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*Multiscale Problems in Nonlinear Analysis*

**Weak convergence models for relating various scales**

When I was taught Physics in the late 60s, the method for describing the relations between different scales, from microscopic to macroscopic (and I only heard much later of the term mesoscopic), was explained in a traditional way, using a probabilistic language. It is still taught that way nowadays.

In the early 70s, I had initiated a different approach, where instead of probabilistic methods one uses weak convergences or more generally convergences of weak type as those appearing in G-convergence which Sergio SPAGNOLO[1] had developed in the late 60s, helped with the insight of Ennio DE GIORGI[2], and H-convergence, which I had developped with François MURAT[3].

When a new idea appears, there are often people who say afterwards that they had known that already, and that may be true, but it becomes doubtful when after many years they show that they have not yet understood the concept correctly. This phenomenon is not new, and when GAUSS[4] read about the work of BOLYAI[5] and the work of LOBACHEVSKY[6] on non Euclidean[7] geometries, he said that he had known that for a long time, but in his case there is no doubt that he had made similar observations but not published them, and he had even mentioned that his reputation would suffer if he admitted in public that he believed in the existence of such a geometry. I had expressed in writing[8] in 1974 the new idea of using weak convergence for relating different scales, which I called microscopic and macroscopic. Could it be that others had thought of the same idea before, but had been afraid to mention it, as it was (and still is) against the well accepted

[1] Sergio SPAGNOLO, Italian mathematician. He works at Universitá di Pisa, Pisa, Italy.

SPAGNOLO S., "Sul limite delle soluzioni di problemi di Cauchy relativi all'equazione del calore," Ann. Scuola Norm. Sup. Pisa (3) **21** (1967), 657–699. "Sulla convergenza di soluzioni di equazioni paraboliche ed ellitiche," Ann. Scuola Norm. Sup. Pisa (3) **22** (1968), 571–597.

[2] Ennio DE GIORGI, Italian mathematician, 1928-1996. He received the WOLF prize in 1990. He worked at Scuola Normale Superiore, Pisa, Italy.

Ricardo WOLF, German-born diplomat and philanthropist, 1887-1981. The WOLF foundation was established in 1976 with his wife, Francisca SUBIRANA-WOLF, 1900-1981, "to promote science and art for the benefit of mankind".

[3] François MURAT, French mathematician, born in 1947. He works at CNRS (Centre National de la Recherche Scientifique) and Université Paris VI (Pierre et Marie CURIE), in Paris, France.

MURAT F., "H-convergence," *Séminaire d'analyse fonctionnelle et numérique*, Université d'Alger, 1977-78. Translated into English as MURAT F. & TARTAR L., "H-convergence," *Topics in the mathematical modelling of composite materials*, 21–43, Progr. Nonlinear Differential Equations Appl., 31, *Birkhäuser Boston, Boston, MA*, 1997.

Pierre CURIE, French physicist, 1859-1906, and his wife Marie SLODOWSKA CURIE, Polish-born physicist, 1867-1934, jointly received the NOBEL prize in Physics in 1903, and she also received the NOBEL prize in Chemistry in 1911.

Alfred NOBEL, Swedish industrialist and philanthropist, 1833-1896. He created a fund to be used as awards "for people whose work most benefited humanity".

[4] Johann Carl Friedrich GAUSS, German mathematician, 1777-1855. He worked in Göttingen, Germany.

[5] János BOLYAI, Hungarian mathematician, 1802-1860.

[6] Nikolai Ivanovich LOBACHEVSKY, Russian mathematician, 1792-1856.

[7] EUCLID of Alexandria, mathematician, 325 BCE-265 BCE (BCE = Before Common Era).

[8] TARTAR L., "Problèmes de contrôle des coefficients dans des équations aux dérivées partielles," *Control theory, numerical methods and computer systems modelling (Internat. Sympos., IRIA LABORIA, Rocquencourt*, 1974), *pp.* 420–426. *Lecture Notes in Econom. and Math. Systems, Vol.* 107, *Springer, Berlin*, 1975. Translated into English as MURAT F. & TARTAR L., "On the control of coefficients in partial differential equations," *Topics in the mathematical modelling of composite materials*, 1–8, Prog. Nonlinear Differential Equations Appl., 31, *Birkhäuser Boston, MA*, 1997.

approach which uses a probabilistic language? Possibly, but I have received no evidence of that yet, and it may clarify the discussion to explain who had contributed to this new point of view in the early 70s, and what the point of view is.

When I had worked with François MURAT on an academic problem of optimization that had been proposed by Jacques-Louis LIONS[9], we used weak convergence in the usual way that we had learned from him in Functional Analysis; there were sequences converging in $L^2(\Omega)$ weak[10], and sequences converging in $L^\infty(\Omega)$ weak $\star$, and there was also a different topology involved; then we found the work that Sergio SPAGNOLO had done on G-convergence, with some ideas from Ennio DE GIORGI, and what we had done was slightly different (so the term H-convergence was coined later), but at that point there were only classical ideas in Functional Analysis, as G-convergence is just the weak convergence of the inverses of some operators anyway, i.e. the convergence of GREEN[11] kernels (and G is a reminder for GREEN), the difficult part of the work being to prove that the limit of GREEN functions of a sequence of elliptic problems is the GREEN function of another elliptic problem. Then we found the work of Henri SANCHEZ-PALENCIA[12], who was doing partially formal computations in a periodic setting, and that helped us realize that what we had been doing was related to properties of mixtures.

That connection with questions in Continuum Mechanics was illuminating for me, and it gave me a way to understand what I had been told in my Physics courses about microscopic/macroscopic relations. Then we proved the Div-Curl lemma, which appeared important for the question of whether or not one always needs quantities like internal energy, which only exist at the macroscopic level. I also observed that the Div-Curl lemma explains why there is an equipartition of energy between the kinetic part and the potential part of the energy for oscillating solutions of the wave equation, and that gave an interpretation radically different from anything that I had been taught in my Physics courses, where the principle is related to counting degrees of freedom; the application of the same idea to MAXWELL[13] equation gives more than just the equipartition of energy between the electric part and the magnetic part of the energy, and the application to nonlinear equations suggests to question the way that physicists have used for discussing equipartition.

During the year 1974/75 which I spent at the University of Wisconsin in Madison, I had clarifying discussions with Joel ROBBIN[14] and it became clear why the classical weak convergence is only adapted to coefficient of differential forms, and why one needs other types of weak convergence.

I had derived a new proof for H-convergence based on repeated use of the Div-Curl lemma, and as I had mentioned my proof to Jacques-Louis LIONS at a meeting in Marseille, France, in the Fall 1975, his article is

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[9] Jacques-Louis LIONS, French mathematician, 1928-2001. He received the Japan prize in 1991. He held a chair (Analyse mathématique des systèmes et de leur contrôle, 1973-1998) at Collège de France, Paris, France. I first had him as a teacher at École Polytechnique in 1966/67, when École Polytechnique was still in Paris, and I did research under his direction, until my thesis in 1971.

*Contrôle optimal de systèmes gouvernés par des équations aux dérivées partielles*, Dunod - Gauthier-Villars, Paris, 1968 xiii+426 pp. *Optimal control of systems governed by partial differential equations* Die Grundlehren der mathematischen Wissenschaften, Band 170 Springer-Verlag, New York-Berlin 1971 xi+396 pp.

[10] The notion of a weakly converging sequence is due F. RIESZ, but I do not know if he had introduced the precise weak topology (which is not metrizable).

Frigyes (Frederic) RIESZ, Hungarian mathematician, 1880-1956. He worked in Budapest, Hungary.

[11] George GREEN, English mathematician, 1793-1841. He was a miller and never held any academic position.

[12] Enrique Evariste SANCHEZ-PALENCIA, Spanish-born mathematician, born in 1941. He works at CNRS (Centre National de la Recherche Scientifique) and Université Paris VI (Pierre et Marie CURIE), Paris, France.

SANCHEZ-PALENCIA Enrique, "Solutions périodiques par rapport aux variables d'espace et applications," C. R. Acad. Sci. Paris Sér. A-B **271** (1970) A1129–A1132.

[13] James CLERK MAXWELL, Scottish physicist, 1831-1879. He held the first CAVENDISH Professorship of Physics (named after Henry CAVENDISH, English physicist, 1731-1810) at Cambridge, England UK.

[14] Joel W. ROBBIN, American mathematician, born in 1941. He works at University of Wisconsin, Madison WI.

the first mention for it[15]; I only wrote about it in the Fall of 1976[16], and it was rediscovered independently by Leon SIMON[17]. My method, which Jacques-Louis LIONS subsequently refered to as the *energy method*, which others have called the *duality method*, and for which I prefer the *method of oscillating test functions*, follows precisely what one does in a physical problem for identifying effective[18] coefficients, and it suggests that the topology adapted to a physical quantity is found by understanding how one measures it, or more precisely how one identifies what it is from the measurement of other quantities; differential forms are defined by their integrals on manifolds, and therefore the topology adapted to them is the weak topology which looks at the limits of these integrals, but other physical quantities have different interpretations and other topologies are adapted for them.

A new point of view had been created, and since that time I have had no hint that this basic idea should be changed, for understanding in a better way some questions relating different scales in Continuum Mechanics or Physics. I am not saying that I could see in 1975 how some particular questions of contemporary Continuum Mechanics or Physics could fit into my program of research, as I understood some of these questions only in the late 80s. I am not saying either that I see clearly now how all questions of contemporary Continuum Mechanics or Physics can be explained in that point of view, as there are some new mathematical objects that must be defined, but it seems that everything will eventually fit into the framework that I have advocated, which does not postulate any probabilistic rules for the way Nature evolves.

When I met John M. BALL[19] in the Fall of 1975, I was puzzled that he had been taught that elastic materials minimize their potential energy, but it was obvious to me from my new understanding of the relations between scales that the strain-stress relation of an elastic material should be stable by weak convergence, and I will only correct that statement now by saying that I was thinking in terms of what I have described as science-fiction elastic materials, which only have an elastic range and do not show any onset of plasticity or apparition of cracks, whatever forces are applied to it (and I have used the term science-fiction because I think now that no such material will ever exist). When in 1977, Clifford TRUESDELL[20] had questioned my framework[21], I had thought that it was due to a difference in generation that he did not see why I was proposing that, and by politeness I had not argued with him on that point. Younger mathematicians have seemed to have trouble with that idea too (and a few have obviously not been educated in a way where one respects

[15] LIONS J.-L., "Asymptotic behaviour of solutions of variational inequalities with highly oscillating coefficients," *Applications of methods of functional analysis to problems in mechanics (Joint Sympos., IUTAM/IMU, Marseille, 1975)*, pp. 30–55. Lecture Notes in Math., 503. *Springer, Berlin*, 1976.

[16] TARTAR L., "Quelques remarques sur l'homogénéisation," *Functional analysis and numerical analysis* (*Tokyo and Kyoto,* 1976), 469–481, *Japan Soc. Promotion of Sci., Tokyo,* 1978.

[17] Leon M. SIMON, Australian-born mathematician, born in 1945. He works at STANFORD University, Stanford CA.

He was told about my previous work by the referee of his article, "On $G$-convergence of elliptic operators," Indiana Univ. Math. J. **28** (1979), no. 4, 587–594.

Leland STANFORD, American businessman, 1824-1893.

[18] For a while I have talked about homogenized equations, but after having learned from George PAPANICOLAOU that physicists use the term effective coefficients, I started using the qualificative effective instead of homogenized.

George C. PAPANICOLAOU, Greek-born mathematician, born in 1943. He works at STANFORD University, Stanford CA.

[19] John MacLeod BALL, British mathematician, born in 1948. He holds the Sedleian chair (founded in 1618 by Sir William SEDLEY) at Oxford, England UK.

[20] Clifford Ambrose TRUESDELL, III, American mathematician, 1919-2000. He worked at Johns HOPKINS University, Baltimore MD.

Johns HOPKINS, American financier and philanthropist, 1795-1873.

[21] "Weak convergence in nonlinear partial differential equations," *Existence Theory in Nonlinear Elasticity,* 209–218. *The University of Texas at Austin,* 1977. "Nonlinear constitutive relations and homogenization," *Contemporary developments in continuum mechanics and partial differential equations* (*Proc. Internat. Sympos., Inst. Mat., Univ. Fed. Rio de Janeiro, Rio de Janeiro,* 1977), *pp.* 472–484. *North-Holland Math. Studies,* 30, *North-Holland, Amsterdam-New York,* 1978.

older people), and I should explain now that if this was not the case, then macroscopic measurements could show the same average strains but different average stresses, so that such a material would certainly not be called elastic. I have learned from Ekhard SALJE[22] the terms co-elastic or ferroelastic for describing some nonelastic materials of that type, whose strain-stress relations show hysteresis effects; they can be studied in the program that I was describing in the late 70s, either by neglecting time or by considering the evolution problem, for which the mathematical tools are obviously still inadequate (and one must have to question a modelization using an equilibrium equation of state anyway). In the way some people have applied the approach by Γ-convergence of Ennio DE GIORGI, they have not seen any hysteresis phenomenon, and they have thought of talking about an elastic material, probably because they had implicitely postulated that a material can only exist in the global minimum of its potential energy; this has also happened because they have only considered the weak topology for the strain (while I have advocated utilising both the strain and the stress, and adding the entropies which are valid), and I do not think that it is a defect of Ennio DE GIORGI's Γ-convergence approach, because he had purposely introduced the notion with a general topology, and I believe that it is only the result of having chosen the wrong topology that the real hysteresis effect had vanished; in order to understand which is the right topology (although there could be more than one), I would prefer to follow my approach, but I do not know the answer of that question at the moment.

Peter LAX[23] has noticed that VON NEUMANN[24] had thought that a numerical scheme which presented oscillations is converging to the right quantity only in a weak topology (and I think that Peter LAX showed that it was not the case), but this remark does not show that my approach was well known. Actually, the usual weak topologies have been used implicitly for a long time, even before the notion of weak convergence had been introduced by F. RIESZ and the spaces named after BANACH[25], FRÉCHET[26], HILBERT[27], LEBESGUE[28] or SOBOLEV[29] had been introduced, because when one replaces a discrete repartition of mass by a (measurable) density of mass, one says that they are very near in a weak topology (and on bounded sets such weak topologies are often metrizable).

It is only when something nonlinear is introduced that my point of view starts being relevant, and although we had been taught that nonlinear mappings are not continuous with respect to weak convergence, we found a way to use weak convergences in nonlinear settings, and to consider other types of weak convergence when necessary.

Considering sequences is just a mathematical artifice used for identifying the right topology, and it is just like the usual mathematical idea of completing a space; although real numbers seemed well known to all, they have only been defined correctly at the time of CANTOR[30] and DEDEKIND[31], and in one approach a

[22] Ekhard K. H. SALJE, German-born physicist. He works at University of Cambridge, England UK.

SALJE Ekhard K., *Phase transitions in ferroelastic and co-elastic crystals: an introduction for mineralogists, material scientists and physicists* 296pp. Cambridge University Press, 1993. ISBN: 0521429366.

[23] Peter D. LAX, Hungarian-born mathematician, born in 1925. He received the WOLF prize in 1987. He works at the COURANT Institute of Mathematical Sciences at New York University, New York NY.

Richard COURANT, German-born mathematician, 1888-1972. He worked at New York University, New York NY.

[24] János (John) VON NEUMANN, Hungarian-born mathematician, 1903-1957. He worked at the Institute for Advanced Study, Princeton NJ.

[25] Stefan BANACH, Polish mathematician, 1892-1945. He worked in Lvov, then in Poland, now in Ukraine.

[26] Maurice René FRÉCHET, French mathematician, 1878-1973. He worked in Paris, France.

[27] David HILBERT, German mathematician, 1862-1943. He worked in Göttingen, Germany.

[28] Henri Léon LEBESGUE, French mathematician, 1875-1941. He held a chair (Mathématiques, 1921-1941) at Collège de France, Paris, France.

[29] Sergei L'vovich SOBOLEV, Russian mathematician, 1908-1989. He worked in Akademgorodok, Novosibirsk, Russia.

[30] Georg Ferdinand Ludwig Philipp CANTOR, German mathematician, 1845-1918. He worked in Halle, Germany.

[31] Julius Wihelm Richard DEDEKIND, German mathematician, 1831-1916. He worked in Brunswick, Germany.

real number is just a CAUCHY[32] sequence of rationals numbers (more precisely an equivalence class of such CAUCHY sequences), and that intellectual exercise is useful when one only knows rational numbers and one wants to prove that real numbers exist, which have the properties that we are used to; after that one may safely work with real numbers. Engineers and physicists may think that mathematicians are a little crazy to spend some time proving such things that they find obvious; after RUSSELL[33] had found his famous paradox, mathematicians realized that one even has to define what a set is; from time to time physicists discard some old theories that they do not believe in anymore, and soon they will have to discard some of the silly games that they have invented in the 20$^{\text{th}}$ Century, but mathematicians do that on a continuous basis, being more careful about what they know and what they only conjecture is true; mathematicians are not supposed to mistake the information coming from an experiment with the properties of solutions of an equation even when this equation is supposed to be a model for a physical effect which the experiment is supposed to test.

Part of what I have been doing in my research work in the 70s and 80s was to give precise mathematical definitions of what mixtures are, and of course it depends upon what properties of the mixture one is interested in; then, after having given precise mathematical definitions of how one relates different scales, one can start discussing about some precise questions. Once the definitions are clearly set, many will say that they are not surprised, but some will prefer to continue using the vague probabilistic language that they have used before as an incantation. Good physicists do not need the definitions of mathematicians in order to go forward and if DIRAC[34] has his name attached to a measure (which mathematicians do not call a function), it is not for having been the first to use the notation of functions for a point mass, but for having done strangely efficient formal computations with his "functions", not being afraid to use their derivatives, and such a bold move was only explained to mathematicians by the theory of distributions of Laurent SCHWARTZ[35]; however, it would be wrong to believe that any formal computation made by a physicist or an engineer must be right, even if it seems useful in a practical problem.

In his forthcoming book on composites, Graeme MILTON[36], who is the world expert on questions relative to bounds on effective properties for composite materials, will probably have used his physical intuition instead of precise mathematical definitions, but I suppose that many of the results will easily be understood with minor adaptations in the mathematical framework that I have developed, while some might not fit so easily.

It is useful to observe that some others who have been initially trained as mathematicians often do not follow a mathematical approach, even in an area where precise mathematical definitions have been given, and prefer to choose the nonmathematician mode where one does not prove everything that one writes, but nevertheless do not emphasize which of their results are only conjectures, as their former mathematical training would dictate; one should then use a detective mode in order to decide if one should trust the statements which they write, or check for gaps in their "proofs" or simply look for their use of terms which have never been defined correctly (my observation is that they often also misattribute the ideas of other mathematicians, perhaps because they resent the fact that others have continued working as mathematicians do).

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[32] Augustin Louis CAUCHY, French mathematician, 1789-1857. He was named baron in 1838. He worked in Paris, France.

[33] Bertrand Arthur William, 3$^{\text{rd}}$ earl RUSSELL, Welsh mathematician, 1972-1970. He received the NOBEL Prize in Literature in 1950. He worked in Cambridge, England UK.

[34] Paul Adrien Maurice DIRAC, English physicist, 1902-1984. He received the NOBEL prize in Physics in 1933. He held the Lucasian chair (named after Henry LUCAS, English clergyman, 1610-1663) at Cambridge, England UK.

[35] Laurent SCHWARTZ, French mathematician, born in 1915. He received the FIELDS medal in 1950. He worked at Ecole Polytechnique, Palaiseau, France, and I had him as a teacher in 1965/66 (when Ecole Polytechnique was in Paris).

John Charles FIELDS, Canadian mathematician, 1863-1932.

[36] Graeme Walter MILTON, Australian-born physicist, born in 1956. He works at University of Utah, Salt Lake City UT.

*The Theory of Composites*, Cambridge Monographs on Applied and Computational Mathematics, 6, Cambridge University Press; ISBN: 0521781256.

In the early 70s, after my work with François MURAT, I was quite puzzled when I discovered that a text book by LANDAU[37] and LIFSCHITZ[38] contained a section giving a formula for the conductivity of a mixture. The reason why it was puzzling was that the results that I had obtained with François MURAT (and probably the results that Antonio MARINO[39] and Sergio SPAGNOLO had obtained too) showed that one cannot deduce the effective conductivity of a mixture from the sole knowledge of the proportions of materials used, except in dimension[40] 1 (in other words, the shape of the pieces play a role). Of course, they could have said that their formula was only an approximation (it was easy to guess why they implicitly considered their mixture to be isotropic), they could have told how it compared with experimental measurements, and from a theoretical point of view they could have explained why, if one mixed in equal parts two (isotropic) conductors of conductivity $\alpha$ and $\beta$, their formula was not symmetric in $\alpha$ and $\beta$. These are details which annoy mathematicians, who are taught to be precise and only write statements which are proved to be true, but the real difficulty lies elsewhere.

LANDAU and LIFSCHITZ talked of a scenario where one first grinds two (or more) different materials into fine powders, then one pours in given proportions these powders into a container before shaking it thoroughly (in order to mix the components of the mixture so that no preferred direction can be discovered in the end), and finally one compresses the result (in order to avoid having too much air as a third component). There is obviously not yet a mathematical formulation for expressing in how many different ways this scenario can be performed so that one may try to answer the question about the conductivity of the mixture. If one talks about grinding, a specialized engineer may understand different things, depending if he/she is a specialist of grinding grains or of grinding minerals with various degrees of hardness, but that is an impossible problem for a mathematician at the moment, as one is talking about going beyond the elastic range and one cannot even stop once a crack appears, and one must admit that the models about cracks which mathematicians play with are problematic anyway. Avoiding to even try to understand what these processes are and forcing probabilistic ideas on the problem is just a way to sweep the dirt under the rug, which cannot result in the cleaning process that mathematicians are responsible for, and one should keep in mind that not everything can be transformed into a sound mathematical framework[41].

It is clear that in front of a problem that they do not understand some people like to pretend that it cannot be understood, and many feel better by inventing a probabilistic game, without realizing that they can only postulate that it is related to the initial problem, of which they may understand near to nothing. In 1981, at a meeting at New York University, Joseph KELLER[42] talked about random waves, and he mentioned that at some time there had been many articles written about the distribution of wavelengths of the waves at the surface of the sea, but there were no measurements; a few years later some measurements were made

[37] Lev Davidovich LANDAU, Russian physicist, 1908-1968. He received the NOBEL prize in Physics in 1962. He worked in Moscow, Russia.

[38] Evgenii Mikhailovich LIFSCHITZ, Russian physicist, 1915-1985.

[39] Antonio MARINO, Italian mathematician. He works at Universitá di Pisa, Pisa, Italy.

[40] It is related to the rule used in electricity that resistances in series must be added. For resistances in parallel the conductivities, inverse of the resistivities, must be added, and in more than one dimension one does not know in advance where the electric current will flow, but if one knew one could consider current tubes where resistances are in series inside the tubes, and the tubes are themselves in parallel.

[41] There is a paradox which states that "for every integer $n$ there is a property $P$, which can be expressed in less than one hundred English words, and such that $n$ has the property $P$ but no smaller integer has that property"; if it was not true there would exist a smallest integer $N$ which does not have such a property, but that would be a contradiction, because $N$ would be the first integer not to have a property such that ..., and one can check that this sentence characterizing $N$ as the first integer having a property $Q$ can be written with less than one hundred English words; however, there are only a finite number of words in English and therefore a finite number of sentences which use less than one hundred English words, and therefore a finite number of properties for an infinite number of integers and there must be $m \neq n$ with the same property for $m$ and $n$, which contradicts the statement for the largest of the two integers. What the paradox shows is that there is absolutely no way to transform the fake mathematical jargon used in the first part of the argument into a sound mathematical formulation.

[42] Joseph Bishop KELLER, American mathematician, born in 1923. He received the WOLF prize for 1996/97. He works at STANFORD University, Stanford CA.

by satellites, and they showed that all the articles were wrong; I guess that the authors had not considered a realistic version of the equations of hydrodynamic and tried to deduce something about the waves (which is too hard a problem at the moment), but they must have postulated that one of their preferred probabilistic game applied to the problem, which ended up not being the case.

I am not opposed to probabilists, and Probability is just a part of Analysis, and I would advocate a probabilistic proof in a case where it would be simpler than a non probabilist one, but I prefer not to use a probabilistic approach because of the wrong ideology which often comes with using probabilities (which has nothing to do with Probability, but is about faith in dogmas); it is mostly because of a misunderstanding of Physics (and Probability) that probabilistic ideas have become so popular in some circles, but they have not been used correctly.

Some people try to prove that BOLTZMANN[43] equation is a good model, and for that they try to deduce it from some Hamiltonian[44] framework of particles interacting with forces depending upon their distances; this project is necessarily doomed, as I had learned a long time ago in a book by Clifford TRUESDELL; if two mathematical observers were looking at the solutions of the Hamiltonian system with a large number of interacting particles, one looking at time flowing forward and the other looking at time flowing backward and reversing the velocities, they would be looking at the same equation but they could not both deduce that this implies that what they observe is near the solution of BOLTZMANN equation, because of the H-theorem of BOLTZMANN, unless they were observing a situation near a global Maxwellian distribution at equilibrium; in other words there is an irreversible process which has been postulated in order to write BOLTZMANN equation, and the equation cannot be used for studying how irreversibility occurs. I have suggested that obtaining the right effective equation could show an effect like the apparition of nonlocal terms by Homogenization, and in this case the two observers would describe their limiting equation, one with an integral term from $-\infty$ to $t$ and the other with an integral term from $t$ to $+\infty$, and these two equations could well describe the same solutions; it would be by getting rid of the nonlocal term and replacing an integro-differential equation by a differential equation that some irreversibility would be introduced into the model. Of course, integral equations can be studied without probabilistic ideas, and if in some instance the integral equation follows from a probabilistic game, there is no reason to prefer the probabilistic game; in the same way if one talks about numerical methods, finite differences, finite elements, spectral methods, wavelet transforms, one is just making a list of tools that are useful to know, and none of them is always better than the others whatever the problem is, each method has advantages and defects and a good numerical analyst should learn what they are in order to choose the most efficient method for the problem that he/she considers at a given time (but some postulate that their method is better than the others).

A celebrated tool in probability is the "Brownian[45] motion", which was developed by WIENER[46] after the initial work of BACHELIER[47] in 1900 and EINSTEIN[48] in 1905, and it is associated with the heat equation $u_t - u_{xx} = 0$ and uses random walks which jumps in position, but this is quite different from what BROWN had observed, which were jumps in velocity, which are more related to the FOKKER[49]-PLANCK[50] equation $f_t + v.f_x - f_{vv} = 0$ and to the ORNSTEIN[51]-UHLENBECK[52] process. Some people like to derive FOKKER-

[43] Ludwig BOLTZMANN, Austrian physicist, 1844-1906. He worked in Graz and Vienna, Austria, and also in Leipzig, Germany.

[44] Sir William Rowan HAMILTON, Irish mathematician, 1805-1865. He worked in Dublin, Ireland.

[45] Robert BROWN, Scottish botanist, 1773-1858.

[46] Norbert WIENER, American mathematician, 1894-1964. He worked at MIT (Massachussetts Institute of Technology), Cambridge MA.

[47] Louis BACHELIER, French mathematician, 1870-1946. He worked in Besançon, France.

[48] Albert EINSTEIN, German-born physicist, 1879-1955. He received the NOBEL prize in Physics in 1921. He worked at the Institute for advanced Study, Princeton NJ.

[49] Adriaan Daniël FOKKER, Dutch physicist and composer, 1887-1972. He worked in Leiden, The Netherlands. He wrote music under the pseudonym Arie DE KLEIN.

[50] Max Karl Ernst Ludwig PLANCK, German physicist, 1858-1947. He received the NOBEL prize in Physics in 1918. He worked in Berlin, Germany.

[51] Leonard Salomon ORNSTEIN, Dutch physicist, 1880-1941. He worked in Utrecht, The Netherlands.

[52] George Eugene UHLENBECK, Dutch physicist, 1900-1988. He worked in Utrecht, The Netherlands.

PLANCK equation from BOLTZMANN equation, and this is illogical, because FOKKER-PLANCK equation is precisely about the grazing collisions which are not well described by BOLTZMANN equation, at least as long as one does not know how to avoid GRAD[53]'s angular cut-off hypothesis (there is a recent tendency to wrongly use the term FOKKER-PLANCK equation for a different equation with no velocity variable, often calling it forward KOLMOGOROV[54] equation too). Except for the work of BACHELIER, which was related to finance (in 1900!), the physical processes where diffusion equations are used seem to have a first step where a scattering effect occurs (the mathematical analysis of which I have not seen), a second step where a large time behaviour creates a diffusion in velocity and a third step which creates a diffusion in space by letting a characteristic velocity tend to $\infty$.

Whatever the physical origin of the heat equation is, one may try to solve it in any way one likes, including the use of "Brownian motion", but I have learned other tools which I find more general; for example, one could use the explicit LAX-FRIEDRICHS[55] finite difference scheme $\frac{1}{\Delta t}\big(U_i^{n+1} - \frac{U_{i-1}^n + U_{i+1}^n}{2}\big) - \frac{U_{i-1}^n + U_{i+1}^n - 2U_i^n}{(\Delta x)^2} = 0$, where $\Delta x$ and $\Delta t$ are mesh sizes in space and time and $U_i^n$ is an approximation for $U(i\,\Delta x, n\,\Delta t)$; one needs a stability condition $2\Delta t \le (\Delta x)^2$, which is exactly what one needs to have $U_i^{n+1} = a\,U_{i-1}^n + b\,U_i^n + c\,U_{i+1}^n$ with $a, b, c \ge 0$, which permits an "interpretation" in terms of expected value for a probabilistic game, which seems to be the basic idea for the "Brownian motion"; however there are also standard Hilbertian methods, which permit also to approach by discretization many other partial differential equations for which no probability game is known.

The point of view of describing the relation between different scales by using convergences of weak type is adapted to working with the partial differential equations of Continuum Mechanics or Physics, and the identification of the adapted topology usually helps solving the important question of adding variables which are necessary for describing the oscillating behaviour of solutions, and the effective equation itself. Forgetting about these added variables automatically gives a model where something is missing, and for correcting the defect of the reduced system many advocate the use of probabilities, but this approach does not seem to lead to the discovery of a better new equation. By avoiding the use of probabilities, one may discover why some phenomenon occurs, and when one discovers that some of the rules of Quantum Mechanics can be obtained without probabilities, one understands that it is not the use of probabilities which is wrong, but the postulate that one cannot explain what one observes without the use of probabilities. If a probabilistic game can be found for describing the solution of an integral equation, why forget that there are elementary results in Functional Analysis which can give the same existence result in a quicker way.

One should first understand what the physical problems are about, and one will probably be led to discover that there are some microstructures which appear in a natural way, and if Nature needs these microstructures it has to be for a good reason, which is certainly that they offer a much better solution than a boring smooth solution showing no microstructures. I believe that the microstructures which one observes in real problems show some kind of optimality, but one should certainly not rush at pretending that one knows which functional is being minimized, because that may not be exactly what Nature is doing anyway.

It is the goal of Homogenization to describe the oscillations (or concentration effects) in solutions of partial differential equations, and doing so it is expected that some effective equations will be discovered, which govern the new important quantities that one will have identified for describing these oscillating solutions, and that one will have identified which topologies of weak type are adapted to the problem at hand. It is the goal of $\Gamma$-convergence to understand similar questions for sequences converging in adequate topologies and trying to minimize adapted functionals, and it is important to discover interesting classes of functionals and which are the interesting topologies to consider in combination with these functionals. I would prefer to see such mathematical questions which may be relevant for explaining something from outside Mathematics, for example problems from Continuum Mechanics or Physics, although I know that in

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[53] Harold GRAD, American physicist, 1923-1987. He worked at the COURANT Institute of Mathematical Sciences at New York University, New York NY.

[54] Andrei Nikolaevich KOLMOGOROV, Russian mathematician, 1903-1987. He received the WOLF prize in 1980. He worked in Moscow, Russia.

[55] Kurt Otto FRIEDRICHS, German-born mathematician, 1901-1982. He worked at the COURANT Institute of Mathematical Sciences at New York University, New York NY.

the process of developing a mathematical theory one must necessarily study a certain number of academic problems (which one should not mistake with reality). There are issues which at the moment are better studied in one aspect than in the other, and Homogenization and $\Gamma$-convergence are two points of view about the study of microstructures which do not coincide, and may be complementary in certain situations, but cannot be antagonistic (except maybe for those who are more interested in politics and ideology than in Science). With the restricted point of view which has been used in the applications of $\Gamma$-convergence, it is not the same than Homogenization, but once the full strength of Ennio DE GIORGI's method will have been used by paying more attention to the choice of the topology, it may happen that the difference with Homogenization (in the way François MURAT and I have developped it first, where no restrictions like periodicity are imposed) may almost disappear.

It is quite common to experience the existence of microstructures in some fluid flows for example, but it is a very arduous task to propose a model that describes accurately the important effects occuring in these flows. A first observation concerns the structure of the interface in front of a rainstorm, as I had observed many times after a hot Summer day in the French countryside, many years before I thought of becoming a mathematician; one knows that a storm is coming, although the air is still, perhaps because the pressure is higher than usual, and then one hears the leaves of the trees moving, while the branches stay still; soon after the small branches start to move too, followed by the large branches a little after and the whole trees may be in motion when the rain arrives. It clearly suggests that the classical idea of a sharp interface with some partial differential equations being satisfied on each side and with some boundary conditions being imposed on the "interface" might not be so efficient for describing the effects occuring in that living layer, with small vortices on the dry side and large vortices on the wet side. A second observation concerns the structure of the "wind", as I had observed twenty years ago, on a week end where I went sailing with some friends, between La Rochelle and Ile[56] de Ré, but the morning had provided us with what one calls "calme plat" in French: there was no wind, and the surface of the sea was extremely smooth and only showing a long swell ("houle" in French), which combined with the steady movement sustained by the small engine of the boat to produce a beginning of seasickness; fortunately, it did not last too long, because after a while we saw what one calls "une risée" in French (my English dictionary translates as "light squall"), the wind waiting for us! It is an amazing fact to come from the windless side with a smooth sea surface to the place where the wind is, with the surface of the sea all wrinkled with wavelengths of the order of 5 to 10 centimeters, and when one crosses the transition line (which seemed stationary, but it might have been moving at a much slower pace that the boat, which was carried by its small engine), the sails inflated, and sailing started. Again, I will not dare to propose a functional, for which minimizing sequences show the precise microstructures which Nature uses in such a marvelous way, because that is probably not the reason they occur, and considering that these microstructures are optimal should be thought as an approximation.

It is useful to familiarize oneself with partial differential equations from Continuum Mechanics and Physics, in order to understand physical meanings which can be attached to a given partial differential equation, even if one is only interested in its mathematical properties. An equation may have lots of properties, some of them being more or less intuitive depending upon one's training, and the same equation may arise as a model in various applications, but some quantities may be more natural to introduce in one model than in another; for example, the equation $u_t - (u^m)_{xx} = 0$ is used as a model in porous media for $m > 1$ (with $u \geq 0$ denoting a density of mass), and the quantity $M = \int_R u(x,t)\,dx$ is independent of $t$ and is the total mass, and the quantity $\int_R x\,u(x,t)\,dx$ is also independent of $t$ and is $M\,x_*$ where $x_*$ is the abscissa of the center of gravity of the distribution of mass; however the case $m = 1$ is the heat equation, and in the interpretation that $u$ is a temperature (out of equilibrium), no physical meaning has been attached to $\int_R u(x,t)\,dx$ and $\int_R x\,u(x,t)\,dx$, although these quantities are independent of $t$. Using physical examples in Partial Differential Equations is a pedagogical approach similar to using pictures in Geometry[57], and it does

[56] Ile is the French word for island, and Ré was still an island at the time, but a bridge connects it to the continent now.

[57] I have been told that Jean DIEUDONNÉ was once giving a course where he was proving some abstract result of Geometry, and as a member of Bourbaki, he was doing that without using any drawing, of course. At some point, he could not figure out the next step in the proof, a difficult situation that every teacher has

not give the physical situations more value than hints: a drawing is not a proof but it is often a sufficient hint for a trained person for telling how to write a proof, if needed.

Of course, I imagine that the students have already heard about the corresponding physical theories, and would like to understand what they have been taught in their Physics course in a more mathematical way; this is often not the case nowadays, as Mathematics and Physics are often taught in a too ideological manner, and very few students have the motivation of acquiring a vast knowledge, and they may have problems understanding examples which could have been useful if they had learned more.

Although I agree with the motto of Hugues de Saint Victor[58] "Learn everything, and you will see afterward that nothing is useless", one must certainly take the time to understand how the various pieces that one has learned fit together before learning a little more.

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experienced; he went nearer to the blackboard so that what he was doing could not be seen by everyone (as he was tall and strong he could hide a reasonable part of the blackboard), and he drew a picture; after he had understood how to proceed from the hint given by the picture, he erased it and he finished his proof. Of course, such a behaviour should be avoided for obvious pedagogical reasons; if pictures are useful hints for the teacher why not explain how to use them to the students.

Jean Alexandre Eugène DIEUDONNÉ, French mathematician, 1906-1992. He worked in Paris and Nice, France.

Nicolas Bourbaki is the pseudonym of a group of mathematicians, mostly French; those who chose the name certainly knew about a French general named BOURBAKI.

Charles Denis Sauter BOURBAKI, French general, 1816-1897; of Greek ancestry, he declined an offer of the throne of Greece in 1862.

[58] Hugo von Blankenburg, German-born theologian, 1096-1141. He worked at the monastery of Saint Victor in Paris, France.