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The Science of Complexity: Epistemological Problems and Perspectives

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Argument

For several decades now a set of researches from a wide range of different sectors has been developed which goes by the name of "science of complexity" and is opposed point by point to the paradigm of classical science. It challenges the idea that world is "simple." To the reductionist idea that each process is the sum of the actions of its components it opposes a holistic view (the whole is more than the sum of the parts). The aim of the present article is to analyze the epistemological status attributed in the science of complexity to several fundamental ideas, such as those of scientific law, objectivity, and prediction. The aim is to show that the hope of superseding reductionism by means of concepts such as that of "emergence" is fallacious and that the science of complexity proposes forms of reductionism that are even more restrictive than the classical ones, particularly when it claims to unify in a single treatment problems that vary widely in nature such as physical, biological, and social problems.

1. Introduction: on the notion of complexity

Scientific terms may be roughly divided into two categories: those that are introduced by means of a precise and even formal definition (which is the case for many of the more recent mathematical terms) and those that are drawn from everyday language and which have further to travel before they attain the status of an unequivocal definition. The word "complexity" (from the Latin *complecti*, grasp, comprehend, embrace) belongs to the second category and is particularly resistant to precise definition. This is also because it is often confused with the word "complication" (from the Latin *complicare*, fold, envelop), and because both terms are mostly used to mean the opposite of "simple."[1] This negative characterization is undeniably effective despite its vagueness, as it captures one of the central aspects of the role played by the concept of complexity in contemporary science: this consists of the radical opposition to a central idea of classical science, namely that the structure of the world is fundamentally simple and that the essence of scientific analysis lies in resolving (dissolving) the apparent complexity of

[1] On these topics see the excellent overview contained in Lepschy 2004a; see also Lepschy 2004b and Lepschy 2000.

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phenomena into its simple constituent elements. However, the search for an exact definition in the literature referring to the so-called "science of complexity" remains fruitless. This search is frustrating because it leaves open the question of whether this definition is intrinsically impossible or whether we are still only half way along the path leading from the vague commonsense notion to a notion defined in rigorous scientific terms but still nonexistent.

It is interesting in this connection to examine the point of view of John von Neumann, as he is often considered one of the scientists having made the greatest contribution to the topic of complexity in modern science.[2] In his work we find both the terms "complexity" and "complication" being used and it is quite difficult to determine whether he uses them as synonyms or as two different situations in the following sense: "complicated" is what presents a web of interrelations that is difficult but not impossible to reduce to simple elements; while "complex" is what is intrinsically impossible to reduce to a simple description. However, it is hard to situate this kind of dichotomy within the framework of von Neumann's thinking. Despite his extraordinary incursions into the topics of "complex" processes, von Neumann is one of the modern scientists who remained most faithful to the reductionist paradigm.[3] This is shown by his way of viewing the various topics of scientific meteorology: however complex meteorological processes may be, an increasingly detailed and precise observation of the empirical data, their real-time transmission to the computing and forecasting center, the use of sophisticated mathematical models, and above all of increasingly powerful computers, would ultimately overcome any complexity, reducing it to a mere problem of "complication." In other words, an increasingly thorough combination of empirical precision and computing power, supported by good mathematics, would always succeed in unraveling complexity and reducing each process to its simple constituent elements.

Nowadays, this optimistic hope is widely considered to have failed. It would be very interesting to explore the historiographic issue of whether the linguistic dichotomy between "complexity" and "complication" used by von Neumann actually contributed – in a manner apparently opposite to his intentions – to the attribution of autonomy to the notion of "complexity," as an expression of something possessing characteristics that were resistant to any attempt at simplification.[4] This analysis could be extended so that it leads to the observation that many acquisitions that contributed to the formation of the "science of complexity" came about in spite of the intentions of

[2] "Von Neumann's interest in 'problems of organized complexity,' so important in the social sciences, went hand in hand with his pioneering development of large-scale high-speed computers. There is a great challenge for other mathematicians to follow his lead in grappling with complex systems in many areas of the sciences where mathematics has not yet penetrated deeply" (Kuhn and Tucker 1958, 120).

[3] Also this statement demands several corrections as, during the final phase of his interest in game theory, von Neumann apparently leaned towards a holistic view. We will examine this briefly in the following.

[4] We shall return to this topic in the new edition of Israel and Millán Gasca 1995 (Israel and Millán Gasca forthcoming).

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the protagonists. We shall see later how the early discoveries regarding the phenomenon of "chaos" – which is today considered as one of the lynchpins of this science – elicited a reaction of rejection. This was aptly interpreted by Pierre Duhem when he said that they were results "forever unusable," that is, a mathematical pathology that was intrinsically interesting but lacking in any practical utility for science.

Essentially, the objective of discussing the "science of complexity" clashes with a two-fold difficulty: the absence of a rigorous definition of complexity and thus the impossibility of arriving at a precise definition of the object and methods of this "science"; the high degree of articulation of the historical pathways leading to the topics of complexity, that is, to something that is ill-defined, "a style of theorizing" rather than "a specific subject matter."[5]

With regard to the definition of complexity, it must be stated from the outset that there is but a single area in which this notion has taken on a very precise meaning – that of algorithmic complexity theory. This is a theory founded by the mathematician Gregory Chaitin on the basis of the work of Shannon and Kolmogorov, which measures the complexity of calculation in well-defined quantitative terms. And this is the only case in which we are up against an exact quantitative definition and not a qualitative idea. It is more than justifiable to claim that algorithmic complexity theory has very little in common with that vast archipelago that goes by the name of "complexity science." In this second context the notion of complexity encapsulates the very general idea that most phenomena cannot be tackled in terms of classical reductionism, that is, they cannot be conceived of as the result of the interaction of their separate parts. The science of complexity is based on the negation of the principle that "the whole is the sum of the parts": it thus predicates a "holistic" view, the need to consider systems as an integrated whole that cannot be broken down into simple elements. The science of complexity aims at visions of the "whole." The idea that "the whole is not the sum of the parts" has as a corollary that in a complex system "new" or "unexpected" properties occur ("emerge") as the analysis of the behavior of the individual components does not allow them to be predicted. In extreme synthesis, *holism* and *emergence* are the principle characteristics of the notion of complexity.

The historical analysis of the interwoven currents that led to the vast and fragmentary field of the science of complexity is extremely interesting but these currents lie outside the scope of the present article, which proposes rather an epistemological discussion of several tenets of this science. However, avoiding any undue schematism, we can group these numerous currents into three main lines. The first line is represented by a series of theoretical considerations that develop in different scientific sectors, such as cybernetics and information theory, and even before this the study of homeostasis phenomena, later known as feed-back, whose better known manifestations consist of the well known

[5] "There is no consensus definition of complexity studies . . . . Indeed the subject matter is often taken to be scarcely distinguishable from "everything," which is perhaps why the disciplines at issue have so far yielded a richer harvest in vague hunches than concrete results" (Talbott 2001, 16).

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work of von Bertalanffy on system theory. These considerations are linked to the more recent ones on self-organizing systems (Ilya Prigogine) and "autopoiesis" (Humberto Maturana and Francisco Varela). The second line is paradoxically rooted in the most classical and reductionist of areas, that is, that of deterministic dynamical systems. "Chaos" theory – which is one of the hobby-horses of the science of complexity – is rooted in Poincaré's research on the three-body problem, in Hadamard's research on geodetics and those of Van der Pol on variable resistance electrical circuits (see Israel 2004a). It was subsequently developed on the basis of the E. N. Lorenz's studies on the meteorological models (Lorenz 1963). In fact chaos theory may be included in the enormous development of studies on differentiable dynamical systems and their properties: stability, attractors, bifurcation phenomena (in particular Hopf's bifurcation), ergodicity, etc. Also the theory of fractals (developed above all by Benoît Mandelbrot) and René Thom's catastrophe theory are closely related to this classical approach in that they have their roots in differential topology and the qualitative study of ordinary differential equations, in accordance with the phase diagrams approach. Therefore, the authors who have observed that "dynamical systems theory has conceptualized many of the fundamental principles on which complexity sciences depend" (Goldstein 2001) are right. And this is the reason why the present article focuses above all on the approach linked to dynamical systems theory. Also the first line – whether it is the idea of system, of analysis of far from equilibrium states à la Prigogine, or of the idea of feed-back which is closely connected to that of limit cycle – has very close links with the classical line.

A separate place must instead be reserved for those developments that are distinctly different from the preceding approaches. This is the case of game theory – the third line – which foreshadows a new type of mathematics based on an analysis of the combinatorial type, on fixed point or convex analysis methods. It is also the case of those theories based on a strictly probabilistic approach – without any underlying differential structure – or else on ideas drawn from Darwin's evolutionary theory. The most striking example, in this context, comes from the theory of genetic algorithms due above all to John Holland (see Holland 1998). Although our analysis, as we have seen, is focused mainly on the first two lines, some mention must be made also of the third as it sets out to propose a radically innovative approach to the question of "emergence" on the basis of the idea of evolution drawn from biology.

2. Resuming the topic of the "unreasonable effectiveness of mathematics"

It may be considered odd that a reflection on the current state of the "science of complexity" should take as a starting point the content of the 1960 article by Eugene Wigner on the "unreasonable effectiveness of mathematics" (Wigner 1960). In our view, from a re-reading of this article more than forty years on several interesting points emerge. The first is that this paper is already imbued with the issue of the

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relationship between simplicity and complexity and this underpins several of its more problematic and even ambiguous aspects. The second point is that it serves the purpose of showing how the issue of complexity is closely linked to the question of the nature of mathematical entities.

The very title of Wigner's article arouses in the minds of many a reaction of astonishment: the linking of "unreasonableness" and "mathematics" – being the latter the "rational" knowledge par excellence – might appear to be an oxymoron. In actual fact, the article seems to be pervaded by a peculiar irrationalistic, and even mystical, attitude. "And it is probable that there is some secret here which remains to be discovered" (Wigner 1960, 1), runs the phrase by C. S. Peirce that Wigner used as the epigraph to the article, in which the words "surprising," "mystery," and "miracle" recur very frequently. It is said that "the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and that there is no rational explanation for it" (ibid., 2), and that "the miracle of the appropriateness of the language of mathematics is a wonderful gift which we neither understand nor deserve" (ibid., 14).

But why talk about "miracle" or "enigma"? Because after all Wigner does adopt a *sceptical* attitude. He does not really believe that there exist *natural laws* that cover a broad range of phenomena. "The world around us is of baffling complexity and the most obvious fact about it is that we cannot predict the future" (ibid., 4). And it is precisely this "baffling complexity of the world" that turns the fact that "certain regularities in the events could be discovered" into a "miracle." To tell the truth these are only very partial regularities, as the "laws of nature contain, in even their remotest consequences, only a small part of our knowledge of the inanimate world. All the laws of nature are conditional statements which permit a prediction of some future events on the basis of the knowledge of the present, except that some aspects of the present state of the world, in practice the overwhelming majority of the determinants of the present state of the world, are irrelevant from the point of view of the prediction" (ibid., 5). In essence, "it is not at all natural that 'laws of nature' exist, much less that man is able to discover them" (ibid.).

Therefore, it is precisely the realization of the complexity of the world – and here we refer only to the inanimate world – that leads to Wigner's sceptical and even mystical attitude, and radically distances him from a view such as Galileo's. Conversely, for Galileo there was nothing mysterious or rationally inexplicable about the effectiveness of mathematics: for him, the truth was that the world had been written by God in mathematical language, and even in a *simple* fashion. Therefore, however complicated this simplicity might be for the human mind, mathematics is the master tool with which to seize the truth of the world. More than one century later this complication would be viewed by Laplace as an insurmountable obstacle to any exact prediction of events, although Galileo's belief that the world is structured in accordance with natural mathematical laws was to remain intact. And this belief was to provide full support also for Fourier's view that the world and mathematical analysis are in a one-to-one correspondence, i.e. they are "co-extensive" (see Israel 1981).

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In this conception of Galilean origin there are two central aspects: the conviction that the world is *simple*, albeit "complicated," and the conviction that its structure was written by God in mathematical terms. There is no doubt that there is a close relation between the idea of "natural law" and the theological view.[6] Even recently Cohen and Stewart pointed out that the physicist's belief that the mathematical laws of a theory actually completely govern every aspect of the universe is very similar to the faith of a priest that laws of divine origin are at work (Cohen and Stewart 1994, 364–5). It is not easy for panmathematism to rid itself of this theological latency. In the very moment in which it strives to do so, furthermore proposing a view of the world as something of inextricable complexity – complexity and not just complication, as the former is an ontological category and the second an epistemological one – it oddly enough leads to an irrationalist or even mystical attitude. This was the point of view of Bourbaki, which was quite similar that of Wigner:

> There is a close link between experimental phenomena and mathematical structures which seem to quite unexpectedly confirm the recent discoveries of contemporary physics; however, we do not understand the underlying reasons . . . and perhaps we will never understand them. . . . mathematics is like a reserve of abstract *forms* – the mathematical structures – and it happens – without any apparent reason – that certain aspects of experimental reality are moulded in several of these forms, as though through a kind of pre-adaptation. (Bourbaki [1948] 1986, 46–7)

If the relationship between mathematics and reality no longer has the nature of absolute transparent necessity, the analysis of this relationship will demand delicate speculation concerning the way in which it should be preserved: is it possible to conceive of an evolution of mathematics that is completely uncoupled from empirical facts and, if this is not the case, what are the limits within which mathematics can proceed independently of these facts without running the risk of becoming lost in the labyrinth of arbitrariness? Reasoning like that developed in this regard by von Neumann (see Neumann 1947) would be completely superfluous in a Galilean context.

Clearly the Galilean view has nothing in common with empiricism. It is well known that the rise of the "new science" is closely related to the rediscovery of Plato. In fact, Galileo is an authentic Platonist thinker. Like Plato he believes that mathematical "ideas" have nothing material about them and that their abstract nature differentiates them clearly from physical objects. But, like Plato, he believes that reality is structured on the perfect ideas of physical objects. Therefore, only in the world of perfect ideas lies the true and intimate reality of the physical world. And the world of ideas on which the physical world is structured is that of mathematical concepts. This provides the metaphysical foundations of the "new science": as Koyré pointed out, "an Aristotelian type of science, which starts from common sense and is based on sensible perception,

[6] On this topic two classical references are Koyré 1957 and Funkenstein 1986. See also Casini 1976.

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has no need for the support of a metaphysics. It leads to and does not start from the latter." On the other hand, a science "that postulates that mathematism is the way to truth . . . cannot do without a metaphysics. Indeed it can do nothing but start from it. Descartes was aware of this. As was Plato, who was the first to have outlined a science of this kind" (Koyré 1944, 80).

As soon as – for a variety of reasons that determine that process known as "loss of certainty" (see Kline 1980) – it is no longer believed that mathematical concepts are the platonic ideas on which nature is structured (or has been structured by God), a two-fold alternative arises: to admit that mathematical concepts are mere constructions, inventions of the mind, or else to transform platonism into a kind of "mathematical platonism" according to which mathematical ideas live in a world of their own, which is not a purely mental one, otherwise it would consist of simple inventions. Mathematical ideas pre-exist, in a world "of their own" and the mathematician discovers them as he passes through them, just as the naturalist discovers and classifies plants and insects as he strolls through nature. This is a strange platonism in which an objective reality is attributed to mathematical entities that is completely separated from the world of concrete phenomena, whatever they are, and which have a profound relationship with it, but one which is now completely mysterious and inexplicable. And yet this is the predominant vision of the mathematical world from the end of the nineteenth century on, and it is one that many mathematicians strenuously defend despite the extraordinary difficulty involved in defining the status of this strange self-contained world.[7]

The development that led to abandoning such a clear but rigid framework of the classical view – namely renouncing the idea of natural law, of a mathematics that represents the essence, the reality of nature and of concepts that are not intended to be pure intellectual inventions – has led to a heavy cost. The difficulty of such a renunciation is shown by the fact that, even today, physics is dominated by the idea of natural law. But this trend led also to extraordinary developments and innovations. We must face here an (apparent) paradox. Precisely because it lost the status of world of ideas that structure nature (and express the laws governing it), mathematics has shaken off the limitations of the classical conception and has galloped over an endless plain of "applications" or "models" that was hitherto prohibited: biology, economics,

[7] See the book-dialogue between the mathematician Alain Connes and the neurobiologist Jean-Pierre Changeux: "AC: . . . [the mathematician] has the impression of exploring a world . . . and of attaining a coherence that shows that a whole region of it has been explored. In these conditions how can it not be felt that this world has an independent existence? JPC: You say "felt"? Your attitude to mathematics is thus a feeling rather than a thought? AC: Rather an intuition, a painstakingly constructed intuition. . . . AC: . . . the reality that forms the object of mathematical knowledge does not change. Once it has been established and demonstrated, the list of simple finite groups will never change. It is indeed the product of a discovery. What has this context got to do with finalism? . . . I am by no means a finalist. And I do not think I can change my mind . . . JPC: And why not? AC: No, no . . . JPC: Perhaps you embarked upon the discussion with somewhat definitive ideas . . . AC: I believe in them . . . JPC: Careful, you are using the word "believe"! AC. Yes, of course. But part of the discussion is meta-physical" (Connes and Changeux 2000, 53, 62).

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social and management science, even psychology and anthropology have become new hunting grounds for mathematics. Of course, the spreading over such a wide area, the heterogeneous nature of the objects – no longer only "natural" and often not even "objects" –, the variety of the criteria of experimental verification – in many cases the impossibility of any such verification – has led to the abandonment of the reassuring rigor of classical reductionism, of the interaction between mathematics and experiment on which knowledge grows in a spiral fashion. The mathematical army is scattered over the land it conquers, its rigor is softened and worn down, the reasons underpinning the effectiveness of mathematics – an effectiveness that is made even more surprising by the wide range of applications – becomes an even deeper mystery and the course of research becomes increasingly difficult to control. As von Neumann asserted nearly half a century ago, now "the sciences do not try to explain, they hardly even try to interpret, they mainly make models. By a model is meant a mathematical construct which, with the addition of certain verbal interpretations, describes observed phenomena. The justification of such a mathematical construct is solely and precisely that it is expected to work — that is, correctly to describe phenomena from a reasonably wide area" (Neumann 1955, 491).

However, the model, precisely because it is an ill-defined object – now approaching the rigor of classical reductionism, now at best validatable in terms of empirical plausibility, now a simple mental image – blurs the boundaries between the exact sciences and the "soft" sciences. In its less rigorous and quantitative forms *the model approaches the "story,"* the sciences come closer to history and to narrative (see Israel 2001a).

There would actually be nothing wrong with this – something we shall discuss in the conclusion – although clearly it is no easy matter to accept that a form of knowledge that for centuries was believed to possess the virtue of drawing upon certainty should be forced to endure a loss of effectiveness and rigor that brings it closer to uncertain knowledge and constantly changing opinions. It is not easy to forego the principles of the classical paradigm: objectivism and predictability.

An attitude like that chosen by Wigner is a coherent one. The admission of the inextricable complexity of phenomena and the fragility of the idea of "natural law" is reconciled with the observation of the effectiveness of mathematics in openly mystical terms, without getting involved in any attempt to construct *ad hoc* epistemologies. On the other hand, when the decision is taken to view complexity no longer as a limit to knowledge but as an object of research, it is difficult to conceal the difficulties that will arise if the requirements of predictability and objectivity are to be satisfied. In the recent post-modern phase of research the epistemological acrobatics that we have witnessed have been far too bold. We shall examine later a few examples. For the time being, let us simply point out that no one has the right to get away with it like the epistemologist and sociologist of post-modern science, David Bloor, who has declared: "Perhaps a truly objective knowledge is impossible? The answer is certainly no. Objectivity is real, but its nature is quite different from what we would expect"

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(Bloor 1991). The idea of a "completely-different-from-expected-objectivity" is not a concept worthy of being put forward in any serious discussion.

3. Objectivism and prediction

One frequent way of getting around the difficulty of interpreting the realistic meaning of phenomena such as deterministic chaos or strange attractors is to appeal to "the idea that scientific laws relate to human knowledge of the external world, rather than to that world *per se*" – an idea which "is a relatively modern one, owing much to the development of the quantum theory" (Deakin 1988, 186). This amounts to saying that science no longer describes anything objective but only our subjective knowledge, our subjective images of the phenomena. This is perfectly all right, provided that we are aware of what the consequences are. It means demolishing scientific objectivism, that is, that which allowed science to be proclaimed for several centuries as a higher form of knowledge than others and that still today is entitled to the halo surrounding it. It means demolishing the concept of prediction, that is, the other superior feature characteristic of scientific knowledge: the capacity to predict and therefore, in principle, to *reproduce* the phenomenon and not merely to describe it empirically.

Furthermore, these two needs are clearly interrelated: it is not possible to conceive of prediction in the absence of objectivism. This crucial aspect of classical science was erected as its real supporting column by Nicola Cusano. It has as its paradoxical starting point precisely the idea that it is impossible to construct a univocal and objective human representation of the universe that possesses any characteristics of absoluteness. However, it is precisely this impossibility of constructing a *complete* and *definitive* representation of the universe that forms the basis of the objectivity of human knowledge! Indeed, only if there is a hiatus, an unbridgeable gap between human knowledge and reality, if there exists an objective reality that serves as *term* and *reference* of the process of knowledge – however *unattainable* –, does the latter rest on a foundation of truth to which it tirelessly and constantly tends. If human knowledge and reality were merged, the latter would have the same nature of finiteness and imperfection as the former and we would have no basis on which to determine the truth of our deductions. But if we admit the existence of an *objective* reality distinct from our thoughts that they cannot attain in absolute terms, we may still conceive of an indefinite, never-ending or never-endable process of approaching and approximating it, in the certainty that our deductions represent an approximation of the truth and thus form an increasingly perfectible part of it.

Therefore, the gap between empirical truth and absolute truth can never be definitively bridged, however hard we try. But just when this observation seems to preclude all forms of objectivity from human knowledge, it actually opens up to the opposite claim. It is actually this very gap which guarantees the existence of knowledge, because its foundations lie in the reference to an ideal and perfect

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entity. As Ernst Cassirer points out, "the cut-off separating the sensible from the intelligible, empirical world and logic from metaphysics, . . . is what guarantees the right of experience. . . . 'Separation' and 'participation' are so mutually non-exclusive that indeed one may not be conceived of except by means of and as a function of the other" (see Cassirer 1927).

It should be noted that, in this conception, mathematics plays a central role in so far as it represents the only way to provide a world of concepts that are representative of ideal and perfect realities – a world that represents the only possible bridge by means of which to link up empirical ideas and partial truths with absolute truth. Mathematization is the highway leading to the construction of a science that participates in the truth.

It is precisely this conception that underpins the well-known "manifesto" of Laplace's causalism (see Laplace [1825] 1986). Laplace makes a very clear distinction between the ontological level and the epistemological and predictive level. Nature is governed by strictly causal laws that are reflected in the mathematical equations describing it (see Israel 1991 and Israel 1992). This in no way means that any exact prediction is possible: it would be attainable only by an "intelligence" so huge as to know exactly the initial state of every single constituent element of nature and the forces acting on each of them at any given instant, and to be able to "subject these data to analysis." But the human mind only "offers a pale shadow of this intelligence" – the well known "Laplace's demon"[8] – and even though it tends constantly to approach it "*will always remain infinitely far from it*." It is difficult to understand how a Cusianan language – which so clearly distinguishes the ontological level from the predictive level – was able to give rise to such a welter of misunderstandings and completely false interpretations. The idea that it is possible to make a perfectly deterministic prediction of events was attributed to Laplace, while in actual fact he says exactly the opposite. And it is certainly no coincidence that he did set out this view in the introduction to a treatise on probability, that is, on a mathematical theory the need for which arises out of our incapacity to attain a perfect knowledge of the causal structure of phenomena, a theory that "partly related to this ignorance, and partly to our knowledge." Furthermore, the causal nature of phenomena cannot be subjected to empirical verification: it is a "self-evident" principle of a purely metaphysical nature, which leads back to the Leibnizian principle of sufficient reason. But it is precisely this essentially metaphysical nature of Laplacian causalism that makes it indestructible "from within": it is fruitless to seek an internal contradiction in Laplace's discourse.

Consequently, the numerous attempts to present Laplace's discourse as a kind of absolute *predictive* determinism, according to which perfect prediction would be attainable by man, purely and simply amount to mystification. And it is even more absurd to attempt to demonstrate that even Laplace's Intelligence is powerless in the

[8] Perhaps this term was introduced as a tribute to Laplace's atheism. When asked by Napoleon what place was reserved to God in his theories, he replied: "Sire, I have no need of this hypothesis." As we see, however, he did need it, and how.

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face of prediction. It is said, for instance, that, although "much cleverer than us," it would not in any case be able to determine exactly a number with an infinite number of decimal places, and would thus be stalled by "an intrinsically uneliminable area of uncertainty in a measurement performed in finite time" (Bertuglia and Vaio 2003, 279–81). The fact is that Laplace's Intelligence is not just "much" cleverer than we are, it is "infinitely" so. It alone – like God or an equally powerful demon – has the power of identifying exactly the initial state of a system and thus of determining its evolution perfectly. On the other hand, the evolutionary trajectories of a deterministic dynamical system – perfectly determined and distinct from one another – exist by virtue of the theorem of existence of the uniqueness of solutions or ordinary differential equations. Instead the crucial point is that for certain systems that verify this theorem – and therefore such that for each point in their phase space passes one and only one solution –, it happens that the prediction is subject to insurmountable limitations because of restricted human calculation capacity – not to mention that of empirical analysis – and the system becomes "chaotic." However, the degree of chaos of a system belongs to the (epistemological) level of prediction and not to the ontological level. It is no coincidence that we speak of "deterministic chaos" and it is only amateur epistemologists who can afford to speak of a self-contradictory determinism: "chaotic behaviours arise in the context of a completely deterministic system and thus provide a means of exorcising the Laplace demon" (Deakin 1988, 190).

The question was raised in a more interesting way a century ago by Pierre Duhem. He spoke of a "forever unusable" mathematical deduction. Indeed – he added – "a mathematical deduction is of no use to the physicist as long as it is limited to stating that a given *rigorously* true proposition has as a consequence the *rigorous* exactness of another given proposition. In order to be useful to the physicist it is necessary to demonstrate that the second proposition remains *approximately* exact when the first is only *approximately* true. And this is still not enough . . ." (Duhem [1906] [1914] 1989, 206–215).[9]

Therefore, the phenomenon of chaos does not allow causalism to be confuted. And not only that. It cannot even be used to demonstrate that the world is "chaotic," or that it possesses the high degree of complexity involved in deterministic chaos. It would be an abuse to consider the existence of chaos *in a model* – which is a purely mathematical fact – as proof of the chaotic nature of the phenomenon this model is claimed to represent. In this way we would surreptitiously be introducing the idea that the model is a perfect image of the phenomenon being examined. However, in order to state this it would be necessary to "verify" the model which, in the presence of chaos, is no easy matter except in qualitative terms and within a time interval in which

[9] Duhem continues as follows: "It is necessary to restrict approximately the amplitude of these two; to fix the limits of the error that may be made in the result, when one knows the degree of precision of the methods used to measure the data; it is necessary to define the degree of uncertainty that may be granted to the data when it is necessary to know the result with a given approximation."

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the effect of exponential divergence is not felt. Here we are in the presence of a circular situation. Clearly some considerations concerning the "butterfly effect" defined not as a characteristic of the model but as an intrinsic characteristic of the real phenomena – as though someone had really demonstrated that the beating of a butterfly's wings in Europe could cause a cyclone in the Caribbean a few months later – are effective in astonishing the general public but are absolutely groundless.

The main difficulties arise concerning prediction. It should be observed that "it is not legitimate to say that in a system following an expansive dynamics prediction is impossible: we can only say that it is expensive, and that its cost grows exponentially with the time interval for which we are seeking for a reliable prediction" (Thom 1986, 19–20). Obviously, for a long-time prediction such cost would be prohibitive. It is therefore legitimate to say that the phenomenon of deterministic chaos entrains the fact that for many models not only is medium- and long-term prediction unreliable but that it seems to be impossible to bridge the gap between prediction and the objective reality of the phenomenon – that is, to "perfect it" – in accordance with the process indicated by Cusano and Laplace, which, while actually remaining infinitely distant from absolute exactness, nevertheless indefinitely brings us closer to it. This indefinite approximation is being challenged. So Vulpiani is correct when he observes that the paradigm proposed by von Neumann for the purpose of founding a science of atmospheric phenomena based on computer science seems to be in crisis: the hope of predicting or actually controlling the behavior of complex systems, such as the atmosphere, by refining the mathematical models, the calculus methods, and the data recording systems, would be groundless if the models in question are affected by chaos (see Vulpiani 1994).[10]

Therefore, while one is obliged to admit that the phenomenon of deterministic chaos is not an incidental pathology, the consequences are much more serious and profound than those implied by some silly consideration on the subject of the indeterminism that is believed to arise out of determinism – a consideration whose sole vain hope is to demolish determinism without touching the objectivism of science. Instead, if it does have any consequences, it is precisely objectivism – the principle of objectivity, the cornerstone of science, as Jacques Monod called it – that is severely affected. If the prediction is not, we shall not say perfect – no one ever expected it to be – but not perfectable; if the objective reality is not only indescribable in exact terms – which is something we already knew – but no longer even represents the term of reference for our representations that allows us to verify the process of

[10] Following Thom we could say that such a program would be too "expensive" and therefore impracticable. However a supporter of the classical approach – like von Neumann, who tended to reduce complexity to "complication" – could always respond that a new mathematical approach could lead to the suppression of the annoying phenomenon of chaos. Such an attitude would be similar to that of someone who hopes one day to return to the causal explanation of elementary physical processes: it is no coincidence that von Neumann himself defined such a perpective as possible although improbable.

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perfection of our knowledge, what does our science become if not the description of itself? It is no longer the image of reality, but the image of our images. The boundary between scientific description and narrative description becomes blurred and dissolves, the scientific model is confused with the tale. For several centuries we believed that the scientific description of the world brings us ever closer to its "truth," unlike other forms of knowledge that are only "relative" and lack any solid basis; now we are obliged to admit that this superiority is not as obvious as it seemed.

The problem is not that of Laplace's Intelligence, which we supposedly knocked off its pedestal, exorcized or whatever you will, but rather lurks calmly in the same place as before. Nor does the problem consist in the fact that we have supposedly demonstrated that the world is not a causal one: we are no wiser than before in this connection. More modestly we must admit that the problem is *ours*, that the crisis is *ours*. That is to say we are no longer able to guarantee that the process of scientific knowledge has a *progressive* nature, that it acquires an increasing quantity of "truth." And this realization opens up the way to the spectre of relativism. Unless we accept the idea of a less restrictive concept of objectivity, not restricted to the world of mathematical concepts, and at the same time clear and precise, but certainly not reduced to some post-modern humbug of the kind "objectivity-of-a-nature-totally-different-from-expected."

4. Reductionism

Much of present-day epistemological confusion stems from the desire to believe that scientific knowledge is passing through a period of glorious growth and, having freed itself from the shoals of the old paradigms, is now proceeding towards the conquest of ever wider areas of reality. But, on the contrary, scientific knowledge is passing through an extremely critical transformation phase. Another typical example of this epistemological confusion stems from the way classical reductionism is presented.

The procedure usually adopted is as follows: a formal definition, as restricted as possible, is given of reductionism; it is then knocked down as an "Aunt Sally" so as to be able to assert the existence of a new non-reductionist science. Unfortunately for this approach the concept of reductionism is an informal concept which has a complex historical development of its own and cannot be "reduced" to a logico-mathematical definition. Therefore, it is necessary to come to terms with the "complexity" of the concept of reductionism and this makes an accurately targeted confutation difficult. Quite frequently, the critics of these *ad hoc* versions of reductionism are actually, albeit unconsciously, reductionists themselves, like Molière's Monsieur Jourdan who spoke in prose without being aware of it.

Among the "reduced" and *ad hoc* versions of reductionism two stand out and are often intertwined. The first consists in presenting reductionism as a procedure for the systematic reduction of the complex to the simple, which is formalized as a tendency to represent each phenomenon with linear mathematical schemata. Reduction of the

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complex to the simple is without doubt one aspect of reductionism but in no way characterizes it completely. Indeed any attempt at explanation or even at description inevitably implies some form of "simplification," unless it is claimed that the only form of scientific representation is 1 : 1 mapping. The translation of the relationship between complexity and simplicity into the relationship between non-linearity and linearity will be dealt with in the following, and we will see that it is often accompanied by unacceptable distortions.

The second *ad hoc* version of reductionism consists in presenting it as the claim that "the whole is the sum of the parts." Also here the concept of "sum" is usually associated with a mathematical meaning, in the sense that it consists of a sum in the narrow sense, or at most a linear combination of the various parts.[11] Let us take the case of dynamical systems. If we accept such an oversimplified version of the second principle according to which "the whole is the sum of its parts," then the distinction between reductionist and non-reductionist systems would be the same as that between the systems in which the eigenvalues of the whole are linear combinations of those of the parts and those in which they are not. The presence of eigenvalues that cannot be "reduced" linearly to those of the parts seems to indicate the "emergence" of "new" properties. This was indeed the approach that led von Bertalanffy to conclude that the theory of dynamical systems was essentially a non-reductionist theory (Bertalanffy 1968). However, the *ad hoc* nature of this distinction is quite clear and is linked to the distinction between linear and non-linear that will be illustrated below. What is neglected here is the fact that reductionism, also in the version of the principle according to which "the whole is the sum of its parts" does not imply the absence of the properties emerging in Nature, if we assume for a moment that this concept of emergence is clearly defined (on this topic see Newton 1997). Quite the contrary, it claims to be able to describe these properties as the effect of the action of the individual parts!

It may be observed that the confusion arises out of the fact that the idea that "the whole is the sum of its parts" is interpreted in an unduly strong sense. In actual fact, when a system has been *defined* as the sum of its component parts, it is pointless to try and refute this procedure from within by positing the "emergence" of "new" properties, and above all by attributing a convenient meaning to the vague concept of "new." In order to proceed correctly, it would be necessary, for the same real system to compare two opposite approaches: the first would be to break the system down into parts and then describe it as the resultant of the behavior of these individual parts; the

[11] "To assert that reductionism is effective actually means that we can break the systems down, study the microdynamics of the parts and obtain the macrodynamics of the system as the sum of the microdynamics. We thus assume that a direct proportionality exists between the microscopic behavior of a "small-sized" subsystem and the macroscopic behavior of a "large-sized" system as a result of which the ratio between microscopic dynamics and macroscopic dynamics is equal to the ratio between the two dimensions (or between the two scales): these hypotheses are the same as assuming a linear relation between micro level causes and macro level causes" (Bertuglia and Vaio 2003, 264).

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second would be to consider the system "holistically," that is, as an indivisible whole, and use global parameters of a completely different kind.

In fact, there is no possible compromise between a holistic and a reductionist approach. Such a compromise would lead to the situation described by Koyré as "a nominalistic misinterpretation of the relationship between a *totum* and its parts": in fact, "a *totum* reduced to the pure addition of its parts is not a *totum*" (Koyré 1968, 42).[12]

Let us consider an example. The same economic system could be described in either "micro" or "macro" terms, which would involve adopting completely different descriptive criteria and parameters of state. It would thus be necessary to show that the global approach is richer than the "micro" one in descriptive terms. It would ultimately be necessary to demonstrate that, in spite of appearances, the object examined is not the same and that the "micro" description is poorer and therefore less valid from the empirical point of view.

One typical example of a correct approach in this context is the project proposed by Werner Hildenbrand regarding the theory of general economic equilibrium. This project consisted in founding the law of demand without the need for a construction in terms of aggregation of the individual demand functions of the economic agents, which Hildenbrand defined as useless and even "pseudoscientific" (Hildenbrand 1994; on these topics see Ingrao and Israel 1990). The procedure consisted in formulating aggregate hypotheses concerning the distribution of the agents' characteristics, thus abandoning the conventional idea that individual actions are described by the hypothesis of individual maximization behavior. The decisive step would thus consist in comparing the results obtained using the Hildenbrand approach with those of classical microeconomics. We shall not go into the details of this comparison which, on the whole, are not particularly comforting for Hildenbrand's holistic approach. What is important is to observe that this is the only correct procedure for ridding oneself of a reductionist approach (in the narrow sense described above). Conversely, there is no point in following a "micro" approach and in continuing to define an object as the sum of the individual parts, and then to say that the whole is not the sum of the parts as it behaves differently from the individual parts: this could serve the purpose solely of demonstrating that "reductionism" takes the "emergent" properties into account and is thus effective.

From a historical point of view, the concept of reductionism is something much more complicated than suggested by these gross oversimplifications and has an essentially epistemological basis. It derives from an image of the tree of knowledge

[12] The concept of holism customary in science of complexity "does not refer to wholes independent of, or antecedent to, the parts. The term 'emergence' testifies to a bottom-up conception of the whole: it is not that the whole generates, and manifests itself through, its parts, but rather that the parts, by interacting, generate the complex behavior of the whole that 'emerges.' It is hardly clear, from the current literature, what this emergent whole is thought to be, beyond the sum of its parts" (Talbott 2001, 17). On "emergence" see the following section.

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which establishes a hierarchy that begins with the knowledge of the physical world and rises towards that of the chemical, biological, psychological, and sociological world. The most radical form of reductionism postulates the possibility of reducing each sphere of knowledge to the preceding one in the hierarchical tree, and thus ultimately of reducing every phenomenon to physical processes and ultimately to the interaction among elementary particles. But even the more radical reductionists do not go so far as to claim that "a knowledge of physics makes the whole of chemistry a futile consequence": they allow that "chemistry is filled with concepts and theories that go no further than merely repeating those of physics; biology has its own local theories that are much more important for biological understanding than the mere knowledge of chemistry and physics on which it is ultimately based. To predict that psychology will be based on biology does not amount to denying that psychologists need to have their own means of comprehension; to expect that one day sociology will be founded on psychology and biology is not the same as claiming that *it is only* the sum of these two sciences" (Newton 1997, 58–59).

However, there do exist less extreme forms of reductionism, like those contained within a single discipline (such as the paradigm that "everything is genetics"[13] in biology), or else between two individual disciplines. Regarding the second case mention could be made of the controversies involving the nature of the reductionist basis of economics, believed to stem from mechanics, according to the paradigm of Léon Walras and Vilfredo Pareto, and from theoretical physics, in the views of Irving Fisher; or else from biology, according to Alfred Marshall; on the other hand, according to the game theory of von Neumann, Morgenstern, and Nash, but also according to Gérard Debreu's approach, economics has logico-mathematical foundations. Therefore, reductionism has a much more complex structure than some convenient syntheses would lead us to believe.

The profound limitation of the reductionists' position is their belief that science y will in any case inevitably "one day" be founded on science x which "precedes" it in a hierarchic order of which they are unable to give any justification. They base their belief on the idea that "Nature is one and interrelated" (ibid., 59). In one sense this idea postulates an undemonstrable and specifically metaphysical thesis, namely that society and the mind are "natural" facts (see Israel 2004b). On the other, it postulates another *a priori* idea, namely that the hierarchic chain linking the spheres of knowledge, which is the specific feature of reductionism, is self-evident and inevitable and is actually the reflection of a kind of intrinsic hierarchy of nature. The most radical critique of reductionism consists in the observation that this hierarchic chain has no characteristic of necessity, that it is merely a contingent epistemological approach, which is neither necessary nor sufficient for the progress of knowledge.

[13] From this paradigm stems the so-called "gene-for syndrome": "the 'gene-for' syndrome (as in 'gene for intelligence' or 'gene for sexual preference'), in which genes that contribute to human traits are instead taken to specify that trait" (Gallagher and Appenzeller 1999, 79).

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5. "Emergent" properties and complexity

One of the hobby horses of the science of complexity is the possibility it gives of accounting for "emergent properties." However, the concepts of "emergent property" and "emergence" – which are closely connected to the concept of holism[14] – are rather ambiguous and hard to define. Indeed, the more precisely it is defined, the less significant it seems to become.[15] It is customary to say that the "emergence" consists in the introduction of "a new order in which the pattern produced by the elements of the system cannot be accounted for in terms of the individual action of the individual components of the system," but rather in terms of the onset of a new phenomenon – "the synergism among the elements" (Bertuglia and Vaio 2003, 298). However, the synergism among elements is a phenomenon that can be detected in very elementary situations. Even a linear dynamical system, the equations of which are not uncoupled, produces a phenomenon of synergism. In the case in which the eigenvalues are distinct it is however customary to get by by saying that the synergism is not significant in that it is expressed as a linear combination of such eigenvalues and can thus be accounted for in terms of the individual components. In the more complicated case in which the eigenvalues are not distinct, the synergistic effect may be said not to give rise to "emergent properties," as the system solutions are linear combinations of known "bricks": exponentials, polynomials, and trigonometric functions. Going on to the non-linear case, the component bricks become increasingly numerous and hard to classify: the classical procedure of enlarging the field of the elementary functions of the analysis nevertheless satisfies the objective of pursuing this classification program. We now know that this program is so complicated as to represent a Herculean task. There is no doubt that using a computer to integrate the differential equations has completely revolutionized the way in which the entire question is now viewed. On the one hand, it has opened up the way to the integration of systems comprising thousands of differential equations, albeit in approximate and qualitative (geometric) terms. On the other hand, it has shown us that phenomena exist, such as that of chaos, of strange attractors, of bifurcations that raise the level of complexity beyond all possible limits and, in some cases, represent out-and-out obstacles.

The most striking fact is the occurrence of such complicated levels as to render unfeasible and even impossible in principle the explicit reduction of the overall mathematical phenomenon to its constituent elements. This blocking justifies speaking of such an irreducible complication as to warrant a new term, that of *complexity*. But between acknowledging that the program of explicit "reduction" to the elementary components is unfeasible and the claim that this means that something new "has

[14] "The difficult and rather obscure notion of emergence is close companion to holism" (Talbott 2001, 17).
[15] Following Holland the "emergence" is connected to the fact that the "behavior of the whole" is much more complex than the "behavior of the parts" (Holland 1998, 1). But if emergence is intended to be as a typical feature of complexity, this characterization is purely tautological.

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emerged" there is a vast gulf. There is not one single plausible reason to justify the claim that the behavior of a system represented formally as the sum of elementary components is not just the effect of the behavior of such components only because we are unable to represent this effect in the terms of the forms of composition with which we are familiar and which are commonly recognized. To paraphrase Laplace, we could say that complexity is due partly to our knowledge and partly to our ignorance. But to confuse our knowledge and our ignorance with the emergence of something "new" is not legitimate.

In actual fact, to speak of "knowledge" and "ignorance" is the only rational and positive way of presenting the developments leading up to the identification of a "science of complexity." It would be absurd to ignore the progress made thanks to the study of far from equilibrium systems, turbulent processes, chaos, and so on. However, the eagerness to extract from these studies the conclusion that the key to the "creative" process, as expressed in "self-organization" processes and "emergence" phenomena, has finally been found, does indeed have something "mystical" about it, to use Roger Newton's term. It corresponds to the age-old ambition of showing that phenomena conventionally considered as the expression of "creativity" actually "emerge" spontaneously inside self-adaptive processes having a lower-order structure. In explicit terms, biological processes are thought not to possess autonomy but to "emerge" from self-adaptive processes of physical matter; conscious thought does not possess autonomy but "emerges" from self-adaptive biological processes, and so on. Indeed, the "miracle" lies in the fact that a system with a structurally deterministic dynamics can produce processes of a non-deterministic nature, or that at least seem to be such and (who knows why) we must consider as such.[16] The only rational meaning of the discourse on "emergent properties" – unless we wish to get embroiled in mystical discourses – thus seems to be the re-proposing of an even more radical form of reductionism. And indeed reductionism it is, because to be able to speak of "emergence" is dependent on the existence of an underlying lower-level structure that determines and produces the apparently creative phenomenon. And this is an even more radical reductionism than the classical form in that the former was mostly limited to pursuing the process of reduction within natural (hierarchically ordered) processes, while here the reduction is performed on the whole of reality, from raw matter to conscious thought. Nothing wrong with this. Each knowledge program is legitimate provided everything is given its correct name. And here we are not up against a critique of reductionism but of the most radical form of reductionism that has ever been enunciated in the history of science, a radicalism comparable with that of Lamettrie and less radical that that of Cabanis, who rejected physicalist

[16] "A complex system, on the other hand, *appears* to react to a stimulus in a coordinated fashion, as though driven by a will to find new responses, that is, new configurations, in order to cope with new stimuli; however, these new configurations are unpredictable for us, which conceals the determinism underlying the system's dynamics" (Bertuglia and Vaio 2003, 307–8).

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reductionism. It appears therefore quite appropriate to say that "it is a live question whether the current developments are indeed a renewal of science or instead represent a retrenchment and strenghtening of the most serious limitations of traditional science" (Talbott 2001, 15).

No doubt is thus being cast on the *specific* progress brought, both now and in the past, by the multifarious front of the science of complexity, but rather on the ambiguity of the overall program. Moreover, the more serious critics have pointed out this ambiguity:

> The approach of complexity . . . is still mainly limited to an empirical phase in which the concepts have not yet been fully clarified and the methods and techniques are completely lacking. The result is often an abuse of the term "complexity" which is sometimes used in different contexts with widely differing meanings and even quite inappropriately. . . . A formalization of complexity would make it possible to turn a set of empirical observations, which is what complexity amounts to for the time being, either into an actual hypothetico-deductive theory, like physics when it is treated using mathematical methods . . . , or into an empirical science . . . , the study of complexity from a mathematical standpoint is currently vague in its definitions, ineffective in its methods and lacking in results. (Bertuglia and Vaio 2003, 344)

In this connection, it is rather extraordinary that the constant reference to a formal mathematical context in which to frame complexity is almost always that of deterministic dynamical systems. And it is precisely here, in this conceptual environment, that the most hazardous acrobatics are performed, perhaps defining complexity as a situation of transition half-way between rigid determinism and the anarchy of chaos (see Kaufmann 1994). Complexity, in other words, is thus neither stable equilibrium nor chaos, but a third condition, in which the system is "creative," as though it were capable of evolving autonomously and improving by adaptation. Just how all this can occur in an environment in which the most inflexible determinism reigns cannot be accounted for solely within the sphere of mystical intuition.

On the other hand, a number of persons quite candidly admit that there is a tendency to pull complexity out of the hat whenever a difficulty of comprehension arises (see Flood and Carson 1986). There is nevertheless a tendency to resist the idea that complexity can be confused with our ignorance, while allowing that it can vary according to the models used (Bertuglia and Vaio 2003, 309). Others end up by asserting that complexity is that property of system models that makes it difficult to formulate the general behavior of the system (see Edmonds 1999).

The concept of complexity thus continues to wander around in limbo between ontology and epistemology. The desire to define it as a property of the real world clashes with the vagueness of the definitions of an empirical nature and with the contradictions to which the formal definitions so far proposed give rise. Furthermore, every time it falls into the purely epistemological sphere it appears as a disillusioned and pessimistic (postmodern) version of the concept of complication.

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6. Old mathematics vs new mathematics

This brings us to the identification that is often performed between the relationship between simple and complex and the (mathematical) relationship between linear and non-linear.

In this connection, the conflict that often seems to exist between "linear mathematics" and "non-linear mathematics" appears overdone, almost as though there were two historically opposed developments in mathematical analysis. If one heeds these simplifications, it would seem that the great eighteenth- and nineteenth-century physico-mathematicians never realized they were almost always manipulating non-linear equations or it would seem that they made systematic use of linear equations. Credit is often given to the singular claim that our mathematics is mostly linear (which is substantially false) because our image of the world is "linear." The latter statement – incomprehensible *per se* – is justified in various ways. Mainly according to the so-called "anthropic principle" – probably enunciated by Monsieur de la Palisse – according to which "physical reality is precisely what we observe among all the possible universes, otherwise it could not be so: if it were not so, we living beings would not exist and would not be here to observe it" (Bertuglia and Vaio 2003, 270). Sometimes in the form of more precise observations according to which if we had lived on a hotter planet, with undue fluctuations caused by the high temperatures, we would not have created such a linear mathematics.

Neglecting such speculations, which have the same value and less literary interest than Isaac Asimov's science fiction, the history of science allows us to understand how and why a "linear" paradigm developed. It is above all to Joseph Louis Lagrange that credit is due for using a "linearization" procedure in the study of ordinary differential equations. Following Lagrange a function is nothing but a series and the series (before Cauchy) were all considered as convergent as they derived from the mathematical representation of physical phenomena. This in a sense was a consequence of the determinacy of physical phenomena. The linearization procedure was based on the assumption that, in a series development, the linear term represents the significant component which embodies the characteristic properties of the function, while the higher order terms represent perturbations that, however significant they may be, are not such as to modify the overall trend of the process. This was obviously an undemonstrated hypothesis, and a false one to boot: this is how it appeared towards the end of the nineteenth century and it must be recalled that one of its bitterest critics was Tullio Levi-Civita, in the wake of Alexander Lyapunov, who was the first researcher to have comprehensively highlighted the importance of the non-linear components. It was a reflection of a metaphysical view and it is not an overstatement to say that it reflected the conviction of the fundamentally *simple* (as well as determinate) character of physical phenomena. Nevertheless, the fact that the linearization procedure enjoyed such success during the nineteenth century and became a kind of *passe partout* – in particular thanks to the British school of E. J. Routh and W. Thompson (Lord

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Kelvin) – does not justify the claim that there is a kind of gap in the history of mathematics before which only "linear mathematics" was done. To speak lightheartedly of "linear mathematics" and "non-linear mathematics," as though there was a line of demarcation between the two, is misleading, as though great scientists like Euler, Laplace, or Lagrange himself, were concerned solely with linear structures and ignored all the others; this would have amounted to striking out practically the whole of mechanics and mathematical physics. It is perfectly legitimate to assert that the centrality of the "linearization" method – not of "linear mathematics," which has never existed as a separate object – is the reflection of a view according to which the natural laws are fundamentally simple. On the other hand, there are no grounds for introducing a strong demarcation that forms the basis of caricatural images like the one according to which we had produced a "linear mathematics" in so far as it is a reflection of the fact that we live in a stable and "linear" niche of the universe.

More in general, it is necessary to distance oneself from a certain kind of excess consisting in accrediting the idea that a "new mathematics" had been created, which is completely different from that known hitherto and that is the outcome of a new awareness of the complexity of nature. It is ironical to observe that this "new mathematics" has developed entirely within the context of the most classical mathematical structure that we know, namely in the context of the theory of dynamical systems, or of ordinary differential equations. The object of this theory did not change merely because we became aware of the extraordinarily complicated – and even irreducibly complicated, that is to say "complex" – nature of the mathematical phenomena emerging in it, and concerning which we are unable to say whether it involves only strictly mathematical phenomena – "forever unusable" developments, to use Duhem's words – or else the image of empirical facts, or lastly of models that can validly be used in the description of empirical facts. In this connection, it is appropriate to emphasize the fact that the vague use of these concepts – now interpreted in an ontological or empirical sense, now in a formal or epistemological sense – is the result of a methodological confusion that should be dispelled.

It is not surprising that, after so much clamor surrounding the birth of a "new mathematics" for complexity, one must admit that this new mathematics still remains to be created in its entirety. In actual fact, the history of mathematics offers us only one case of the birth of a truly "new" mathematics: infinitesimal calculus and the theory of differential equations. It is not necessary to point out that the birth of this new mathematics arose out of the need to represent physical phenomena, in particular those of motion, for which Euclidean mathematics proved quite inadequate.[17] The unique nature of this development was quite apparent to John von Neumann when he enunciated the possibility of a second turning point consisting in the creation of a

[17] In this connection, see the classical books of Alexandre Koyré (Koyré 1966, Koyré 1968) and also Giusti 1993.

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new mathematics for the analysis of processes so different from physical processes such as social and economic ones. Rightly or wrongly he identified it as game theory. Von Neumann customarily insisted on the idea that, while it had taken several centuries to achieve a sophisticated and satisfactory mathematization of physical phenomena, it would take even longer to construct a mathematics suitable for the analysis of far more complex phenomena such as social phenomena. And it may seem surprising to observe that von Neumann – often considered to be conservative on the subject of complexity, which he actually tended rather to interpret in the sense of complication – recommended an increasingly holistic approach in the analysis of economic and social phenomena through game theory. To the extent that Nash's point of view, insofar as it reappraised the methodological individualism approach, represented a return to a reductionist approach, as Nash himself had to admit (see Israel 2004d, Israel forthcoming, and Mirowski 2002). The history of the relations between simplicity and complexity and its mathematical images is thus more complicated and tortuous than may appear at first sight.

At this stage a few considerations are warranted concerning the attempts to develop the science of complexity outside the customary schemata of classical formalism: among these, even more than the use of probabilistic methods, the use of the idea of evolution borrowed from biology seems innovative and original. Here we are actually addressing an issue that verges on the one that will be treated in the following section. It is indeed from the study of processes occurring in the biological or social sphere that the idea emerged of using schemata of representation that differed from those suggested by the physical sciences. It is nevertheless curious to note how strong the force of reductionism is, as a result of which, setting aside physical reductionism, one turns to biological reductionism. One has the distinct impression of being up against an inertia or laziness of reason as a result of which one is able to think only in terms of schemata suggested elsewhere. Moreover, the attraction of the evolutionist idea, considered as a way out of the difficulties of the mechanistic schemata, has manifested itself in numerous forms, including the attempt by Thomas Kuhn to explain the growth of scientific knowledge in terms of selection mechanisms (see Kuhn 1992). Such an idea is debatable to say the least and has indeed not given rise to any significant developments. However, it is not much more convincing to claim that "most organisms evolve by means of two primary processes: natural selection and sexual reproduction" (Holland 2005). Organisms – whether biological or social – display a strong capacity for goal-directed behavior, so that the reduction of a random process governed by an objective parameter such as "fitness" appears as parody of reality.[18] In this way mechanism is

[18] There is "an extremely peculiar complication into the mechanism governing living beings, a complication which does not exist in the statistical dynamics of molecular physics. Not only is the living organism capable of accomplishing at the macroscopic level achievements similar to those that in the world of molecules are allowed to figments in the imagination, such as Maxwell's demon, but this power 'to beat chance,' as it were, is possessed to varying degrees by the different living organisms, and the mechanisms underlying the systems

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abandoned in favor of an even more stringent type of physical reductionism. In actual fact, vis-à-vis the theory of genetic algorithms, one naturally wonders whether it is a question of using biological ideas to enhance the performance of calculus or of a projection of information technology ideas onto a biological context. In any case, it is significant that the more important applications mentioned to account for the efficacy of this branch of learning refer to engineering contexts and far fewer are known in a biological or socioeconomic context.[19]

One further illuminating example of the reductionist force in the science of complexity is the frequent use made of the well-known "prisoner's dilemma" model taken from game theory. It is a known fact that this model touches upon the problem of cooperation as it provides a simple example of a situation in which both players should avoid cooperation in order to limit the damage due to the situation in which they find themselves.[20] It was apparently overlooked that this model was invented as a parody of the extreme consequences to which Nash's notion of equilibrium led, that is, its unduly non-cooperative nature. It is therefore not a critical attitude to refer to it as evidence of the intrinsic difficulties of cooperation: if anything it is proof of the inability of Nash's theory to represent the cooperation factor and, more generally, to correctly describe rationality.[21] It is a fact that one way out towards the representation of rationality given by the prisoner's dilemma consists in the observation that its repeated application leads to the emergence of forms of cooperation. However, this discovery may be viewed solely as the observation that a dynamic re-interpretation of the model waters down the opposition between the rigid formal rationality of Nash's equilibrium and the rationality of real behavior. Conversely, it cannot be used to subscribe to a kind of fetishism of formal schemata and models. The principle of natural selection or cooperation envisaged in the prisoner's dilemma are not universal schemata referring to every aspect of reality. This kind of fetishism of the primacy of formal reasoning seems to have been embraced by Daniel Dennett when he asks why trees in the forest expend so much energy growing tall. His answer is that trees behave irrationally: "If only those redwoods could get together and agree on some sensible zoning restrictions and stop competing with each other for sunlight, they could avoid the trouble of building those ridiculous and expensive trunks, stay low and thrifty shrubs, and get just as much sunlight as before!" (Dennett 1995, 253). In fact the only thing that is truly ridiculous here is the idea of wanting to teach nature to be rational, in compliance with the formal rationality of game theory. It is not realized that compared with the

containing living matter must necessarily take into account, in addition to this capacity, also the degree to which this capacity is possessed, as it plays an important part in the determination of the place occupied by the various biological species in the scale of evolution" (Gause [Gauze] 1935, 42–43).

[19] Holland mentions the development of algorithms that learn to control gas pipeline systems, to design communication networks, or are used in the design of high-by-pass jet engine turbine.

[20] A detailed description of the prisoner's dilemma may be found in any book on game theory.

[21] Nash's lack of sensitivity to the question is shown by his reaction to the model, as reported by Sylvia Nasar: he apparently observed that in the "dilemma" there was still too much cooperation (Nasar 1998).

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complexity of ecosystems the simplicity of game theory formalism is seen to be of a ludicrous oversimplification.[22] This shows the extreme lengths to which the myth of formal reasoning and reductionism can take us.

7. The problems of the non-physical sciences

We now come to the topic of the mathematization of non-physical phenomena, in particular socio-economic phenomena, regarding which a few rapid remarks may be made.[23]

We have seen how one of the things the theory of complexity (unjustifiably, in our opinion) prides itself on is that of accounting for emergent properties and thus of allowing analysis to rise from a lower to a higher level context, accounting for the higher processes characterized by self-organization and creativity. This is an ideal subject for the theory of complexity which aims at providing a response to the problem of the scientific analysis of contexts, such as those of life and consciousness, previously not available to reductionist type analysis. But precisely here the apparent nature of progress is discounted and that fact that, in particular in the formal and mathematical field, nothing new has occurred which allows the old aporiai to be dispensed with.

In fact, in the context of mathematical analysis, the difficulty is an old one and it is no coincidence that there were attempts to get rid of it starting in the eighteenth century: this is related to the fact that the deterministic and probabilistic descriptive framework both seem unsuitable for representing the fundamental peculiarity of a conscious subject, that is, to be capable of making a conscious choice of actions dictated by a *goal*. The eighteenth-century physical mathematicians – in particular, Maupertuis and Euler – believed that the problem could be solved upstream, showing that even physical processes are guided by purposes that cannot be reduced to a rigorous determinism and that are believed to be represented mathematically by the principles of minimum action.

It was already clear to Lagrange and then explicitly showed by Ernst Mach and von Neumann that any such hope was an illusion. As von Neumann pointed out, the causal approach and the teleological approach seemed to be different – the second apparently being more general than the former – although they were actually rigorously equivalent. And he added that the question of whether mechanics is causal or teleological has no meaning, as it depends solely on how we decide to write the equations. Therefore,

[22] It is undeniable that the most "rational" forest in Dennett's view would be composed of a small number of sparse low trees. Allowing that getting sunlight is the only function of trees in a forest. . . . Considering a forest as an aggregate of isolated units (the trees) in competition is a striking manifestation of reductionism, something as far remote from holism as can be imagined.

[23] For further details see Israel 2001a, Israel 2001c, Israel 2004c.

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teleology in mechanics is a mere formal fact and is totally reabsorbed by determinism. Von Neumann added that there was nothing to joke about regarding the importance of taking into account teleological principles when dealing with biology, although he believed it was plausible that also here the problem could disappear as in mechanics (see Israel and Millán Gasca 1995).

But the fundamental misunderstanding was due to the groundless identification of the concept of teleology (or finalism) with the very *ad hoc* mathematical finalism involved in the principles of minimal action of Maupertuis and Hamilton (see Bachelard 1961). So, the problem has by no means disappeared, otherwise it would be impossible to account for the efforts made to find a more satisfactory explanation of biological phenomena than the deterministic one – and *a fortiori* in the field of human phenomena. However, the formal tools still do not offer anything different than the representations in deterministic or probabilistic terms. It should not be necessary to spend too many words on the fact that a goal-directed subjective behavior is something that is diametrically opposed to a random process. And yet it still seems necessary to persist in the criticism of theories that stubbornly repropose these prospects. One could merely refer to René Thom's biting comment that it is a "wretched way out to determinism" to try to "save the possibility of radical innovation, of creative invention" by appealing to the "Brownian movement of the drunken sailor" and to the image of a "crazy God forced unceasingly towards the burlesque alternative of Buridan's ass" (Thom 1986, 23). Or to recall the recent remark by the mathematician Keith Devlin, who, in an interview, pointed an accusing finger at the methods used to check passengers in American airports: "They talk about random checks but we know perfectly well that if the decision is left to a human being it is impossible to achieve a random behavior. What probably happens is that, in addition to suspicious types, those who are checked are the most unassuming looking passengers, like myself, who would cause fewer problems" (Anonymous 2002, 15). In this way, Devlin tells us quite frankly that nothing more than a random process is more remote from the decision-making method used by a conscious subject.

Of particular interest in this connection are the difficulties facing game theory in the attempt to provide a formal representation of what is meant by the "rational" behavior of a decision-maker. The theory postulates that the decision-maker is rational "in the sense that he is aware of his alternatives, forms expectations about any unknowns, and chooses his action deliberately after some process of optimization" (Osborne and Rubinstein 1994, 4). And this is regardless of whether or not he is acting in the presence of uncertainty.

Osborne and Rubinstein point out that "the assumptions that underlie the theory of a rational decision-maker are under perpetual attack by experimental psychologists, who constantly point out severe limits to its applications" (ibid., 5). Less recently Luce and Raiffa (Luce and Raiffa [1957] 1987) observed that the heart of the matter lies in the concept of "rationality" and in the way, too often vague or implicit, with which this concept is used, as though it was an obvious concept and not the starting point of

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all possible analysis in a social context. Then Luce and Raiffa observed that the severe restriction placed on description by the need for logic and mathematical schematization

> seems to be regarded by many social scientists as a terrible inadequacy, and yet it is a common difficulty in the whole of physical science. It is analogous to a physical prediction based on boundary conditions which may be subject to change during the process, either by external causes or through the very process itself. The prediction will only be valid to the extent that the conditions are not changed, yet such predictions are useful and are used even when it is uncertain whether the assumed invariance actually holds. In many ways, social scientists seem to demand from a mathematical model more comprehensive predictions of complex social phenomena than have ever been possible in applied physics and engineering; it is almost certain that their desires will never be fulfilled, and so either their aspirations will be changed or formal deductive systems will be discredited for them. (Ibid., 7)

So why ever should one demand of mathematized social sciences what is not demanded of physics? Is it only a hidden idiosyncrasy for mathematization, or a concealed irrationalist attitude? Sometimes this may also be the case, although it is quite evident that a real problem does actually exist. The success of physics is due to the fact of having chosen as its guiding principle the Galilean approach of "pruning the impediments" (see Israel 1996a and b), that is the belief in the fact that there is a mathematical order underlying nature which is simpler than it appears, and that represents the essence of phenomena, and with regard to which the complications, the peculiarities and the individual specificities are secondary and non-decisive aspects. This principle – although its validity cannot be scientifically demonstrated, as it is a metaphysical principle – has proved to be effective. It represents the key to the success of the mathematization of physics since it underpins the notion of "law of physics," that is, precisely that type of law that the socio-economic sciences do not have. However, the "attrition" and the "impediments" that physics has successfully reduced to ancillary aspects of the phenomena it studies are unfortunately the *object* of the analysis of the social and economic sciences. Physics does not take individual objects, or objects taken in their individuality, into consideration. Quite the contrary, it discards them, otherwise it would not exist as a science. But is not the *subject*, the *individual*, in his specificity, precisely the object of the socio-economic sciences? The extraordinary challenge at the center of these sciences – that is, the determination of *objective conclusions* concerning the behavior of *subjectivity* – is legitimate, provided however that the procedure of "pruning the impediments" is not adopted in an unconditional and brutal fashion. And it is certainly not due to chance if all the results of some formal value in the field of theoretical mathematical economics, and very often also in game theory, take into consideration *standard* classes of utility functions of the agents whenever they do not actually consider them all as identical. These are formally valid but empirically insignificant results. And it is certainly no coincidence if Hildenbrand's program, described above, honestly took stock of the failure of mathematization procedures and, on the basis of aggregate

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hypotheses, relied on the distribution of the agents' characteristics, abandoning the usual strictly "micro" approach. The fact that this program has so far not led to any important results is further evidence of the extreme difficulty of these problems.

8. Conclusion

All this leads back to our main topic and allows us to make some general final considerations. There is no doubt that the viewpoint of the champions of the theory of complexity is closer to the type of critique of the formalization of social and economic systems illustrated above. Indeed it singles out the inadequacy of the reductionist approach which, in these fields, appears in the form of methodological individualism. However, in order to be coherent through and through, the point of view of complexity should renounce – at least in the sphere of human phenomena – any attempt, however subtle or camouflaged, to make use of methodological individualism and the claim to be able to cancel out subjective characteristics in an arbitrary objective representation and to consider subjects as material points. In our view, since the 1 : 1 cartographic description cannot be considered as a scientific analysis, the only remaining approach is that of pursuing global or holistic analyses, or whatever they are called, although in full awareness of their insurmountable limits, of the inevitable narrowness and of the need to integrate them with non-formal analyses carried out in the specific context. It is hard to believe that a scientific analysis of the socio-economic processes can do without the historical aspect. It must therefore be related to a complex of elements and concepts that can in no way be exhausted by a formal analysis.

We have already observed how the development of applied mathematics – in particular, under the influence of the modeling method – led to the invasion of territories previously deliberately excluded by mathematization, and we have seen that this process suggests the image of an army that has enormously enlarged the territory it occupies, paying the price of an increasingly more difficult and controversial control. The new domains are not firmly in the hands of a process of mathematization like the "motherland" (i.e. the territory of physical phenomena). Of course, the science of complexity is not just mathematization. However, there is no doubt that the mathematical-formal approach remains the heart of the theoretical elaborations of the science of complexity. And this is precisely where the ambiguity lies: since, in this way, the science of complexity runs the risk of becoming an updated version of reductionism, that is, of the attempt to subordinate every aspect of reality to the same interpretative key. Any such attempt inevitably clashes with the realization that the "army" of reductionism, however effective, does not have the strength to occupy and control such huge territories.

The science of complexity, if it hopes to have a future, should therefore radically cut off all links with reductionism, it must work around the paradox that, precisely when it proclaims itself critical of this ideology, ends up by taking on its objectives and functions.

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It should accept coexistence and collaboration with other forms of knowledge having a different nature to formal knowledge, and that are essential in order to account for the dimension of historical time and subjectivity. No intellectual acrobatics can render plausible the inference of the "laws of history" from an analysis of differential dynamical systems. Moreover, all forms of knowledge aim at achieving progress and have an underlying belief in some form of objective order: even literature pursues the identification of types having some value of generality and aims at inferring some teaching from a tale. Giving up the restriction of the idea of objectivity as something strictly deriving from the physico-mathematical approach is therefore an essential prerequisite for developing a knowledge of those "complex" fields characterized by forms of "autonomy" that it is necessary indeed to consider in their *full autonomy* instead of trying to the "reduce" them to "emergent" properties.

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