Padé approximants?

For the nonlocal effects in t studied by Luísa, I wrote an approach by power expansion in my article for the 60th birthday of Bernard COLEMAN, but this approach fails for the hyperbolic situations studied by Youcef AMIRAT, Kamel HAMDACHE, and Hamid ZIANI, since all the terms are supported by characteristic lines moving at velocity a_∞ while the effective equation does not, except if there are no oscillations (the Young measure is a Dirac mass, and $\mu = 0$): the power series does not converge in the sense of distributions; it might converge in the sense of analytic functionals, for which there is no notion of support.
For a nonlinear ODE case, the power expansion reminds of Feynman diagrams: a difficulty of bookkeeping for all the terms, and of avoiding divergent sums.

If one starts from a problem which has a solution, and one approaches the solution by a power series which diverges, one may find a different approach for computing the solution: one has seen plenty of examples of this kind since the work of CAUCHY on holomorphic functions and extension by analyticity.
Physicists often are in a different situation, working on an equation which does not describe well the phenomenon which they are interested in (like using a linear framework for studying an interaction, or using models corresponding to $c = +\infty$), so that they should not be so interested in its solution.
Nevertheless, they may compute the solution and end up with a divergent series, and it then becomes sheer logical nonsense that they are happy if they find a way to “sum the divergent series” which gives numbers looking like the measurement: worse than just confusing ‘A implies B’ with ‘B implies A’, they do not build

an intuition for rejecting wrong equations! Are students aware of the mistakes? Must they stay silent to not damage their career?

Galilean invariance is basic in mechanics, but it must take a different form (to be discovered) in situations where the velocity field oscillates, which is the basic hypothesis for turbulent flows: too many "specialists" are still deluded enough to play probabilistic games, without observing that they are not compatible with the equations of hydrodynamics, but the equations one uses now for hydrodynamics are not that good, and this question should be discussed.
In the early 1970s, Jacques-Louis LIONS taught courses on singular perturbations and boundary layers, and I heard specialists of continuum mechanics say that his ideas concerning fluids were not so good, since he expected that the equation named after EULER (1707–1783) would appear at the limit of "Navier–Stokes" equation for vanishing viscosity, but one needed to use ideas by OSEEN (1879–1944), which I never looked at. For boundary layers, one only mentioned the ideas of PRANDTL (1875–1953), and Olga OLEINIK (1925–2001) had studied the existence of the solutions of Prandtl's equation.

It was only in 1982 in Madison that Richard MEYER (1919–2008) mentioned to me Stewartson's triple deck, and Jean-Pierre GUIRAUD gave me afterward some intuition about it. No one has assessed the relation of these models to "Navier–Stokes" equation, but my impression was that the specialists of boundary layers dropped some terms in "Navier–Stokes" equation, not so much because they are negligible but because the equation is not so good near the (solid) boundaries!
At a meeting in Saint-Étienne in the beginning of 1986, after hearing a talk about deriving Euler equation from "Navier–Stokes" equation under strong hypotheses (a technical way to prove that if there are no problems, then everything is OK), Sergeï GODOUNOV said that he did not understand why one was doing all that, since the good fluid models use Maxwell laws.
It is more easy to write articles on subjects which one already knows, for example for having already published on it, but it seems that some equations are not so good models, although no one talks about that.

Jacques-Louis LIONS once said that semigroup theory cannot be deep since it applies to too many equations. One can discuss *conservation of energy* in some cases, related to invariance with respect to translations in t, but hiding the space variable x inside the definition of a functional space does not help understanding *conservation of linear momentum*, related to invariance with respect to translations in x, or *conservation of angular momentum*, related to invariance with respect to rotations in x.
Once energy is conserved, one may look at its density and wonder where it is located, and since weak convergence was a classical tool it was easy to see that energy is not conserved by taking weak limits, and it gives a way to "hide energy" at mesoscopic levels (since physicists and material scientists prefer to keep the qualifier microscopic for the level of atoms), but the wave equation, the Maxwell–Heaviside equation, the linearized elasticity equation share an important (physical) property of *equipartition of hidden energy*.

This follows from simple applications of the *compensated compactness theory*, developed with François MURAT in 1976, as a generalization of our Div-Curl lemma of 1974, sufficient for the wave equation.
In order to go a step further, and understand what happens to the "hidden energy", one uses H-measures, which I developed first (in the late 1980s) for proving theorems of small amplitude homogenization, which was my way of interpreting guesses by physicists. I then checked that they could follow the propagation of oscillations and concentration effects, which is possible because these objects are (quadratic) microlocal objects.
One should observe that Lars HÖRMANDER (1931–2012, 1962 Fields Medal, 1988 Wolf Prize) did something quite different: he defined microlocal regularity (which does not seem connected to physics at all), and he proved results of propagation for it; his followers do a lot of propaganda by calling these results "propagation of singularities", which they are not.

H-measures use no characteristic length, since none was necessary for the small amplitude homogenization results which were my initial motivation.
Patrick GÉRARD developed similar objects (which he called microlocal defect measures) for a question of compactness by averaging (which I had failed to prove using H-measures), but he wrote that only the

support is important (in the spirit of Lars HÖRMANDER's wave front set): it is totally unfair to attribute him my results which are not qualitative but quantitative, like getting numerical values in homogenization, or transport equations for quantities on light beams.

My idea for using a characteristic length was to introduce an extra variable x_{N+1}, multiply by a function of $\frac{x_{N+1}}{\varepsilon_n}$ and use H-measures in $N+1$ dimensions, while Patrick GÉRARD did something a little different, which he called semi-classical measures. His definition has a problem at ∞, and both our definitions have a problem at 0, which I corrected by introducing a new variant, using a compactification of $\mathbb{R}^N \setminus \{0\}$ with a sphere at 0 and a sphere at ∞.

My new definition also served in correcting an initial mistake of Pierre-Louis LIONS (1994 Fields Medal) and Thierry PAUL, who had written the silly statement that one can recover H-measures from semiclassical measures, and they introduced the same object than Patrick GÉRARD but called it after WIGNER (1902–1995, 1963 Nobel Prize in Physics) because they used his transform.

With Patrick GÉRARD, we found a quicker way to interpret their approach, by considering 2-point correlation functions (which need L^2 bounds, but are not defined without a characteristic length) and using a characterization of the Fourier transform of non-negative measures by BOCHNER (1899–1982) (taught by Laurent SCHWARTZ in his course at École Polytechnique).

We also observed that one can define an object using 3-point correlation functions (which need L^3 bounds) and obtain transport equation for a diffusion equation with drift (and small viscosity), but without an analogue of Bochner's theorem it is not clear what the correct way to look at this result is.

However, such objects are not adapted to the wave equation, since it is not well posed in spaces like $W^{1,p}$ for $p \neq 2$. One possibility is that although one cannot define all cubic quantities (in u_x, u_t) some special cubic quantities make sense by effects of *compensated integrability* or *compensated regularity*, and I hope that it could help in proving better existence theorems for the coupled system of Dirac's equation (without mass term) and the Maxwell–Heaviside equation, and I have stated a conjecture using a remark which Raoul BOTT (1923–2005, 2000 Wolf Prize) told me in the late 1980s.

Finally, I have introduced multi-scales H-measures, but it is too early to say if they are the right kind of object needed for boundary layers and creeping rays in Joe KELLER's GTD, which seems a much better reason for light rays to be curved near the surface of the sun than the silly theory by EINSTEIN, who did not seem to realize that light is electromagnetism, and that there are intense electromagnetic effects near the surface of the sun!