Complex Systems

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Abstract

Traditional modes of system representation as dynamical systems, involving fixed sets of states together with imposed dynamical laws, pertain only to a meagre subclass of natural systems. This reductionistic paradigm leaves no room for final causes; constrained thus are the simple systems. Members of their complementary collection, natural systems having mathematical models that are not dynamical systems, are the complex systems. Complex systems, containing hierarchical cycles in their entailment networks, can only be approximated and simulated, locally and temporarily, by simple ones. Anticipatory systems are, in this specific sense, complex, hence this introductory chapter on Complex Systems in the *Handbook of Anticipation*.

Keywords

Complex system • Simple system • Anticipatory system • Dynamical system • Impredicativity • Closed path of efficient causation • Hierarchical cycle • Emergence • Difference in kind • Simulability • Algorithm

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Complexitas

There is no unique definition of 'complexity'.

The only agreement is that scholars disagree: there are almost as many definitions of 'complexity' as schools involved in the study of the topic. The babel of the usage of the *avant-garde* word 'complexity' and all its derivatives is evident. A common intersection, however, of all characterizations of 'complexity' contains as the very minimum the requisite

$$\text{emergence of phenomena from a plurality of interactions.} \tag{1}$$

A complex system entails emergent novelties, things that are surprising, unexpected, and apparently unpredictable. A simple (i.e., noncomplex) system does not engender these counterintuitive things. But complexity is not employable as an explanatory principle of (1), 'complexity' and 'emergence' being fashionable labels of the same concept. It is the *source*(*s*) and the *cause*(*s*) of this 'emergence' in (1) that are the contentions of what constitute 'complexity'.

This chapter on Complex Systems is not meant to be a comprehensive survey of this vast subject. It is, after all, a chapter appearing in *The Handbook of Anticipation*, so the presentation will be on the connection between anticipation and complexity, with emphasis on one species of the latter. This particular species of complexity explicated herein is *impredicativity*, and is proposed in the Rashevsky–Rosen school of *relational biology* as a necessary condition for life:

$$\textit{Complexitas viventia producit.} \tag{2}$$

Complexity brings forth living beings. More explicitly, a living system anticipates, and an anticipatory system is impredicative (complex):

$$\text{Impredicativity} \supset \text{Anticipation} \supset \text{Life} \tag{3}$$

(See the chapters on "▶ Introducing Anticipation", "Relational Biology", and "Mathematical Foundations of Anticipatory Systems" in this *Handbook* for further explorations of these inclusions.) We shall begin informally with an exposition on the strategies of studying complexity before homing in on the mathematical intricacies of the beyond-algorithmicity that is impredicativity.

The antonymy of 'simple' and 'complex' (used as attributes of natural systems) has been studied in many modes and contexts, by natural scientists, social scientists, mathematicians, and myriad others. But their various distinctions, just as many other dealings with differences, may be broadly classified as either 'difference in degree' or 'difference in kind'.

If one takes a material view of nature, and believes that physics equips one with universal laws that encompass all (natural) systems, then there is only one kind of 'system' (namely, a subset of the universe), whence 'simple' and 'complex' only differ in degree. A representative proponent of this stereotypical view is von Neumann (1951, 1956), who contended that 'complexity' is a measurable (or even computable) quantity of systems that might be used to totally order them, i.e., that complexity was a kind of taxonomic index, or ranking, of systems. Further, he suggested that there was a critical value, or *threshold*, of complexity. Below this threshold were simple systems that behaved in their conventional mechanical modes, and above the threshold populated complex systems that were capable of manifesting new, counterintuitive, unanticipated modes of behavior. In this scenario, (a necessarily finite number of) repetitions (and removals) of rote operations sufficed to cross the (unavoidably fuzzy) threshold, to carry systems from one realm to the other (and back). This in-degree difference between simple and complex devolved into the equivocation of complexity with 'complication': the more constituent elementary units a system had, and the more elaborate the modes of interconnection between them were, the 'more complex' the system was. Note that the in-degree difference allows the possibility of the comparative 'more complex'.

On the other hand, an in-kind difference between the class of simple systems and the class of complex systems would require an absolute partition of the universe into two complementary sets. In the universe U of natural systems, one defines the collection P of simple systems as all those natural systems that satisfy a specific property p:

$$P = \{x \in U : p(x)\}. \tag{4}$$

(See the chapter on "▶ Relational Biology" in this *Handbook* for an explication of the Axiom of Specification and other nuances of set theory). For an in-kind distinction, one defines the collection of complex systems as its complement, the set P^c of all the natural systems that do *not* satisfy the property p; equivalently, all those that satisfy the property $\neg p$ (*not* p), i.e.:

$$\begin{aligned} P^c &= \{x \in U : \neg\, p(x)\} \\ &= \{x \in U : x \notin P\} = U \sim P. \end{aligned} \tag{5}$$

The partition $\langle P \mid P^c \rangle$ of U then entails an 'impermeable' boundary: a system is either simple or complex (but not both), and the two categories of simple systems and complex systems are mutually exclusive. From the outset, a dichotomy is established; a complex system is defined as a system that is not simple, and vice versa. In-kind difference is absolute: a natural system is complex or it is not; there is no 'more complex' in-degree comparisons among systems.

As an illustration, consider the cardinality of sets. An in-degree difference between simple and complex is akin to the classification of the size of sets into ‘small’ versus ‘big’. Instinctively, a set containing a few elements is small, while a set containing, say, a googolplex of elements is big; but the transition from small to big is fuzzy, it being context dependent. (Is 2703068 a small number, or a big number? How about 2703069?) Contrariwise, an in-kind difference between simple and complex systems is analogous to the distinction between ‘finite’ and ‘infinite’ sets. The partition between finite sets and infinite sets is impermeable. Infinite is not ‘big finite’: from within the finite realm, (a finite, however big, number of) repetitions of mechanistic operations such as ‘add one element’ will not make a finite set infinite.

Analysis and Synthesis

How should one study a given natural system? The conventional strategy is to break down the system into its constituent elements. If the resulting elements are still too complicated, the same procedure is repeated until one arrives at simple-enough elements to be able to understand them. Ideally, once the ‘elementary’ elements or particles have been found, the original system can be reconstituted from them.

This strategy goes back to Descartes’s methodological rules. It is based on two implicit and usually tacit assumptions. The first assumption is that fragmentation implies simplification: that is, the idea is that particles are indeed *simpler* than the system they compose. In this regard, it is worth noting that elementary-particle physics is apparently as good a counterexample as any other. The second assumption is that fragmentation does not eliminate *essential* information. Otherwise stated, the implicit assumption is that all the relevant properties of a system can be recovered by taking into account its elements and their relations.

This strategy of system analysis has even been elevated to the *principle* of composition, one of the fundamental assumptions of classical science. According to the principle of composition, a given entity under analytical investigation is decomposed into parts. The guiding idea is that the entity is literally made of these parts and can be reconstructed from them, and decomposition into parts misses no relevant information. This assumption is universally valid, providing that the following conditions are respected: (*a*) the interactions among the parts do not exist or are negligible, (*b*) the relations describing the behavior of the parts are linear, and (*c*) the whole resulting from the parts does not perform any functional behavior. These are, however, very severe restrictions; very few natural systems meet them. The parts of a system are in interaction, their relations in general are nonlinear, and systems are encapsulated within other systems. Systems so restricted, described as “not organized complexity”, are well represented in classic physics, and systems not so restricted (almost all natural systems), “organized complexity”, are well represented in biology (Weaver 1948).

Fundamental for organized complexities is the concept of hierarchical order, according to which systems are decomposable into subsystems and these into further sub-subsystems. One cannot fail to note that the starting point of this new vision is the system (the whole) and that systems are decomposed into subsystems, not into elements or atomic components.

While the suggestion is proposed that this divide-and-rule strategy has proved immensely successful, the systems that are entirely governed by their elements (from below, so to speak) are rare. The vast majority of systems follows a different pattern: these systems depend not only on their elements but also on the system that results from them and eventually also on higher-order systems of which they are parts (e.g., organisms, communities) (Poli 2011; Rosen 1985a). The fact is that analysis through fragmentation may inadvertently destroy the relational linkages that are crucial in the study of many kinds of systems (such as 'living' ones). Other forms of analysis (e.g., through 'subsystems') may offer better results. Synthesis, on the other hand, is a natural procedure with which to study emergence: the (unanticipated) relational connections that appear when a multitude of component systems interact. The main problem is that at least some systems cannot be fragmented without losing relevant information.

Admittedly, our understanding of non-fragmentable systems is still deficient: there is no denying that robust methods of subsystem analysis and synthesis need to be developed. Anyway, the availability of both strategies (analytic and synthetic) will enable the development of a more articulated, integral, respectful, and responsible vision of the world.

Analysis and synthesis are the two general strategies to which we may resort to understand any given system. The former strategy claims that a system is what results from its parts (look downward), while the latter strategy claims that a system is what results from the higher systems to which it pertains (look upward) (Poli 2011, 2017).

System Theory

In the same sense in which the Copernican revolution was far more than the ability to better calculate, albeit slightly, the movement of the planets, and relativity has been much more than an explanation of a small number of recalcitrant physical phenomena, the introduction of system theory is more than the study of nonlinear dynamics. What systems bring in and make visible is the idea of complexity. However, something more is at stake, namely, the difference between predicative and impredicative science.

System theory faces its difficulties too. In fact, system theory has raised both enthusiastic appreciations and even more severe denigrations. For a scathing attack on the whole "systems movement", see, e.g., Lilienfeld (1978). As Midgley (2003, *p.* xxxv) notes,

> Although Lilienfeld's book might have been a little hysterical, it struck a chord with a social science research community that was aware of some of the expensive failures and disastrous social experiments being perpetrated in city planning departments in the name of systems [*sic*] thinking.

The systemic perspective encounters resistance from other directions as well. Many scientific questions have the annoying habit of crossing departmental and faculty boundaries. Not by chance, having learnt to properly frame one's questions is customarily taken as indicative of successful training. The subsequent fragmentation into more and more restricted areas of specialization has provided such an astonishing amount of results that it almost annuls the possibility of any alternative strategy. The very idea that one science could have something to learn from another science – say sociology from biology, or the other way round – is dismissed out of hand. Dissenting voices have been feeble and substantially ineffective. The common wisdom is that there is only one universal science, namely, physics. To this end, biology deals with inordinately rare contingencies, and, say, sociology deals with second-order inordinately rare contingencies and therefore lacks any general value.

Out of darkness emerges a robust alternative strategy. Nicolas Rashevsky (1899–1972) initiated relational biology in the 1950s, and the subject was subsequently expanded and fine-tuned by his student Robert Rosen (1934–1998). Rosen explicitly posed two disturbing questions: Is physics indeed the most general science? Do we not have something to learn not only from the differences among sciences but also from their similarities?

The first question raises the possibility that physics could be very special – even inordinately so – and that a proper understanding of the duality between 'speciality' and 'generality' opens new avenues for science. The second question entails the distinction between two different modes of analysis, the structural and the functional. Learning to distinguish the two modes and to use them properly will become the gateway to a new vision of science.

A promising strategy is to distinguish between what a system is *made of* (structure) and what a system is *made for* (function) (Rosen 1971). The former attitude is isolative; the latter is relational. To fix ideas, the distinction is introduced between two different modes of *analysis*: the analysis of a system into its elements and the analysis of a system into its subsystems. One should be careful to avoid the assumption that each functional activity implies a given structure that supports it. In fact, the relation is far from being one-to-one; eventually a many-to-many relation is implied, in the sense that each function can be implemented by different structures and each structure can express different functions.

For an illustrative example, consider a production company. To survive and develop, the company should perform a variety of different functional activities, including designing new products, producing, storing, and distributing them, managing employees and workers, etc. Any of these activities may be performed by a specialized unit, or it may be split among a variety of units in many different ways. Companies make different choices in this regard. All the possibly different structural choices notwithstanding, the functions to be performed are analogous. Structures divide, functions unify.

One of the major differences between analysis via elements and analysis via subsystems is the following: given a system S, there is only one maximal set of component elements, while there are many ways to decompose the system S into functional subsystems, both at different hierarchical levels and from different perspectives. This difference underlies Rosen's claim that "there are many ways for a system of entailment to be complex; only one way for it to be simple" (Rosen 1991b).

To compound the problem, the functional perspective is not limited to the subsystem–system relation. The system itself enters into functional relations with its environment or, better, with other systems in its environment. And, as the case may be, it can establish different functional relations with different systems. Moreover, different functional subsystems can develop different functional relations among themselves. The social realm offers as many relevant exemplifications as one may wish: one may consider functional subsystems such as the economic, political, legal, scientific ones, etc., and the network of their functional interdependencies.

Each subsystem has its own models – one could say its own codes. However, to communicate with other subsystems or the overall systems, a given subsystem cannot but exploit the structures to which it has access.

An awkward and often misunderstood issue emerges here. The problem is the difference between 'doing' something and 'making sense' of what is done. Beside the difference between *ex ante* and *ex post* sense-making, i.e., between the sense of an action before it is performed and the different sense that it may acquire after it has been performed (Schutz 1967), all the systems' interactions depend on, and can be performed only through, their material structures. What a system *does* depends on its structure; what a system *means* depends on its functional interconnections.

Note that the very distinction between structural and functional organization is an outcome of the interaction with our scientific and technological capacities. Apparently, nature does not distinguish them in the same way as we do. Consider, for instance, an airplane and a bird. The airplane distinguishes the engine (power) and the lift mechanisms (the airfoil) and segregates them into separate 'organs'. The bird, instead, unifies the propeller and the airfoil into a single organ, the wing. As Rosen notes, "there is no physical mechanism which can dissect the bird wing apart in such a way that the functions are separated" (Rosen 1974). Interestingly, holograms are the only artifacts similar to natural organs.

We have seen the difference between analysis through fragmentation into elements and analysis through distinction into subsystems. Before entering into further details, the reader should take notice that the distinction between analysis through *elements* and analysis through *subsystems* is not exhaustive. A third kind of analysis should be considered, namely, analysis though separation into natural *levels*. Levels here correspond to what has elsewhere been called 'levels of reality' as distinguished from either levels of organization or levels of representation (Gnoli and Poli 2004; Poli 1998, 2001, 2006, 2007, 2011).

The availability of different kinds of analysis (and, likewise, different kinds of synthesis) shows that different strategies can be used. It is therefore important to understand the capacities and the limitations of each strategy.

The following two examples reveal something more of the tangled network resulting from the interactions among system, subsystem, structure, and function. In the case of the 'vertical' relation exemplified by the subsystem-system situation, the relevant structure automatically pertains to both of them. Even if what the structure does can be (and usually is) interpreted differently, because the system and the subsystem may adopt different models, the presence of a shared structural unit forces a level of mutual adjustment. On the other hand, the 'horizontal' relation between systems (or subsystems) is much more subject to misunderstanding, in the sense that more translations are required: the communication from system S_1 to system S_2 includes the translation from S_1 to the structure $\delta(S_1)$ of S_1 that should interact with a corresponding structure $\delta(S_2)$ of S_2, the translation between $\delta(S_1)$ and $\delta(S_2)$, and finally the translation between $\delta(S_2)$ and S_2:

$$\begin{array}{ccc} S_2 & \xleftarrow{\delta^{-1}} & \delta(S_2) \\ \uparrow & & \uparrow \\ S_1 & \xrightarrow[\delta]{} & \delta(S_1) \end{array} \tag{6}$$

Not only may each of these translations go awry, the selection itself of the structures that materially open a channel between the two systems is also a source of possible mistakes. Therefore, the unfolding of $S_1 \rightarrow S_2$ into the composition $S_1 \rightarrow \delta(S_1) \rightarrow \delta(S_2) \rightarrow S_2$ is far from being a trivial affair. It is even more complex when one realizes that, as far as social systems in particular are concerned, the value of the usual 'structural map' $S \rightarrow \delta(S)$ is rarely uniquely determined, it often being instead of the type $S \rightarrow \{\delta_1(S), \delta_2(S), \ldots, \delta_n(S)\}$. On the connection between set-valued mappings and anticipation, see Louie (2013) and Poli (2017).

Systems and Subsystems

Each subsystem uses only some of the degrees of freedom of the overall system. As a consequence, the dynamics of the subsystem and the dynamics of the overall system may and usually do diverge. The dynamic equations of the overall system include all the system's degrees of freedom. Similarly, the dynamic equations of the subsystem include all the subsystem's degrees of freedom. However, since the degrees of freedom of the subsystem is smaller than that of the overall system, the question arises as to the roles performed by the system's degree of freedom that do not contribute to the subsystem's dynamics. They may characterize other subsystems, and in general they are free to interact with other subsystems and even with other systems in the environment of the overall system. What they do, however, is outside the window of relevance of the subsystem (given by its degrees of freedom), so that they may follow codes incomprehensible from the point of view of the subsystem.

Systems (or subsystems) endowed with different models read the same underlying situation differently. "Our choice of models . . . is important because it affects how we think about the world" (Maynard Smith 1987, *p*. 120). Essentially, this is the source of both innovation and conflict. The following question arises: "how can the behaviors of different systems, perceiving the same set of circumstances but equipped with different models, be integrated?" (Rosen 1979). To begin with, conflict is a natural outcome of any differentiated society. Not only do different underlying models see the situation differently (e.g., because different observables are encoded and/or the observables are differently structured), but they generate different anticipations about the future evolution of the situation.

Not surprisingly, "most of what we call 'conflict' arises not so much in an objective situation, but in the fact that widely different predictive models of that situation are harbored by the parties to the conflict" (Rosen 1984). Indeed, many *objective* differences, such as those connected to the social division of labor, the differences in social capital (economic, cultural, relational; Bourdieu 1984); gender, age, health – that is, all the variants of social differentiation – contribute to the development of different models and are therefore sources of possible conflicts. One way to mitigate conflicts is to develop strategies for the embedding of partial individual and group models in more comprehensive ones.

Models

It is now time to address the question: "to study social systems, why should one study biological ones?" (Rosen 1979, 1984). The converse question may be raised as well: "to study biological systems, why should one study social ones?" And, more generally, the basic question is: "to study a system of type *X*, why should one study a system of type *Y*?" As Rosen notes, science is replete with relevant exemplifications. To mention but one of the examples presented by Rosen, in order to understand biological membranes, biologists study collodion films, ultrathin glass, and ion exchangers. This and many other similar cases show that it is simply untrue that "the only thing about a system which is important is the arrangement of matter within it". In fact, if it were so, "how does it happen that the study of such 'model systems' is possible at all?" (Rosen 1974).

A condition is needed for this to make sense, namely, that the two systems, as different as they are from a structural (material) point of view, are nevertheless similar enough from a functional point of view, so that one can learn something about the behavior of one system by looking at the behavior of the other.

The simplest way to exploit this intuition is to understand the dynamics of a given system as a representative of a class of systems with the same dynamics. When different systems (and related subsystems) have the same dynamics, one can use the knowledge arising from any of them to better understand any other system of the class. Since different sciences are usually differently successful in understanding different aspects of the relevant systems, each of them can have something analogous to offer to its fellow sciences.

Besides the possibility of using materially different systems (such as a biological and a social one) pertaining to the same dynamic class in such a way that one could be used to gain better understanding of the other, the same idea can be exploited for hierarchically organized systems. When different levels of organization of a biological or social system pertain to the same dynamic class, the level that is better understood can be used as a specimen for those that are less understood.

Models anticipate. In fact, the anticipatory exploitation of models is possibly the single most important reason for developing models. According to Rosen, "An anticipatory system is a system containing a predictive model of itself and/or its environment, which allows it to change state at an instant in accord with the model's predictions pertaining to a later instant" (Rosen 1985a, *p*. 339). This definition states that anticipation concerns the capacity exhibited by some systems to tune their behavior according to a model of the future evolution of themselves or the environment in which they are embedded.

The following quote, after Rosen (1979), helps developing a somewhat more concrete grasp of the situation:

> The vehicle by which we anticipate is in fact a *model*, which enables us to pull the future into the present. The stimulus for our action is in fact not simply the sight of the bear; it is the prediction or output of our model under these conditions . . . This simple example contains the essence of anticipatory behavior. It involves the concept of *feedforward*, rather than feedback. The distinction between feedforward and feedback is important, and is as follows. In a feedback system, as we have seen, control is error-actuated, and serves to correct a system performance which is already deteriorating. In a feedforward system, on the other hand, system behavior is *preset*, according to some model relating present inputs to their predicted outcomes. . . . The essence of a feedforward system, then, is that present change of state is determined by an anticipated future state, computed in accordance with some internal model of the world.

As natural as model-based anticipation may appear, its potentialities are restrained by the main assumption hidden in the modelling of physical systems championed by Newton: namely, that the dynamics of a natural system depends exclusively on present and past states of the system. No future information is ever allowed to play any role whatever. This is captured by what Rosen (1991a, *p*. 49) succinctly calls

The Zeroth Commandment *Thou shalt not allow the future to affect the present.*

For the most part, physics may consider only present states and present forces; biological, psychological, and social systems need to include also past states and forces (memory). This is already a first major difference between physical (or nonliving) and living systems. The inclusion of memory, however relevant it may be, is still not sufficient for precise distinction between nonliving and living systems. Memory-based systems can still be purely mechanical systems. Living systems require more, namely, future states and forces. It is simply impossible to perform even the simplest action without involving in one way or another the future as an active force. Therefore, underlying the idea of anticipation is

Anticipatory System's Main Assumption *Future states may determine present changes of state.*

Causes, Complexity, and Dynamics

The explicit introduction of anticipation into the scientific framework developed by Rosen rehabilitates the supposedly antiquated Aristotelian theory of the four causes: material, formal, efficient, and final. Even more importantly, Rosen advances the Aristotelian theory by showing not only that the causes can overlap with but even merge into one another. Two steps are needed to arrive at this result. The first step is to find a way to show how the first three Aristotelian causes are customarily translated into the machinery of physics. Rosen's translation in this regard is to embed the material cause in the state space, the formal cause in the space of parameters, and the efficient cause in the family of operators (Rosen 1984). The second step adds anticipation as the scientific counterpart of the final cause. The Newtonian framework does not have room for anticipation. The claim is therefore advanced that Newtonian science is too limited a framework to give proper account of the structures of reality. A more general new framework is needed, one able to include all the causes at work in reality. One may note that the theory of Memory Evolutive Systems arrives at the same result. Specifically, the merging of the causes is a consequence of the "Iterated Complexification Theorem" (see Ehresmann and Vanbremeersch 2007, Chapter 4, Section 6.1).

During the past few decades, the idea has been repeatedly put forward of using the network of causes to distinguish between complex and complicated systems. It is often claimed that complicated systems originate from causes that can be individually distinguished, can be addressed piece by piece, and that for each input to the system there is a proportionate output. On the other hand, complex systems result from networks of multiple interacting causes that cannot be individually distinguished, must be addressed as entire systems (i.e., they cannot be addressed piecemeal), and are such that small inputs may result in disproportionate effects. Unfortunately, the theoretical support for these claims is fragmented and often lacks the generality required to be fully convincing.

An intimately connected issue is the mutual transmutation of the causes, generating the collapse of the framework supporting the theory of dynamic systems. Dynamic theories have two components: instantaneous states (the values of observables at a given time point) collected into the system's state space and the modification in time of the state space (i.e., the changes of the values of states of the system) as captured by dynamic equations. When the different categories of cause are mutually interrelated, new states emerge and others may disappear. As a result, the system no longer has a preestablished, fixed once-and-for-all, state space. As soon as the state space changes, the idea of a set of dynamical equations able to capture the dynamics of the system vanishes as well.

A different way to present the same problem is to say that we do not have a dynamic theory of functional systems. The dynamic frameworks that we can exploit are limited to structural systems. The failure of the theory of dynamic systems opens interesting new avenues, among them the ontological priority of open systems over closed ones. In this regard, it is worth noting that the very idea of open system is relational, in the sense that it makes no reference to particles. Even more interestingly, the failure of the theory of dynamic system paves the way for a full-fledged theory on the emergence of new, higher-order systems from underlying preceding systems. One may note that the emergence of new systems follows a characteristic pattern: often the new emergent system is initially simpler ('more primitive' in a suitable sense of 'primitive') than the systems from which it results; then, once generated, it starts its own developmental trajectory and in time acquires new capacities.

Rosen's relational-biologic framework is the only one proposed so far that is wide enough to fully resolve the above issues. The move from predicative to impredicative science suffices. *Impredicativity*, indeed, is the definitive 'complexity'; it encompasses anticipation and life (see inclusions (3) above). And on this πέτρα the Rashevsky–Rosen school of relational biology is built. It is thus toward a formal exposition of this topic that we now turn.

Beyond Algorithms

The Rashevsky–Rosen school of relational biology resides definitively on the in-kind difference moiety of the simple-versus-complex distinction. The introduction of the Rosen essay (Rosen 1985b) serves as its manifesto:

> The thrust of this essay is that the theory of organisms, and of what we shall call *complex systems* in general, requires a circle of ideas and methods that, from the very outset, depart radically from those taken as axiomatic for the past 300 years.
>
> What we shall conclude can be stated succinctly here at the outset, as follows.
>
> 1. Our current systems [*sic*] theory, including all that presently constitutes physics or physical science, deals exclusively with a very special class of systems that I shall call *simple systems* or *mechanisms*.
> 2. Organisms, and many other kinds of material systems, are not mechanisms in this sense. Rather, they belong to a different (and much larger) class of systems, which we shall call *complex*.
> 3. Thus the relation between contemporary physics and biology is not, as everyone routinely supposes, that of general to particular.
> 4. To describe complex systems in general, and organisms *a fortiori*, an entirely novel kind of mathematical language is necessary.
> 5. A simple system can only *approximate* to a complex one, locally and temporarily, just as, e.g., a tangent plane can only approximate to a nonplanar surface locally and temporarily. Thus in a certain sense, a complex system can be regarded as a kind of global limit of its approximating simple subsystems.
> 6. Complex systems, unlike simple ones, admit a category of final causation, or anticipation, in a perfectly rigorous and nonmystical way.

One may offer the explicit.

Definition A natural system is a *simple system* if all of its models are simulable.

Definition A natural system is a *complex system* if it is not a simple system.

Here is a terse explanation of the terms, in their formal incarnations, appearing in the definitions. (Most of them are discussed in detail in the chapters on "▶ Relational Biology" and on "▶ Mathematical Foundations of Anticipatory Systems" in this *Handbook*. We shall also have more to say about them presently in this introductory chapter.) A *model* is a commutative encoding and decoding between two systems in a *modelling relation*. A model is *simulable* if it is "definable by an algorithm". (There is no need to get into the intricacies of algorithms here. It suffices to note that the crucial characterization of algorithmic and simulable as applied to formal systems is that these are concepts restricted by *finitude*, whence their simplicity. A *formal system* is "an object in the universe of mathematics". It includes, but is not limited and therefore not equivocated to, Hilbert's formalism. In this context, then, a simple system is a natural system with the property that every formal system that encodes it through the modelling relation is simulable.

Let U be the universe of natural systems and let $N \in U$. Let M be a model of N (i.e., $M \in \mathbf{C}(N)$; see the chapter on "▶ Mathematical Foundations of Anticipatory Systems" in this *Handbook* for the notations). Further, let $s(M)$ be the property 'M is simulable'. Then, by definition, the collection of all simple systems is

$$S = \{N \in U : \forall M \in \mathbf{C}(N)\, s(M)\}. \tag{7}$$

The negation of the statement 'all models are simulable' is 'there exists a nonsimulable model'; the complementary set, the collection of all complex systems, is accordingly

$$S^c = \{N \in U : \exists M \in \mathbf{C}(N)\ \neg\, s(M)\}. \tag{8}$$

As a consequence of our in-kind distinction between simple systems and complex systems, we do not equate 'complexity' with mere 'complication'. A simple mathematical example serves to illustrate the difference between the two terms. There are many methods of matrix inversion, and they are all algorithmic. The mechanism to calculate the inverse of an (invertible) $n \times n$ matrix is, therefore, simple. For small n, say up to 20, one may feasibly do the inversion 'by hand' (i.e., with pencil and paper). For larger n, modern electronic computers pick up the baton with alacrity. Whatever the size of n, the same simple-in-principle algorithms apply. For very large n, however, the matrix inversion problem becomes 'complicated'. 'Technical difficulties' include the polynomial computation time (viz., the usual 'it won't finish before the end of the universe' hyperboles), computer memory and page-faulting issues, and numerical errors due to truncation and magnitude-disparity cancellations. These 'complications', however, do not negate the fact that the processes are algorithmic and therefore *simple* (i.e., by definition *not complex*) and, indeed, some of the difficulties will disappear (or at least diminish) with technological advances. The corresponding 'complex' problem would be, say, the calculation of the inverse operator on an infinite-dimensional Hilbert space; then, of course, the problem requires a completely different solution and is not the algorithmic extension to ‘invert the $n \times n$ matrix but with $n = \infty$’.

The simple system/complex system partition is an ontological divide. But how does one epistemologically distinguish the simple from the complex? Since one cannot practically check *all* models of a system for simulability, how does one recognize a simplex system when one sees one? Chapter 8 of Rosen (1991a) and Chapter 8 of Louie (2009) contain expositions and mathematical proofs of the consequences of simplicity of systems. The reader is cordially invited to consult these two works, especially for explanation of those terms that appear below but are not explicitly defined. We will not repeat the philosophical and formal discussions here but will only give a summary of the conclusions.

These are properties of a simple system:

Theorem *If a natural system N is a simple system, then*

i. *N has a unique largest model* $M^{\max}$.
ii. *N has a finite set* $\{M_i^{\min}\}$ *of minimal models.*
iii. *The maximal model is equivalent to the direct sum of the minimal ones,* $M^{\max} = \bigoplus_i M_i^{\min}$, *and is therefore a synthetic model.*
iv. *Analytic and synthetic models coincide in the category* $\mathbf{C}(N)$.
v. *Every property of N is fractionable.*

A simple system contains no closed path of efficient causation (hierarchical cycle). The five statements in the following theorem are equivalent to one another. (See the chapter on “▶ Relational Biology” in this *Handbook* for the definitions and notations.)

Theorem

i. *If all models of a natural system N are simulable, then there can be no closed path of efficient causation in N.*
ii. *There can be no hierarchical cycle in a simple system.*
iii. *If a closed path of efficient causation exists in a natural system N, then N cannot be a simple system.*
iv. *If a closed path of efficient causation exists in a natural system, then it has a model that is not simulable.*
v. *In (the relational diagram of) a simple system, there cannot be a cycle that contains two or more solid-headed arrows.*

Simplicity of systems therefore has an equivalent, and graphically verifiable, characterization (Fig. 1).

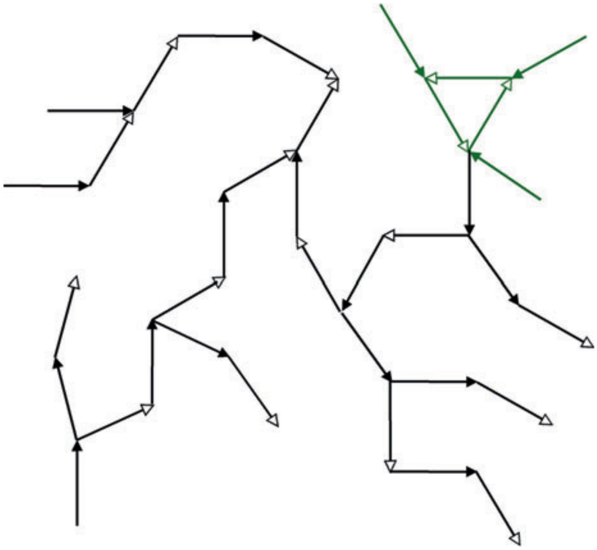


Fig. 1 Entailment network of a sample simple system: no closed path of efficient causation (The *green cycle* is a closed path of material causation, a sequential cycle)

Theorem *A natural system has no closed path of efficient causation if and only if all of its models are simulable.*

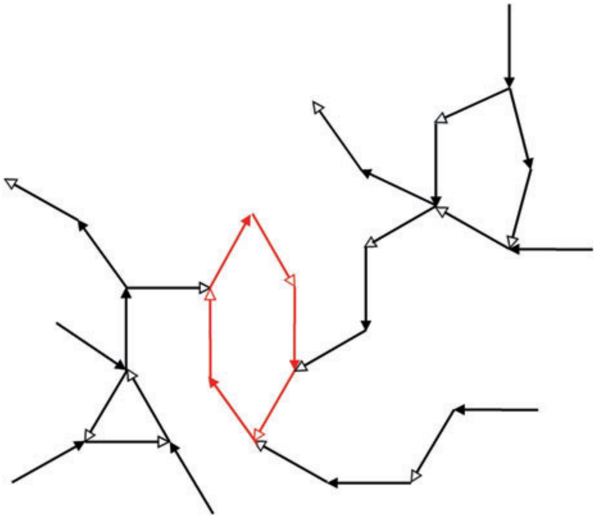
Toward Impredicativity

A complex system is one in which there must exist closed paths of efficient causation. Such hierarchical cycles cannot exist in a simple system; therein lies its feebleness, in the sense that there is insufficient entailment structure in a simple system to close a cycle of hierarchical compositions. In mathematics, cycles of this kind are manifested by impredicativities, or self-references – indeed, by the inability to internalize every referent. Simplicity is thus equivalent to predicativity. (*Predicativity* is complete, algorithmic, inferential syntacticization. *Impredicativity*, its antonym, is the property of a self-referential definition and may entail ambiguities. Cf. the chapters on "▶ Introducing Anticipation" and "▶ Relational Biology" in this *Handbook*.) One has, therefore, the following collection of five equivalent statements:

S i. *N* is a simple system.
S ii. *N* has no closed path of efficient causation.
S iii. All models of *N* are simulable.
S iv. *N* has no hierarchical cycle.
S v. *N* is a predicative system.

Complementarily, one also has the following set of five equivalent statements (Fig. 2):

Fig. 2 Entailment network of a sample complex system, with a closed path of efficient causation (shown in *red*)



S^c i. *N* is a complex system.
S^c ii. *N* contains a closed path of efficient causation.
S^c iii. *N* has a nonsimulable model.
S^c iv. *N* contains a hierarchical cycle.
S^c v. *N* is an impredicative system.

Theorem *A natural system is impredicative* (i.e., '*complex*') *if and only if it contains a closed path of efficient causation*. *A natural system is predicative* (i.e., '*simple*') *if and only if it contains no closed path of efficient causation*.

In view of this last Theorem, every appearance of the characterization 'all models are simulable' heretofore may be replaced by 'has no closed path of efficient causation', or equivalently 'has no hierarchical cycle', or equivalently 'predicative' (taking care, obviously, in rephrasing to avoid redundancies). So all discussions of simple systems may be taken without ever mentioning simulability (computability, effectiveness, 'evaluability by a mathematical (Turing) machine', or any other similar computing-theoretic concepts). In fact, the only property of simulability that is used in the proofs of the above theorems is the obvious fact and almost-tautological statement that the program of a simulable model must be of *finite* length. It is opportune to reemphasize that complex systems are 'noncomputable' in the 'non-algorithmic' sense. A suitably altered definition of computability (e.g., drop the finitude requirement, or if ever a true 'heuristic computer' is developed) can make complexity (indeed, anything at all) computable.

Although it has been proven (in Rosen 1991a and Louie 2009) that certain processes are not simulable, it is really beside the point! The sidetrack into the domain of the Church–Turing thesis is incidental. The most important conclusion is that causal entailment patterns *without* and *with* closed paths of efficient causation are *different in kind* and that the barrier between the two classes is 'nonporous':

"there are no purely syntactic operations that will take us across the barrier at all." The in-kind difference of predicativity and impredicativity is the very dichotomy between simple systems and complex systems.

The Newtonian paradigm invariably images natural systems as dynamical systems, dual structures consisting of sets of states and imposed dynamical laws. However much the languages that one uses to construct system models of whatever kind may differ, in detail and emphasis, all represent paraphrases of the language of Newtonian mechanics. Two separate ingredients are necessary for the process of system description; they are: (i) a specification of what the system is like at any particular instant of time, with the associated concept of the *instantaneous state* of the system, and (ii) a specification of how the system changes state, as a function of present or past states and of the forces imposed on the system, i.e., the *dynamics*. The characterization of the instantaneous state involves the specification of an appropriate set of *state variables*, while the characterization of how the system changes state involves a specification of the *equations of motion* of the system. Further, each natural system has a maximal image, which behaves like a free object (in the mathematical sense), and of which all formal models of the natural system are homomorphic images. All dynamical systems are simulable. In this regime, therefore, all natural systems are simple. Stated otherwise, a natural system is simple if a single description (namely, its maximal dynamical system model) suffices to account for its processes (and all one's interactions with it).

Relational biology contends that there are natural systems that possess mathematical images that are not dynamical systems; these are the impredicative complex systems. There is, in particular, no maximal description of a complex system. Complexity is a consequence of the plurality of inequivalent models. Complexity is not an intrinsic property of systems but, rather, arises from qualitatively different possible interactions. A complex system offers a multitude of partial descriptions, each an appropriate model of a different aspect of its behavior under consideration. A complex system cannot be completely and consistently described as a whole but admits a plethora of (mutually independent) partial descriptions. Each partial description represents a *subsystem* and by itself appears to model a simple system. A living system is complex. An anticipatory system is complex. A social system is complex. They are complex because one can interact with them in many ways, with no singular model sufficient for their complete characterization.

On the other hand, because complexity is subjective, depending entirely on modes of available interactions, *any* system can be made complex. A rock is usually considered a simple system, since one can only interact with it in a few mechanistic ways. Indeed, as a Newtonian particle it can be completely characterized as a simple dynamical system. However, for a geologist, say, who is equipped to interact with the rock in a multiplicity of distinct ways, a rock becomes a complex system.

It is important to note that while a complex system admits many models that are simple subsystems, the former is not a superposition of the latter. This inherent non-invertibility that a complex system is not the synthesis of its analytic parts may in fact be taken as a definition of our species of complexity that is impredicativity. (The reader is cordially invited to read Sects. 7.43–7.49 of Louie 2009 for an exposition on the amphibology of analysis and synthesis.) This departure from superposition (or 'direct sum') is the root cause that defeats attempts to characterize complexity in reductionistic terms (which have been successful for simple systems).

Summary

$$\text{Life} \subset \text{Anticipation} \subset \textbf{Impredicativity}$$

Complexity is reflected in the absence of a single overreaching system description. An immanent cause of complex systems is the multiplicity of partial descriptions, each one by itself a simple system. That is, each partial description accords with the Newtonian paradigm of a dynamical system, a state set with imposed dynamical laws.

Complexity manifests itself operationally through the failure of these partial descriptions, either individually or collectively, to account for the whole system's behavior. That is to say, a simple subsystem of a complex system is always more open to interaction than it would have been if it were merely a simple system. Such a failure to conform to predictions based on simple models is equivalent to the emergence of new modes of system behavior. Emergence manifests itself as a qualitative difference (rather than a mere quantitative difference) between what is expected and what is observed.

Closed paths of efficient causation provide a rigorous platform on which to discuss categories of final causation. This kind of finality, in turn, is scaffolding for the exploration of function (which dictates structure) and anticipation (in which the end entails the means). In short, causality in a complex system can also include what an effect entails rather than exclusively what entails the effect.

Acknowledgments We dedicate this exposition on impredicativity to Robert Rosen (1934–1998), iconoclastic mathematical biologist, whose permeating presence in this Handbook of Anticipation is keenly felt. His next monograph would have been Complexity.

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