My discovery of mechanics

Paul Germain
Emeritus Professor, Université Pierre et Marie Curie
Formerly, Professor of Mechanics at Ecole Polytechnique
Secrétaire Perpétuel Honoraire de l'Académie des Sciences de Paris

Some of my former students have been kind enough to dedicate to me for my eightieth anniversary a volume of their original contributions and they asked me to write a few lines as a preface. It was impossible to turn down this proposal. So I decided to try and tell how I discovered this discipline to which I have devoted my professional life, and how I became fascinated by some of its various aspects. The main reason is that these discoveries have been made mostly with them and for them. Therefore, it is quite natural to present this introduction to this volume as a testimony of my gratitude.

Ever since I was a teenager I have always intended to become a professor. I have been happy enough to realise my dream. I am ready to agree with the statement of a very interesting young boy who filled the tank of my car at the nearest gas station of Brown University and who told me one day, "Let me be straightforward, a professor is somebody who was put to school when he was five years old and who had not enough imagination to get out". It is true that there exists a great continuity between learning and teaching. I was lucky enough to have enjoyed sometimes the feeling of having discovered a new result or to be the first to solve a new problem. But I must confess that probably my greatest satisfaction came from teaching. I spent many hours trying to get a new and deeper understanding of a concept or of a method and to find the best way to make them easily understandable by the students. What can be actually a greater gratification than the one you feel when you realise that your teaching has been meaningful, that your students experience themselves the deep beauty of the discipline and, above all, when you see their joy when they obtain themselves new results, partly thanks to some ideas you have tried to give them.

1. STARTING SITUATION

1.1 Researcher in mathematics?

I was very happy to be admitted to the "Ecole Normale Supérieure" in 1939. This school has been created by the French revolution and its main purpose is to train young people who aspire to become professors, teaching normally in the upper grades of our "lycées", and in particular in those that prepare students for admission - always a matter of stiff competition - into one of the French "Grandes écoles": Ecole Polytechnique, Ecole Nationale des Ponts et Chaussées, Ecole Nationale d'Administration,.., and in particular, the Ecole Normale Supérieure (ENS). This last is a very famous school because the selection is very hard. At that time, for the whole country and for all scientific disciplines, only twenty students were admitted each year. I was delighted by the perspective of teaching mathematics to the candidates to these difficult competitions. The studies at ENS lasted three years. And then, war came. The times were distressing and, needless to say, not very favourable for absorbing the new mathematical and physical concepts, methods and knowledge. During the first year, we were very few; only those who were not drafted; and later on we suffered the hardships of the German occupation.

Consequently, in my class at ENS we missed many chapters of a good, modern mathematical training in comparison with our older or younger fellows. Roughly speaking, we covered the first two volumes of Goursat's "Cours d'analyse" published during WWI and also a few topics of differential geometry. In mechanics our knowledge was pretty poor: kinematics and dynamics of rigid bodies, and Lagrange's equations.

In November 1940, the Germans closed for a few weeks the Sorbonne, the sciences faculty where most of our courses were taught. The direction of the ENS advised us to get in touch with a professor to work on some research mini-project. I saw Georges Bouligand, a very enthousiastic professor, who suggested reading some chapters of the famous four-volume treatise of Gaston Darboux, "Théorie des surfaces". The presentation was old fashioned; but the contents fascinated me, since geometry was my pet topic. I succeeded to write a paper which improved and completed what was written in Darboux, on a family of surfaces presenting some curious properties. Georges Bouligand advised me not to accept, in October 1942, the position in a lycée which was offered me, and to try rather to do some research in view of becoming, later on, a university professor.

There were no regular seminars at the time. So, I spent many hours in one of the best libraries in mathematics and I studied some papers, looking for a domain of research opening more prospects than the classical theory of surfaces in three-dimensional space. In February, I was strongly recommended to replace immediately a professor, arrested by the Germans, who was teaching a class of students preparing the competition for admission to one of the most renowned engineering schools. Nine hours of mathematics and a lot of work! Nevertheless, I found the experience very interesting and stimulating. At the end of the academic year, I had to work in a factory in the eastern part of France, to fulfil my "Service du Travail Obligatoire"*. In September 1944, this area was liberated and I went back to Paris.

1.2 The choice of mechanics

During the last eighteen months, I was thinking hard on what I wanted to do. I became very dubious about my capacity to get the knowledge and the deep understanding of modern mathematics – in fact, the Bourbaki literature – which would be necessary in order to do fruitful research in this field. I was still tempted by teaching in a lycée, but also to follow the suggestion of my friend Raymond Siestrunck, a physicist of my class

* "Compulsory Work Duty", imposed by Vichy authorities in agreement with the Germans

at ENS, who was beginning to work in aerodynamics. He thought that theoretical fluid dynamics would be an attractive field for a student who was keen on geometry. He gave me the book of Joseph Pérès, a mathematician who wrote a few books with Voltera on functional analysis and who began to learn fluid mechanics when, at the age of forty, in 1930, he was appointed director of the newly founded "Fluid Mechanics Institute" at Marseille. The book deals with the theory of irrotational flows of an inviscid and incompressible fluid – two-dimensional and axisymmetric mostly – and also with Prandtl's lifting-line theory. Finally, I decided to try this last proposition. I met Joseph Pérès, Lucien Malavard and Raymond Siestrunck in their small laboratory in the basement of ENS, where they were doing interesting work using electrical analogy. Malavard proposed for me to study a numerical method described in a NACA Technical Note, in order to compute the pressure distribution along a given airfoil. It was based on a conformal mapping of the exterior domain of the airfoil onto the exterior domain of a circle. Everything was straightforward and easy, except for the computation of the imaginary part, on a circle, of an analytic function of a complex variable, defined outside this circle, when its real part on the circle was known. Analytically, the result is obtained through the Cauchy principal value of an integral. Numerically, it is not easy, and in applications, all the accuracy of the method is greatly affected by this operation. My contribution was to do this computation without using the representation through a Cauchy integral, but by appealing to the special and very simple behaviour of the operator acting on trigonometric functions. The gain of time and accuracy was very significant.

I was just obtaining this result, when Pérès received a letter from Jacques Valensi, who was a member of the French Scientific Mission, located at Carlton Gardens, in London. Valensi was writing to him that he was working at NPL, the National Physical Laboratory, and that he thought he might succeed to obtain for one of his research assistants or collaborators the possibility to work at NPL during a few months, as a temporary member of the French Scientific Mission. Malavard and Siestrunck declined the offer. "Do you speak English?" asked Pérès. "Not a single word", was my answer. "Are you ready to go?" said Pérès with a smile. "Yes", was my reply, quite surprisingly for Pérès, my friends and also for myself. Deciding to go to NPL during the war, knowing neither English nor fluid mechanics, was quite an adventure. In January 1945, after two weeks in London, Valensi succeeded to have an appointment with the director in charge of fluid mechanics, who sent me to the department dealing with theoretical aerodynamics, headed then by Sydney Goldstein. With the help of Valensi, I tried to describe the only thing I knew and I had done. "We also have a method to do this kind of numerical computations. Come Monday morning, we will give you an example to compute, and we will compare your result with ours". The test was satisfactory. Goldstein was not able to keep me in his laboratory that dealt with confidential problems. But he recommended to the direction of NPL that I be admitted as a visitor. This way, I was able to participate every day, for three months, at the NPL, in the team headed by Dr. Falkner and then, for two months, in the team of Dr. W.P. Jones. Fortunately, I had access to the library, where I spent much of my time. When discussing with people, my complete ignorance of the basic concepts and methods of fluid mechanics was hidden by my extreme difficulty to understand and to speak English. Back in Paris, in July, I finally wrote some reports on the questions I had studied and explained to Pérès and to my friends what I had learned, without being able to make the difference between what was new and what was known already for some years. Then, after a really incredible combination of happy circumstances, I knew that I could try to work in Mechanics.

2. MECHANICS, A GOOD FIELD FOR A YOUNG MATHEMATICIAN

During my first British experience, I had the happy occasion to read, on a mimeographed pre-print, what is now well known as "Supersonic flow and shock waves". This celebrated book by Courant and Friedrich was a wonderful way to convince me that a good classical mathematical training may lead to interesting contributions in theoretical fluid dynamics. When I began to work, I was inclined to attach more interest to the mathematical problems and their solution, than to their mechanical origins and significance. My thinking and my research were therefore rather mathematically oriented, but in a few years, progressively, my job and my relations with other scientists brought about a reversal in my attitude. First, as early as October 1946, I was appointed head of a small research team at the *Office National d'Etudes et de Recherches Aéronautiques* (ONERA.) Second, I attended in 1948 the International Congress of Mechanics in London, where, thanks to Goldstein I had the good fortune to meet a large number of talented colleagues. With some of them, particularly the British, it was the beginning of a long-lasting friendship. I must namely mention the two invitations to spend one or two weeks in the famous department of Applied Mathematics of the University of Manchester, headed in 1949 by Sydney Goldstein and in 1951 by my old, much admired, and now regretfully departed, friend, James Lighthill. With James I remained closely related until his dramatic death, sharing in common the status of professor, the professional experience of being directors of aeronautical research establishments in our respective country, and becoming both members of the IUTAM bureau. Let me finally mention my appointment as a senior lecturer at the University of Poitiers, to teach the subject of compressible and incompressible fluid flows in a school of mechanics and aeronautical engineering, a program which fitted very well with my work at the ONERA.

A paper devoted to my discovery of mechanics is not the place to spend too much time on my research activity during this period. The two main themes of my research were the mathematical linearised theory of supersonic aerodynamics and the theory of transsonic flows. But, in fact, for my teaching and for answering some questions at the ONERA. I was also led to work on other topics concerning subsonic aerodynamics and gas dynamics.

2.1 Linearised supersonic aerodynamics

In France, we had the quite old tradition of the “Doctorat ès Sciences”, closer to the German Habilitationsschrift than to the American Ph.D. I worked hard to get my first professorship at Poitiers, writing a thesis of some 200 pages, published by the ONERA. and translated as a NACA Technical Memorandum, on the subject of conical supersonic flows. The field was opened by a pioneering work by Buseman who proved that mathematical solutions of the problem might be built through analytic functions of one complex variable. Of course I was lucky, because I had do work on a part of mathematics that I mastered. But I must recognise how much my training in fluid mechanics was poor, when I started to work in the fall of 1946. I spent a lot of time before understanding that, dealing with a linear problem, I had not to worry about the non-linearity of the boundary conditions on the wing, a delta one, for a typical application of special interest to engineers at O.N.E.R.A. I spent again a lot of time hesitating about the choice of the proper solution, because I was actually not aware of the role of the Kutta condition in supersonic flows and of the properties of the wake.

Anyway, when appointed in Poitiers, I was mature in fluid mechanics, but this is not the place for reporting on some other of my works on linearised supersonic aerodynamics. I will just mention the invitation to deliver a general lecture on that subject at the Brussels’ 1956 International Congress. I chose to give a review of known results, mainly by others than myself, trying to improve and unify them, using Schwartz’s theory of distributions, which was not familiar to most scientists of the mechanics’ community. Needless to say, my purpose was to stress that distribution theory sheds light on many facets of supersonic wing theory, rather than to illustrate distribution theory with some problems, like the one of minimum drag. I take this opportunity to mention that most of my contributions were improvements of some results found in literature or new results soon to be improved by another scientist. Reading was very stimulating; it helped me to get a better understanding of the subject matter, which proved very useful in teaching and building closer links with foreign colleagues who became good friends.

2.2 Transsonic flow and equations of mixed type

During the early fifties I was involved in research about transsonic flows, a subject of interest for aeronautical engineers and one which fascinated me as a mathematician. I remember being enthusiastic while reading papers by the Soviet scientist Frankl, whom I never had a chance to meet. I worked mainly on mathematical aspects of linear equations of mixed type, which apply to steady, two-dimensional potential flow of an inviscid fluid, in the hodograph plane. I paid also attention to some special equations for which approximated solutions may be found, providing either special or approximated solutions to, or shedding light on, problems of technical interest: nozzle or jet flow, upstream flow around an airfoil; flow at Mach number one around a wedge; behaviour near special points, like the one at infinity, or special lines like the sonic one or the transsonic boundary. But my main interest was in mathematical problems proper, investigated with Roger Bader. They dealt with the Tricomi equation and its Euler-Poisson-Darboux solutions, their evolution when the singular point crosses the parabolic, i.e., the sonic, line, the singularity changing from logarithmic to Riemann's type; the theorem of maximum; a new proof of the existence theorem of the Tricomi problem and its Green's function.

All these topics gave me the great satisfaction of bringing me back to mathematics and the occasion to have close and friendly relations with mathematicians, especially at the Courant Institute. Mathematics, the beloved discipline of my youth has never ceased to fascinate me and excite my admiration. But I felt the necessity to be consistent with my choice and my decision and not to continue to use mechanics as an excuse to do mathematics. It was time to get a good knowledge of what really was this discipline.

3. GETTING A DEEPER INSIGHT INTO THE REALM OF MECHANICS

During the period 1952-1955, while pursuing my routine work, I was much concerned with thinking about the concepts, the trends, the understanding, and the reasons that sustained my vocation for mechanics. It was not only important for me, but also for the students and scientists working with me, now, and even more, in the future. One might feel that the example of my British colleagues could deliver an answer; but they would not be able to understand my uneasiness. They grew up in the best tradition since Newton's times. They were doing mechanics as a natural thing, just like breathing. I had to cope with the special situation of mechanics in France, at that time. During many decades, and especially after WWII, most talented young scientists were attracted either by pure mathematics or by hard physics, while mechanics was considered as some reminiscence of the 19th century. As a consequence, I felt myself rather isolated and I had to get, all by myself, an overview of what should be the discipline to which I wanted to devote my efforts in research and teaching. Of course, I was not fully conscious of this evolution in my mind, but at least three major events helped me in elaborating meaningful answers and I want to report on them.

3.1 First contact with Paco Lagerstrom.

The first of these was my meeting with Paco Lagerstrom during the Istanbul 1952 International Congress. He was, like myself, the author of an important report on conical supersonic flows. He started by studying Roman languages – I believe – in Sweden, and then attended courses in pure mathematics at Princeton. At the time I met him, he was professor at Caltech, in the department of aeronautical engineering, so that both our cultural backgrounds were similar. He spoke to me about some questions which he thought to be of the utmost importance for the understanding of fluid mechanics and which might be ripe for solution at that time. One of them was the mathematical basis of the boundary layer concept, discovered by Prandtl, nearly fifty years before. Another one concerned the steady flow of an inviscid fluid as the limit of a class of corresponding flows of the same fluid, involving a vanishingly small viscosity, so that, in some sense, solutions of the Euler equations might be related to a class of solutions of the Navier-Stokes equations, through some limiting process. He brought my attention to another of Prandtl's discoveries, namely the constancy of vorticity in a closed, streamlined, two-dimensional steady inviscid flow, as a result of vanishing viscosity, acting during an infinitely long time. I began to foresee a new link between mathematics and fluid mechanics, provided by asymptotic techniques; as a matter of fact, not simply a link, but a way of thinking at an enormous variety of problems. Needless to say that all this was opening avenues without a clear vision of getting the right way in.

3.2 Visiting professor at Brown University

During the spring 1952, being invited to present a paper on transsonic flows at a small colloquium held in Belgium, I had the chance to meet William Prager. A few days later he mailed me an invitation to deliver a course in gas dynamics at Brown University. I was grateful to him for this attractive and unforeseen proposal, but I asked to have it postponed for a year, to give myself, in the meantime, the opportunity to improve my English. I was very fortunate to spend the full academic year 1953-54 as a member of the graduate division of Applied mathematics, which was very close to the division of Engineering. For a mechanician, this was one of the best places in the world, providing many good courses, covering most of the main fields of mechanics and running a famous seminar attended by outstanding mechanicians from the United States and abroad. I took the measure of some fantastic gaps in my knowledge while attending these courses, mainly in solid mechanics, discovering plasticity, linear and non linear elasticity and studying Truesdell's papers, in which he gave the first systematic treatment of continuous mechanics, starting from fundamental concepts. I also learned a lot in fluid mechanics, boundary layer and viscous fluids, and even in hydrodynamics and incompressible inviscid fluid flows. I also had the opportunity to discuss many things with some of the best mechanicians in the world. Coming back to France, with so much new material, I felt worthy to be named professor of mechanics.

3.3 Professor of rational mechanics

This is what actually happened in October 1954. I was appointed professor in the chair of "rational mechanics" in the University of Lille – this is the third event that I have announced. At that time, in France at least, rational mechanics was considered as a branch of mathematics, dealing mostly with the application of the Newtonian theory to rigid body motions and Lagrange's analytical mechanics. Usually, a chair of rational mechanics was a position for a mathematician who worked there in anticipation of being appointed, as soon as possible, to a chair of calculus or of advanced geometry, or still, advanced analysis. With the choice I had done, this could not be my prospect. Actually, I had to understand more deeply the foundations of Newtonian mechanics as a mathematical model to the physics of the equilibrium and motions of bodies in the neighbourhood of the Earth, of the solar system, and of the Universe. The system of reference was a fundamental notion which was to be adapted to each situation. For the first time I realised how wonderful was the mathematical schematisation of the reciprocal action of one body on another. That the fact had required nearly twenty centuries to be recognised was not anymore a surprise to me! Lagrange equations for systems of rigid bodies with perfect constraints were also very attractive for a man who was very keen on geometry. I was able to cover nearly the whole classical programme in less than the full academic year and then, to have about six to eight weeks to present a topic, among the following ones: advanced analytical mechanics (Hamilton-Jacobi-Maupertuis); basic notions of relativity theory; basic concepts of continuous media with simple examples on incompressible inviscid fluid and classical linear elasticity; vibration theory. I mentioned these three experiences because they set up the starting point and the corner stone of what I have tried to build afterwards. I have discovered, above all, new facets of mathematics. It is not enough to solve problems and to study their solutions. One must also build the concepts and the models, which will be able to offer a deeper view of the phenomena than the one given by mere observation, and even by experiments. Mathematics has to build what I have called, in a talk at the Académie des Sciences, "la mathématique du monde". Moreover, these experiences have contributed to enrich my own cultural personality. Mechanics was not anymore a thing external to myself. It was becoming not only a part of my intellectual life but also a constituent of my spirit which, since that time, shaped my deepest convictions.

4. MODELLING AND ASYMPTOTICS IN FLUID MECHANICS

Let me repeat what I intend to describe in this introduction. It is not the discoveries I was able to make in the field of Mechanics – they are very limited. It is the progress of my understanding during all my life of what is this discipline, more precisely by discovering new problems, new ideas, new methods, new fields of applications. In fluid mechanics this progress of understanding was due mostly to the close relations with some colleagues and to the good knowledge of their works. Let me first mention some favourable occasions that offered these possibilities. These were, my stay at Caltech during four months near Paco Lagerstrom, Saul Kaplun, Julian Cole; later on, the academic year spent by Paco in my laboratory; the two visits in Paris, for one full year each, of Milton Van Dyke. I also benefited a lot from the long stay of W. Eckhaus in my laboratory. All of them theorised on asymptotic singular expansions which I came to use frequently. But, my best contributions to fluids were due to discovering, at the end of 1955, Jean-Pierre Guiraud. I succeeded to convince him that fluid mechanics was a very interesting topic and I asked the direction of ONERA. to recruit him to work in my small research team. During six years we met at least once a week, often for a full day. Jean-Pierre began to work for his doctoral thesis, on the small perturbation theory applied to hypersonic flows. But we did not limit our discussions to this topic. We exchanged our ideas about our recent readings, our small discoveries; we have learnt together, we have built our views together; our vision on mechanics was very similar. That was extremely stimulating and fruitful. In the beginning, I was the leader of the discussions; but very soon the positions were reversed.

4.1 Shock waves in gas dynamics and in MFD

Many of my personal studies dealt with flows of an inviscid fluid in which the variables defining its kinematics and its physics, suffer discontinuities on some manifolds in space-time. These are the shock waves. In order to explain my contributions, I must give some reminders. Burgers' equation, as it is well known, is the very simple mathematical model that gives the best physical meaning of this phenomenon. It involves one unknown u – which may be interpreted as a velocity – one space variable x, and one "viscosity" coefficient ν. As shown by Hopf, one may write explicitly the solution of this second order non-linear, partial differential equation which, for the initial time t=0, takes a given value $u_0(x)$. It is a continuous and differentiable function $u(x, t)$. Now, let ν tend toward zero; the limit is a "weak" solution of the inviscid Burgers' equation (ν=0) which, in general, presents discontinuities – that are shock waves (in a point of discontinuity the x-derivative of u has to be computed by an appropriate device, for instance, distribution theory). Locally, at a point of the shock, the jump of u must check not only an equation J, as any weak solution, but also an inequality which characterises a "shock solution" among all the "weak solutions". Let us recall also that, clearly, in this simple model, a shock wave appears as the result of two conflicting influences, first the non-linearities of the propagation for ν=0, the inviscid Burgers' equation, which tends to steepen the profile of the variations u along converging characteristics and, second, the weakening effect on this profile, due to viscosity.

One may now formulate the general situation, which is to be faced. It concerns the motions of a fluid in which are present some physical mechanisms of dissipation – let these motions be called P. These mechanisms may be mathematically schematised by some terms and then the motions we are looking for may be described as solutions of a general system of differential equations – let us call N this schematisation. But N also is too complicated. If the dissipations are very small, a further modelling may be considered by neglecting all the dissipations – call E the system of equations so obtained. Some solutions of E may involve surfaces of discontinuity. One wants to study the solutions of E which may be considered as limits of solutions of N for any vanishing dissipations. The shock S appears, then, as the limit of a small layer – called shock layer – of a solution of N which is the result of two mechanisms with opposite effects, the steepening due to the converging progressive waves of the limiting inviscid fluid and the weakening due to the dissipations. In order for it to be a shock solution of E, on each point of the shock, the jumps have to check some equations J and some inequality j.

In classical gas dynamics, the situation is clear and simple. The equations J are the Rankine-Hugoniot relations and the inequality says that the specific entropy cannot decrease when a particle crosses the shock. That is a necessary condition; it is also a sufficient one, as it may be seen when one studies "the shock structure". This can be done by the same method that was applied to study the ordinary boundary layer. At a fixed point of S and at a fixed time, a stretching variable ξ is introduced along the normal to S, becoming infinite when the dissipation coefficient tends towards zero, in order to obtain a "significant degeneracy" – see below. The values of the variables on the sides of the shock become the values for ξ=+ ∞ and ξ=-∞ according to the matching conditions between the distal (outer) expansion (the original solution) and the proximal (inner) expansion (for the stretched geometry). It is easy to show the existence of the structure which is the solution of the N equations in the stretched geometry, when ν tends towards zero.

Everything we recalled above is very classical. But I felt necessary to do it in order to understand the situation that is met when one wants to study shock waves within the frame of the classical magneto-fluid-dynamics theory (MFD).

In the classical MFD, when all the dissipative effects are neglected, the jump relations convey the conservation laws (mass, momentum, energy) and the Maxwell equations. They can be written with four variables $q_1 .... q_4$ - say **q** – which are respectively the specific volume, the temperature, and the tangential components of the relative velocity and of the magnetic induction. The four constants $c_1 .... c_4$ - say **C** - represent the quantities conserved across the shock. The jump relations J may be written $L_k[\mathbf{q}, c_i] = 0$ (k=1, 2, 3, etc.) on both sides of the shock. One may check that for given values of the shock constants **C**, it is possible to define a function P which takes in the **q** space stationary values at the two points, images of the values of the **q** on both sides of the shock. One may show also that for given values of the constants **C**, there exist at most four points in the **q** space, S_1, S_2, S_3, S_4 in which P is stationary, the index of these points being chosen by non-decreasing values of the specific entropy.

One may be tempted to write the inequality condition j by imposing that the specific entropy cannot decrease when crossing the shock. That was the proposal of the first authors working on this question. In order to see if the statement is correct, one must investigate the structure of the shock, by taking account of the dissipation in the proximal representation of the shock layer with the ξ stretched distance to the inviscid shock. In the most simple representation, one has to take into account the four coefficients of dissipation – two for viscosity, one for Joule's dissipation, one for heat conduction – and assume the existence of a dissipative function as a linear combination of $\dot{q}_i^2 = \left(\frac{dq_i}{d\xi}\right)^2$, say $D(\dot{\mathbf{q}})$. Consequently the shock structure equations may be written

$$\frac{\partial D}{\partial \dot{q}_i} = \frac{\partial P}{\partial q_i} \qquad i = 1,2,3,4 .$$

The structure of the shock, defined by the two points S_a and S_b $(a < b)$ is the integral of the system connecting S_a and S_b along which ξ increases from -∞ to +∞. One has to discuss the existence of such an integral and its limit when all the coefficients of dissipation tend towards zero, independently. The MFD shock $S_a \to S_b$ is admissible only if this limit is S_a for $\xi < 0$, and S_b for $\xi > 0$. What is found is that for given constants of the shock, only two are physically admissible: the fast shock $S_1 \rightarrow S_2$ and the slow shock $S_3 \rightarrow S_4$. Of course, if some of the dissipation coefficients vanish, the structure of the shock may present a discontinuity which is called a subshock.

I have presented this question in some detail, because its result led me to some conclusions of a more general nature which may be useful not only in fluid mechanics but also in other modellings of macroscopic theories of physics. The first concerns the significance of entropy. Roughly speaking, I would say: *The non-decreasing property of entropy is a necessary condition for a process to be physically admissible, but it is not always sufficient.*

Another comment concerns the validity of a simplified theory which gives rise to a mathematical system S of equations. Such a system admits *classical solutions* which must check the well-known Hadamard conditions: existence, uniqueness, continuous dependence on the data. Inviscid fluid dynamics shows that the set of classical solutions is too restrictive for the description of physical situations. One has also to consider *weak solutions*; but now, the set of weak solutions may be too large in order to have admissible solutions. Many papers have given conditions which may be imposed to weak solutions in order to achieve a satisfactory requirement. What was proposed above is to prescribe a "continuity" between the set of more or less refined theories which may describe the behaviour of a physical situation.

A solution of a mathematical schematisation S of a physical situation is not acceptable if it cannot be obtained by the limit of a solution of a more refined schematisation S' when S' tends to S.

One may try to apply this statement in order to see how the previous results are the limit of a more refined description. One possibility that has been investigated is to introduce a model with two fluids – ions and electrons – and, for simplicity, to neglect viscosity and heat-conduction dissipations. The differential system which rules the structure involves three parameters: one that rules the Joule's effect and the other two, β and χ, which are respectively proportional to the product and the differences of the densities of ions and electrons. When β and χ are zero, one recovers the differential system which governs the structure of a fast shock in the MFD model. Some interesting situations have been noted: first, of course, subshocks may be present. Then, oscillations may be found either in the front side or in the back side of the shock. Finally, if Joule's effect is neglected, one finds a structure which is a model of what is called a "collisionless shock", in plasma theory.

4.2 General theory of jump conditions and structures in gas dynamics

Kinetic heating during re-entry gave aeronautical engineers a strong impulse to improve the boundary layer theory. But if viscosity and heat conduction have to be taken into account outside the boundary layer, that means that the Rankine-Hugoniot equations which rule the jump across the bow-shock wave have to be rewritten. A first result given in the literature was roughly criticised by noting that one must take into account the thickness of the shock. But the proposed evaluation was not completely satisfactory. Both, Jean-Pierre Guiraud and I, we were convinced that the only way to get correct results was to apply matched asymptotic expansions. This is a quite complicated problem, because the powers of the inverse Reynolds number $\left(R_e^{-1}\right)$ which arises, may be fractional and even more complex than the powers of the square root. I do not intend to discuss the whole matter here; I prefer to restrict myself to the special topic which has been worked out, namely the structure of the expansion when the sole presence of the bow shock wave is taken care of. Here one has to deal with simple R_e^{-n}, n integer, powers. Two expansions are needed, the so-called outer one, the leading term of which gives the inviscid solution, and the inner one, which provides the well known internal shock structure, to leading order. It is possible to build the whole expansions, at least formally, including matching. What is remarkable is that one may write out the jump conditions to be applied to the whole of the outer expansion. This comes from the conservative form of the Navier-Stokes equations. As a consequence, one may write out jump conditions by a very simple process, like the one that leads from inviscid conservation equations to jump conditions. Then viscous and heat conducting terms appear to have been taken care of. But the result is illusory because one has to add a contribution from the inner expansion. At least formally, this contribution may be written straight to any order R_e^{-n}. Of course, this is correct only for the terms of the expansion which are forced out by the shock. To order R_e^{-1}, the jump conditions are very easily written out when one knows the internal shock structure to leading order only. This is very well documented. Of course, the boundary layer brings in half powers. The result to order R_e^{-1} has found applications in kinetic re-entry heating.

4.3 Other topics involving singular asymptotics

I did not theorise on singular asymptotics but got a deep knowledge of this methodology through teaching and research work. Let me mention a few examples.

The first one arose with a few lectures on progressive waves I had to deliver in 1970 at Stanford and Berkeley. I read a number of outstanding papers. I do not want to choose among them here, but rather, to simply report on what was my view after that reading. A progressive wave occurs generally when a physical phenomenon is thought to be represented by the occurrence of steep gradients in one variable only, across three-dimensional manifolds, in four-dimensional space-time, with much smoother gradients in other directions. The mathematical structure of the representation looks like one of a phenomenon in five-dimensional space-time. We need some notation in order to avoid confusion. Let the phenomenon be quantified by an n-dimensional vector U and let t be the time, and $\boldsymbol{x}$ be the position vector in three-dimensional space. Assume that the manifold across which the gradients are steep is $F(t,\mathbf{x}) = \text{Const}$. Then, the mathematical progressive wave structure is $U(t,\mathbf{x},F/\varepsilon,\varepsilon)$ so that $\xi = F/\varepsilon$, is considered as a fifth variable. There is apparently nothing in the equations, which allows us to single out the dependency of U on ξ. But all is changed when we add the ansatz that the proper physical solution may be obtained as an expansion with respect to ξ, t and $\mathbf{x}$ being fixed when proceeding to the limit of vanishing ε. As a matter of fact, the multiple scale technique, through the requirement of vanishing of secular terms, provides the way, by means of which that dependency can be figured out. A key to the existence of progressive waves is that *U*, supposed to be dependent on ξ only, at leading order, exists as a planar wave solution ruled by a linear system with $\partial F / \partial t$, $\partial F / \partial \mathbf{x}$ obeying a dispersion relation. That relation defines a wave speed and may be considered as an eikonal or Hamilton-Jacobi equation, the solution of which is built by means of rays. The planar wave being a solution to a linear system is determined only up to a scalar amplitude factor. This amplitude obeys an equation which is got when going to higher order in the expansion and eliminating secular terms. The details depend on the particular phenomenon considered, and there exist quite a variety of situations that may be described mathematically by such a procedure. It is not my purpose to enter into the details, but let me frame a few remarks. The small parameter ε characterises the steepness of the transversal gradient. If the physical process is non-linear, and non-linearity is measured by the order of magnitude of the amplitude and, if, furthermore, the initial equations are first-order quasi-linear, as is the case with inviscid gas dynamics, then the amplitude obeys a partial differential equation which is generally an inviscid Burgers'equation along each ray. If there are second order derivatives present in the equations, with a small coefficient, then the amplitude obeys a partial-differential equation which is generally Burgers'. The role of time is played by the distance along each ray, while the role of space is played by ξ. One may deal with third order derivatives, and another small parameter, yielding then the Korteweg-de Vries equation. Both phenomena may occur simultaneously. The equation for the amplitude is called the transport equation. One may even treat cuspidal rays, corresponding to caustics of the wave, and get a kind of Tricomi equation for the amplitude but I have to stop here.

I have been too long and shall go faster with the other two examples. The second one was an invitation to give a course in theoretical fluid mechanics at the famous summer school in “Les Houches”, during the summer of 1973. I chose the topic “Asymptotic methods in Fluid Mechanics”. I lectured to brilliant young physicists who began to be somewhat attracted by mechanics and not simply by hard physics. As physicists, they knew the usefulness of approximations and of non-dimensional scaling. But they did not know that a systematic technique was available for building approximate mathematical models and trying to measure quantitatively their validity. I showed that the approximation is very often tied to the existence of a small parameter, coming out from the non-dimensional form of the equations, and I intended to show that the process is sustained by asymptotic singular expansions. I gave an account of the various methods of building the approximations, as asymptotic expansions, and insisted on the methodology, in particular the matching conditions and the concept of significant degeneracy, recently created by Eckhaus. I liked very much this last one, because it gives a systematic way to find out what should be the various stretchings. I thought that this might be attractive to physicists, because it is a quasi-systematic way of comparing the respective weights of various terms in the equations, which measure the physical importance of phenomena they are likely to describe.

The third and last example, which is quite recent, is issued from an invitation to write a paper on the extraordinary heritage of Prandtl. I was not long to focus on two topics, out of a large number initiated by this giant, namely, boundary layer and lifting line theories. It was wonderful to read anew Prandtl's original 1905 paper and to find in it, not only the essentials of the boundary-layer theory, but yet more wonderful, the query of Prandtl about separation, for which he run quite beautiful experiments. Of course, the history of the boundary-layer theory provides a lot of crucial events, but the most impressive one is the construction of the "triple deck" in 1969, independently, by Stewartson and Neiland, through matched asymptotic expansions. A more systematic alternative way to establish this beautiful "triple deck" would be to use the concept of significant degeneracy. This construction which would, I think, have been impossible without the matched asymptotic expansions gave, fifty-five years after the query by Prandtl, a satisfactory explanation of separation, at least for steady laminar flows. The lifting-line concept built by Prandtl in 1917-18 waited till 1964 to find, with Van Dyke using matched asymptotic expansions, not only a justification but also directly one approximate solution to the famous Prandtl's singular integral equation, which is consistent with the order of approximation at which Prandtl's equation itself is consistent. And it was yet more wonderful for me to discover that in 1991, eighty five ears after Prandtl's construction, two young French scientists, Guermond and Sellier, gave a fascinating asymptotic approach to full lifting surface theory of high aspect ratio, allowing, at least in principle, to build the expansion up to any order.

4.4 Final remarks on Fluid Mechanics

Looking back to the main ideas, methods and results I have related on above, I must recognise that I have benefited a lot from them. My research field covered a very limited part of the very large domain of Fluid Mechanics. There is nothing on very important topics: turbulence, rarefied gas dynamics, hydrodynamic stability, to cite a few. I have read many papers, particularly when I was the principal editor of the "Journal de Mécanique" during seventeen years; and I have listened to many talks, in particular at the regular seminar of my laboratory. But, after ten, fifteen, twenty years, what remains is just a feeling of how fascinating it could be to understand deeply the fundamental questions raised by many of these papers. Perhaps, I might say, what remains is a very pleasant "cultural" feeling, something close to the statement of a member of the Académie Française who said that "culture is what remains when everything else has been forgotten".

5. FORCES AND STRESSES VIA VIRTUAL POWER

5.1 General formulation

It is worth to relate the origin of this discovery. In a meeting with mathematicians and physicists which was organised in order to discuss who will assume the task of teaching mechanics to the students in their first two years of university, I had to explain the programme proposed by the mechanicians. For rigid body mechanics, our proposal was to introduce the torsor ("torseur" in French) concept, which gives an adequate mathematical representation of the action, exerted by exterior bodies on a rigid body. This concept is very seldom introduced in the English speaking universities, which prefer to start with the classical Newton laws. It is nevertheless an interesting concept, because it may also be used to describe the kinematics of a rigid body. A torsor is a field of moments and a resultant when it describes the forces. It is a velocity field and a rotation-rate vector, if it describes the kinematics. At that meeting somebody asked me: What is a "torsor" in a space of n-dimension? I was ashamed not to be able to answer and, above all, not to have thought, myself, to ponder on this question. A few weeks after this unhappy event, I found what I consider the best way to define the mathematical representation of the action. Let us consider the "forces" exerted on a given mechanical system by an outside system. It is based on the concept of virtual motion introduced nearly two, or more, centuries ago. Roughly speaking, the "mobility" of a system B at a fixed time t, in a given reference frame R, is the vector space V of all the possible velocity field of the virtual motions, which one has decided to consider. A system of actions F exerted on B is defined by a linear form on V – to each motion there corresponds a scalar P – which is its virtual power. In other words and briefly, F is an element of the dual of V defined by the linear form. The "force" is the dual of the "mobility"!

This definition may, at first sight, look a little abstract. In fact, it is not and, moreover, it presents many advantages. First, it is very natural: if you want to see if a suitcase is heavy, you try to raise it a little bit. Second, it gives immediately the known result for a system reduced to a material point [the virtual velocity is a vector; the force is a dual vector], or for a rigid body [the kinematics of a virtual motion which keeps the system rigid is a "distributor"defined by a field of the velocities vectors and its associated rotation rate anti-symmetric second-order tensor and the forces are then described by a "torsor", a field of antisymmetric second order tensors field of moments and its associated resultant force]. These two concepts, distributor and tensor, may be identified only in the 3-dimensional space.

The concept of virtual motion was used in analytical mechanics since Lagrange. So, it is not a new one. What is new, is, in addition to using it in order to write equations of motion, its use from the very beginning, that is, in order to define the mathematical representation of "forces". One must notice also that what is proposed is similar to what is done in distribution theory when a function (and its generalisation to a distribution) is defined by a linear and continuous functional in a space on "test functions". A virtual motion is a "test function".

The concept is also very flexible. Given one system, you may choose the definition of the mobility – if you refine the representation of the mobility, you will automatically refine the representation of "forces". You may also choose the linear functional. This remark will find its best application in continuum mechanics.

5.2 "Stresses " in continuum mechanics

When on deals with deformable bodies, it is convenient to introduce separately the (virtual) power of exterior action P^e – exerted on the given body B by the system exterior to B – and the power of interior actions P^i mutually exerted by the elements of B. In classical continuum mechanics the following axiom concerning interior actions plays a fundamental role: For *any rigid virtual motion of B, P^i is zero*.

It is equivalent to state: *The power P^i of internal forces is independent of the frame in which the virtual motion is defined.*

This axiom is another formulation of what is often called the principle of material indifference.

The best way to build a continuum theory is to start by writing P^i. The most simple choice is to consider P^i as the integral over B of a local p^i and to assume p^i as a linear function of the local values of the velocity and of its space derivatives. The coefficients of this function define the local representation of the interior forces, which may be called the "stresses" inside the body B.

If B is a 3-dimensional mechanical system, p^i is a linear function of the symmetric part of the gradient of the (virtual) velocity field – The velocity field itself and the antisymmetric part of the gradient cannot be present, on account of the axiom of internal forces. Then the "stresses" here are simply defined by the field of the stress tensors, i.e., symmetric second-order tensors. It is, of course, the classical result, but here it is defined directly and in a rather simple way.

If B is a compressible fluid, a medium sensitive only to the rate of the specific volume, the "stresses" are simply defined by a field of scalars – the pressure p.

If B is incompressible, for the virtual motions which satisfy this constraint, this rate of the specific volume must be zero. The "stresses" are reactions to this constraint and (if the constraint is ideal) are defined by a field of "Lagrange multipliers", i.e., pressures – but the latter are not of the same physical nature as the pressure in a compressible fluid.

The advantages are all the more important, when the situation is complex. For instance, they are very appreciable in plate and shell theories. In the natural theory of plates of small thickness, the mean plane being $x_3 = 0$, one may consider the virtual velocity fields whose components are

$$v_\alpha + x_3 \lambda_\alpha, w \; (\alpha = 1,2)$$

which give for the components of the tensor of deformation rate

$$D_{\alpha\beta} = d_{\alpha\beta} + x_3 K_{\alpha\beta},\; D_{\alpha 3} = b_\alpha = \lambda_\alpha + w_{,\alpha},\; D_{33} = 0.$$

One may write

$$p^i = N_{\alpha\beta} d_{\alpha\beta} + M_{\alpha\beta} K_{\alpha\beta} + Q_\alpha b_\alpha$$

The "stresses" are defined by

$$N_{\alpha\beta} \text{ membrane stresses, } M_{\alpha\beta} \text{ flexure stresses, } Q_\alpha \text{ shear forces}$$

The Love-Kirchhoff theory considers virtual motions such that the small segments perpendicular to the plate remain perpendicular to the mean surface in the (virtual) deformation in such a way that $b_\alpha = 0$. Then, $p^i = N_{\alpha\beta} d_{\alpha\beta} - M_{\alpha\beta} w_{,\alpha\beta}$. This theory does not take shear forces into account.

In the previous examples, one starts with the definition of the mobility. As "mobility" and "forces" appear as a dual concept, one may also start by the definitions of "forces". In such a case, one must first derive what must be the rate of deformation of the virtual motions and after that, what is the deformation of the medium.

Let us emphasise what is, maybe, the greatest advantage of the method of virtual power. In classical presentations, one usually defines independently the "deformation" and the "stress" and then one is not sure that the assumptions made to define them are in agreement: one may be too refined compared to the other. The definition of "stress" via the virtual power avoids this difficulty.

In the above considered examples, p^i was a linear function of the first derivatives of the virtual velocity field – one says that these theories are of the first grade – except the theory of Love-Kirchhoff for plates, which is a theory of the second grade. It shows that one may refine a theory usually by assuming p^i to be a linear function of first and second derivatives of the velocity field.

One may also build the "stresses" of a micropolar medium, the local particle being not a point of matter, but an infinitely small rigid body whose kinematics are defined by a distributor (a velocity field and a skewsymmetric tensor, which represents the rotation rate of the particle). A liquid crystal is an example of such a medium. One may also build, by a similar process, a theory of micromorphic media by assuming that the particle is no more rigid but deformable. A polymeric solution is an example of such a medium.

Finally, let us note that Gérard Maugin and his co-workers have extended this theoretical scheme to study continuous media in which, physical interactions other than mechanical, are present, in particular electromagnetic interactions, as shown in many papers of this author.

6. CONTINUUM MECHANICS AND CONTINUUM THERMODYNAMICS

This is a new way to look at mechanics, a way by which one discovers its deep unity: mechanics of rigid bodies, fluid mechanics, solid mechanics, appear then as branches of a large tree. To have participated in this adventure is for me a great satisfaction. Within continuum mechanics, new physical phenomena receive a scientific treatment. Continuum mechanics appears today as the foundation of macroscopic physics. I owe Clifford Truesdell and Ronald Rivlin the first discovery of this field; in particular, I am grateful to them for pointing my attention to the works of Pierre Duhem who, at the beginning of the 20th century, had a clear and prophetic view of what it would be like, but unhappily, without being heard in France at that time.

As I try to tell how I have discovered this new important field, I must stress again that, above all for this point, everything that I have to report now was very closely connected with my teaching. I think I may distinguish three steps, which will be called: Mechanical interactions – Continuum thermodynamics – Mechanics of materials.

6. 1 Mechanical interactions

One very important decision for the development of continuum mechanics in France was the creation, in 1958, of a new curriculum called “licence de mathématiques appliquées″ in which a semester optional course on mechanics of continuous media would be offered to the students in every university which would want to open such a possibility. I had just been appointed professor in Paris, and I was asked to teach this course. This was for me the occasion to develop a little what I had began to teach in Lille. The course comprised three parts: (i) a general introduction to the Euler and Lagrange representations of motion, deformation in a small-perturbation framework, (ii) stress tensor and the conservation laws of mass and momentum, and then, (iii) some examples of steady flows of an incompressible inviscid fluid and some examples of the classical linearised elasticity for homogeneous and isotropic bodies. The principal goal of these examples was to give the students an idea of the interest and usefulness of fluid mechanics and solid mechanics.

Another decision, a few years later, was to give to universities the possibility to organise more advanced courses in the curriculum towards a DEA (Diplôme d’études approfondies). In Paris, it was not difficult to create a DEA in fluid mechanics because competent professors were present in the department of mechanics. But solid mechanics was not a very well developed discipline. A DEA in solid mechanics was created as soon as the appointment of a new professor gave the department this possibility. I was then in charge of the creation of a more advanced course in continuum mechanics. To do that I had to learn new topics. Roughly speaking my teaching dealt with the general concept of the discipline, much inspired by Truesdell, Toupin, Noll, Coleman. I was using their notations, their reasoning and some of their examples, in particular the marvellous theory of simple fluids and materials with fading memory. But I included also, in the framework of small perturbations, applications of theories of linear viscoelasticity with the use of the Laplace transform, and elastoplasticity with a fixed yield surface in particular, in order to give notions of the beautiful limit analysis.

6.2 Continuum thermodynamics

It was clear that thermomechanical interactions are involved in most of the evolutions of the bodies that have to be considered in a general study of continuous media. It means then that something like thermodynamics was needed. But how to build such a satisfactory theory?

The principal basic question is to introduce the entropy and the absolute temperature. In classical thermodynamics, dealing with systems in equilibrium, many answers have been proposed, some of them, like that given by Caratheodory, very satisfactorily. But mechanics considers systems in motion. There a true *thermo-dynamics* is needed, and not the classical one, which, in fact, is thermostatics.

Two classes of answers were produced. In the first one, it is proposed to assume that entropy and absolute temperature are primitive concepts – or at least, entropy. This standpoint implies a drastic change in the concept of thermodynamics. In the second one, one wants to maintain what is known in thermostatics and to adapt the necessary requirements needed by the new situation.

The starting point of both approaches is to write the three conservation laws: mass and momentum, as above, and the conservation of energy, which implies the introduction of the specific internal energy, and the heat flux vector. Among the quantities involved in these five scalar density equations, some are the "principal unknowns", density, velocities and an (empirical) temperature; the others are "the complementary unknowns" – for instance, in the classical theory, internal energy, stress tensor and heat flux vector **q**. The main question is to write for a given medium the *constitutive equations* which, with the conservation laws, will allow one to find all the equations that need to be solved. In all the theories presented, the only basic statement appears to be the *Clausius-Duhem inequality*.

After a long hesitation, during two or three years, I decided to adopt for my future research and teaching the second standpoint. I have tried to explain this choice at an international seminar in Portugal in 1973 and also in a review paper I was invited to write with Quoc Son Nguyen and Pierre Suquet for the 50th Anniversary issue of the Journal of applied mechanics, in 1983.

It is, maybe, worth recalling how I reached that conclusion. First, it was the discovery, a few years after its publication, of a paper by H Ziegler in a volume of the *Progress in Solid Mechanics*. Writing, as Lord Rayleigh did in fluid mechanics, the function of dissipation, it was noted that it is a homogeneous function of order two for viscoelastic materials, like for a fluid, but of order one only, for a perfect elastic plastic material. For the professor I was, it was very interesting: it opened the possibility to use the same procedure in order to derive basic constitutive laws for two different types of materials. The second ingredient came from a wonderful note of Jean-Jacques Moreau in the *Comptes rendus de l'Académie des Sciences*. Once again I must confess that it took me at least some months in order to see that it contained what, in fact, I was, more or less unconsciously, looking for: the possibility, at least for a large class of materials, to introduce a pseudo-potential of dissipation. The third ingredient came from my colleague and friend, Joseph Kestin. In my opinion, he is one of the few scientists who had a deep understanding of what may be a correct extension of thermostatics to thermodynamics, in order to deal with complex situations in physics. Most of what I will report below finds its sources in the reading of his papers and in my fruitful discussions with him.

I do not want to enter into the system of equations and into the conclusions which permit to write the constitutive equations. But it may be worth answering the fundamental question about entropy and absolute temperature in this theory. And for that, I must comment a little on the significance of internal variables, a concept that appears in any theory of continuum thermodynamics. We state the possibility to introduce some variables describing some physical properties of the matter in the neighbourhood of any particle of the given system – they may be scalars, vectors or tensors – such that, with the variables which describe the deformation of the medium near this particle, it may be considered as the set of normal variables of a local thermostatic system associated to the particle, the l.a.s. (local accompanying state), when density and specific internal energy are the same as those of this particle. All the physical properties of this l.a.s. will be considered as physical properties of the neighbourhood of the particle of the given system. In particular, the entropy and the absolute temperature at each point of this system depend on the modelling chosen for describing the l.a.s.. Let us note the great flexibility offered for adapting this theory to a particular situation. One can choose

1. the internal variables $\alpha_1,......\alpha_n$ and their geometrical nature;
2. one thermodynamical potential ψ, for instance, the free energy to describe the thermostatic properties of the l.a.s.. The derivatives of ψ with respect to the α are the forces A associated to the α. [*equations of state*] ;
3. one writes down the Clausius-Duhem inequality, which gives the dissipation - composed of heat-conduction dissipation and internal dissipation. In the simplest case [normal dissipation, standard material] one may choose a pseudo-potential of internal dissipation, for instance a function χ of the $\dot{\alpha}$, time rate of the α. The derivatives give A as function of $\dot{\alpha}$ - they are the *complementary constitutive equations*, which, together with the equations of state, give the complete constitutive laws of the material.

The dissipation mechanisms which have to be kept in mind for a good description of the system are those whose time rate derivatives have an evolution with time comparable to the rate of deformation. Namely, a dissipation mechanism described by internal variables α such that $\dot{\alpha}$ is very small, may be, approximately, neglected, because α will keep its initial value during the deformation of the particle. Now, a dissipative mechanism such that $\dot{\alpha}$ is very large, may also be neglected, because α reaches very quickly its asymptotic value and consequently, it is nearly constant during the deformation of the particle. The entropy and the absolute temperature of the l.a.s. depend on the number of mechanisms which are retained. Then, the entropy and the absolute temperature are not physical properties of the particle itself; but they are those of the l.a.s. which depends on the choice of the mechanisms of dissipation one wants to take into account.

6.3 Mechanics of materials

The continuum thermodynamics, which have been defined in the foregoing section, provide a frame that must be filled out by observations and experiments. The thermodynamic potential – the free energy – describes principally the reversible and elastic part of the behaviour. The pseudo-potential of dissipation describes principally the main physical properties of the material.

In the simplest cases of standard materials, ψ and χ are convex functions. Many unusual materials may be considered as standard. All their important physical properties must appear in the expression of ψ and χ. For instance, in damage mechanics, in the small-perturbation framework the ψ may be a quadratic function of the deformation, as in elasticity, but its coefficients will be affected by a damage variable, which is an internal variable. In plasticity, very often, the dual convex function of χ is the characteristic function of a closed convex set of the space of the A's, which, in many examples, is just the stress tensor. In such a case, one internal variable must be a tensor which represents a plastic deformation. Most of the physical properties of the material can be read off the two convex functions and their dual variables which describe completely the constitutive equations of the material.

7. TOWARD AN ANALYTICAL MECHANICS OF MATERIALS

I arrive now at a new facet of mechanics as a scientific discipline that I am presently discovering or I have just discovered recently. I am not able to organise them in a convincing way. Nevertheless, I will mention some of them briefly, without long comments, because I think they may become important and also because it is now for me a great satisfaction to learn something of these new developments.

I mention first what may be called the balance of material momentum, which may be considered as a primary notion and is a most adequate concept to exhibit nicely the material properties of a system, as shown by many recent publications of Gérard Maugin. For me, one of the best ways to get this new look at the mechanics of any system is due to Pierre Casal, in a not very well known paper of 1978, that I have, once more, really understood many years later: you define at a fixed time, forces and stresses, not by a virtual motion of the system, keeping fixed the reference configuration, but by the virtual motion of this reference configuration, keeping the position of the system itself, fixed. So, you obtain directly the Eshelby stress tensor and the suitable forces to describe singularities and inhomogeneities inside the material. Pierre Casal obtained directly a very elegant formulation and extension of the Rice integral to compute the stress intensity factor at the tip of a crack.

I indicate now what concerns the global formulations of statics and dynamics of structures, starting with the elegant presentation of energy theorems, variational equations, Castigliano theorems for Lagrangian and Hamiltonian integrals and equations in elasticity, including finite elasticity, and extended to a large class of materials, in particular to standard materials. It is impossible to mention all the questions which are treated: stability, buckling, rupture and all the industrial operations on materials, stamping, forging... On account of my personal interest I will just note the question of phase transition and shock waves and the possibility to extend significantly the concept of a shock generating function. One general result worth mentioning here is a kind of generalisation to systems of relations which have been introduced locally by the continuum thermodynamics.

It explains, at least partially, one of the reasons of the success of what is often called micro-macro description of the properties of the materials. The flawless case is the homogenisation of periodic structures, introduced by Sanchez-Palancia. The most evident is the study of a polycrystal as a collection of monocrystals. More generally, by a self-consistent scheme of localisation-homogenisation, one may relate the variables which appear in the constitutive equations at each point of the macrostructure, to average values (or global values) of the physical properties of the local microstructure – a representative volume element – in which one takes into account its own mechanical properties described as above with the convenient material variables. But, a last remark is in order. The scale of the microstructure is very large in comparison with the scale used by the people in solid-state physics, working with grains, dislocations and disclinations!

I would be tempted to evoke many other questions. But it would not be reasonable, because I cannot pretend to have for them a thorough understanding.

8. BEYOND THE SCIENTIFIC DISCIPLINE OF MECHANICS

Beyond the discipline or rather before the discipline, one has the community of men and women who are doing mechanics, professors and researchers. We had in France many important scientific societies: Société Mathématique de France, Société Française de Physique, but no similar association in Mechanics. Joseph Pérès was the founder of the "Association Universitaire de Mécanique des Fluides" a few years before his death in 1962. An "Association Universitaire de Mécanique des Solides" was created a few years after. The unity of the disciplines of mechanics was only recognised in 1973, with the foundation of AUM - "Association Universitaire de Mécanique", fifteen years after the introduction of continuum mechanics in the new curriculum of applied mathematics and the creation of a laboratory of theoretical mechanics, (now Laboratoire de Modélisation Mécanique) in the Sorbonne, the Paris faculty of science. That was, of course, favourable to my "discovery" of mechanics, which was the main project of my scientific activity.

But it was also in 1962 that a certain event has compromised greatly this project. Despite my firm resolution to devote all my professional activity to my job as a professor, I finally had to yield to friendly pressures and accept to become general director of ONERA. I was not prepared to assume such a function. During five years, this has taken up, approximately, two thirds of my time. I was able to continue with my teaching, to take care of some students and to give time to my duties as the principal editor of the Journal de Mécanique", a newly created journal, in order to give a tribune to the researchers in mechanics, particularly to the younger ones. Needless to say that during this period I have not been very active in research.

I tried to recover a little bit after this experience which gave me the opportunity to have a direct contact with the aeronautical industry and with the new activity of the country in space, by launching a new activity for increasing my knowledge and new research projects in mechanics. One sabbatical year as Visiting Professor in Stanford, thanks to an invitation of Nicholas Hoff, was very helpful. I had practically all the time to learn and to have fruitful discussions with my colleagues and friends, especially Lee and Van Dyke. I had, at that time, a big project: to publish a four-volume treatise for graduate students and researchers on the "Mécanique des milieux continus". The first of them appeared in 1973.

Something that I had never anticipated led me to a new serious desertion of mechanics. Our "Académie des Sciences" needed an important reform (statutes had received only slight modifications since 1816), A new *Secrétaire Perpétuel* had to be elected, after the resignation of Louis de Broglie, in 1975. I had accepted to be the candidate of the fellows who wished to move forward this reform. I was elected. I fulfilled the job during twenty years. I was, and still remain, convinced that a strong Academy must be able to deliver independent advices with the highest vision of what science must represent in the life and culture of a modern society. This decision meant for me the need to give up definitely some personal research activity. I was led to leave my laboratory and to take a professorship at the Ecole Polytechnique, without research obligations. I had an office near the Laboratoire de Mécanique des Solides de l'Ecole Polytechnique, one of the best research units in mechanics in the country and I used this wonderful possibility to talk with my young colleagues and to discuss with them, just as if I were a young student once again. This time I had to teach very well prepared students, who had passed the arduous entrance examinations to this school. Just a few hours were sufficient to give them the basic notions of mechanics, (rigid bodies, fluids, solids) at the level of the first year graduate studies. My colleagues of the Laboratory and the students helped me not to consume too quickly my capital of knowledge.

If I talk about this last period of my activity, it is because I had the opportunity to discover something I feel important about mechanics. Two weeks after the signature of the new statute of our *Académie* , in 1979, President Valéry Giscard d'Estaing asked our Fellowship to write a report about the strengths and the weaknesses of the mechanical sciences and industries in France, and to make the appropriate proposals. The last report of the *Académie* had been written in 1916! By this demand, the Président de la République wanted in particular to test whether the capacities of the *Académie* were at the same level as its claims. With a small team, we worked very hard, in order to give a satisfactory answer. But what is worth to be mentioned here is that I have discovered that mechanics was at the same time a science, a technology and an industry. Evident, of course! But an evidence I had never before realised. It is not the place to discuss the conclusions and the consequences of this report – nearly six hundred pages in length. It is certain that it had a big influence on the orientation of many people working in mechanics, on scientists in universities and research establishments, on engineers and directors of companies and on the orientation of long-term programmes. A committee *Haut Comité de Mécanique* of twenty people (one President, one secretary, six scientists, six engineers and six directors of companies) was created in order to study together the many problems of the activity of mechanics in France and to make suggestions in order to improve the mutual relations between these different groups represented. After a lapse of some fifteen years, in 1997, twenty small scientific and technical associations decided to join together in a single society, the *Association Française de Mécanique*. I have been very lucky to be a participant to this significant evolution of mechanics in France. Fifty years ago, you could not find mechanics among the basic Curricula in Universities; you found some courses, but no laboratories in most of the engineering schools, even in the most famous ones. The mechanical and aeronautical industries were very weak. Now mechanics is a scientific discipline which plays an important role in the present development of sciences and which is directly connected with the industries that have to build goods and equipment with the resources of modern technologies and new materials.

As the famous motto we have adopted in France goes: "*Mechanics? In the heart of a moving world!*" And a professor of mechanics? One of the best spots to look at and to participate in this moving world. That would be my answer today to the remark of the nice guy who filled my tank in the gas station near Brown University.

(English version by Eleni MAUGIN; published in pp.1-24 of **Continuum Thermomechanics, The Art and Science of Modelling Material Behaviour** (Paul Germain's Anniversary Volume), Eds: G.A.Maugin, R.Drouot and F.Sidoroff, Kluwer Academic Publishers, Dordrecht, The Netherlands, 2000.

A selection of scientific works and publications by Paul Germain

(established by G.A.Maugin)

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