MARCELLO CINI (*)

Criteria of Choice Adopted by Scientific Communities for the Control of Disciplinary Development

1. The dividing line between science and other claim of knowledge

It is by now an almost trivial statement that the growth of scientific knowledge is not a cumulative linear process. But also the less obvious claim that a sharp line cannot be drawn dividing the rational reconstruction of reality performed by science by means of purely logical procedures firmly grounded on factual data, from other kinds of belief based on individual or collective experiences leaving more or less space to irrational and subjective factors, also this claim is no longer considered as unconconventional as it used to be up to recent times.

To make the issue clear let me stress that I am not questioning the claim that science approximately represents existing features of the piece of reality whose knowledge is being sought. My point is that it does so only within an interpretative frame invented by the subject of this cognitive atcivity. The result therefore cannot be looked at as if it were a pure reflection of the object's properties, but reproduces always a relationship whose form leads back to the subject's active role, in spite of the accuracy of the object's portrait, no matter how abundant its details may be.

Traditional wisdom sometimes admits that is the case, but adds immediately that it is possible, and indeed necessary, to erase all the tracks left by the subject, thereby attaining objective knowledge, provided definite universal rules are followed in the assessment of the truth value of propositions about facts. My claim is, on the contrary, that these rules are neither universal nor given once for all, but rather depend strongly on cultural traditions and social environment. This means therefore, that even when a new scientific contribution, after having been scrutinized in the light of the accepted rules, has been recognized as valid knowledge, we cannot assert that the sunjective " impurities " introduced in the cognitive activity have been eliminated by the sieve of abstract rationality. The validity recognition simply means that the accepted contribution

(*) M. Cini, Dipartimento di Fisica, Università di Roma " La Sapienza ".

conforms to the standards established by the social subject entitled to perform this task.

The first thing to do, however, in order to explain what my claim implies, is to clarify the meaning of the word "subject". A careful distinction must be made between the individual scientist and the community to which he belongs. To quote Gregory Bateson: "There is a deep gulf between statements about an identified individual and statements about a class. Such statements are of *different logical type*, and predictions from one to the other are always unsure" (1).

This distinction corresponds to two different moments in the process of growth of scientific knowledge. The first, truly individual, moment is that of invention, in other words, the moment when a proposal is formulated for an innovation in the body of shared knowledge. The second truly social, moment refers to the evaluation of this proposal by the relevant disciplinary community and its final acceptance or rejection.

Innovation always causes a change in the rules of the game: by claiming that certain facts become irrelevant compared to other ones, by discovering analogies between groups of phenomena that had hitherto been considered unrelated, by inventing new concepts to explain the collection of already known empirical data, by eliminating unsolved problems as pseudo-problems, or by turning into questions statements which had been considered obvious up to that moment. The causes which lead the individual scientist to the formulation of this proposal of change are however numerous, different and hidden. It may well be that his idea has been originated by analogies and suggestions which are extraneous to his discipline, or inspired by his metaphysical convictions; or, as Kuhn says, by a rearrangement Gestalt switch in the global perception of the relevant facts.

In all cases no reconstruction, however accurate, of the historical circumstances, the cultural traditions and the social environment in which the new idea was born, can provide a really satisfactory explanation of its origin. These reconstructions are on the contrary fundamentally important for the understanding of a scientific revolution because they throw light on the main factors of the mechanism of acceptance and validation by the disciplinary community of the individual new contribution to the construction of scientific knowledge. The keepers of the rules of the game may accept or reject requests for their change. However, in order to reach a decision, they must base their judgement on other rules, of a different logical type, which are not given once for all, but are, nonetheless, not arbitrary.

2. The hierarchy of levels of knowledge

It is one thing to judge whether a given contribution satisfies the validity conditions which stem from the ensemble of formalized rules that characterize

(1) G. Bateson, *Nature and Mind*, Wildwood, London 1979.

a given discipline at a given moment in time. It is quite another one to judge whether a given proposal to change those rules is acceptable in the light of metarules that fix the norms that should not be abandoned, at that given moment in time, by the practioners of that particular discipline.

The distinction is completely analogous to the one made by Gregory Bateson between different levels of learning in his celebrated essay " The logical categories of learning and communication " (2). In particular, the distinction is analogous to that between two different types of change, introduced by Paul Watzlawick, a pupil of Bateson's, in his studies on the " pragmatics of human communication " (3).

But it is clear that we are not dealing with simple analogy. In all cases, we are talking about separating the changes that come about within a given context from the changes of the context (and ultimately, in their turn, from the changes in the class of changes of the context). The growth of scientific knowledge, therefore, does not escape the general modalities of knowledge acquisition by humankind. Whether one is dealing with collective or with individual knowledge, these modalities are still based on the possibility of ordering the information contained in the messages that produce this acquisition in a hierarchy of classes, each of which is an element of the one immediately above it and which, at the same time, includes those at the next lower level. It is thus the identification of the information contained in each message, and the attribution of each piece of information to the level that is considered appropriate, which produces a growth of knowledge. Knowledge is not, therefore, any longer seen as the simple, undifferentiated accumulation of new contributions, but as a process of enrichment and reordering of this complex system of relationships between classes of propositions about the surrounding, world, classes of propositions about the preceding propositions, and so on.

For the purpose of example, we may recall that the process of learning " by trial and error " is for Bateson only one given level, which must not be confused either with the lower one (learning level zero: a given response for each given stimulus) or with the higher one (learning level two: ability to change the ensemble of alternatives from which one makes the choice at the lower level).

This example appears as praticularly significant for the purposes of our argument if we recall that Popperian evolutionary epistemology considers the growth of knowledge to be equivalent, in fact, to a process of learning by trial and error. ' The development of knowledge " writes Popper " proceeds from old problems to new problems, by means of conjectures and refutations ". And, elsewhere he specifies: " The solution of problems proceeds always through trial and error: new reactions, new forms, new patterns of behaviour,

(2) G. Bateson, " Le categorie logiche dell'apprendimento e della comunicazione ", in G. Bateson, *Verso un'ecologia della mente*, Adelphi, Milano 1976.

(3) P. Watzlawick, J. Beavin, D. Jackson, *Pragmatic of Human Communication*, W. Norton & Co., New York 1967; P. Watzlawick, J. Weakland, R. Fish, *Change*, Astrolabio, Roma 1974.

new assumptions are always provisionally proposed on trial, and are controlled by elimination of the error" (4).

In the light of what has been said above, the limit of this conception seems, then, clear: it consists in the reduction of the complex hierarchy of the levels of control of the process of the development of knowledge to the sole level of the elimination of error in a context assumed given once and for all. It is not by chance that to this reduction there corresponds in Popper a unidimensional vision of reality which sees clouds at one extreme and clocks at the other (5). But we are not something that stands between the former and the latter of these: we are more complicated than either.

This point of view also facilitates the discussion of Popper's traditional opponent, Thomas Kuhn. It is clear that his periodization of the history of science into phases of normal science, separated by brief periods of revolutionary change (6), comes much closer to the process I have outlined, in the sense that the former clearly correspond to the acquisition of knowledge within a given context, while the second correspond to changes of the context. This dichotomy is, however, excessively schematic. For here, too, there is a tendency towards this flattening down of the levels which can lead to damaging confusions. Paradigmatic change is assuredly a change in the criteria that regulate the bounds of acceptability of the contributions that are regarded as valid for the development of a given discipline, but it too is also subject to *metarules* which are no less binding even if less formalized. What seems to be missing from Kuhn's conception is again an awareness of this hierarchy of levels of selection and control which has the function of ensuring the maintenance of the identity of a given discipline, at the same time as allowing the acceptance at the lowest possible level of the changes necessary for its survival.

This hierarchical structure explains, moreover, why the entire discussion between Kuhn and his opponents (7) on the nature of the discontinuity implied by the concept of scientific revolution is a discussion that can go on *ad infinitum* if it is not understood that a change in paradigm consists simulateneously in a discontinuous change of the rules that define *how to do physics* and the maintenance of the metarules that define *what physics is*.

This articulation of the concept of scientific revolution also resolves another problem that has been the subject of a long dialogue of the deaf: that of the circularity implied by the relationship between paradigm and community. If the community is defined as the ensemble of scientists who share a given paradigm, and this latter is nothing other than the rules which the scientists belonging to a given community have inherited and which they utilize for the development of their discipline, then neither the former nor the latter is defined.

(4) K. Popper, *Objective Knowledge*, Oxford at the Clarendon Press, Oxford 1972.
(5) K. Popper, *quot.*
(6) T. S. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago 1962.
(7) G. N. Gilbert, M. Mulkay, *Soc. Stud. Sci.*, *12*, 1982, 383.

If, however, the levels are distinguished, everything becomes clearer: the community is composed of everyone who shares the *metarules* to be used for changing the rules of "normal" research activity by safeguarding what they maintain constitutes the indispensable identity of the discipline. This definition is not hollow nominalism. It is the only one that explains why an Einstein or a Schrodinger, who most certainly had every right to form part of the physics community, was in fact pushed out onto the sidelines after 1927. In the same way it also explains why a von Neumann, an outsider with respect to the physics community in that by profession he was a mathematician, became on the other hand one of its most authoritative members.

3. Sciences of laws and sciences of processes

Let us now have a look at the whole spectrum of natural sciences. It is common practice to divide them into two types: on the one hand normative sciences, characterized by the search for, and the statement of, necessary and universal laws of nature, and thus capable of making rigorous predictions, and, on the other, the evolutionary sciences, considered incapable of acceding to the sphere of universality since they are given over to the investigation of irrepeatable processes, and thus at most able to provide a hypothetical reconstruction of a succession of events within a context that can no longer be modified. To this distinction corresponds a substantial difference in epistemological status. In point of fact, it is the former (the sciences of laws, or of *why*) — based on the repeatability of the experiment, on the modifiability of the initial external conditions, on the eliminability of factors considered to be of a secondary nature — which are regarded as having the full right to be called sciences since they are verifiable or falsifiable (according to differing points of view) with respect to nature. The latter on the other hand (the sciences of processes or of *how*) are often maintained by epistemologists to be second class sciences, which owe to the former those general laws that are held to be capable of providing the sole convincing explanations of the concatenation of events which these latter are limited to describing as plausible. Only the complexity of the context, the multiplicity of the accompanying factors, the incompleteness of the documentation relating to the events that have actually happened would, according to the dominant conception, justify recourse to these "surrogates" of science which, are, it is said, essentially lacking in any autonomous epistemological status.

A number of questions, then, immediately spring to mind. What is it that fixes the criteria of definition of the different disciplines and places them on one side or the other of the watershed? Up to what point do these criteria stem from the nature of the subject, that is to say from the collection of phenomena and facts that constitute the (approximately closed) ensemble to be interpreted and explained, and to what extent on the other hand do they depend on the choice made by the scientists who select the data and the experiment to construct their science, thus placing it on the one side or on the other? It is commonly maintained that it is the object investigated that determines the nature (the science of *why* or the science of *how*) of the discipline that studies it, the scientist being limited to taking note of what there is before him or her. But, in actual fact, this demarcation is much less objective than one might think. The case of the most recent developments in the field of biology provides an obvious demonstration of this.

Up to the middle of the seventies, this " science with a schizophrenic personality " — as M. Ageno defined it recently (8) — was present in two completely different guises. On the one hand functional biology, essentially a reductionist discipline, was given over to the analytical study of each organism, determining its structures and internal processes right down to the minutest details. It thereby tended to bring the explanation of all biological phenomena down to the events that take place at this latter level and therefore, in point of fact, to reduce biology to the physics and chemistry of the molecules involved. On the other hand, evolutionary biology, instead, considered living organisms as indivisible entities whose particular characteristics come out only at the level of the totality and are only partially deducible from ana analysis of the constituent subunits.

To these two faces correspond two opposed ideological conceptions that characterize their respective scientific communities. On one side the molecular biologists have arrived at the point of pushing their reductionism as far as the assertion that when " we know all the details of all the chemical steps that take place in the cell in the course of the entire cell cycle, there will be nothing else left to know about the cell itself, and its life mechanism will have been completely deciphered ". On the other side, the students of evolutionary biology reject this position that professes to reduce the whole explanation of living phenomena to a knowledge of the structure and interactions of the atoms and molecules that constitute the organism, falling back in their turn on an integralist ideology, holistic in nature, that can be summarized in the somewhat empty and generic slogan " the whole is greater than the sum of the parts ". We are faced, then, with two biologies: the first is to be located in the group of the sciences of laws, or of *why*, while the second is that of the sciences of processes, or of *how*. But the revolution that has taken place over the last few years following on the discovery of unexpected properties of eukaryotic DNA has changed this picture quite radically. Let me quote Ageno: " Far from being something invariant, which is in essence conserved within the framework of the dynamic of the organism, DNA now appears to be involved in a dynamic all of its own, one that is incessant and to a great extent dominated by random events. Molecular biologists now see themselves forced, a little at a time, to change the type of questions which, within the framework of their research into the functionality

(8) M. Ageno, " Importanza della concezione darwiniana nella biologia odierna ", unpublished manuscript.

of the organism, they were wont to ask... Face to face with the multiplicity of *a priori* equivalent solutions, the search for causes, the quest of the *why*, shows itself to be surprisingly indecisive and unimportant, and molecular biologists are ever more led to ask themselves *how* each solution has come into being, through what chain of events and in what general environmental conditions.

Thus natural science and functional biology are, in actual fact, finding their common root in the theory of biological evolution. For biological phenomena, there are no possible explanations other than evolutionary ones.

What has been happening in biology thus indicates that the traditiona demarcation, in this case at least, seems to have been created more by choice of the scientists themselves than by the properties of the objects under study. These events show, in point of fact, that the molecular biology community, aiming at retaining for its own discipline the normative science character that distinguished it from the start, has as far as is possible eliminated from this disciplinary field all those phenomena which would have compromised its image by the impossibility of fitting them into the framework of general and atemporal laws. Similarly, they show that evolutionary biology, by accepting this division of spheres of competence, continued — as long as this division did not enter into crisis — to consolidate its own original nature as a science of irreversible and non-repeatable processes.

At this point then, the doubt arises as to whether this mode of procedure, consisting in choosing the objectives of the investigation and the corresponding interpretative categories so as to ensure consistency between the development of the discipline and the epistemological status attributed to it by definition, is typical not only of biology but might be a common praxis adopted by the different scientific communities to define their own identities. That this happens in social sciences probably does not come as a surprise. But that it also constitutes the rule in physics, which has always been considered the science of laws *par excellence*, may seem a difficult assertion to maintain. I am here proposing to show however, that in this case too, at least in respect of a recent historical period, affairs have in fact developed just in this way.

As we shall see, the history of twentieth century physics acquires a new interpretative dimension if one sees it as the result of a consistent undertaking by the scientific community to keep for this discipline the characteristics of a science " of laws " by as far as possible excluding from its bounds all those phenomena, together with their interpretations, which could have introduced into it some characteristics of the sciences " of processes ".

From this point of view, one can for example understand why the sensational developments that have latterly taken place in the field of the dynamics of complex systems (9) have been fifty years late with respect to the appearance of

(9) See for example L. Galgani, A. Scotti, " Recent Progress in Classical Non-linear Dynamics ", *Riv. N. Cimento*, *2*, 1972, 189.

the pioneering work of Poincaré (who in essence laid the basis for them), despite the fact that this work has been available since the end of the last century. And one can also understand how these developments have come about in disciplinary sectors that have now become autonomous with respect to the fields that physicists define as physics.

The victory of quantum mechanics at the end of the twenties represents, if seen in this light, the success of this operation of reasserting physics as the science of laws — albeit at the price of giving up determinism in the strict sense — by thrusting back and even outside its boundaries all that has to do with the unpredictability, irreversibility, randomness that characterize phenomena such as turbulence, dynamic instability, stochastic processes, the thermodynamics of irreversible processes, etc. This point of view also explains why physics has maintained and protected its image as science of the simple, excluding complexity as much as possible as one category that characterizes the reality selected as the subjects of its investigation. Thus it is that we witness an unchallenged dominance of reductionist ideology, with priority being assigned to the search for the "elementary" constituents of particles, which still today constitutes the essence of the most prestigious branch of physics.

This operation revolves around one key figure, that of John von Neumann, the man who quite definitively transformed the retreat that physics had to make with the renunciation of classical determinism into the reassertion of its supremacy, by succeeding in bringing chance back into the laws of logic.

Opposition to this operation, as clearly emerges from the reconstruction that Steve Heims (10) makes of the two personalities in his well-known book, came from Norbert Wiener. It was not however in the sense that Wiener ever systematically presented an alternative programme in direct competition with von Neumann's on this latter's own terrain. Wiener was contraposed to von Neumann because he proposed a strategy that was to be developed on a different terrain, in essence consisting in an enalrgement of the boundaries of physics to encompass all those random phenomena which, far from being exhausted within the confines of the quantum realm, would have represented the rule even for other fields of physics, within which the reversible determinism of Newtonian mechanics would then be the exception.

This reconstruction of the conflict between different strategic choices seems to me more satisfying than the traditional one that sees, above all, the opposition (at the 1927 Solvay Congress) between the submissive supporters of a classical deterministic vision of physics (Schrödinger, Einstein: "God does not play at dice") and the victorious upholders of a conception based on indeterminism (11). This contraposition certainly took place in the stages that preceded the victory of quantum mechanics. But the very lightning nature

(10) S. Heims, J. von Neumann, N. Wiener, *From Mathematics to the Technologies of Life and Death*, MIT Press, Cambridge Mass. 1981.

(11) See for example M. Jammer, *The Conceptual Development of Quantum Mechanics*, McGraw-Hill, New York 1966.

of the victory won shows how this conflict was more a rearguard action than a real debate between perspectives that potentially could have given rise to alternative developments. Paul Forman's reconstruction (12) of the cultural and ideological climate of Weimar Germany that saw the establishment of quantum mechanics, however accurate and acute it is, seems to carry little weight if assumed as the principal explanation of the ready conversion to the new theory of the German physics community, given that a hypothetical faithfulness to a deterministic conception could not have been translated into a scientifically valid alternative. It becomes, on the other hand, much more convincing if seen as the background to a compromise that saved the fundamental nature of the scientificity of the discipline (i.e. its being the source of an unchallenged logical and empirical legality), rather than as justification for an opportunistic surrender to the dominant irrationalism. It may be said in passing that it is in this way that the apparent contradiction — for which Forman provides no convincing explanation — between the adhesion to the theses of the Vienna Circle on the part of many of the founders of the new physics and the tendency, that certainly was present, towards adaptation by the community to pressure exerted by the dominant ideological-cultural environment, is resolved. At the same time, it also explains why it was that Dirac, who, contrary to Heisenberg, saw quantum mechanics more as a logical outcome of than as a radical break with classical mechanics (13), immediately found his place among the fathers of the new theory alongside the German physicists.

We shall now therefore, look in more detail at the reconstruction of this contraposition.

4. The logic of chance

I have al ready had occasion to refer to the role played by von Neumann (14) in codifying in the form of real and proper vetoes, formulated in scientific language, the two ideological strongpoints of the Göttingen-Copenhagen school:

a) the ultimate and definitive nature of QM;

b) the impossibility of an objective description of reality because of the indispensable role of the observer.

Both these assertions of a metatheoretical nature were transformed by von Neumann into propositions that belong to the theory itself. This is a point

(12) P. Forman, "Weimar Culture, Causality and Quantum Theory", *Hist. Stud. Phys. Sci.*, *3*, 1971, 1.

(13) M. De Maria, F. La Teana, "Dirac's 'Unorthodox' Contribution to Orthodox Quantum Mechanics", *Fund. Sci.*, *3*, 1982.

(14) M. Cini "L'identificazione dei criteri di scelta degli scienziati nella storiografia e nella ricerca scientifica: il caso della meccanica quantistica", *Internal Note*, Istituto di Fisica, Università di Roma, no. 797/1982, paper at the 3° Congresso Nazionale di Storia della Fisica, Palermo 1982.

that must be emphasized since it is exactly here that we find the proof that the axiomatization of QM is an operation of definition of the boundaries of the discipline, boundaries of whose integrity the community had to become guarantor. Which was the goal of von Neumann's program? A biographer, Steve Heims, writes:

This logical mastery may have affected von Neumann's views and premises concerning the world. It became his mathematical and scientific style to push the use of formal logic and mathematics to the very limit, even into domains others felt to be beyond their reach. He seemed to regard the empirical world, probably even life and mind, as comprehensible in terms of abstract formal structures. He seems to fall under that tradition in Western thought in which it is believed that only rigorous logics will ever succeed in containing the timeless, universal truths that govern everything.

The guiding characteristic that informs so much of von Neumann's work — Heims goes on to say — is the effort to devise as far as possible, and even further, a formal or mathematical structure within which to contain the mysteries and complexities of life. It is a naive and optimistic faith in mathematical machinery. He is pushing out the bounds of the subject matter amenable to logic.

In this way, even chance can be brought back within the compass of a purely logical scheme, and seen as the manifestation of definite, general and atemporal laws. Everything that belongs to the physical world is thus brought back to the sphere of logico-mathematical legality. Only consciousness remains outside it. But through this, chaos, disorder, the unpredictable are all driven out of science which thus reacquires its uncontaminated purity.

In the limits imposed on this contribution, it is not possible to say more on this. I should like, however, to emphasize, even though we shall come back to this later on, that von Neumann can well be considered the most representative exponent of the overwhelming majority of the scientific community of those theoretical physicists who were his contemporaries.

His commitment to the tools of reason — concludes Heims — and his prowess in their use, together with his apparent disinterest in philosophical issues beyond the assertion of the primacy of formal structure and faith in scientific progress, helped making him a paragon among early twentieth-century scientist-mathematicians.

We now turn our attention to the figure of Norbert Wiener, the person who most consistently expresses — at the research programme level as much as at the epistemological one — a conception of the world and of science that is contrary to that which we have just discussed.

It would be too long to illustrate in detail the manifold aspects of Wiener's personality that are reflected in his concrete way of working as a scientist. I shall limit myself to underscoring three fixed points of his conception of the world which appear to be of particular significance. At the age of ten, Wiener wrote an essay entitled "The Theory of Ignorance" in which he gave "a philosophical proof of the incompleteness of any form of knowledge" (15). (Remember von Neumann's aspiration to demonstrate the *completeness* of knowledge!) This theme remained a constant conviction for the whole of his life. Knowledge by its nature is limited, hence so too is the ability to control events; this is humankind's inevitable destiny, but at the same time a perennial stimulus to its insatiable thirst to know. And it is this theme that we again find in his scientific interests. The choice of Brownian motion as the fundamental problem of his most important line of research is exactly the choice of a problem in which the incompleteness of information and partial ignorance play an essential role. The aim that Wiener set himself was that of giving a rigorous mathematical form to the description of a sequence of events even in the case when they are the product of chaotic actions.

The second fundamental point of his vision of reality regards the non-eliminable nature of time in any valid description of phenomena. If von Neumann is to be placed within the tradition that comes down to us from Parmenides, Wiener, on the other hand, belongs to that of Heraclitus. For Wiener "science is the explanation of processes that take place in time" (16). For him, every scientific problem ought to be formulated in terms of time-varying statistical processes.

Lastly, the third point regards his holistic conception of reality: everything is connected with everything else. It is this which lies at the base of the other fundamental contribution that Wiener made to twentieth century science: cybernetics. "Control and communication in animals and in machines" is, as we know, the subtitle of his book (17). Control and communication imply an end-directed behaviour. In this sense, Wiener brings back unto scientific thought a concept that was discredited: the Aristotelian final cause, the goal. But this reacquires scientific importance in so far as it is identified with the self-regulatory mechanism of a system using feed-back. The mathematical treatment of this mechanism, an omnipresent one in the most varied and recondite aspects of the world that surrounds us, according to Wiener, makes it possible to extend physics from the realm of matter and energy to that of communication and information.

How did the scientific community of physics welcome Wiener's ideas? Very differently, one may say, from how they reacted to von Neumann. This latter was as much "the paradigmatic model of the mathematical scientist" as the former was considered the prototype of the eccentric mathematician with his head in the clouds. So it was that, despite all his efforts, Wiener's interlocutors remained throughout the whole of his scientific career — apart from the close-knit community of mathematicians who recognized him as the founder of their disciplinary sector — the systems engineers, the students of applied

(15) S. Heims, *John von Neumann...*, *quot.*, 140.
(16) *Ibidem*, 151.
(17) M. Wiener, *Cyberbetics*, Wiley, New York 1948.

mechanics dealing with turbulence, the medics looking at biomedical technologies, even the psychiatrists and psychologists studying behaviour, but not the physicists.

5. Who decides what physics has to be?

In the introductory part, I maintained the thesis that the scientific community, through its choice at the end of the twenties, wanted to restore to physics the character of being a science of general and immutable laws, a Galilean science founded on the conviction that "the great book of nature is written in mathematical language and its symbols are triangles, circles and other geometrical figures", a character that the crisis of the first decades of the century had thrown much doubt on. The essential element of this reassertion consisted in bringing the chance, probabilistic aspects of the new mechanics back within the rules of a logico-abstract algorithm through the elimination of their temporal character. From this point of view, the new mechanics emerges more as the legitimate heir of Newtonian mechanics than as its implacable opponent. For as we see, the equations of motion in both theories are deterministic and reversible, and the temporal evolution of the quantities that represent the state of a system is no other than the deployment of a succession of changes that contain nothing new, inasmuch as they are potentially included in any one of the past or future states of the succession itself, chosen arbitrarily.

I now propose to corroborate this thesis by showing how the way out selected was anything but obligatory, and how it has even implied the removal of a series of basic problems that the crisis of classical physics had raised by postponing their being taken into consideration for several decades. Here, I should like to make it clear that I do not mean to support a position that the physics community ought at all costs to have tried to formulate an alternative theory to QM by the use, for example, of the techniques already developed by Wiener at the beginning of the '20s. I should, rather, wish to underscore the ideological hostility that for a long time hindered any attempt to explore possibilities that existed in other directions.

The first fact that should make one reflect is constituted by the disappearance from physics of the concept of irreversibility from the end of the nineteenth century right up to our times. It is well-known that first Boltzmann, and then Planck, maintained that the second principle of thermodynamics was an absolute law of nature. And as such, it played a central role for them in physical science.

I would recall only that Planck's programme, which then unexpectedly led to the black body law and to its interpretation in terms of quanta, set off with the intention of demonstrating that the electromagnetic field contained within a cavity would irreversibly reach the equilibrium state at a given temperature by virtue of no more than the equations of motion (18). Only when this

(18) T. S. Kuhn, *Black Body Theory and the Quantum Discontinuity*, Clarendon, Oxford 1978.

objective proved unreachable (because of the reversibility of the equations themselves) did Planck abandon the description in terms of a temporal evolution in order to adopt the statistical one in terms of the probability of states that Boltzmann had already adopted after having himself run into the same difficulties. From then on the question of irreversibility remained confined within the community of the chemical physicists. From the Onsager relations of 1931 right up to the work of Prigogine's school this line of research has remained, however, on the sidelines of physics, so much so that Prigogine was awarded the Nobel Prize for chemistry.

Time thus disappeared from physics in the sense that the evolution of a statistical ensemble towards equilibrium ceased to be a problem worthy of interest: statistical mechanics was reduced to the calculation of partition functions at equilibrium. And it is exactly this point on which was based the introduction of discontinuity, a discontinuity that was to become the seed from which QM germinated. One had to wait until 1954 for the Kolmogorov theorem to discover that the property of ergodicity postulated by Planck is far from obvious for complex systems. The problem, then, was openend up again, but history had by now run its course.

This became the subject of research of another community: other journals, other congresses, other professorships. Whoever tries to ask him/herself how physics would have been if the KAM theorem had been known in 1900 is now looked upon as an eccentric.

Another expulsion happened for another subject born within classical mechanics at the end of the nineteenth century with Poincaré's famous note on the three-cody problem: that of dynamic instability. This was a theme which in its turn opened up an unexpected breach in Laplacian determinism, which seemed a necessary consequence of Newtonian mechanics. Developed in particular by the astronomers in the '60s, and taken up independently in numerous other contiguous and allied disciplines, the subject is based on the fact that completely deterministic non linear dynamic systems can have a "wildly chaotic" behaviour. Only in 1977 was there held a conference at Como on the stochastic behaviour of Hamiltonian systems which, for the first time, brought together astronomers, biologists, economists, physicists and mathematicians who were all working in this area. It is significant that the conference was called thirty years after the famous one at Como that gave QM its official baptism, thus at least in the intentions of the organizers making the explicit claim that the '77 conference, analogous to the previous one, represented an historic turning point (19).

A third field of research, which was pushed out onto the sidelines by the physics community and which only recently has received a great impetus — coming back into physics by the window after having been pushed out through

(19) G. Casati (ed.), *Stochastic Behaviour of Classical and Quantum Hamiltonian Systems - Volta Memorial Conference*, Como 1977, Springer Verlag, Berlin 1979.

the door, as one might say — is that of stochastic processes. We have already seen how the pioneering work of Wiener at the start of the '20s were in essence ignored by the physicists who, right up to the '70s, never took into serious consideration the possibility of utilizing the mathematical tools developed at that time to deal with problems of interest for their discipline. It is of interest to underline the fact that these instruments, as Battimelli has shown in his work on the birth of the discipline that grew up around the research into turbulence, were, in the 1940s, adopted with notable success by this community through the direct links between Wiener and the founder of this theory, J. C. Taylor. In the 1930s, the very act, on the part of Kolmogorov and the Russian school, of having founded such a fruitful new discipline as the classical theory of probability passed practivally unobserved by physicists right up until the most recent times. The techniques of stochastic processes began, in point of fact, to become fashionable among theoretical physicists who dealt with statistical mechanics and field theory in the 1970s.

It is too soon to say whether this change, which for the moment seems to consist essentially in the adoption of more efficient and flexible techniques, is the prelude to a conceptual change by one part of the community in respect of the attitude to be adopted towards problematics typical of the sciences of processes. Personally, I would maintain that the situation is evolving in a direction analogous to that depicted by Ageno in the biological field.

This is the end of my story. It shows the type of things that someone like myself, an active member of a scientific disciplinary community who refuses, at the same time, to consider the behaviour of his fellows as the only one ratio nally possible, has the opportunity of understanding.