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Filomena Maggino *Editor*

Complexity in Society: From Indicators Construction to their Synthesis

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Complexity in Society: From Indicators Construction to their Synthesis

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Introduction

Complexity and Its Implications in Dealing with Indicators

The quantitative observation of reality and the aspects defining it (like wellbeing, quality of life, and so on) requires a multifaceted approach and a compound methodology.

The usual practice, aimed at quantitatively observing reality, is to section and divide the observation in single elements called indicators.

The risk of using indicators is to consider reality like a machine, made up by elementary components. Since the world is actually an inseparable network of relationships and reality is a self-regulating system, the approach to indicator construction should respect those characteristics and require relationships, schemes and contexts to be defined and considered.

No indicator can be considered separately and independently from the others. Each indicator is important, but what makes the monitoring exercise meaningful is represented by the relationships that can be observed and analysed between and among indicators. The integrated view allows the phenomenon we are monitoring to be located diffusely in the system of indicators. In other words, a (social, economic, environmental) system is an integrated set/totality which can be understood only by examining the features of the whole, in line with the well-known saying *the whole is greater than the simple sum of its parts*.

The systemic approach and view find their theoretical reference in the theory of complexity, which applies also to the world of indicators. Complexity is actually a mathematical theory technically known as non-linear dynamics. Its application to the study of reality allows the understanding of the fundamental characteristics of social phenomena by using a systemic view, requiring the identification of relationships, networks and organisational schemes.

In dealing with indicators, complexity affects the following:

- *The construction of indicators*. Consequently, indicators should be many in order to, in a systemic view, focus not only on a single element. Each single

element should be considered as an integral part of a variety of elements related to each other, defining that reality; no element or no indicator has an intrinsic validity in itself; consequently, if we are measuring a phenomenon, e.g. wellbeing, we should be aware that it is not located in one single indicator but is a global characteristic of the group of indicators, even though each indicator has a meaning in itself; moreover, the dynamics of the system do not require a rigid selection of indicators.
- *The analysis of the indicators*. The analysis should respect the non-linear relationships among indicators and require a multi-technique and multi-method approach; since indicators are actually mutually related (i.e. each indicator is influenced by the others and influences the others), the analysis should not produce a result represented by a simple and single number but should produce a meaning; the relationship between two indicators yields a meaning and produces new exchange, new contacts or interactions; in this perspective, the analysis in the ambit of a system of indicators allows the system to generate itself.
- *The interpretation of the results*. The systemic characteristic of the relationships among indicators requires a particular attention in the interpretation of the results obtained through the analytic process; the attention should be based on the idea that any increase of complexity introduces more refinements, fragilities and uncertainties in the statistical analyses.

In this frame, the synthesis of indicators plays an important role in:

- Reducing the complexity
- Allowing analytical processes to be conducted at higher levels of the system
- Allowing easier communication of the results

However, synthesis should not be pursued inconsistently. We should avoid aggregating many indicators, inevitably producing a meaningless value.

For this reason, complexity should be preserved in constructing, managing and analysing indicators and should guide in the representation exercise (telling stories through indicators).

The complexity approach should guide not only academic researchers but other actors like policymakers. In fact, this debate always points out that dealing with complexity shows challenges which are institutional, methodological, statistical and technical.

The Volume

This volume aims at disentangling some important methodological aspects and issues that should be considered in measuring complex social phenomena through indicators and in dealing with those indicators in order to construct syntheses.

Even though apparently dealing with these issues is merely a technical problem to be faced and possibly solved by statisticians or information scientists, the construction of indicators presents also other crucial aspects to be considered, starting from philosophical and political concerns. The ultimate success or failure of constructing and using indicators depends upon, as Alex Michalos pointed out in many occasions, the negotiations involved in creating and disseminating the indicators or the reports or accounts that use those indicators.

The volume has 13 chapters organised in four parts:

The **first part** is focused on *conceptual issues*.

Alberto Peruzzi introduces some important epistemological issues related to the notion of complexity by discussing how its increasing use in social sciences actually warns against excessive expectations and abuses of the notion resting on an appeal to rhetoric. He aims at providing a step towards clarifying the scientific meaning of complexity.

Rocco Sacconaghi proposes to disentangle the problem of how to synthesise analytical data and illustrates how the phenomenological approach can contribute to an effective interpretation of the relation between heterogeneous elements, moving from a list to a synthesis without causing an undue homogenisation of the elements themselves.

The **second part** deals with *methodological issue*.

Marco Fattore, in Chap. 3, raises an important issue related to the so-called information-based policymaking and the role that socio-economic statistics and indicators play in that context. In particular, he discusses the role of big data and data science on future socio-economic statistics and their potential effects on the construction of social and economic indicators.

The editor of the volume enters into the merits of developing indicators in Chap. 4, by stating the importance of having a systemic view and illustrating the challenge, needs and risks of this exercise. Chapter 5 deals with the methodological issues related to the synthesis in a system of indicators, by distinguishing also between aggregative and non-aggregative approaches.

Kenneth C. Land, Vicki L. Lamb and Xiaolu Zang, in Chap. 6, face important questions related to the construction of synthetic indicators: Can properties of a society described by synthetic indicators be scaled across time periods and levels of analysis – from the whole system to subunits thereof? Starting from the idea that indicators describe complex systems like societies, they address the question within the context of two general sets of equations of state for complex systems. The first complexity model is a non-linear deterministic dynamics model, defined by difference or differential equations. The second one incorporates stochastic (uncertainty) elements into the model specifications, leading to the various classes of statistical models.

The **third part** explores and investigates different *technical issues* related to the construction of synthetic indicators. In particular, three main approaches are illustrated.

Matteo Mazziotta and Adriano Pareto, in Chap. 7, illustrate the consolidated methodology aimed at constructing composite indicators, by including this in the worldwide movement (“beyond GDP”) aimed at identifying the best approach for measuring wellbeing and by paying attention on the pros and cons of this approach.

Marco Fattore, in Chap. 8, illustrates how the synthesis of indicators can be managed by using a non-aggregative approach, based on partial order theory. He shows how the application of partial order theory can overcome the limitations of aggregative approaches. The proposed method, in fact, is able to deal with indicators measured through data ordinal in their nature and measuring phenomena not necessarily correlated to each other.

Arranged together with Michela Gnaldi and Simone Del Sarto, Chap. 9 starts from the idea that one of the aims of measuring social phenomena is to identify, quantify and possibly explain the differences between units of analysis (individuals or countries) starting from some characteristics. In this context, the most applicable statistical approaches are those which allow us to deal with two different analytical perspectives, clustering (or grouping) units into homogeneous classes, by taking into account at the same time the multidimensionality of the phenomena under study. One of such methods is the latent class (LC) multidimensional IRT model.

The **fourth part** includes some *particular experiences* in dealing with the synthesising exercise by using also concrete data.

In Chap. 10, Chang-Ming Hsieh starts from considering how the synthesising process can account for potential societal, cultural and/or individual differences in values associated with different facets or domains represented by the considered indicators. Such topic is continuously discussed in the field of social indicators and is related to the use of a weighting system in order to reflect individual different values associated with different life domains. As illustrated in the chapter, the topic has important implication not only at the conceptual and methodological level but also at the technical level.

The content developed in Chap. 11 allows Ludovico Carrino to compare different strategies in normalising indicators while building syntheses. He discusses not only the rationale of the different approaches but also the consequences of their adoption in terms of results which show how the normalisation process can play a crucial part also in defining variables’ weighting.

Giovanna Boccuzzo and Giulio Caperna, in Chap. 11, illustrate the construction of a synthetic indicator through the application of the non-aggregative approach proposed in Chap. 3. The application uses official data produced by the Italian National Institute of Statistics and aims at defining a measure for life satisfaction in Italy. The application shows how the partial order theory can represent an important resource for social statistics.

Chapter 13, authored by Tommaso Rondinella and Elena Grimaccia, focuses on the big challenge of comparing different territorial areas according to multiple factors to be synthesised. The proposed solution, requiring the adoption of different statistical methods, is applied on indicators regarding the Europe 2020 indicators involving European countries.

The volume has no presumption to be complete and is not able to cover all methodological approaches and technical applications. The topic is continuously evolving and also expanding the boundaries of interest.

Other reflections could be added.

Some of them include representation (possible use of graphics in the perspective of synthesising in a complex context) and communication issues (how to obtain understandable data, and results, how to correctly present them).

Another topic to be more systematically explored concerns how to analyse indicators in a systemic view and context. This process requires particular attention: indicators representing a complex reality should be analysed through a systemic approach by considering from one hand non-linearities and multiple reciprocal causal relationships and on the other hand the uncertainties. The latter issue is particularly delicate especially in the perspective of defining possible futures.

In other words, our work is still in progress . . . to be continued . . ..

Roma, Italy Filomena Maggino

Part I
Conceptual Issues

Chapter 1
Complexity: Between Rhetoric and Science

Alberto Peruzzi

Premise

What is called "complexity theory" contains a core of ideas of scientific relevance, surrounded by features which, although intuitive, are in mutual tension. If complexity really marks a turn in our scientific image of the world, that core should not be burdened by vagueness and ambiguity, associated with features in mutual tension. Rather than relying on the intuitive idea of complexity as a sort of universal glue between different theoretical approaches worked out so far, caution is suggested. The reasons for caution do not imply any choice of specific theoretical framework, but their consideration is preliminary to any such choice.

This caution reacts to widespread rhetoric which ends in adhesion to a cult of complexity, and cult is not science. Although rhetoric may help bring a change of perspective, it obstructs development of a theory satisfying those standards of rigour and testability constitutive of the scientific method ever since Galileo: a method which relies on observational and experimental procedures in terms of quantities to test (the axiomatic presentation of) a theory expressed in mathematical language.

That method has various features. One is measurability of the quantities we talk about. For instance, consider the notion of "length": its measurement needs the introduction of units, which though conventional have to satisfy constraints (a rigid rod must be length-invariant under transport and this invariance rests on theoretical assumptions); data don't tell us how to interpret them, and when more than one interpretation is at hand, it is the theory we adopt which makes the difference; the consistency of data with a given theory (in turn supposed to be self-consistent) neither implies its truth nor is it a warrant of its explanatory power. It is plain that any

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mathematical model omits some aspects of reality, but this omission can be justified by the gain in explanation and prediction it ensures. No matter whether we adopt a verificationist or a falsificationist viewpoint, or a probabilistic variant of either, such a method is more demanding than qualitative taxonomy and both more prudent and precise than any background "philosophy". Since the seventeenth century the growth of scientific knowledge has relied on this method and no other path has shown itself more reliable or effective. In principle, the new perspective focused on complexity, intended as a notion common to natural and social sciences, is no exception. But is this really the case?[1]

To free complexity from the "noise" of rhetoric, we need to examine the constituents of the notion, with their different sources, motivations and examples. If we miss the composite nature of the manner in which complexity is held to be crucial for distinct scientific domains, we risk turning up the "noise". This risk is witnessed by much of the flourishing secondary literature on complexity in social sciences, educational studies and philosophy. That this increase in background noise has been due to both scientists and scientifically oriented philosophers – who, in the garb of prophets, have led their followers to treat "complexity" as a mantra – is a further concern.[2] The new dialogue between science and philosophy, initiated in the early twentieth century, gave rise to what is presently named "philosophy of science" and it might have been expected to produce a different outcome. Thus, research on complexity demands an epistemological analysis unbiased by enthusiasm or hostility; and while no sophisticated meta-theoretical outlook produced the "complexity turn", logical rigour remains indispensable to assessing its import and relevance, as under that rhetorical noise (and blur) there is one of the most original and fascinating developments of our time, something involving mathematical sophistication and careful protocols to store/process the data pertaining to theoretical models.

Here, technicalities will be by-passed but this is not to suggest we can dispense with them. Their omission is due only to the limited aim of the present paper, that of a conceptual analysis to help clear away rhetoric and confusion found in much talk about complexity. Once detected, these can be avoided. Once avoided, the scientific

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[1] Of course, an adequate account of the scientific method calls for many more details, including the recognition that its features combine in different ways for different subjects; therefore, it is hard to prescribe one and only one way of "doing science". Yet these preliminary remarks already furnish a couple of suggestions for research on "QOL-exity" (i.e., Quality-Of-Life-complexity), namely, (1) there is no direct inference from any plurality of data to "Synthetic Indicators", (2) the hypotheses used in collecting and organising the data should be explicit and crisply stated. If the study of complexity is part of science, it does not justify any shift to a new-style, computer-aided, inductivism (the rumour surrounding Big Data is a case in point), exactly just the appeal to "complexity" does not, by itself, imply a new framework for research in the social sciences, still less one ensuring unprecedented advances. It rather points to a theory of Synthetic Indicators as leading parameters of a dynamical system to be precisely defined.

[2] Systemic thinking does not need any particular "philosophy". The ability to take into account the consequences of a decision, together with their unintended feedbacks, was always recognised as an ingredient of rationality, of direct relevance to any strategy in politics, business and conflict.

content and explanatory power of theoretical models of complex systems in the social sciences can be properly evaluated.

Two Razors

From the first attempts to form a unified image of nature, the explanation of a given body of evidence accessible through relatively restricted data appealed to entities which were not part of the evidence to be explained, e.g., after Democritus, the existence of different kinds of atoms, or, after Aristotle, the existence of stable combinations of the four elements, each admitting continuous variation of density. Metaphorically speaking, the more kinds of such entities, the thicker the beard on the face of theory. Such Inflationary tendencies in the positing of metaphysical entities to account for empirical evidence provoked practice in the wielding of no less metaphorical razors. Two are relevant here. One was honed in Ockham (England), the other in Santa Fe (New Mexico).

First, the famous Ockham's Razor (OR): named for the birthplace of one of the greatest philosophers of the Middle Ages, though the version that became famous was one William of Ockham never used. That version in Latin reads *Entia non sunt multiplicanda praeter necessitatem*, whereas Ockham wrote: *Numquam ponenda est pluralitas sine necessitate*. It is the former version which became standard: paraphrased into English, it means that the entities we suppose to exist should not exceed what is strictly necessary to account for the data. While not the authentic version of Ockham, it remains a reasonable suggestion, recommending that the number of kinds of entities appealed to beyond evidence be minimised. Equivalently, the razor expresses the idea of maximising "ontological" economy.[3]

If we stay with Ockham's actual words, OR can be updated as follows: *Numquam ponenda est complexitas sine necessitate* – i.e., never posit complexity, unless necessary. Most scientists are familiar with this kind of razor. The value of simplicity was classically stated in Newton's *Opticks* as a rule of scientific research, one since shown to be fruitful, and strong motivations recommend it. The adoption of such a razor promotes an austere habit of thought though it is not so easily wielded in accordance with a precise and consistent methodology. Inherent within it is also a dangerous propensity to treat the idealised model of a system as reality.

The Santa Fe Razor (SFR) is seemingly cheaper and easier to use but in reality more tricky. To keep Latin as the language for razors, it might be rendered as *Numquam ponenda est simplicitas sine necessitate* – i.e., never posit simplicity unless necessary. The SFR requires no less careful management than OR. It takes its

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[3] It is precisely from a consideration of OR that one of the best books on complexity and its models starts: Bertuglia and Vaio (2011). This is recommended reading, especially for those social scientists who intend not just to build models by exploiting the machinery of complex systems, but also want a synoptic view of different methodological options.

name from the Institute founded by George Cowan in 1984, which has as its motto "Science for a Complex World". It was at the Santa Fe Institute that the by now widespread view of the economy as a complex system was first promoted, dating from a conference held in 1987. The SFR shifts our attention from numbers (of theoretical posits) to interactivity, from saving to spending, and from issues of conceptual economy as in OR to the large variety of aspects involved in understanding *quality* by means of *quantity*. But, it comes with a symmetrical and no less dangerous propensity to that deriving from OR: to take the foam for the sea.

Much contemporary debate pivots on the rigid opposition between these two razors. What if we think of them as complementary rather than mutually exclusive? After all, one could say, the SFR prompts us to look at simplicity as an emergent property grounded in complexity and the "necessity" it refers to concerns the underlying process which allows for such emergence, through correlations between the components. In the absence of such correlations, it would be an instance of "disorganized complexity", to use Warren Weaver's words. Thus, one might think, the second razor makes the first possible. But this scenario seems immediately to tie us in knots, as the issue appeared to be deciding which of the razors to use in social sciences at the other's expense, but now the two razors complement each other. And if so correlated, how can they be opposed? To go back to Popper's dichotomy between two perspectives in the modelling of systems, "clocks" vs "clouds", we could even look at a clock as resulting from a cloud (of atoms) as well as at clouds as the result of many (say, asynchronous) clocks: this suggests a means to overcome the divide between conservative and progressive minds, namely, between those who deny complexity is a science and those who rely on it as the keyword to understand everything.

But then the price to pay for a (non-rhetorical) appreciation of complexity consists of having to sit on the edge of not one, but two razors, and we cannot expect such an uncomfortable position to recommend itself to either skeptics or enthusiasts for complexity. The *bonus* is that once both principles, associated with OR and SFR, are taken carefully into account, much of the rhetoric surrounding discussion of complexity is, if not banished, at least brought into view. The *malus* is that the resulting picture is even less clear. Against the "fast thought" so frequently advertised to save the effort of rigour, classical "slow thought" has its rights. Thus, let's start clarifying the terms of the opposition.

Back to Origins

Roughly speaking, a system is complex when it's made up of many interrelated components which, owing to their number and/or the number of their mutual relationships, exhibit properties which are not only different from those of any component but also *are* irreducible to (properties of) the set of components. Such irreducibility is intended as lying in the "things" and the way their whole behaves: a complex system is one with a behaviour hard, if not impossibile, to understand and to compute in terms of the components (be they elements or parts of the system).

So, complexity has from the start both an objective and a subjective aspect. Moving from intuition to the concepts of physics: the trajectory followed in the state space of the system depends on a particularly intertwined structure of its components and/or a very large number of simultaneous interactions among an equally large number of components. With a further concept-stretching (to use Imre Lakatos' term), we can add another feature: a small difference here can produce a vast difference everywhere – the "butterfly effect" leading to the land of chaos. Accordingly, such a system is supposed to be extremely sensitive to tiny input-variations. The exact conditions at the source of the flow of "information" through the boundary, i.e., the interface of the system with the environment, turns out to be an essential factor for any forecast (as a constraint, it is assumed that periodic orbits must be dense).

Finally, some complex systems are capable of self-organisation -which implies a capacity for the system to enter stable states conferring tolerance to the above changes. Granted that self-regulation in the presence of changing background conditions may lead the system to stabilise on unprecedented regimes, both (a) the idea of a self-regulating system and (b) the idea of non-hierarchical organisation are typically associated with the system being complex. Ad (a): if the system is the output of a program, as in the case of cellular automata, the program is supposed to be given in advance (differently of a neural network lacking any external algorithm of correction) and yet its behaviour can display behaviour we would call "complex". Ad (b): the emergence of macro-structural properties implies a hierarchical architecture which induces constraints on micro-structural dynamics, with top-down feedbacks contributing to the complexity of the system.[4]

Each of these different aspects has been formally articulated in areas of research as far removed from each other as e.g. game theory and meteorology. But different interests gave rise to different notions and reveal further aspects of complexity, which call for different methods. If, as someone also claimed, being "complex" were not intrinsic to a system but relative to its description and the method adopted to probe it, such conceptual fission would be inevitable in the presence of many non-equivalent descriptions and methods; and since a theory arranging the set of alternative descriptions and methods within a unifying picture allows to confer the concept a grip on an intrinsic property, the claim that complexity is language-laden prevents the existence of such a theory.

Two years after the Santa Fe Institute, the Center for Complex Systems Research (Urbana-Champaign) was founded by Stephen Wolfram. Many other institutes have

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[4] This is just an anticipation of conceptual difficulties to be analysed in the following. For the time being, let me add that, were complexity only an "approach" or a "style of thought", there would be no reason to worry about such difficulties. There is a reason, i.e., they are obstacles to overcome, if we care for a definition of complexity that is cross-disciplinary, not so much to grasp a Platonic essence as to sustain the search for a set of axiomatic principles. That such a set is yet to be found is a challenge not to give up, and the appeal to a new "style of thought" does not relieve us of the task.

been created since then, mainly devoted to complexity-focused research and development. Today there is a long list of centres which promote research on complex systems around the world. The number of journals having "complexity" or "complex" in their title is impressive and many books, from advanced monographs to popular science, cover a wide range of applications. There are also dedicated series, by publishers with a worldwide market, centred on complex systems, not to mention a number of websites and mail groups; and the growth rate in the literature on complexity has been rising for many years. This is noteworthy, considering it was in 1987 when a reference journal such a *Complex Systems* (also founded by Wolfram) first appeared.[5]

The first issue (1995) of another journal, *Complexity*, had an opening paper by Murray Gell-Man entitled "What is complexity?". Twenty years later, notwithstanding the subject's growth, that question is still with us and has become more compelling, because of the many ways in which complexity has been approached in in dealing with a rapidly increasing number of topics.

To provide an idea of the manifold "ingredients" associated with the notion of complexity one might think of listing a set of formal features. Such a project assumes that a unitary meaning has been shown to capture the generality expected of the notion. This has not yet happened. An alternative strategy consists of returning to the origins of the interest in complexity.

This is the way adopted here, for both contingent and conceptual reasons: an adequate survey of research trends on complexity, just as a "rational reconstruction" of their development up to the present state of the art, is beyond the length of one paper; moreover, detection of the different ingredients of complexity is favoured by looking at the seminal works, in which the ingredients have not yet been superposed, so to say, and, ultimately, the issue about the meaning of the so-called "complexity turn" was already present in the original literature.

Many of the works marking the tipping point in the turn towards an increasing interest in complexity appeared between 1968 and 1981, in roughly a decade. Even when they took the form of collections of previously published work, their impact was amplified (an effect in line with ideas stressed in the works in question!) and actually the underlying ideas were not born in those years. For instance, some specific "seeds" of complexity go back to von Neumann and Ulam's work on cellular automata, while Schrödinger and Turing demand mention in view of their pioneering contributions to the analysis of the notion of living system; Heisenberg had also contributed with his statistical investigation of turbulences, and other names could be mentioned as well.

But the new picture, with its "style of thought", did not come directly from those who anticipated one aspect or other of complexity: their contribution was recognised with hindsight, after the change of perspective which matured in the late 1960s and came to the fore in a set of seminal works during the 1970s. These works or at least the names of their authors are probably known to any researcher on

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[5] The *Physica* series of journals had already been enriched in 1980 by *D. Nonlinear Phenomena*.

complexity who pays any attention at the sources of the notions in use. What is mostly relevant here is that a minimal list of books is sufficient to make apparent the different ingredients which jointly inspired a generation and became "classic" references.

General Systems Theory, 1968, by Ludwig von Bertalanffy.
Towards a Theoretical Biology, in 4 vols., 1968–1972, by Conrad Hal Waddington.
Steps to an Ecology of Mind,1972, by Gregory Bateson.
Stabilité Structurelle et Morphogenèse, 1972, by René Thom.
Synergetik: eine Einführung, 1977, by Hermann Haken.
Self-Organization in Non-Equilibrium Systems: From Dissipative Structures to Order Through Fluctuations, 1977, by Ilya Prigogine (with G. Nicolis).
Autopoiesis and Cognition, 1980, by Humberto Maturana and Francisco Varela.
Observing Systems, 1981, by Heinz von Foerster.[6]

Since our present aim is neither a complete survey nor a critical review, still less a chapter on the history of ideas in the twentieth century, the rich content and sense of exploration those works convey cannot be given adequate discussion here. Nonetheless, some hints may be useful.

Already in 1945 Von Bertalanffy, a former member of the Vienna Circle, had sketched a cross-disciplinary investigation of any kind of dynamical system and introduced his readers to the study of the nested hierarchy of open systems in nature as the new frontier of science, searching for laws that apply to generalised systems, "irrespective of their particular kind, the nature of their component elements, and the relation or 'forces' between them".[7] Waddington introduced the notion of "epigenetic landscape", rather than assuming the environment of a system as an independent variable, and proposed a non-standard way of looking at the micro/macro relationships in Darwinian evolution. Unlike the other authors listed, Bateson was not a specialist in one field, in search of a wider perspective. He acknowledged himself to be an autodidact, with only Nature as his book, in Galilean style. Yet his freedom of mind had a contagious effect, reinforced by brilliant prose, and the unusual connections between the most different topics he led his readers to consider, when seen in the light of general principles, contributed to a heightened attention to systemic complexity.[8]

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[6] Let me emphasise again that for the most part the books listed were collections of previously published papers, but the joining-up of ideas thus presented boosted their impact. (The entries in the list will be included, in standard reference format, within the bibliography at the end of this paper.)

[7] Similar suggestions, pointing to a cross-domain analysis of hierarchical organisation, came from Herbert Simon in the 1960s.

[8] Some years ago, on the occasion I met Nora Bateson, I referred to her father as the "Twentieth Century's Socrates". The original Socrates was not expected to provide his unlucky interlocutors with any theory and those who sold his brainstorming as a theory were simply cheating. Yet his questions made Plato's theory possible. Indeed, Bateson was criticised for his lack of step-by-step arguments and his dealing with epistemological problems in a non-professional way, but many

Thommade use ofanalytic methods developed in a branch of differential topology (singularity theory), and relied on his own classification of elementary "catastrophes" for systems governed by a potential function, as a decisive resource for understanding the most various natural phenomena, with an emphasis on biology. In the succeeding years catastrophe theory became a hot topic. Though its applications to social sciences met objections, the mathematical models had a penetrating effect and remain a source of essential tools for dealing with any process through which a small continuous change ends in a sudden transition to a different regime, corresponding to a global "qualitative" change.

According to Haken, who regarded laser beams as a paradigmatic example of self-organised coherence, the variety of types of self-organisation in nature can be covered by a small set of unifying concepts, pivoting on "the enslaving principle" through which global order parameters reduce the degrees of freedom of a system's constituents. Prigogine's research on the thermodynamics of open systems led him to describe"dissipative systems" (i.e., systems having stability far from equilibrium) as the centre of a new view of nature, in which chaos also held a place.

Maturana and Varela proposed a view of any living system, from single cells to societies, as an autopoietic machine, that is, as a "network of processes" of self-transformation. Accordingly, the components of an organism are no longer parts (organs), but rather processes such that "(i) through their interactions and transformations continuously [they] regenerate and realise the network of processes (relations) that produced them; and (ii) constitute it (the machine) as a concrete unity in the space in which they [the components] exist by specifying the topological domain of its realisation as such a network", Maturana and Varela (1980, p. 78).[9] Influenced by Bateson, this view resulted in what at Santa Fe was called as a "Complex Adaptive System" (CAS) by John Holland and Murray Gell-Mann, taking account of its feedbacks, and in fact research at the Sant Fe Institute was focused on CAS's as the key for unifying other research trends on complexity, see Holland (1995). The underlying characterisation of autopoiesis had in turn a strong influence on Niklas Luhmann's view of social systems.

Finally, the last entry in the list presents ideas which seem to have been conceived first: the research of von Foerster, linking "second order" cybernetics and biology, had been ongoing for about 20 years before converging with the growing body of work on complexity. He also established the Biological Computer Laboratory at Urbana-Champaign in 1960, which became a crucible of ideas on self-organising systems.

As for explicit philosophical commitments, both von Bertalanffy and von Foerster were originally influenced by logical empiricism, Waddington by the philosophy of A. N. Whitehead. Bateson, strongly interested in cybernetics and

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popular books on complexity have appeared in the 50 years or so since he wrote and none have matched his mastery of style.

[9] This space has its own dimensions and complexity enters the scene with the interactions among processes. Some updated formulations of such an approach can be found in Petitot et al. (1999).

itsapplications to anthropology, had a radically critical attitude towards the epistemology of modern times as “autocratic”. No less immune to twentieth century debates in philosophy of science, Thom gave a neo-Aristotelean inflection to his emphasis on emergent qualities.

In fact, the general biological inspiration of most of the seminal works listed above prepared the ground for a progressive link with emergentism, as a general philosophy, and in particular with the diachronic structure of cognitive development according to Jean Piaget, to whose ideas explicit reference is made by von Foerster, whose work, together with that of William Ross Ashby, played a key role in connecting research on cybernetics with systems theory. But von Foerster’s philosophical views also separated epistemology of complexity from mainstream philosophy of science, as independently did Bateson and Maturana and Varela, who gave expression to a dialectical view of a scalable hierarchy of systems: since each of these is anchored to its specific inner space, their position is nearer to an idealistic dialectics than to one of a materialistic kind, since no property of an observed system S is independent of the observing system S' (the supposedly neutral ambient space hosting S and S' is affected too). Subsequent work of Varela with Evan Thompson and Eleanor Rosch contains an explicit endorsement of a Buddhist worldview, see Varela et al. (1991).

The same years in which these books appeared also saw the paper by Edward Lorenz: “Unpredictability: Does the flap of a butterfly’s wings in Brazil set off a tornado in Texas?”, Lorenz (1972),[10] which had a worldwide effect in drawing attention to complexity.

Further developments in the 1980s introduced concepts and tools which strengthened the framework for treating complexity and led to new fields of application.[11] Among the chief advances were Stephen Wolfram’s use of statistical mechanics to study cellular automata, see Wolfram (1984, 2002), and Per Bak’s “self-organised criticality” deserve mention. The latter notion (introduced by Bak together with Chao Tang and Kurt Weisenfeld), corresponds to the conditions in which “mass effects” become possible and a system, so to say, lives on them, rather than dying with them, see Bak et al. (1987).

Until recent years few professional philosophers of science have paid attention to these ideas which from the 1970s onwards were becoming a source of new models of nature. One of the first exceptions was Mario Bunge, who in the fourth volume of his *Treatise on Basic Philosophy*, Bunge (1979), sought to embed an ontology of systems into the logical and algebraic framework of formal semantics.

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[10] Lorenz published no book, even one of collected papers. His name (and research) became known to a large audience through the brilliant science writer James Gleick. His book about chaos, Gleick (1987), presented a fascinating collection of case studies intended to point at a theory left to the reader’s imagination.

[11] A chronological map of the history of research on complex systems is in Baianu (2011), p. 23, which also provides information about authors and lines of research not mentioned here. Let me remind the reader that the few and cursory historical references in this paper are supposed to provide an introduction to epistemological questions about complexity.

Though complexity was not his focus of concern, the rigorous setting of his presentation shows how much care is needed when we talk of theories, models and systems. This lesson was not taken to heart in some of the cursory reflections by expert researchers in particular fields who lacked professional expertise in philosophy of science. The risks exhibited in such writings are illustrated not only in the work of past decades. There is still a persistent habit to think that the care put into one's own work within the field of one's particular scientific expertise can be dispensed with in talking about the meaning of such work or that of others. Such an unscientific habit seems widespread especially when complexity is the subject of discussion. Combined with the widespread lack of scientific education on the part of philosophers, this has made it difficult for a serious dialogue between scientists and philosophers on the topic of complexity to get off the ground.

Curiously, the above list of books offered synthetic and mainly informal expression to ideas about complexity so as to reach a wider audience, but they had a solid epistemological background, and one at odds with mainstream philosophy of science. This was true of *La Nouvelle Alliance*, a book published in 1978 by Ilya Prigogine (with Isabelle Stengers), see Prigogine and Stengers (1978), in contrast with later general books intended to communicate the meaning of the turn to complexity to a large audience.

Indeed, discussion of further aspects remained confined to journals. This especially concerns a source of conceptual seeding of the field we have already mentioned, namely the work of von Neumann and Ulam on cellular automata (the very term "theory of complexity" seems to have been first used by Christopher Langton). John Conway's algorithmic world, LIFE, is an excellent didactic reference, illustrating emergence and stability of configurations in a discrete space and time governed by a small set of simple laws. Wolfram's research in this area led him to advance a general and philosophically controversial view, whereby such an experimental approach to computation yields "a new kind of science" whose paradigm is how simple algorithmic rules give rise to complexity, Wolfram (2002).

In retrospect, we can identify which concepts from these listed works came to play the role of co-ordinate axes in research on complexity:

1. *Co-evolution* of a system and its environment (be it natural or social), together with the variability of constraints acting on large-scale processes.
2. *Emergence* of relatively stable systems through self-regulation, by taking their consistency with the laws of thermodynamics as a reference model.
3. *Non-linearity* as a recurrent feature in the dynamics leading to systems of increasing complexity. (So-called *bifurcations* are a major instance of such a "sensitivity" to small changes amplified into divergent lines of evolution, so that, when these non-linear state-transitions repeatedly occur, the dynamics of a system becomes predictably unpredictable.)
4. *Morphogenetic laws*, at work everywhere, in the growth of crystals as well as in visual gestalts.

5. *Sudden phase transitions* (loss of continuity for specific values of control parameters), for instance in case of "conflictual" states, through which a system passes from one pattern of behaviour to a radically distinct one.
6. *Attractors* of different shape for different systems, each endowed with its own set of attractor basins, in terms of which both von Foerster's slogan "order from noise" and the concept of top-down causality can finally achieve precise formulation.

These ideas were not independent of one another and a rigorous presentation might reduce the number of axes. Each was rich in meaning and its range of applications went well beyond borderline "case studies" that might leave our images of the world or of science unaffected.[12]

The widespread impression was that a new "paradigm" had been born. But what exactly? Notwithstanding that the original lines of research had different motivations and were elaborated on different theoretical backgrounds, they shared the project of identifying concepts which *transfer* from one domain of science to another. That such transfer turned out to be less smooth than claimed does not alter the fact that a path was cut across traditional boundaries between sciences: not in virtue of any "reductionist" ontology, guided by the idea all kinds of entities within a domain can be defined in terms of a single basic-level kind, but based rather on a cross-boundary language, possibly associated with an abstract theory of dynamical systems, in which patterns of complexity find expression.

An Analogy with Category Theory

Patterns of structure, order, and complexity across domains call for a theoretical framework suitable for dealing with domain-independent features, and these call, in turn, for a notion of *universality*, which is something more than independence of specific hardware.

In this respect, there is an analogy with the focus on universality characteristic of the category-theoretic approach to the foundations of mathematics. Formal details aside, this cross-boundary character is shared by category theory and "complexity theory"[13] insofar as what both intend as *universal* is the outcome of invariant relationships that are also *trans-categorical*. The categories are, in mathematics, those of each specific kind of mathematical structure (such as the category of topological spaces or the category of Abelian groups) and, in the study of

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[12] Each provided a condition on a system and, jointly taken, the set of conditions could orient to characterise complex systems, though none of them could be taken as sufficient. Some of them might also be non-necessary. For instance, a system governed by a set of linear equations can salso how a kind of complex behaviour.

[13] Inverted commas are due to the manifold ways of approaching complexity which thus far have not reached unification. In what follows I prefer to use "complexity framework".

complexity, those of each kind (class) of dynamical systems. Such “universals” are correlated with the emergence, respectively, of more cohesive totalities with respect to point-like sets of entities.

On the complexity side there are instances of different systems with the same kind of dynamical patterns; on the category side are instances of different kinds of objects and maps with the same categorical properties. In the first case, we find one and the same type of attractor for systems composed of different types of materials; in the second, within the different models of a given mathematical theory we can identify a generic model, through which any member of the intended class of objects-and-maps can be “functorially” obtained.

The underlying worry is not new at all. Already in medieval times a term was introduced for notions having a similarly cross-domain status: they were called “transcendentalia” (in English, “transcendental” [entities], not to be confused with “transcendent” [entities]). The main difference is that such a notion lacked any consideration of the maps involved as well as any consideration of dynamical aspects – concepts which are central to the categorical and complexity frameworks. In philosophy, the meaning of transcendental concepts changed after Kant, but that lack remained. In a nutshell, philosophers detected a problem without being able to solve it. It is all the more striking when the solution comes from scientific research which was not originally addressed to that problem. But in order to provide a firm solution, it must be the outcome of a theory. Now, category theory exists while no complexity theory is at hand. But what if they could work together?

It is noticeable that in both the categorical and the systemic framework the source of unity across manifold domains calls for a new language: one which does not identify a foundational project in the traditional sense – it proposes no new kind of elementary/underlying/ultimate ingredients. In the mathematical case there is no longer a uniquely defined ontological hierarchy and thus the unification of mathematics does not issue from a *reduction* of any theory to one basic theory – say set theory; in the systemic case, the demarcation between social sciences and natural sciences is no longer sharp or deep as previously thought and the unification in sight is not the result of a finally achieved *reduction* of the social to the physical – rather, there is a recurrent set of dynamical patterns, exactly as there is set of structural patterns across different areas of mathematics.

Can this analogy be relevant to scientific explanation in natural and social sciences? In recent years the pioneering work of Robert Rosen in mathematical biology has come to be acknowledged, and the width of his view that attempted to unite emergent organic complexity with category-theoretic notions has begun to be recognised, see Rosen (1987, 1991). One can doubt if the class of systems defined with this aim in view (“anticipatory systems”) will be adequate to the task, but the conceptual resources to improve on his suggestion have now become available.

On the other hand, the similarities listed so far may suggest more than we are justified to claim. Long-range order out of small-range interactions is not the same thing as structure-laws independent of the elements. Moreover, an attractor for a dynamical system is an end configuration in the set of possible states and may not be unique, whereas a generic model for a mathematical theory is initial, in the sense that any other model of the same kind factors through it, and it is unique up to isomorphism. Thus, unless we confine the analogy to a special subclass of systems, much work remains to be done to clarify the manner and the extent to which category theory can provide a fruitful setting for modelling emergent features of dynamical systems.[14] But, as already noted, whereas category theory is well-defined, complexity theory is not, being just a class of models divided by one common language.

Bypassing the Dual Language

We return to the question: what can we learn from the seminal works on complexity? There is a specifically epistemological point to emphasise: the "turn" related to the systemic approach to complexity appealed to quantitative language in treating subjects which were formerly thought to admit only qualitative characterisation. Since the "complexity turn" is often presented as standing in radical opposition to the previous tendency of post-Galilean science, it should be emphasised that its appeal to mathematical models is in fact a continuation of the Scientific Revolution. Insofar as mathematical models of complex systems provide an explanation of hitherto unexplained facts, this represents a further accomplishment of that Revolution, rather than its reversal.

It may be objected that this argument misses the point, for it overlooks the *radical difference* in the scientific image introduced by a focus on complexity. My reply involves two steps. First, motivations for the claims in the previous paragraph. Second, clarification of the problems related to such "radical difference". This section will briefly deal with the first step, the two following sections ("Some doubts" and "The complexity of science and the range of compositionality") will deal with the second one.

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[14] A personal remark may be telling at this point. In the early 1970s, as a student, my interest in complex systems was sparked by one of the most open-minded Italian physicists of that period, Giuliano Toraldo di Francia, whose courses in Florence treating foundational problems of physics I had the good fortune to follow. He gave me the opportunity to meet Prigogine and Thom. My research centred on topics, such as models of semantic cognition, which then appeared distant from applications of the complexity framework. But I was already searching for a general setting for the "cross-boundary" universality mentioned above. I found it in category theory. However the link between category theory and dynamical systems theory was at that time unclear to me, I later became aware that Bill Lawvere had already developed a categorical approach to dynamical systems Lawvere and Schanuel (1986), and that Rosen had explored applications of category theory to biological systems. Recent papers on category-theoretic treatments of dynamical systems have investigated issues of complexity from a perspective which draws on Rosen's work. Further advances are in prospect. But so far proposals to bridge the two theoretical frameworks remain too generic to provide an insight into specific open problems, or too tied to the study of particular systems which appear of little relevance for social sciences. This picture may change. If so, it will provide further evidence that my theses in Peruzzi (2006) apply to the *emergence* of cognitive patterns. See section Emergence below. One of the first papers in this direction was Ehresman and Vanbreemersch (1987).

From the seventeenth century Scientific Revolution onwards, the language of the natural sciences became progressively more mathematical. But the steady expansion of the appeal to quantities was accompanied by a recurrent debate: can a purely quantitative account cover every aspect of reality? The progress of physics (and chemistry) was made possible by the adoption of a formalism centred on the role of measurable quantities (supposed to be continuous and additive) subject to operationally defined standards in their application. Its success was so stunning that many scientists were led to identify the *real* with the *measurable*. Nonetheless, the search for (and characterisation of) *causes* in biology, psychology, economy and sociology remained confined to qualitative language. Anyone convinced that qualitative aspects are intrinsic to the characterisation of life, mind and society faced a dilemma. If such aspects are law-governed, either the laws can be expressed in quantitative terms or the whole framework of quantitative inquiry is conceptually insufficient, thus, if knowledge of such subjects is possible, our method of investigation has to be radically different.

This dilemma remains with us in social sciences, and also in psychology the debate has never ended. Something has changed, however. Quantitative methods have entered both fields and if a qualitative assertion admits an empirical test, the test is designed in quantitative terms.

There was an underlying tacit hypothesis common. Namely that *the use of quantities is committed to linearity, additivity, compositionality, all supposed to be reducible to the interactions of sharply identifiable (if not pointlike) constituents of a system*; any cause can be accordingly factorised and its effects are uniquely determined and hence predictable (in principle, with certainty). Determinism and predictability were strictly linked, if not simply confused with each other.

The rejection of such underlying hypothesis in the works mentioned above might have been expected to pose a fundamental challenge to the conception of irreducible qualities, since what the authors of the above seminal works were proposing was an image of the world strongly dependent on the study of variable quantities. What actually happened was different. The complexity framework was frequently taken as proof of the limits of quantitative science. But if qualities become *emergent quantities*, the dynamics which makes their emergence possible is expressible in equations just like the law of falling bodies or of electric repulsion. Thus, rather than bringing rhetorical slogans in support of the complexity framework or sarcastic remarks to dismiss it, the discussion would have benefited from a precise methodological analysis of the role of the two razors and their interplay.

What is at stake is more than an issue of language. The complexity framework by-passes the classical Quantity vs Quality debate, in a way reminiscent of the thesis of the transformation of quantity into quality familiar from nineteenth century dialectical materialism. But with this difference: the transformation is now formulated in precise mathematical language – that of the theory of dynamical systems. While not all aspects of dialectical materialism are preserved in this setting, it is nonetheless curious that the traces of that philosophical position have almost completely disappeared. This is strange when one considers that the complexity framework effectively undermines the fundamental opposition of "Quality" and "Quantity" which had been a recurrent theme in metaphysical thought in the Western tradition until challenged by dialectical materialism. To suggest how various have been the shapes that this opposition has taken, we mention some of them here.

Aristotle: mathematics is not suitable for the investigation of nature.

Knowledge of nature pivots on the essential qualities of any being: everything in nature tends to an inherent goal, associated with such qualities. The very motion of a body signifies that this process is ongoing – and motion is qualitatively sharply distinct from rest in an irreducible way. Moreover taxonomic classification is the paradigmatic task of science, whose aim is to assign every entity its place in nature – its proper slot in the great (and static) cabinet of Being. Equivalently, "real" definitions designed to capture essences are the aim of inquiry.[15]

Descartes: the mental is beyond the range of science.

Rather than through (qualitative) subject-predicate logic, the grounds of rationality take the form of algebraic equations. Science is a rational endeavour based on mathematics, and mathematics is essentially geometry, and geometry investigates quantities that can only be defined for something "extended" in space. Minds are not "extended": mental properties, concepts, thoughts, arguments and any other feature such as belief, desire, hope, have no length, area or volume. The conclusion, by contraposition, is straightforward.

Eddington: the scientific and the perceptual account are in mutual contradiction.

When we sit at a desk, what is in front of us is not one, but two things: the perceptible desk, as a full, rigid, piece of wood with a smooth, flat, surface of, say, brown colour, i.e., the macro-table, and the desk as described by physics, i.e., as a set of atoms bonded together, with the whole volume mostly made of empty spaces between atoms in constant motion and a surface which, if considered at the microphysical level, is not flat at all (and colour is not in the thing, but in which wavelengths of light are not absorbed).

Husserl: the quantitative worldview has led us to confuse nature with its mathematical models.

The natural sciences reduce anything to measurable objects. With such objectification, the subjective aspects of experience disappear, together with the constitutive process through which we assign meaning to what we say about nature: we become things among other things. In order to recover the very sense of scientific investigation – a sense not itself belonging to nature – a

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[15] In Popper's view it was the persistence of this idea in the social sciences which was mainly responsible for their backwardness in contrast to the natural sciences. Note here, however, that if "complexity" denotes an essential quality of a system, Popper's distinction becomes blurred.

> different kind of investigation is needed, one directed at the subjective roots of the very ideas of object, nature and science. But to identify these roots we have first to suspend, or “to bracket”, beliefs and existential assumptions and such “bracketing” paves the way to focus on pure phenomena and their essences. This is the task of *phenomenology* and it must be free of any quantitative method, on pain of a vicious circle.

Wittgenstein: quantitative language is just one among others.

> We are involved in a plurality of language games, none of which is entitled to primacy. Ordinary language is not one but rather a plurality of language games: the womb of every specific, technical, formalised, language, and in particular of any language dealing with quantities. Each specific language is legitimate as any other, provided the context of its practical use is made explicit. There is no master context and any socially shared context is equally valid.

All these views were ignored in the texts mentioned above (section “Back to origins”) though in different ways. If we look at any object around us as a relatively stable outcome of an underlying dynamics governed by principles of self-organisation, and if we apply that conception to the mind itself, the oppositions stressed by Aristotle, Descartes, Eddington and Husserl all break down, and Wittgenstein’s juxtaposition of a plurality of language games only concerns end-products. A new frontier for science is before us. This is the new frontier named “complexity”.

The problem becomes how to re-conceive the relationship between Quantity and Quality. In the first place, the opposition is no longer static: qualities emerge from a dynamics in which only variable quantities are involved. For purposes of explanation and prediction a “qualitative” dynamics may be sufficient in many cases, as for instance, when the positive, null or negative derivative of a function provided the needed “qualitative” information on the evolution of a system. The exact values of the quantities involved are irrelevant to the dynamical behaviour in certain regions of the state space but may be extremely relevant in other regions due to non-linearity. This too is something to be explained in giving an account of the emergence of certain kinds of qualities from certain kinds of quantitative change.

Some Doubts

Ontology concerns “what there is” and in recent times ontology makes use of systems theory, but the dynamic view of anything real, a view which is part of contemporary science, asserts that “what there is” is a (possibly stable) outcome of many different, ongoing, changes. This step from statics to dynamics in treating the distinction of Quality and Quantity calls for precise examination of its meaning. But instead of efforts to make the meaning precise we frequently find mere rhetoric, opening the door to conceptual confusion.

Confusion can remain even when rhetoric is avoided. The recurrent idea of complexity as at the centre of a circle of so many topics that there will be "something for everyone" invites such confusion. Its acceptance spells the end of logic, which if it does not immediately bring the end of science, certainly prepares the way. *The End of Science:* so is titled a book by John Horgan, published right after his editorial "From Complexity to Perplexity", which appeared on *Scientific American* 20 years ago, see respectively Horgan (1995, 1996).

In that editorial Horgan expressed a negative attitude to complexity in asking "is the 'New' Science of Complexity more than soup yet?"(More politely put: the idea is fine, but what are the results?) while in the book he depicted contemporary scientific research, when not a mere a list of footnotes to "Newtonian" science, as a case of dubious literary criticism, or "ironic science". Horgan's paper fuelled a harsh debate. Its very title was laden with sarcasm, whereas that of his book carried a more dramatic tone. The mention of both is meaningful here, as those who reacted to the paper did not coincide with those who reacted to the book.[16]

Horgan presented complexity as a viral marketing campaign, which by means of a sequence of assertions of predictable unpredictability, nested like Chinese boxes, threatens to end science. Twenty years later, the growth of knowledge about complex systems deserves neither sarcasm nor drama. The use of the notion simply urges care, prudence and theoretical rigour when we take complexity as a source of scientific explanation. Horgan's call for a precisely defined concept of complexity is here endorsed. His complaint that complexity violates the "Newtonian Paradigm" as the royal road of science is not. We may note in passing that Quantum Mechanics already looks non-Newtonian, yet cannot be seen as "ironic science" considering the fact that it predicts precise effects made *certain* by the *uncertainty* principle. Thus there are two problems before us:

Problem 1. Which facts are *explained* in terms of complexity that could not be explained otherwise?

Problem 2. What *predictions* does complexity offer which could not be predicted otherwise?

An epistemologically informed reader will immediately spot a further question, preliminary to Problem 1, namely which model of explanation to adopt? As for

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[16] On the conviction that (a) the achievements of science left about nothing substantial to be explained, Horgan claimed that (b) science is by now becoming postmodern, with complexity as part of this mutation. This claim was subsequently shared by some of those who advocated for (c) complexity as the new paradigm. Eminent proponents of (c) shared his idea that (d) mathematics is no longer the land of proof in announcing that (e) the deductive method has to be replaced by wide-ranging experimentalism through engineering of mathematical models. So far, none of (a)–(d) has been convincingly argued; rather there is evidence against each of them. In addition, there are doubts about the consistency of the conjunction of the four claims.