Mathematical Research Today and Tomorrow

Viewpoints of Seven Fields Medalists

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Leaving Mathematics for Philosophy

by

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Fields Medal 1958

for inventing and developing the theory of cobordism in algebraic topology. This classification of manifolds used homotopy theory in a fundamental way and became a prime example of a general cohomology theory.

Abstract: This lecture is an overview of the author's own work from homotopy theory and homological algebra to the recent books *Esquisse d'une Sémiophysique* and *Apologie du Logos*, with emphasis on the rôle played by different ideas from biology in catastrophe theory.

The attitude adopted is critical towards experimentation with no underlying theory and towards an improper use of statistics as a justification of analogical thinking in science.

Leaving Mathematics for Philosophy

I must say, to begin, how happy I am to be in Barcelona again. Visiting Barcelona, I think, is a kind of continuous surprise and it is something which is always what the Germans would call *ein Ereignis.*

My talk consists of what happened to me after being awarded the Fields Medal and how, finally, I survived this event.

When, a little while before the once fateful limit of 35 years, you receive the Fields Medal, when you have been subjected to the rituals of the International Congress of Mathematicians, where you get the medal, and, tired but happy, you are left to yourself again, it is then that you are assailed with doubts. Am I worthy of this honour done to me? Were there not to be found, amongst younger mathematicians, some who were more deserving? And, more subtle in its perversity, a suspicion creeps into your mind: Was not this the highest level of achievement I shall ever attain? Whatever I do later on, is it not bound to be something less? Will my mathematical capacities always stay at this level or am I doomed to an inevitable falling off?

All these thoughts were at the back of my mind already in the spring of 1958. Shortly before the date of the Congress came Barry Mazur's proof of the Schönflies conjecture —to my mind a pure gem of the deepest mathematical inventiveness. A little before that, Milnor's exotic sphere structures had appeared. (Milnor did later receive the Fields Medal, this is true.) It seemed more and more evident to me that my capacities for rigorous mathematical proof were lessening as time went by. It was then that I abandoned the more algebraic theories (like homotopy, homological algebra, K-theory, etc.) which bore little attraction for me, and turned again to study the singularities of differentiable maps, a subject to which Hassler Whitney had given a brilliant start in 1951–52, and which appeared to me more flexible and more concrete.

Singularity Theory

I then tried to find out whether the idea of "genericity" of a singularity (like the classical cusp for smooth mappings of one plane onto another) might have applications in everyday life. I remember as I lay on my berth on the liner *Île de France* bearing me to New York and Princeton during my first Atlantic crossing in 1951, that the gentle rocking of the swell gave me the idea of comparing the unfolding of a wave to the transformation of a smooth regular curve obtained as an image of a map $f : I \to \mathbf{R}^2$, which, passing through an ordinary cusp, develops a double point that subsequently breaks, liberating (by surface tension) an isolated "drop" of the initial wave (a phenomenon which can be easily observed in a linear pencil of cubic curves). So, I already had this general idea that looking at a very simple phenomenon of ordinary type, it is not impossible to find for it simple algebraic models for which one can find a sort of a priori generalization or justification.

At Strasbourg University, towards 1959–60, I began, with the help of a physicist colleague, to study the singularities of caustics in optical geometry, as well as their deformations. I was amazed to discover that caustics developed more singularities than required by the simple theory of transversality for plane curves. The singularities of curves in the plane are fairly well known and very easy to describe. To my surprise, I found that in caustics, organized by some very simple optical instruments like spherical mirrors and rectilinear diopters, one may find a singularity that should not theoretically exist. This was the case of the so-called *umbilic points*, which I will discuss later. It took me several years to understand that this was a consequence of Fermat's principle of optimality. An observer aware of this could have foreseen the existence of the optimality principle simply by playing around with caustics.

In 1963 I left Strasbourg for the IHES at Bures-sur-Yvette. There I enjoyed total freedom, with no teaching or administrative tasks imposed on me. About that time, the general idea of sets and stratified morphisms took shape in my mind. In particular, one year before (this was the beginning of the movement of modern mathematics), people wanted, in general, to drop geometry out of the curriculum. Especially the theory of envelopes, because the theory of envelopes was a rather difficult theory to explain and to make precise. So they decided to eliminate the theory of envelopes from the curriculum. This irritated me quite a lot, so I wrote an article trying to explain that the notion of envelopes was just a special case of the general notion of singularity of a map, and if we knew how to deal with generic singularities of maps, then we should also know about envelopes. Thinking about that, I developed essentially the theory of singularities; in particular, the extension of transversality theory to the special jets, which I had already made in 1956.

Then I tried, using Whitney's formulation of the (a, b) property, to get the extension of transversality theory to spaces satisfying, for the connection between the corresponding subsets which were later called *strata*, the corresponding property to the classical stability property of transversal intersection for smooth manifolds.

But the axiomatics I worked out were still inadequate, and it was not until John Mather's work, around 1966, that a satisfactory description of the chief properties of stratified objects and strata —in particular, the topological stability of almost smooth proper maps between manifolds— was proved. This work culminated in my article in the Bulletin of the American Mathematical Society in 1968, and was practically my last piece of strictly mathematical research. After that, I turned into —or "degenerated into"— a philosopher.

The Origins of Catastrophe Theory

This departure from mathematics towards philosophy did not happen at once. Around 1963–64, whilst undertaking the study of classical embryology, I pursued my reflections on the "concrete" use of transversality theorems in the direction of biology. In some sense, I got the feeling that, as a mathematician, I was quite replaceable; that anything I could find would certainly be found without me shortly after, and, in general, in a better way. As a result, I tried to get out of the general program of mathematical production and I wanted to study, precisely, the "concrete" use of transversality theory for natural forms. So in 1965–66 I started to work on what was to become, six years later, catastrophe theory. In these beginnings, the ideas of two biologists, very different one from the other, played an essential part. One was Conrad H. Waddington, with his model of the *epigenetic landscape.* The epigenetic landscape is a sort of landscaping hill with several valleys running down from the top of the hill. It was supposed to describe the succession of cellular differentiations in embryology, each valley describing a common fate of a clone of cells, and every valley bifurcating into two valleys. Following one branch or another would correspond to the choice of one cell between one type of cellular differentiation, and another cell following the other type of differentiation.

The other biologist who had more or less the same kind of idea was Max Delbrück in a paper presented —I think in 1946— in a congress of the CNRS in Paris, where he linked cellular differentiation with what is called a *stable mitotic regime* of embryo cells; that is, the idea that in an embryo dividing by mitosis there could be stable asymptotic regimes of mitosis, which could eventually bifurcate (locally) and create the corresponding clones.

It was in trying to apply this model to embryology that I ended up with my famous list of seven *elementary catastrophes* in space-time.

Meanwhile, singularity theory made considerable headway, already in 1960, with Malgrange's proof of the C^∞ preparation theorem, the use of transversality in stratified set theory, and later (1968–69) with the explanation by Arnol'd and his Moscow school of the general theory of singularities of analytic functions and its mysterious connections with the classification of Lie groups. My "biological" list of seven elementary catastrophes thus found its place in a much wider framework. I did not understand its real nature till some years later (1970–72), when some people explained to me the theory of flat deformations of an analytic set. As you know, if we have an analytic set, i.e., a set defined by a set of analytic equations, and introduce a parameter deforming these equations, we get an analytic family. About these deformations of analytic sets, algebraic geometers define a very specific class, which they call *flat deformations.* Even now I am not sure I really understand the algebraic definition of flat deformation, which, in fact, arises naturally from the theory of flat modules in homological algebra. But, roughly speaking, a flat deformation is a deformation in which the fundamental class of the generic fiber remains of the same dimension when we move the parameters, i.e., we are not allowed to jump the dimension of the fundamental cycle of the complex analytic family when we perturb the parameters. It turns out that, if we are given the germ of an analytic set at one point, then we may try to find out only its flat deformations, and it can be proved by general theory that the family of all flat deformations of a given germ corresponds to an analytic space of universal flat deformations. This space itself can be given an analytic structure.

Staying at the IHES at that time, I was a colleague of Alex Grothendieck. But we were never able to talk or to have any kind of mathematical connection between ourselves. I tried sometimes, but, after a few minutes of talk, Grothendieck was immediately embarked in his own terminology, in his own way of describing things, and I was too lazy to follow Grothendieck's seminar and learn his terminology. The result was that we worked more or less independently of each other. Perhaps because of this, Grothendieck wrote me a personal letter saying that, in this period, I was too lazy. He might have been right in this. I was never able to do as he did: to work all through the night, to go to bed at 3:00 in the morning (or eventually later) typing his machine all the time. This is the type of work I am perfectly unable to do.

At that time, Grothendieck already had a general theorem about universal deformations of analytic sets; but this was, of course, a very abstract result, as was the work generally done by Grothendieck. It turns out, in this problem, that if we take an arbitrary singularity of an analytic set, then its universal deforming space is itself an analytic space, which is in general a very bad set to manipulate; it is in general of infinite dimension and has a large amount of singularities. If the singularity is isolated, then automatically the unfolding space becomes finite dimensional but with singularities. Trying myself to explain the origin of natural forms I had, of course, in mind the basic scheme explained in my first book *Structural Stability and Morphogenesis*, which I wrote in 1967–68.

In this scheme one starts by considering a natural medium, and one defines an equivalence relation between its points. Two points x and y in this domain are said to be equivalent if there exist local neighborhoods U of x and V of y, and a mapping g of U to V having the property that, if we pick a point z in U and take its image $z' = g(z)$ in V, then the local phenomenological properties of the medium in z and z' are the same. The neighborhood of x cannot be phenomenologically distinguished from the neighborhood of y. This defines an equivalence relation between points, and then one asks what are the equivalence classes.

This is a very natural thing to do for any kind of natural system. In some sense, the shape of any object is described by this sort of equivalence class of phenomenological equivalence between points. In geology, when people cut in a trench, they speak about the *facies* of the mineral and would put in the same stratum two points having the same facies. Two facies may be not exactly the same, but they may deform continuously one into the other. This is, in some sense, the basic scheme for descriptive morphology for any natural system. This was the original concept introduced in my book, originating what later became catastrophe theory.

This book was not immediately published. Initially entrusted to the American publisher Benjamin, the edition ran into difficulties, namely the publisher bankrupted and was taken up again by Addison–Wesley only in 1971–72. A few copies were however circulating under the hat and some found an enthusiastic reception by Christopher Zeeman at the University of Warwick. Zeeman helped a great deal in making it possible for the book to appear in French in the spring of 1972. His own reflections on this subject led to a grandiose extension of the theory. In *Stabilité Structurelle et Morphogenèse* I had restricted myself to a single universal substrate, namely $\mathbf{R}^3$ space, or, at most, space-time $\mathbf{R}^4$. But Zeeman, setting the theory ("catastrophe theory," as he called it) in the framework of a "general theory of systems," took for substrate any locally Euclidean space, sometimes defined in an abstract way by some semantic quality.

In fact, in general system theory one considers a system contained in a black box, having inputs occurring at discrete time (e.g. at integral values of time) and outputs at discrete values of time too. Suppose these are vectors. Then one gets a cloud of points in a vector space —product of the input vector space and the output vector space— and the basic program of general theory of systems is to deduce from the shape of the cloud of points the mechanism inside the black box, instead of smashing it into pieces and looking what is inside. The general system theory would look at the shape of the correspondence between inputs and outputs and try to find out, by a sort of interpretation of the shape of these *characteristics* (as they call them), what is inside the black box.

This way of looking at things was a radically new viewpoint opening the way to the creation of multitude of "models" that could be applied to the most varied disciplines, from physics (geometrical optics), to biology and human sciences. Invited to the Vancouver International Congress of Mathematicians in 1974, Christopher Zeeman delivered a dazzling lecture which he was later asked to repeat. As a result, catastrophe theory (which I will call CT from now on) took off like a rocket, propelled by the principal media all over the world. This glory was short-lived, however, and the brief success of CT soon fell to the slings and arrows of trans-Atlantic criticism.

Criticism and Defense of Catastrophe Theory

This experience played a central rôle in my subsequent "calling" as a philosopher of science. There were two main criticisms levelled against CT. The first went like this: Our universe is what it is, and if a phenomenon in it does not possess the "generic" form, there is no way of creating in the world the small deformation that could restore this generic form. Thus, for example, classical dynamics is described by Hamiltonian systems, which are not "generic." The second criticism bears on the pragmatic inefficiency of catastrophe models; the theorem of the unfolding of a singular point of a C^∞ function does not allow quantitative prediction, but at best a qualitative prediction in the neighborhood of the singular point.

It is not impossible to counter the first accusation to some extent; for example, one can invoke the implicit necessity of satisfying certain constraints. (In the case of Hamiltonian systems, a local micro-reversibility with energy conservation.) The second, on the other hand, seemed to me from the start to be fully justified. Since catastrophe models are defined only up to one C^∞ change of coordinates, we cannot use them for prediction in the way that we can apply quantitatively exact physical laws. I did not find it hard to admit this pragmatic shortcoming of CT (although Zeeman was more reluctant to admit it and, in fact, I am afraid he does not admit it even now). But, without subscribing to Rutherford's assertion "Qualitative is nothing but poor quantitative," I had to accept that an inefficient model might nevertheless be "worth thinking out," inasmuch as it confers on the global configuration of the system studied a global intelligibility which would be difficult to acquire otherwise —for example, through experimentation— or to describe by conceptual thinking linguistically expressed.

For those who know about the classical model of dogs' aggression which starts Zeeman's book *Catastrophe Theory*, it is true that I find this model still very useful in the sense that, if we want to express this model in a linguistic way, we cannot do it very easily. We have to use a lot of paraphrases all the time, and the thing which is very easily expressed by the mathematical picture is not easily described by ordinary language. I think this is the fundamental usefulness of catastrophe-theoretic models: They give a picture of a situation which is not immediately amenable to linguistic description.

Anyway, this controversy about the rôle and importance of CT, which finally ended in the years 1977–78 by a kind of disaffection of classical science with respect to CT, obliged me to think about what can be expected from science, and whether the general principle of science that everything has to be justified experimentally —this a priori dogma of experimental justification— has to be admitted.

Just at that time, the theory of catastrophes was superseded by the theory of the so-called *order to noise*, and essentially by the Prigoginian theory of dissipative structures. I was quite surprised by the fact that this theory won such a huge sociological success. In fact, Mr. Prigogine got the Nobel Prize for thermodynamic irreversible phenomena, of which practically nothing is known. I am still surprised that this was not obvious for the people in charge of the Nobel Committee that they could have crowned a nonexisting theory. Anyway, it is true that Prigogine forced most scientists to be aware of the importance of irreversibility in phenomena and about the irreversibility connected to morphological events in the medium. This is a great merit of the Prigoginian theory.

Nevertheless —getting back to my proper region of thought— the success of chaos was also, of course, a factor that took people away from the theory of catastrophes. CT was associated with local dynamics of potential functions, and potential functions is an extremely special type of dynamics. No doubt that it is a very special case, but, in some sense, it is perhaps the only dynamics for which the prediction problem can be solved. That is, given the initial data, can one predict where the trajectory will finish? This can be solved, in a universal way, only for dynamics made by gradients. Otherwise, in general, it is never known what is the outcome of a given trajectory. In that respect, I think that it is extremely important to understand first the simple case in which a complete prediction of the future of any trajectory can be given.

But the success of chaos started in 1975; essentially, after Ruelle–Takens theory in 1972 and, later, the theory of the so-called *weak turbulence*, which was also a great surprise among scientists, who did not expect turbulence to be deterministic. I must say that Arnol′d told me, already in 1966, that he was aware of the weakness of the Landau theory of turbulence and, in that respect, one should certainly recognize his position. But, very rapidly, people discovered the so-called *strange attractors*. Then came the classical examples of Lorentz, Rösler, etc.

From the point of view of practical applications for describing natural phenomena, I do not think —except for phenomena associated with physics— that we have a lot of examples of validity of the "chaosological" approach. Let us say that the idea, which was expected to be very useful, of explaining epileptic attacks by some sort of chaotic process, has turned out to be very "enervant" or quite inadequate.

Among physiologists, for example —there have been many physiologists interested in the chaos explanation of epilepsy, but there are already two radically different schools—, it is said that normal physiology is regular, and it is pathology which leads to chaos; whereas there are a few other physiologists which took the opposite position and said that the reverse is what is true: Normal state is chaotic and many pathologies appear by creating a very simple attractor. The typical example is, precisely, epilepsy. The normal

α-rhythm of EEG is extremely oscillating (there are a lot of nearby variations), whereas the epilectic feature is characterized by an extremely rigid, periodic attractor. So, this is a good example where interpretation of chaotic concepts in concrete phenomena leads to fairly difficult problems.

Going Back to Aristotelian Logic

Another criticism of CT was about analogy. Some people said that the bad thing about CT is that it leads to metaphoric thinking whereas the pure scientists do not admit analogy; they examine reality in itself. I think it is, more or less, philosophically an illusion to distinguish between reality and metaphor. In fact, analogy is, to some extent, a deep phenomenon of our thinking and if we want to understand what analogy is, then we are led to very fundamental philosophical problems, which have now been dropped, more or less, out of the consideration of people. But, in the Middle Ages, with Scholasticism (essentially, the ideas emanating from Aristotle), the problem of analogy was a fundamental problem, for the following reason. Aristotle defined what he called a *genus* as being a class of predicates, a class of adjectives, that had some sort of continuity property. Aristotle used this notion as a kind of classifying scheme for qualities or even for objects in the world. This led him to the notion that a genus might either be a subgenus of another genus, or it might be completely "uncommunicable." That is, it could not be added to other genera. In Platonic arithmetic, there were numbers that could not be added one to the other.

This kind of independence of objects is something very important if we think in the usual way. I was aware of this fact because, already in 1969, I wrote an article against the use of set theory in elementary schools, even in kindergarten. For instance, they put in front of a child a box containing some cubes; large and small, red and blue, and they asked the child to take out from the box the cubes that were *large or blue*. I do not know what the children did —maybe the most intelligent did it— but the fact is that, in usual language, the copula *or* cannot be placed between two adjectives which do not belong to the same genus. The opposition between large and small occurs in the genus of quantity, whereas the opposition between blue and red is an opposition in the genus of colour, and these genera are completely alien one to the other. Thus, asking the children to extract cubes that are *large or blue* is a task which is completely against the natural structure of the mind. We cannot say that a fellow is *short or intelligent*, because these two qualities do not belong to the same genus, whereas we can say that a fellow is *tall and intelligent*, or *short and intelligent*. When two genera are completely independent, we can put the copula *and* between the two qualities, because, in some sense, we take the transverse intersection of the two genera in the universal space of qualities. However, we cannot use *or*.

This is, of course, completely against the usual logic, for which the copulas *or* and *and* are considered as corresponding to union and intersection in the classical interpretation of first order logic. For me, first order logic is basically unsound from the point of view of usual thought. When Boole wrote his books saying that it was an investigation into the laws of thought, he was entirely mistaken. He was totally in the wrong direction from the way we really think, because this independence of genera of qualities is a very important structure of the mind which completely disappears if one takes for propositional logic the set-theoretic interpretation.

So this led me back to consider the old logic of Aristotle and to look at his metaphysics. As a result, I wrote a book which appeared recently in Spanish translation. I am happy to have the opportunity of doing some publicity, although this has been purely coincidental, because I received this translation just before leaving Paris. In this book, I tried to get back to the original ideas of Aristotle and discuss what kind of relation one can see between the way he looked at the world and my own view of applications of CT. There are basically two main points of concurrence between Aristotle and my point of view. First of all, it is obvious that we cannot use any kind of syllogism in Aristotelian logic. Take the following syllogism: "Any man is bipedal; a one-legged man is a man; hence, a one-legged man is bipedal." What is wrong with this reasoning? Of course, if you are a logically-inclined man, you would say that it is the first step which is wrong, because a one-legged man is not bipedal. But if you are a bit more philosophical, you would say "Well, it is natural to say that any man is bipedal, because normally a man is bipedal; there are very few exceptions, which are of course accidental, and we do not take account of these exceptions in the concept of man when we claim that all men are bipedal."

I think that Aristotle had the idea that logic has to be founded in the ontological nature of the concepts. It has to express something in reality. The fact that any man is bipedal is associated with a fundamental fact of biology, namely the fact that, expressed in modern terms, it is "inscribed" in the genoma of the animal that the animal normally developing will have two legs. This is associated in Aristotelian metaphysics with a well-known notion, which is what the old Aristotelian people of the Middle Ages called the *substantial form* of the concept. The concept has a sort of ontological ground in reality, which is its substantial form, its "essence." And it is part of the essence of the man to be bipedal.

So, if we want to use logic in a natural way, in a way which is founded in reality, we have to introduce this sort of essential quality which is associated with any concept, and we will have accidental references of this concept which modify this quality. If we take this point of view, the syllogism fails most times. In Aristotelian physics, the basic notion is that, in front of processes, one has to distinguish those which arise most of times and let pass those which are accidental.

This is a very sound way of looking at logic, and also a very sound way of looking at science itself. We should have a place for accidents for which we do not have any good explanations. In the point of view of CT, one makes the assumption of *genericity* and, when one wants to interpret morphology by means of dynamics, one always assumes the hypothesis of genericity. Doing this hypothesis one makes an appeal to the notion of "something occurring most of the time." Of course, genericity in the framework of smooth potential dynamics is a much stronger notion, because genericity implies an *open* property (the opposite case as, let us say, *meager* in the space of functions). Of course, this distinction did not occur to Aristotle, but the basic idea that natural processes are generic is, in some sense, fundamental in Aristotelian physics.

So I discovered that the things for which I took so much time to develop in CT were already known, to a large extent, by Aristotle. This is, essentially, what I express in my book *Esquisse d'une Sémiophysique*.

Qualitative Description Versus Exact Quantitative Modeling

Of course, it is not to deny that modern science was an advance. Modern science was an advance in the following sense. When Aristotle looked at the process of throwing a stone upwards vertically, he said the stone had first a sort of motion which he called a "forced motion." The upward motion for a stone is not a natural motion, because it goes against Nature. But, arriving at the top of the trajectory, the stone starts falling again, because it wants to reach the center of the Earth, which is the "natural locus" of the stone. So, for Aristotle, there is a sort of catastrophe at the top point of the trajectory, and then the motion of the stone changes its nature. You would probably say that now it is completely silly to look at this process in this way. First of all, I will tell you that saying that gravitation is described by the potential

$$V = gz$$

is perhaps no more explanatory than Aristotle's concept of natural locus. It is perhaps more precise, but it is not very much explanatory. The second point is that the great success of Galileo was to discover that the two motions had the same equation

$$z = v_0 t - \frac{1}{2} g t^2.$$

So, the falling motion was the analytic continuation of the ascending motion. Of course, this was a tremendous discovery, because once we know that some phenomena are directed in their evolution by a law associated with an analytic function, then we are able to make quantitative predictions. In fact, when we know part of a trajectory, analytic continuation gives —at least theoretically— the whole trajectory. We have a kind of canonical way of extrapolation and, hence, the possibility of prediction. I believe it is still true that, even now, strict quantitative prediction in science is associated with analytic continuation. As soon as we get out of the field of analyticity, analytic continuation is not possible. As a result, there is no strict way of extrapolation and no strict quantitative prediction becomes possible.

But the main point is that the qualitative description of a function is still useful in mathematics. In the first year at the university, if we want to teach people how to construct the graph of a function $y = f(x)$, we tell them first to compute the derivative, localize the zero points of the derivative, compute the value of the function at these points, draw the corresponding horizontal segments and then match these segments by a continuous curve monotonously increasing or decreasing between these segments.

This says that the qualitative description of a mathematical object remains useful in any case. This was the basic philosophy of Henri Poincaré. When, in 1880, it was found that the three-body problem was not solvable in any reasonable sense, Poincaré turned to qualitative study of differential equations in the plane and founded qualitative dynamics. This was a tremendous project which is, in some sense, a sort of anti-Galileo success. But now science is still very rigid; everything has to fit into the Galilean scheme. This concerns especially the people using computers, because for them everything has to be finally computed. Thus everything has to be described by, essentially, analytic processes.

But the number of phenomena which are amenable to specific analytic descriptions is relatively very small. We are not willing to accept this, because we think immediately about fundamental physics. Fundamental physics, of course, is expressed by laws which have an analytic nature. Nevertheless, these laws are analytic essentially because they follow from hypotheses of fundamental symmetry of the Universe. So, we have to make assumptions about the fundamental symmetry of the Universe to be able to derive laws explaining the motion of very huge phenomena, like, for example, the origin of the Universe out of the Big Bang and, finally, ending in the subquantum levels of very tiny particles.

People think that because one has laws for very large phenomena and also for extremely small phenomena, one should also have similar laws for phenomena in between. But this belief is quite likely unfounded. There are good reasons to believe that, in descriptions of theoretical physicists, phenomena like, for instance, metastability are still completely outside prediction. Take a glass pane in a window, which is a system in a metastable situation. According to the theory of statistical mechanics, after some time it should fall into its basic components: to a dust of glass particles. Nevertheless, we still believe that our glass will not break so easily, barring accidents.

So, a large number of natural phenomena in our scale still do not obey the general principle of exact quantitative prediction, and this should oblige us to take into account the possible data of qualitative approach. This is why I think that after some time people will, of course, say that CT is not able to give practical results, but nevertheless it may bring a lot of qualitative understanding that could not be easily obtained in any other way. It is my conviction that, after all this turn-over on CT and chaosology, one will get a most balanced appreciation of the situations.

Living as a Philosopher

All this is associated in this book with philosophical conceptions. As a result, I gained some reputation as a philosopher of science. I discovered, to my distress, that the sociology of philosophers is completely different from the sociology of mathematicians. Mathematicians form, in general, a community which —perhaps because of the Fields Medal after all— has a sort of worldwide unity. If we ask a generic mathematician to order the mathematical value of Mr. X with respect to Mr. Y or Mr. Z, in general we get a fairly substantial consensus among the corresponding ordering. When we look at philosophers, this is by no means the case.

I think philosophers live in their own nations; practically there is no worldwide community of philosophers, not even philosophers of science. In France, in particular, there are many philosophers who think that a good philosopher has to be a good writer, has to have style. Since Jean-Paul Sartre, we have been accustomed to think that a philosopher should write plays, novels, and so on. As a result, to do strict scientific philosophical work in France is something which is not very wealthy. I do not want to generalize too much, but certainly it is not something which is very important.

As I said, it is very difficult to know, even now, who are the most brilliant philosophers in Germany, in Italy, even in the United States. Of course, people in the United States have better opportunities of getting a reputation, essentially for editorial reasons still. This sort of situation of the philosophical public is something which is rather difficult to accept for the people who write outside their original speciality.

This is perhaps the lesson which has to be deserved among the Fields Medalists who might perhaps be tempted to give up their original speciality and specialize in other directions. They should not expect to find things easy. As I said, the community of mathematicians has a great knowledge of itself, yet, in the domains associated with human sciences, you would see that the situation is completely different.

I am particularly interested in linguistics. I think that my ideas have opened up some interesting aspects of linguistic theory but, up to now, although I have not been ignored by the linguists, nobody really cares about that. They say that these are things of no interest for them.

This is perhaps an aspect to be considered. I think we need to reach culture with respect to science, so that one finally recognizes that what is important in science is not the distinction between true and false. This might seem strange to mathematicians, but I will say that if I had the choice between an error which has an organizing power of reality (this could exist) and a truth which is isolated and meaningless in itself, I would choose the error and not the truth. There are many examples of errors which are scientifically important, and there are many, many examples of meaningless truths in science.

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