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Organisms, Machines, and Thunderstorms: A History of Self-Organization, Part Two. Complexity, Emergence, and Stable Attractors

ABSTRACT

Part Two of this essay focuses on what might be called the third and most recent chapter in the history of *self-organization,* in which the term has been claimed to denote a paradigm shift or revolution in scientific thinking about complex systems. The developments responsible for this claim began in the late 1960s and came directly out of the physical sciences. They rapidly attracted wide interest and led to yet another redrawing of the boundaries between organisms, machines, and naturally occurring physical systems (such as thunderstorms). In this version of self-organization, organisms are once again set apart from machines precisely because the latter depend on an outside designer, but—in contrast to Kant's ontology—they are now assimilated to patterns in the inorganic world on the grounds that they, too, like many biological phenomena, arise spontaneously.

KEY WORDS: self-organization, organisms, dynamical systems, organized complexity, edge of chaos, dissipative systems, self-organized criticality

INTRODUCTION

The term *self-organization* is often used to describe a paradigm shift or revolution in scientific thinking that, according to numerous authors, has been in motion for the last three decades. New theoretical developments, it is argued, have finally enabled scientists to transcend their traditionally static and reductionist worldview, to turn their attention to the global dynamics of complex systems, and to expand their domain of competence from the world of being

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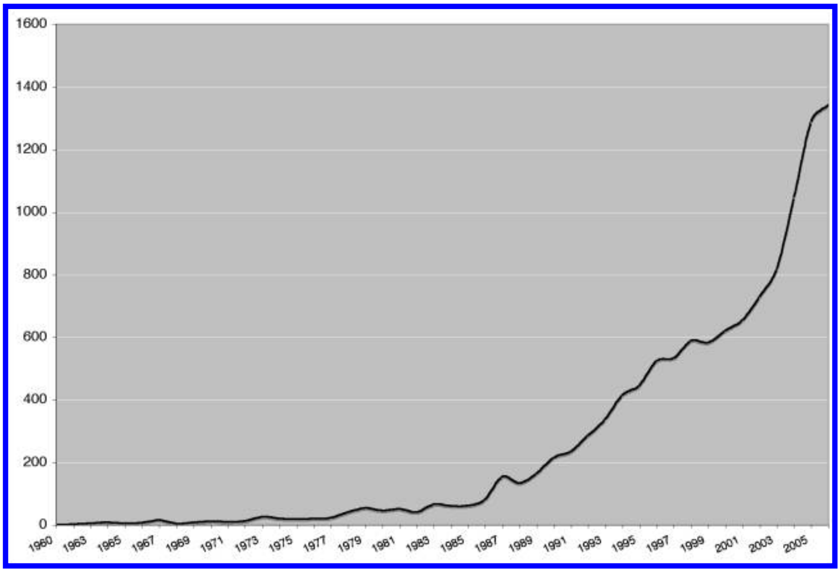

FIG. 1 Cumulative citations of self-organization (all spellings) in *Inspec, Compendex,* and *NTIS* (*National Technical Information Service*), 1960–2006.

to that of becoming; indeed, to life itself. More specifically, it is claimed that new techniques of analysis make it possible to account for the spontaneous emergence of macroscopic architectures from microscopic interactions—i.e., for the origin of order in complex systems.

Such claims derive considerable support from citation figures. Numbers alone, of course, are not generally very informative, but in this case they clearly indicate a change of some magnitude. Figure 1 graphs the frequency of the term *self-organization* as it appears in the cumulative citation index of *Inspec*, *Compendex*, and *NTIS*.[1] Still, questions remain: What exactly do these numbers tell us? What is the nature of the change to which they attest? And who are the critical players in this transformation?

One of the first to call general attention to such a transformation was the Austrian physicist Erich Jantsch, who dedicated his 1980 book *The Self-Organizing Universe* to Ilya Prigogine, a "catalyst of the self-organization paradigm."[2]

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[1] *Inspec, Compendex,* and *NTIS* (*National Technical Information Service*) are databases providing access to bibliographic citations searching over 7.7 million abstracts from scientific and technical literature. Materials covered include journal articles, conference proceedings, reports, dissertations, patents, and books; http://www.engineeringvillage2.org (accessed 15 Jul 2008).

[2] Erick Jantsch, *The Self-Organizing Universe: Scientific and Human Implications of the Emerging Paradigm of Evolution* (Oxford: Pergamon Press, 1980), v. This book was published the same year as Ilya Prigogine, *From Being to Becoming* (New York: Freeman, 1980).

Others making similar claims have pointed to earlier, more diverse and expansive origins, crediting, in addition to Prigogine, cyberneticists Ross Ashby and Heinz von Foerster, mathematician René Thom, physicists Hermann Haken and Manfred Eigen, and others, interpreting their interventions as providing different perspectives on, and different contributions to, an essentially unified concept. For example, in their 1990 book *Self-Organization: Portrait of a Scientific Revolution*, Wolfgang Krohn and his colleagues[3] argued that, while certainly not homogeneous, ideas developed from the 1960s to the present (or at least to 1990) can be integrated into a single paradigm, all relying on the idea of "self-organization." These authors began their modern chronology with the work of von Foerster (much as they began their volume with an essay by Humberto Maturana, the Chilean biologist who, in the early 1960s, first developed the concept of *autopoiesis*[4]), and they moved smoothly from those beginnings to the work of physicists Prigogine and Haken and the mathematics of limit cycles and attractors in nonlinear dynamical systems.

My own view is somewhat different. While it is certainly possible to find connections linking the various developments that find shelter under the umbrella term *self-organization,* I argue that they represent fundamentally different approaches to the problem of spontaneously self-organizing systems, only some of which are reflected in the mainstream scientific literature and, hence, in the citation data reported here. Furthermore, I assert that the change these data indicate has additional roots that tend not to be included in these accounts. If a new paradigm of self-organization is in evidence in the scientific literature, I contend both that it is considerably narrower than these accounts suggest and that it grew less out of efforts in cybernetics and philosophy (such as the efforts described in Part One of this essay) and more out of technical developments, primarily in the Soviet Union, that had little if anything to do either with Kant's understanding of this term or with any of the nineteenth-century or early twentieth-century thinking on this subject—and indeed, at least in the early days, had little to do with living organisms.[5]

In my interpretation of this history (and the data of Figure 1), the view of self-organization that arose out of cybernetics—especially the efforts of

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[3] Wolfgang Krohn, Gunter Küppers, and Helga Nowotny, eds., *Self-Organization: Portrait of a Scientific Revolution* (Dordrecht: Kluwer Academic, 1990).

[4] See Maturana's account of the development of his concept in Humberto Maturana Romesin, "Autopoiesis, Structural Coupling, and Cognition," http://www.isss.org/maturana.htm (accessed 20 Jan 2008).

[5] Evelyn Fox Keller, "Organisms, Machines, and Thunderstorms: A History of Self-Organization, Part One," *HSNS* 38, no. 1 (2008): 45–75.

Marshall Yovits (director of the Information Systems Branch at the U.S. Office of Naval Research), von Foerster, and their colleagues to promote self-organization as a central topic for mainstream research in the U.S.—reached an end with the triumph of the vision of artificial intelligence promoted at MIT by Marvin Minsky and the demise of Frank Rosenblatt's Perceptron project (Rosenblatt himself dying a few years later).[6] Indeed, lists of conferences on self-organization show a substantial gap of almost twenty years, starting in the mid-1960s and continuing to the early 1980s. Furthermore, when such conferences did resume, they bore little relation to the work on self-organization that was just then bursting onto the scene.[7]

The history of contemporary understandings of self-organization has been recounted in a number of popular accounts that have tended to emphasize either particular technical achievements[8] or larger cultural dynamics responsible for a trans-disciplinary shift in *episteme*.[9] My own recounting of this history is more complex, perhaps even chaotic: it admits of no simple narrative, but instead seeks to recognize (and briefly describe) the many different dynamics crucial to the story.

CONTRIBUTIONS TO SELF-ORGANIZATION FROM MATHEMATICS AND PHYSICS

Nonlinear Mathematics and Dynamical Systems

My account begins in the early 1960s (thus demarcating the third chapter of my history of self-organization), but instead of looking to second-order cybernetics, I choose a different starting point: the sudden awakening of

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[6] For a fuller account of these events, see E. F. Keller, "Marrying the Pre-Modern to the Post-Modern: Computers and Organisms after WWII," in *Growing Explanations: Historical Perspectives on Recent Science*, ed. M. Norton Wise (Durham, NC: Duke University Press, 2004), 181–200.

[7] Also, the citations that did appear during the 1960s and 1970s refer primarily to literature published in the Soviet Union, where the term *self-organization* had a still different set of references. There, although many of the techniques were taken from the work in the U.S., the term was more likely to refer to applications of Rosenblatt's Perceptron algorithms to pattern recognition in data fields than to the design of animal-like machines or to basic principles biological or machine organization. Oddly enough, almost none of the Soviet citations relate to the robust tradition of mathematical work on nonlinear dynamics that had been flourishing in the Soviet Union since the early part of the century.

[8] See, e.g., Heinz R. Pagels, *The Dreams of Reason: The Rise of the Sciences of Complexity* (New York: Simon & Schuster, 1988).

[9] N. Katherine Hayles, *Chaos Bound: Orderly Disorder in Contemporary Literature and Science* (Ithaca, NY: Cornell University Press, 1990).

American researchers in the late 1950s and early 1960s to the remarkably rich and flourishing tradition of work in the Soviet Union on stability in nonlinear dynamical systems—a tradition beginning with the Russian mathematician A. M. Lyapunov (1857–1918) in the late nineteenth and early twentieth centuries, but one which had been virtually unknown in the U.S.[10] Although virtually none of this literature made explicit use of the term *self-organization*, I claim that its new availability after 1960 had a direct and unmistakable impact on the course of research (especially American research) in a number of different, albeit related, fields (mathematics, theoretical physics, and control engineering) in ways that were critical to the emergence of modern understandings of self-organization.

Its impact was most immediately evident in control theory, and for obvious reasons. Control theory is the branch of engineering concerned with guaranteeing that the output of a dynamical system with a finite input remains itself finite, i.e., that the system remains stable. Classical control theory flourished in the U.S., but prior to 1960 it had been limited to systems in which the output was a linear function of the input; most real systems, however, are nonlinear. In 1960 the first conference of the newly formed International Federation of Automatic Control was held in Moscow, and in that same year R. E. Kalman and J. E. Bertram introduced American researchers in control theory to the early work of Lyapunov, thereby helping to inaugurate the era of modern control theory.[11] The crucial realization of Kalman and Bertram was that Lyapunov's methods made it possible to extend the classical theory into the domain of nonlinear systems. A year later, the U.S. Atomic Energy Commission published a comprehensive account of Lyapunov's methods and their applications in English.[12] Similarly, in the early 1960s, the work of A. A. Andronov, L. S. Pontryagin, N. Kryloff, N. Bogoliuboff, and the Romanian V. M. Popov began to be widely distributed in the U.S.

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[10] Lyapunov's work is now sometimes credited as the beginning of nonlinear stability theory, i.e., of the analysis of the stability of solutions to nonlinear differential equations describing the behavior of dynamical systems.

[11] R. E. Kalman and J. E. Bertram, "Control System Analysis and Design via the Second Method of Lyapunov," *American Society of Mechanical Engineers* D 82 (1960): 371–400. See also R. E. Kalman, "Lyapunov Functions for the Problem of Lur'e in Automatic Control," *Proceedings of the National Academy of Sciences* 49 (1963): 201–05.

[12] V. I. Zubov, *Mathematical Methods of Investigating Automatic Regulation Systems*, AEC-tr-4494, Office of Technical Services, Department of Commerce, Washington, DC, Sep 1961, and *Methods of A. M. Lyapunov and Their Applications* (1957 in Russian); English trans., AEC-tr-4439, Office of Technical Services, Department of Commerce, Washington, DC, Oct 1961.

To be sure, some American mathematicians were already familiar with the Soviet literature on nonlinear dynamical systems, primarily as a result of the efforts of the Russian-born mathematician Solomon Lefschetz at Princeton University. Lefschetz was introduced to the rich vein of research on nonlinear oscillations and stability by Nicholas Minorsky in the course of his work for the U.S. Navy during World War II, and he immediately set out to redress its long neglect among his colleagues and students. In 1943, he translated the work of Andronov and Chaiken on the theory of oscillations, and in 1949 translated an introduction to nonlinear mechanics by Kryloff and Bogoliuboff.[13] In 1946, with the support of the Office of Naval Research, Lefschetz organized a research center at Princeton on differential equations that he directed until he retired in 1953. The center was phased out after Lefschetz's retirement, but in November 1957, one month after the launching of *Sputnik*, Lefschetz received a new mandate to establish a center for differential equations, this time based in industry. Originally called RIAS (Research Institute for Advanced Studies), the center moved to Brown University in 1964, where it was renamed the Lefschetz Center for Dynamical Systems.[14] By this time Lefschetz's efforts were joined by a massive (mostly government-sponsored) program to make the Soviet research on this subject available to English-speaking readers—a program launched independently of Lefschetz's own endeavors. Translation projects initiated by, among others, General Dynamics Corporation, the Wright Air Development Center (WADC/WADD), NASA, the AEC, and the NSF, as well as a private initiative of the Consultants Bureau (later to become Plenum Publishing Corporation), resulted in the sudden availability to English-speaking students in

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[13] A. A. Andronov and C. E. Chaikin, *Theory of Oscillations* (Princeton: Princeton University Press, 1949); N. Kryloff and N. Bogoliuboff, *Introduction to Non-Linear Mechanics* (Princeton: Princeton University Press, 1943).

[14] This brief account is taken mainly from Sir William Hodge, "Solomon Lefschetz," *Contemporary Mathematics* 58, pt. 1 (1986): 7–46. As Hodge wrote, "By making available translations of the leading Russian workers in the field, by lecturing, by writing textbooks and review articles, as well as by original papers of his own, he stirred up enthusiasm and created one of the leading schools in the country. Many younger men owe their introduction to the subject to his books" (41). In a similar vein, H. A. Antosiewicz wrote in his review of Lefschetz's 1961 book (co-authored by Joseph LaSalle), *Stability by Liapunov's Direct Method with Applications* (New York: Academic Press, 1961): "The fact that [these methods] begin to be used now is in no small measure due to the authors' persistent efforts to acquaint engineers and mathematicians alike with this area of stability theory, which was developed almost entirely in the Soviet Union." Quoted in H. A. Antosiewicz, *Bulletin of the American Mathematical Society* 69, no. 2 (1961): 209–10.

mathematics and control theory of a huge literature that had previously been largely unknown.[15]

Why this sudden expansion of interest in Soviet mathematics, and especially in the work of Lyapunov? The answer seems clear: the successful launching of *Sputnik* in October 1957 stunned American scientists and engineers and served as a wake-up call. Before *Sputnik* (and apart from Lefschetz's school), not much attention was paid to the work being done in the Soviet Union on nonlinear dynamics, but the situation changed, decisively, in its aftermath. Looking backwards, F. L. Lewis observes that, "Given the history of control theory in the Soviet Union, it is only natural that the first satellite . . . was launched there."[16]

Studies of nonlinear dynamical systems in the West exploded after 1960, and not only in control theory. Indeed, one of the more conspicuous features of the Soviet literature on nonlinearity is the remarkably close (and apparently seamless) interaction it suggests between mathematical theory and at least some areas of engineering practice.[17] Given this proximity, it was perhaps inevitable that it would also have a noticeable impact on American researchers in mathematics (both pure and applied) and theoretical physics. But there were other pathways through which this literature exerted influence, such as via earlier translations or the intermediary of Russian-speaking mathematicians in the West. Stephen Smale, for example, began his important mathematical work on dynamical systems in 1958, just prior to this wave of translations, by extending the earlier works of Kolmogoroff, Andronov, and Pontryagin. David Ruelle entered the fray in the late 1960s and early 1970s with his pioneering work on the

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[15] By 1963, over half of the items in the bibliography for stability theory in ordinary differential equations provided by Cesari's widely used textbook were from Soviet publications; two years later, R. L. Drake from NASA updated that bibliography, adding another 891 references, the overwhelming majority from Soviet sources. Lamberto Cesari, *Asymptotic Behavior and Stability Problems in Ordinary Differential Equations*, 2nd ed. (Berlin: Springer-Verlag, 1963); R. L. Drake, Reference List for Stability Theory in Ordinary Differential Equations, Contract No. NAS 9-11196, Drexel Institute of Technology Project No. 243, 1965.

[16] F. L. Lewis, *Applied Optimal Control and Estimation* (New York: Prentice-Hall, 1992); http://www.theorem.net/theorem/lewis1.html (accessed 3 Nov 2007).

[17] Bissell suggests that the role of Aleksandr Aleksandrovich Andronov was of particular importance in these interactions. See Chris Bissell, "A. A. Andronov and the Development of Soviet Control Engineering," *IEEE Control Systems* 17, no. 1 (1998): 56–62. See also his "Control Engineering in the Former USSR: Some Ideological Aspects of the Early Years," *IEEE Control Systems* 19, no. 1 (1999): 111–17, as well as A. Dahan (in collaboration with I. Gouzevitch), "Early Developments of Nonlinear Science in Soviet Russia: The Andronov School at Gor'kiy," *Science in Context* 17, nos. 1/2 (2004): 235–65.

mathematics of turbulence and phase transitions, and he too made good use of the Soviet literature.[18] Similarly, Edward Lorenz, working at MIT in meteorology in the early 1960s and relying on the computer to simulate global weather patterns, drew on earlier translations of Nemytskii and Stepanov for his understanding of the aperiodic behavior he had observed in a simple, deterministic model, and perhaps even for his recognition that minute variations in initial conditions could yield dramatically different results (the so-called "butterfly effect").[19] The actual term *chaos* was not introduced until 1975 when T. Y. Li and James Yorke[20] used it to refer to the unpredictable behavior of nonlinear deterministic systems, in which small changes in initial conditions can lead to very large changes over time; all the same, Lorenz's 1963 paper is often claimed to have initiated the history of chaos theory. By the mid-1980s, the study of equilibrium states into which such systems may settle until disrupted by a perturbation (stable attractors and limit cycles) had become a veritable industry. Since then, chaos theory has inspired numerous historical accounts; but for my purposes the important point is that, despite the diversity of those accounts, they all begin in the early 1960s.

Thermodynamics, Statistical Mechanics, and the Term *Self-Organization*

There are, of course, other ways to chart the history of this third chapter of *self-organization*—ways that make no explicit mention of the mathematical prehistory in the Soviet Union—and these too date back to the 1960s. Perhaps it is not accidentally so; indeed, I suggest there are at least implicit links tying these different histories together. Here is one, particularly familiar, alternative account that features the Russian-born physicist who later received a

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[18] For a more complete account see, e.g., David Aubin, "From Catastrophe to Chaos: The Modeling Practices of Applied Topologists," in *Changing Images in Mathematics: From the French Revolution to the New Millenium,* ed. A. Dahan Dalmedico and U. Bottazini (London: Routledge, 2001), 255–79, and David Aubin and Amy Dahan Dalmedico, "Writing the History of Dynamical Systems and Chaos: *Longue Durée* and Revolution, Disciplines, and Cultures," *Historia Mathematica* 29, no. 3 (2002): 273–339.

[19] Edward N. Lorenz, "Deterministic Nonperiodic Flow," *Journal of Atmospheric Sciences* 20 (1963): 130–41; V. V. Nemystkii and V. V. Stepanov, *Qualitative Theory of Differential Equations* (Princeton: Princeton University Press, 1947, 1960).

[20] Tien-Yien Li and James A. Yorke, "Period Three Implies Chaos," *American Mathematical Monthly* 82, no. 10 (1975): 985–92. Interestingly, this paper contains no references to the Soviet literature, relying instead on references to Lorenz and Smale.

Nobel Prize for this work. Ilya Prigogine was director of the Solvay International Institutes of Physics and Chemistry in Brussels from 1959 and of the Center for Statistical Mechanics and Complex Systems at the University of Texas, Austin from 1967, holding both positions until his death in 2003.There is no doubt about Prigogine's role in the surge of papers and conferences in the self-organization industry, but he was far from the sole contributor. In particular, his contributions need to be set not only against the backdrop of the growing prominence of nonlinear mathematics and dynamical systems, but also alongside the work of such contemporaries as Hermann Haken and Manfred Eigen. Furthermore, the role he did play in this history was a complex one—part scientific, part synthesizing, and part promotional.

In 1967, after over two decades of work on the thermodynamics of irreversible (but close-to-equilibrium) processes, Prigogine and his student Grégoire Nicolis published a paper, "On Symmetry-Breaking Instabilities in Dissipative Systems."[21] The principal scientific aim of the paper was threefold: first, to underscore that the kinds of instability familiar from fluid dynamics (as in Raleigh-Bénard convection, where a homogeneous horizontal fluid layer heated from below becomes unstable at a critical rate of heating and subsequently settles into a stable hexagonal pattern) also occur in "purely dissipative systems" that involve chemical reactions and diffusion but no hydrodynamic forces; second, to integrate all of these findings into the language and established principles of thermodynamics; and third, to show that such phenomena require that the system in question be both thermodynamically open and far from equilibrium. The authors began with the reaction-diffusion model that Alan Turing had introduced fifteen years earlier to illustrate a possible mechanism for the biological phenomenon of morphogenesis,[22] and proceeded to submit this model to detailed analysis over a far greater range of parameters than Turing had done.

Prigogine and Nicolis's primary focus was not nonlinearity per se, but the thermodynamic properties of a system in which instabilities can give rise to new stable states that "break" the symmetry of the original states. That such effects depend on nonlinearity was less striking to them than was their dependence on the flow of free energy that keeps the system far from equilibrium. There is no mention in this paper of *bifurcation*—the term that mathematicians

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[21] I. Prigogine and G. Nicolis, "On Symmetry-Breaking Instabilities in Dissipative Systems," *Journal of Chemical Physics* 46, no. 9 (1967): 3542–50.

[22] A. M. Turing, "The Chemical Basis of Morphogenesis," *Physics Today* B237 (1952): 37–72.

would have used in lieu of *symmetry breaking*[23] (and a term most authors used in subsequent years)—nor is there any mention of *phase transitions*—another analogy that later became ubiquitous.[24] For now, the authors were content to unify Turing's reaction-diffusion patterns with the spontaneous structures observed in fluid dynamics under the category of dissipative structures and to draw out some rather striking and large implications.

For one, they argued that the formation of novel spatial and temporal structures, far from being rare, is to be expected wherever there is a large flux of energy through a system. To my knowledge, the first time the term *self-organization* appeared in this context is on the second page of the Prigogine and Nicolis paper, where it was introduced, almost as an aside, by way of indicating the second significant implication the authors wanted to draw. After summarizing their main argument, they wrote: "We also have the opportunity to indicate briefly the implications of such 'self-organizing' open systems for biogenetic processes."[25] Here the term *self-organizing* appears as an effective synonym for the spontaneous formation of "dissipative structures,"[26] or, more specifically, for the emergence of such structures in low-entropy, far-from-equilibrium systems.

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[23] In the 1960s, given its enormous importance in high-energy physics, the term "symmetry breaking" probably carried considerably more cachet than "bifurcation," and this too may have influenced the authors' choice of terms. By 1974, however, they seem to have incorporated the mathematical term as well (see G. Nicolis and I. Prigogine, "Introductory and Inorganic Oscillations: Thermodynamic Aspects and Bifurcation Analysis of Spatio-Temporal Dissipative Structures," *Faraday Symposium of the Chemical Society* 9 (1974): 7–20).

[24] I would guess that the analogy with phase transitions originated with Hermann Haken (see below) and first showed up in Prigogine's work in 1971. See G. Nicolis and I. Prigogine, "Fluctuations in Nonequilibrium Systems," *Proceedings of the National Academy of Sciences* 68, no. 9 (1971): 2102–07.

[25] Prigogine and Nicolis, "Symmetry-Breaking" (ref. 21), 3543.

[26] In Prigogine's autobiographical notes (http://nobelprize.org/nobel_prizes/chemistry/laureates/1977/prigogine-autobio.html) (accessed 5 Nov 2007), he wrote that he introduced this term in 1967 in his paper on "Structure, Dissipation and Life," presented at the first International Conference on Theoretical Physics and Biology, held in Versailles, Jun 1967. See M. Marois, ed., *Theoretical Physics and Biology* (Amsterdam: North-Holland, 1969), 23–52. In fact, however, what was novel was not so much the term as his focus. The expression "dissipative systems" had been the subject of extensive study in fluid dynamics and thermodynamics ever since the work of Lord Raleigh and Lord Kelvin in the nineteenth century, and the term "dissipative structures," though not common, was used on several occasions in the twentieth century to refer to various kinds of structures that arise in dynamical systems. See, e.g., Gregory H. Wannier, "The Threshold Law for Single Ionization of Atoms or Ions by Electrons," *Physical Reviews* 90 (1953): 817–25. Nevertheless, after 1967 the term became so closely associated with Prigogine that it can be taken as his trademark.

The principal point—and what distinguishes such patterns from, for instance, the formation of oil drops or snowflakes—is precisely their dependence on a flow of energy (and, in some cases, of matter). The link to biology is, at this point, merely a promissory note; indeed, the authors would have to wait three years for the first proposal of a direct link between such symmetry breaking and a real biological system, and this came with the publication of a Turing-like model of spontaneous aggregation in cellular slime mold that was developed in the U.S.[27] The promise of a link to biology was not entirely new, however, for the whole point of Turing's original model was an attempt to account for embryogenesis. But Prigogine and Nicolis were confident "that the importance of such instabilities goes far beyond the morphogenetic problem discussed originally in Turing's paper"[28]—they envisioned an important role for such instabilities in the origin of life.

A particularly important landmark in this history of self-organization was the publication in 1971 of Manfred Eigen's extensive monograph on "Self-organization of Matter and the Evolution of Biological Macromolecules."[29] Eigen, awarded the Nobel Prize for chemistry in 1967 for his work on high-speed chemical reactions, here turned his attention to biology, specifically to the problem of emergence of large macromolecules (like DNA) in the origin of life (as if taking up Prigogine's challenge). He explicitly referred to the work of Prigogine and Nicolis on instabilities in the vicinity of far-from-equilibrium steady states, but he drew an important distinction: "The type of organization we need at the beginning is not so much organization in physical (i.e., geometrical) space. We need functional order among a tremendously complex variety of chemical compounds. . . . We need organization in a different 'space,' which one may call 'information space.'"[30]

Indeed, Eigen focused directly on the recent findings of molecular biology and asked how such a complex informational molecule as DNA, and such a sophisticated relation to protein synthesis as suggested by the genetic code, might ever have arisen in the first place. Although Eigen did not completely solve the problem, he made two crucial contributions that appeared in all

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[27] E. F. Keller and L. A. Segel, "Slime Mold Aggregation Viewed as an Instability," *Journal of Theoretical Biology* 26 (1970): 399–415.

[28] Prigogine and Nicolis, "Symmetry-Breaking" (ref. 21), 3550.

[29] Manfred Eigen, "Selforganization of Matter and the Evolution of Biological Macromolecules," *Naturwissenschaften* 58, no. 10 (1971): 465–523; see also "Molecular Self-Organization and the Early Stages of Evolution," *Quarterly Review of Biology* 4, no. 2 (1971): 149–212.

[30] Eigen, "Selforganization of Matter" (ref. 29), 470.

subsequent discussions of the topic: first, he discovered the "error catastrophe" (i.e., the limit that the need for fidelity in replication placed on the size of nucleic acid molecules that might spontaneously evolve), and second, he developed the idea of hypercycles (referring to a doubly autocatalytic system in which the synthesis of two molecules is mutually reinforcing). This model had its difficulties, but generations of future researchers were inspired to improve upon it.

Eigen's name may not be as well known today as Prigogine's, but his work was influential, and certainly sufficiently so as to draw serious scientific attention to the new context in which Prigogine had begun to use the term *self-organization*, whether or not the focus was the origin of life. In June 1971—shortly before his papers appeared in *Naturwissenschaften*—Eigen presented his work at the Third International Conference on Theoretical Physics and Biology, held in Versailles.[31] Prigogine, Nicolis, and Hermann Haken were all in attendance.

Haken, a theoretical physicist from Stuttgart, was known for his work on the statistical mechanics of lasers and nonlinear optics (the branch of optics concerned with the behavior of high-intensity light, such as that emitted by lasers, in nonlinear media). He also had been present at the Versailles conferences in 1967 and surely would have recognized a connection between his own interest in cooperative phenomena in nonlinear systems and those of Eigen and Prigogine. But Haken's focus was neither on the origin of biological macromolecules nor on thermodynamics; rather, he aimed to forge a new discipline linking nonlinear dynamical systems theory with statistical physics and, relatedly, linking what Prigogine and Nicolis had called "symmetry breaking" with phase transitions. He called this discipline *synergetics*.[32] In 1972 Haken organized the first of a series of workshops on synergetics (subtitled "cooperative phenomena in multicomponent systems"), and in 1975 he published an extensive review of what he called "cooperative phenomena in systems far from thermal equilibrium."[33]

As already mentioned, Prigogine focused on thermodynamics rather than on statistical mechanics, and he had his own preferred terminology. By 1975, the terms *self-organization* and *dissipative structures* had begun to assume a

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31. M. Marois, ed., *From Theoretical Physics to Biology: Proceedings of The Third International Conference on Theoretical Physics and Biology, Versailles, 21–26 June 1971* (Basel: S. Karger, 1973).

32. Haken introduced the term synergetics in 1971 to describe the cooperative behavior (*Zusammenwirken*) of individual atoms in a monochromatic light beam. H. Haken and R. Graham, "Synergetik—Die Lehre vom Zusammenwirken," *Umschau in Wissenschaft und Technik* Heft 6 (1971): 191–95.

33. H. Haken, "Cooperative Phenomena in Systems Far from Thermal Equilibrium and in Nonphysical Systems," *Reviews of Modern Physics* 47 (1975): 67–121.

characteristic prominence in his published papers, and finally, in 1977, a comprehensive account appeared in a book he co-authored with Grégoire Nicolis, *Self-Organization in Nonequilibrium Systems: From Dissipative Structures to Order through Fluctuations.*[34] Part One of this book provided a brief overview of the "Thermodynamic Background," while Part Two gave a more extensive introduction to the "Mathematical Aspects of Self-Organization," including lengthy discussions of classical analysis of stability (linearization), Lyapunov instability, bifurcation theory, and so on. In the same year Prigogine was awarded the Nobel Prize in chemistry "for his contributions to non-equilibrium thermodynamics, particularly the theory of dissipative structures."[35]

Haken published his own book in 1977, *Synergetics, An Introduction: Nonequilibrium Phase Transitions and Self-Organization in Physics, Chemistry and Biology*.[36] This work expanded on his earlier review article, with the notable difference that he now claimed *self-organization* as a synonym for *synergetics*. In fact, the subject matter of the two books was strikingly similar: although Haken's work included a discussion of laser physics not found in Nicolis and Prigogine's, and the latter included an extensive discussion of thermodynamics not found in the former, both books covered much the same mathematical ground, each emphasizing nonlinearity. (In a second edition appearing a year later, Haken added a final chapter on "the rapidly growing field of chaos."[37]) The reference to biology in Haken's title was borne out with examples in ecology and morphogenesis.

A New Paradigm

With the publication of these two volumes, things took off quickly. A number of international conferences on self-organization convened over the next few years (Rostock in 1977; Berlin and Bavaria in 1982; Austin, Texas, and the Soviet Union in 1983).[38] Both Haken and Prigogine were cited extensively in

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[34] G. Nicolis and I. Prigogine, *Self Organization in Non-Equilibrium Systems: From Dissipative Structures to Order through Fluctuations* (New York: John Wiley & Sons, 1977).

[35] Royal Academy press release, 11 Oct 1977.

[36] H. Haken, *Synergetics, an Introduction: Nonequilibrium Phase-Transitions and Self-Organization in Physics, Chemistry, and Biology* (Berlin: Springer-Verlag, 1977).

[37] Ibid., 2nd ed., vii.

[38] Werner Ebeling, ed., *Stochastische Theorie der nichtlinearen irreversiblen Prozesse ausgewählte Vorträge der Tagung Nichtlineare irreversible Prozesse und spontane Strukturbildung* (Rostock: Wilhelm-Pieck-Universität Rostock, 1977); W. Ebeling and R. Feistel, eds., *Physik der Selbstorganisation*

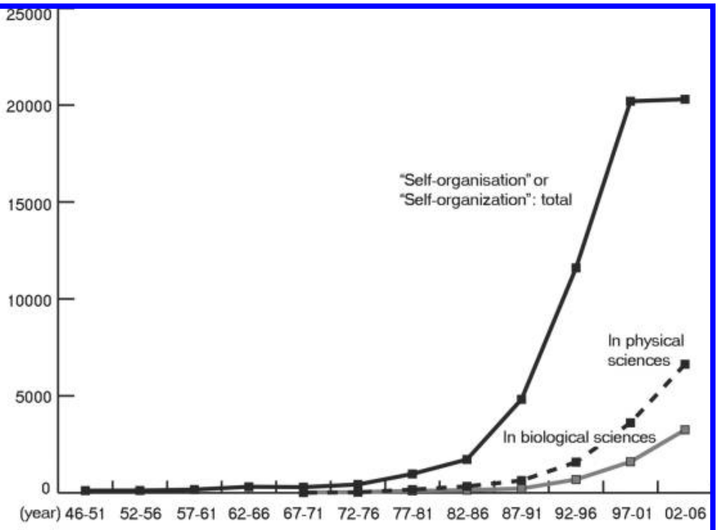

FIG. 2

"Self-organisation" or "Self-organization" (all spellings): total	113	120	168	311	292	430	974	1820	4820	11600	20200	21000
In physical sciences					6	26	167	333	633	1580	3610	6630
In biological sciences					23	35	100	139	220	688	1600	3250

all of the published proceedings. A more detailed analysis of the literature on self-organization during this period, as cited in Google Scholar,[39] shows more than a doubling in the overall (exponential) growth rate, with a particularly marked growth in the physical science literature (see Figure 2). Figure 3 displays a further breakdown of this growth in which the relative influence of Prigogine and Haken in the scientific community is parsed by tracking the frequency of the characteristic (and idiosyncratic) terminology of each author

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und Evolution (Berlin: Akademie-Verlag, 1982); "Evolution of Order and Chaos in Physics, Chemistry, and Biology," in *Proceedings of the International Symposium on Synergetics at Schloss Elmau, Bavaria*, 26 Apr–1 May 1982, ed. H. Haken (Berlin: Springer-Verlag, 1982); "Chemical Instabilities: Applications in Chemistry, Engineering, Geology, and Materials Science," in *Proceedings of the NATO Advanced Research Workshop*, ed. G. Nicolis and F. Baras, Mar 1983 (Dordrecht: Reidel, 1984); and V. I. Krinsky, ed., *Self-Organization: Autowaves and Structures Far from Equilibrium* (Berlin: Springer-Verlag, 1984).

[39] Because of its ease and versatility, I chose Google Scholar for this analysis rather than, for example, the Science Citation Index.

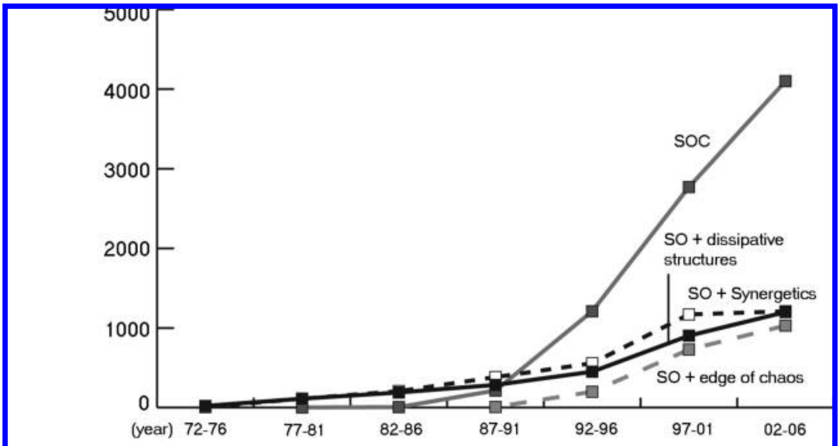

FIG. 3

SO + dissipative structures	21	112	190	285	451	903	1200
SO + Synergetics	6	112	206	383	557	1170	1210
SO + edge of chaos				8	199	729	1210
SOC		2	6	215	1210	2770	4100

in the technical literature: *synergetics* in the case of Haken, *dissipative structures* in the case of Prigogine.[40]

The explosive growth of the literature at this time surely counted as a serious factor in the emerging view that a *scientific revolution* was in process and a *new paradigm* had emerged. But taken alone, citation counts can never fully reveal the enthusiasm with which a new paradigm is heralded by the larger community—and perhaps especially not at this moment when popular accounts of "hot" scientific advances were fast becoming a surprisingly lucrative publishing industry phenomenon.[41] The dissemination of Prigogine's vision was particularly important. Following the relatively technical overview that he had published with Nicolis in 1977, a series of remarkably successful accounts popularized the wider implications of what he had seen. The first of these, *La Nouvelle alliance*,

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[40] Figure 3 also shows the frequency of what I take to be the two trademarks of the Santa Fe Institute, "edge of chaos" and "self-organized criticality," as will be discussed later in the text.

[41] Especially noteworthy is the phenomenal success, starting in 1980, of John Brockman's recipe for "How to Entertain the Smartest People," http://artnode.se/artorbit/issue4/i_brockman/i_brockman.html (accessed 25 Oct 2007), and of the literary agency he founded to turn this recipe into an engine for "Turning the Mind into Money," http://www.edge.org/documents/press/focus.e.html (accessed 25 Oct 2007).

co-authored by Isabelle Stengers, appeared in French in 1979 and in English in 1984 (under the title *Order Out of Chaos: Man's New Dialogue with Nature)*; the second, *From Being to Becoming*, was published in 1980.[42] Both books became runaway best sellers, attesting to the importance of the expanding cultural resonances that N. Katherine Hayles has described.[43] But at the same time, it also attests to the influence that Prigogine himself was able to wield, especially as a Nobel laureate. By all accounts, he exercised consummate skill in both promoting his vision and rallying together the various kinds of interest to which this vision appealed, and he deserves much of the credit attributed to him (at least in the popular literature) for its success. That he also tends to emerge with the lion's share of credit for its origin—despite the fact that he was not directly associated with the development of the methods of nonlinear dynamics itself, and despite the critical role played by many others in drawing attention to the peculiar features of dynamical systems, far from equilibrium, governed by nonlinear interactions—is equally unsurprising.

Prigogine may have prevailed over his European rivals, but even so, he was still not the only claimant to credit; nor was his the only characterization to attract attention to the new understanding of self-organization. The new paradigm could be equally well described without reference to either *dissipative structures* or *synergetics*, but simply in terms of nonlinear dynamics and stable attractors—i.e., as the production of stable patterns observed in nonequilibrium systems governed by nonlinear dynamics, for which the dominant methods of analysis were those of nonlinear differential equations and, after the mid-1980s, their computational analog of cellular automata as well.[44] Indeed, it was in the terms of this (ostensibly more neutral) formulation, now subsumed under the label of *chaos*, that a major new contender for popular attention appeared on the scene in the U.S.

Complexity, Self-Organized Criticality, and the Edge of Chaos

The Santa Fe Institute (SFI) was founded in 1984, in Santa Fe, New Mexico, by a group made up primarily of physicists (many of them from Los Alamos)

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[42] Ilya Prigogine and Isabelle Stengers, *La Nouvelle Alliance: Métamorphose de la Science* (Paris: Gallimard, 1984), and *Order Out of Chaos: Man's New Dialogue with Nature* (London: Flamingo, 1984); Ilya Prigogine, *From Being to Becoming* (New York: Freeman, 1980).

[43] Hayles, *Chaos Bound* (ref. 9).

[44] A brief history of the use of cellular automata in simulating biological processes can be found in E. F. Keller, chapter 9 in *Making Sense of Life* (Cambridge, MA: Harvard University Press, 2002).

under the enthusiastic leadership of Murray Gell-Mann. Fifteen years earlier Gell-Mann had been awarded the Nobel Prize for his work in particle physics, but his interests were now greatly expanded. The aim of the new institute was to bring the experience of physicists in nonlinear dynamics and phase transitions to the study of large complex systems in as many disciplines as such systems appear, doing so in an environment that fostered productive and adventuresome interdisciplinary exchange. SFI was located in an exquisitely beautiful setting, and it soon attracted a core of smart, ambitious, intrepid, and mostly young scientists devoted to the creation of a new science of complex systems. It also attracted a great deal of public attention. In 1986, Jim Crutchfield and his colleagues published a celebratory essay on the powers of the new science that was widely read and widely cited; entitled "Chaos," it was published in *Scientific American*, and a year later, James Gleick published his widely-acclaimed book under the same name.[45] Other popular accounts, focusing more specifically on the new institute, followed soon after.

The scientists drawn to SFI were well aware that self-organization, as it was understood at that time, had its home in the physical sciences (referring to patterns of fluid dynamics, optics, chemical reactions, phase transitions, and so on), and many of them had their original training in physics. But at SFI their horizons greatly expanded to include the social, computational, economic, behavioral, and biological sciences as well. Indeed, to them, self-organization knew no bounds. As the physicist Paul Davies somewhat provocatively wrote, "Mathematically we can now see how nonlinearity in far-from-equilibrium systems can induce matter to 'transcend the clod-like nature it would manifest at equilibrium, and behave instead in dramatic and unforeseen ways, molding itself for example into thunderstorms, people and umbrellas.'"[46] Life itself was reconceptualized as a self-organizing system, with the same kinds of properties that had previously been encountered in fluid dynamics and statistical mechanics.

In the late 1980s, at roughly the same time, Chris Langton and Norman Packard, building on Steven Wolfram's analyses of the behavior of cellular automata, intuited that complex phenomena (patterns) might not simply emerge

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[45] J. P. Crutchfield, J. D. Farmer, N. H. Packard, and R. S. Shaw, "Chaos," *Scientific American* 255 (1986): 46–57; James Gleick, *Chaos: Making a New Science* (New York: Viking Penguin, 1987).

[46] P. C. W. Davies, "The Physics of Complex Organization," in *Theoretical Biology: Epigenetic and Evolutionary Order from Complex Systems*, ed. B. Goodwin and P. Saunders (Edinburgh: Edinburgh University Press, 1989), 101–11. The secondary quotation is from C. H. Bennett, "On the Nature and Origin of Complexity in Discrete, Homogeneous, Locally-Interacting Systems," *Foundations of Physics* 16 (1986): 585–92.

spontaneously, but under certain conditions might even evolve, giving rise to ever more adaptive and more complex patterns of organization. Langton referred to what he regarded as the necessary conditions for such evolution as the "onset of chaos"[47]; Packard's term was the "edge of chaos," and it was the latter that stuck.[48] In 1987, Per Bak (an expert on phase transitions who was based at the Brookhaven National Laboratory, but also a member of the faculty at SFI), together with two postdoctoral researchers, Chao Tang and Kurt Wiesenfeld, introduced the closely related notion of *self-organized criticality*. The concept referred to the spontaneous approach of complex systems to states exhibiting behavior like that seen at the critical points of phase transitions in statistical mechanics. In their paper, Bak and his colleagues presented a general mechanism by which systems that are out of thermal equilibrium may evolve to a fractal, or scale-invariant, distribution.[49] *Self-organized criticality* was another term that took hold at SFI, and even though the original Bak-Tang-Wiesenfeld paper made no reference to biology, others were quick to apply this sort of analysis to spontaneously forming patterns wherever they might appear, from earthquakes, forest fires, traffic jams, and economic markets to biological phenomena ranging from natural selection to the distribution of species of trees in a forest.[50]

Stuart A. Kauffman was in some ways the odd man out. He had no training in physics; indeed, his only advanced degree had been a medical degree. But he had established himself as a theoretical biologist, first working with Warren McCulloch and later taking his place as an important proponent of the nascent discipline of mathematical biology during the 1970s and early 1980s. SFI provided the ideal setting for Kauffman to integrate and develop his ideas about biological organization, development, and evolution, and in the late 1980s he worked vigorously to promote the view that the dynamic created by the notions of *edge of chaos* and *self-organized criticality* demanded a radical

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47. For further discussion, see Roger Lewin, *Complexity: Life on the Edge of Chaos* (London: Orion Books, 1993) and M. Mitchell Waldrop, *Complexity: The Emerging Science at the Edge of Order and Chaos* (New York: Simon & Schuster, 1992).

48. N. H. Packard, "Adaptation Toward the Edge of Chaos," in *Dynamic Patterns in Complex Systems*, ed. J. A. S. Kelso, A. J. Mandell, and M. F. Shlesinger (Singapore: World Scientific, 1988), 293–301. Neither "edge of chaos" nor "onset of chaos" has a precise definition, but both refer to the idea that complex adaptive systems, including life itself, naturally evolve toward a regime of maximum complexity that is delicately poised between order and chaos.

49. Per Bak, Chao Tang, and Kurt Wiesenfeld, "Self-Organized Criticality: An Explanation of the 1/*f* Noise," *Physical Review Letters* 59 (1987): 381–84.

50. See, e.g., Lee Slomlin, "Per Bak 1948–2002: A Remembrance"; http://edge.org/documents/bak_index.html (accessed 15 Nov 2007).

revision of the traditional approaches to biological evolution. At a philosophy of science conference held in 1990, he began his summary of the main argument of his forthcoming book *The Origins of Order* by announcing: "A new science, the science of complexity, is birthing. This science boldly promises to transform the biological and social sciences in the forthcoming century." In particular, it promised to transform our understanding of evolution. As Kauffman put it, "Since Darwin, we have viewed organisms, in Jacob's phrase, as bricolage, tinkered together contraptions" but then he asked, "Must selection have struggled against vast odds to create order? Or did that order lie to hand for selection's further molding? If the latter, then what a reordering of our view of life is mandated!" Order, he concluded, "is in fact ready to hand"; "extremely complex systems exhibit 'order for free'"; and they achieve this order "in a lawlike way," at "the edge of chaos."[51]

What Kauffman was after, and believed he had found, was an alternative to the neo-Darwinian worldview—a view in which

> organisms are *ad hoc* solutions to design problems [and] the answers lie in the specific details wrought by ceaseless selection. In contrast, the explanatory approach offered by the new analysis rests on examining the statistically typical, or generic, properties of an entire class, or "ensemble" of systems all sharing known local features of genomic systems. If the typical, generic, features of ensemble members correspond to that seen in organisms, then explanation of those features emphatically *does not* rest in the details. It rests in the general laws governing the typical features of the ensemble as a whole. Thus an "ensemble" theory is a new kind of statistical mechanics. It predicts that the typical properties of members of the ensemble will be found in organisms. Where true, it bodes a physics of biology.[52]

By the time *Origins of Order* appeared in 1993, the Santa Fe Institute was widely perceived as the center of a new science of complexity. Analysis of the frequency of the terms *edge of chaos* and *self-organized criticality* in the literature of self-organization at this time shows much of the rhetorical control of this literature already shifting away from the "Brussels school" of Prigogine to SFI (see Figure 3). Indeed, enthusiasm for the achievements (or perhaps one should say, for the promises) issuing from Gell-Mann's brainchild was at its height, and the expectations generated by that enthusiasm were extraordinary. Kauffman's

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[51] Stuart A. Kauffman, "The Sciences of Complexity and Origins of Order," *Philosophy of Science Association, vol. 2: Symposia and Invited Papers* (East Lansing, MI: PSA, 1990), 299–322, on 299; Stuart A. Kauffman, *Origins of Order: Self Organization and Selection in Evolution* (Oxford: Oxford University Press, 1993).

[52] Kauffman, "Sciences of Complexity" (ref. 51), 300–01.

book followed on the heels of the immensely popular (and exceedingly celebratory) accounts of Santa Fe, first by Mitchell Waldrop, and soon after by Roger Lewin, and although considerably more difficult to read, Lewin's book, too, became a scientific best seller.[53]

In his own book *How Nature Works*, published three years later, Per Bak referred to *Origins of Order* as "the first serious attempt to model a complete biology," and he lauded Kauffman's efforts to apply his (Bak's) own ideas about self-organized criticality to evolution.[54] That Kauffman's vision of evolution operating on "coupled dancing landscapes"[55] spontaneously approaching criticality turned out to be premature was of little consequence, for almost immediately afterwards, Bak and Sneppen did succeed in developing a highly simplified model for a kind of evolutionary dynamics that exhibited the desired characteristics.[56] However artificial their model may have appeared to biologists, it contained the essentials required to produce the desired behavior. Here was a model of an open and dissipative system that organizes itself into a critical state simply by virtue of its intrinsic dynamics, independent of any control parameter. Drawing on an analogy with the physics of phase transitions, the existence of a critical state was said to be signaled by a power-law distribution in some variable—to physicists familiar with the behavior of systems at thermodynamic critical points, a seemingly clear indication that short-range interactions had induced long-range correlations and a form of global organization had emerged in which details of the particular system got obliterated. Bak and his colleagues built on this model to propose a "comprehensive theory" for complex systems, suggesting that

> systems that are far from equilibrium become critical through self-organization. They evolve through transient states, which are not critical, to a dynamical attractor poised at criticality . . . The system jumps from one metastable state to another by avalanche dynamics. These avalanches build up long range correlations in the system.[57]

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[53] Waldrop, *Complexity: The Emerging Science* (ref. 47); Lewin, *Complexity: Life* (ref. 47).

[54] Per Bak, *How Nature Works: The Science of Self-Organized Criticality* (New York: Copernicus, 1996), 127.

[55] Kauffman, *Origins* (ref. 51), 243.

[56] P. Bak and K. Sneppen, "Punctuated Equilibrium and Criticality in a Simple Model of Evolution," *Physical Review Letters* 71 (1993): 4083–86.

[57] Maya Paczuski, Sergei Maslov, and Per Bak, "Avalanche Dynamics in Evolution, Growth, and Depinning Models," *Physical Reviews* E 53, no. 1 (1996): 414–43. The term "avalanche

To many, self-organized criticality provided the latest key to an understanding of emerging structure in complex systems. In his preface to *How Nature Works*, Bak reported that since the coining of the phrase in 1987, "more than 2,000 papers have been written on the subject," making this initial paper "the most cited in physics."[58]

One might argue, however, that by 1996 the allure of SFI's sweeping assertions had already begun to fade. One year earlier, a lead-in on the cover of the June issue of *Scientific American* provocatively asked, "Is Complexity a Sham?" The reference was to an article by John Horgan reporting a growing disillusionment with promises of a comprehensive or unified theory of complexity, even among some of the major figures at SFI.[59] Horgan quoted Jack Cowan (one of the founders of SFI) complaining of the high "mouth-to-brain ratio" of the excessive hype, as well as John Maynard Smith's reference to Langton's "Artificial Life" as "a fact-free science." Theories about the edge of chaos and self-organized criticality came in for particular critique, above all for being applied too expansively. Philip Anderson, a member of the SFI board, said that he did not believe in "a theory of everything": "You mustn't give in to the temptation that when you have a good general principle at one level it's going to work at all levels." And even Murray Gell-Mann expressed concern about "a certain tendency toward obscurantism and mystification."[60]

Nevertheless, the lure of a science of complexity, and especially of self-organized criticality as the basis of a unified theory of complexity, persisted. SFI may have lost some of its allure, but just a few years later Laszlo Barábasi and his colleagues extended Bak's idea to the world of network topology, claiming to have found a universal architecture for complex systems occurring in biology, sociology, technology—in short, everywhere.[61] This work provided

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dynamics" refers to the behavior of the system commonly used to illustrate self-organized criticality: sand dripping onto a sand pile. The claim is that once the sand pile has attained a critical slope, it retains its conical shape as more sand is added; it manages this by setting off small avalanches. The timing and size of individual avalanches is unpredictable, but the *distribution* of avalanches (in both size and timing) displays the prototypical regularity of the power law.

58. Bak, *How Nature Works* (ref. 54), xii.

59. John Horgan, "From Complexity to Perplexity," *Scientific American* 272, no. 6 (1995): 74–79.

60. Ibid.

61. See, e.g., R. Albert, H. Jeong, and A. L. Barabási, "Diameter of the World Wide Web," *Nature* 401 (1999): 130–31; A. L. Barabási and R. Albert, "Emergence of Scaling in Random Networks," *Science* 286 (1999): 509–12; A. L. Barabási and Z. N. Oltavi, "Network Biology: Understanding the Cell's Functional Organization," *Nature Reviews Genetics* 5 (2004): 101–14.

yet another boost to expectations about what the physics of phase transitions might do for biology, and it sparked yet another upsurge of scientific enthusiasm and wider media acclaim.[62] It would seem that, to many readers, the attractions of such a program were just too strong to resist.

REFLECTIONS

Organisms and Thunderstorms

One great appeal of the analogy between biological organization and phase transitions has been the hope that looking at organisms in this way might emancipate biology from its traditional dependence on and commitment to particularity. A major triumph of modern statistical physics was its theory identifying the universal aspects of critical phenomena—a theory in which the macroscopic (thermodynamic) properties of a system near a phase transition are insensitive to the particularities of the system, namely, its underlying microscopic properties. Perhaps, some hoped, a similar approach would finally make it possible to identify those aspects of biological organization that were universal, and we could stop worrying about all the messy details.

This is a literature that began in the world of physics (especially in thermodynamics and statistical mechanics), written for the most part by physicists and published primarily in physics journals. Yet it rapidly spread to other fields, and was soon taken up by the flourishing industry of science popularization. Juxtaposed with earlier literatures on self-organization, however, this literature severed the term *self-organization* from both its original biological meaning and its later engineering sense, stripping it of all resonance with either natural or engineering design and appropriating it instead to categorize complex phenomena arising out of random ensembles, essentially uniform distributions of simple physical entities. Not only eddies, whirlpools, and Bénard cells were to be understood as arising from homogeneous gases, fluids, and lattices, but also more dramatic eruptions such as thunderstorms, earthquakes, and living organisms. Indeed, this literature claimed that self-organized criticality could describe the emergence of life itself.

In this assimilation of life and familiar physical processes, is biology being reduced to physics, or is physics being revived by the infusion of life? To some

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[62] For an analysis of this resurgence of the promise of a unified theory of complexity, see E. F. Keller, "Revisiting Scale-Free Networks," *BioEssays* 27 (2005): 1060–68.

it is undoubtedly the former; but Lee Smolin, one of the subject's more thoughtful writers, has seen in self-organization the possibility of revitalizing physics. He has argued that viewing the universe as a nonequilibrium, self-organizing system has many advantages: in particular, it allows for a world in which "a variety of improbable structures—and indeed life itself—exist permanently, without need of pilot or other external agent, [and] offers the possibility of constructing a scientific cosmology that is finally liberated from the crippling duality that lies behind Plato's myth."[63] But from the perspective of the life sciences, such assimilation seems to carry a considerable cost. Despite Smolin's caution, and for all his hopes, the wedding of statistical physics to biological processes effects a serious elision, hints of which are concealed in the semantic spread of the terms stability and complexity. Let me first address the question of stability, and then turn to complexity.

Meanings of Stability

Part One of this essay described the prominence of notions of equilibrium, stability, and constancy in the thinking of Fechner, Bernard, and others, and the range of meanings these terms took on in nineteenth-century discussions of vital phenomena. What does it mean to speak of the constancy (or stability) of the internal environment of an organism, and what relation is there between this notion and the properties discussed by physical chemists under the same name? A.V. Hill and others took a significant step forward in the early twentieth century with their clarification of the distinction between equilibrium (as understood in closed mechanical and chemical systems) and steady states (which necessarily refer to open systems). This important distinction was not enough to remove all ambiguity, however. Many different kinds of steady states could be described, some clearly amenable to treatment by the available methods of physics. But, crucially, the kinds of "steady state" in which physiologists were most interested—"conditions maintained constant by delicate governors and by a continual expenditure of energy"—could not.[64] Indeed, it was because physiological stability (or equilibrium) could not be assimilated either to the equilibria or to the steady states of traditional physics that Walter B. Cannon felt compelled to introduce the new term *homeostasis*.

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[63] Lee Smolin, *The Life of the Cosmos* (New York: Oxford University Press, 1997), 160.

[64] A. V. Hill, *Adventures in Biophysics* (Philadelphia: University of Pennsylvania Press, 1931), 79.

But with the arrival of techniques for analyzing nonlinear dynamical systems, the compass of physics was substantially enlarged, now extending beyond the lowest-order approximations mandated by the assumption of linear interactions. Of particular significance here is the fact that physics now encompassed the entire realm of phenomena for which mathematically stable solutions could be found to the corresponding different equations. This extension encouraged the assumption that all phenomena exhibiting stability, in whatever sense of the term, could be subsumed under the new set of modeling techniques, and with that assumption came a widespread reversion to the category of stability as the operative umbrella for self-organizing phenomena.

Attractors are, as it were, attractive, and in more senses than one. Technically, the term refers to a subset of phase space (a point, a curve, or a space) to which the solutions of a nonlinear set of differential equations (a dynamical system) eventually converge, provided that the system starts out in what is called a *basin of attraction*. Trajectories that get close enough to the attractor must remain close even if they are slightly disturbed. In other words, attractors are lures for trajectories in phase space. But they are conceptual lures as well, inviting the expansion of the term to refer to many different kinds of stability, robustness, or homeostasis. Thus, for instance, Stuart Kauffman wrote, "For a dynamical system . . . to be orderly, it must exhibit homeostasis; that is, it must be resistant to small perturbations. Attractors are the ultimate source of homeostasis as well, ensuring that a system is stable."[65] And a little later he asked, "Is homeostasis hard to create, making the emergence of stable networks vastly unlikely? Or can it, too, be part of order for free?"[66] His answer: yes, homeostasis too can be part of order for free.

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[65] Stuart Kauffman, *At Home in the Universe: The Search for the Laws of Self-Organization and Complexity* (New York: Oxford University Press, 1995), 79. A good example of the kinds of confusion caused by attempts to assimilate the new dynamical systems theory with earlier work on self-organization can be seen in Gordon Pask's retrospective remarks about "Different Kinds of Cybernetics" in *New Perspectives on Cybernetics: Self-Organization, Autonomy, and Connectionism*, ed. Gertrudis van de Vijver (Dordrecht: Kluwer Academic, 1992), 11–31. "Limit points, cycles and other equilibrial behaviours are often called goals and the system is said to be 'goal-directed'" (13). And "In summary, it used to be the case that stability meant something like homeostasis. . . . Currently, that meaning of stability, although it rightly persists for special cases, is quite rare. More often we speak of a self-organizing system as 'semi-stable' [a term he evidently (if somewhat mysteriously) takes to be synonymous with Ashby's term 'ultra-stable'] even if informationally open and evolving. Formally, this can be expressed in terms of several equilibrial conditions or cycles that are 'chaotic'; in particular those chaotic 'attractor' classes (distinct and different equilibrial cycles) associated with regions maintained on a phase-transition boundary" (15).

[66] Kauffman, *Home* (ref. 65).

In a similar extension, Kauffman also subsumed the phenomenon of robustness (of long-standing interest to engineers, and one that has become of enormous interest to biologists in recent years) under the same category. Concerned with how evolution could have led to the construction of more robust systems, he wrote, "Nonequilibrium systems can be robust as well. A whirlpool dissipative system is robust in the sense that a wide variety of shapes of the container, flow rates, kinds of fluids, and initial conditions of the fluids lead to vortices that may persist for long periods."[67] These various terms—stability, homeostasis, robustness—have been employed in many different contexts to describe many different kinds of phenomena, and the question must be asked: Can the various phenomena to which they refer in fact be so easily assimilated, the terms so readily interchanged? Have Cannon's (or Hill's) concerns been met by the mathematics of nonlinear dynamical systems? And what about the robustness of engineered systems (e.g., airplanes or the Internet) with respect to the kinds of disturbances engineers worry about, disturbances that need not be small? Can the robustness of systems in response to such disturbances also be explained in terms of Lyapunov stability?

The terms *stable* and *robust* have a common colloquial sense, and even in the technical literature they are often used interchangeably. But neither has a clear technical meaning unless we first specify both the feature of interest and the kinds of disturbance that threaten its survival or persistence. And generally, the term *robustness*, especially as used in engineering and biology, encompasses a far greater range of both features and disturbances. In particular, many features of complex systems, and many kinds of disturbance, are difficult if not impossible to quantify. For example, what if we are interested in the persistence of a system's architecture through changes in its composition; the persistence of a function through changes in components or architecture; or the persistence of an organism through changes in the structure or composition of its environment? Furthermore, as David Krakauer has pointed out, it is often necessary to consider the effect of multiple disturbances acting on multiple levels.[68] Stability theory provides powerful methods for analyzing the effects of small perturbations of well-defined parameters on equally well-defined variables,

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[67] Ibid., 187.

[68] D. C. Krakauer, "Robustness in Biological Systems: A Provisional Taxonomy," in *Complex Systems Science in Biomedicine*, ed. Thomas S. Deisboeck and J. Yasha Kresh (New York: Plenum Press, 2005), 185–207.

but these comprise only a small fraction of the problems with which engineers and biologists are concerned.[69]

Also, although many of the systems treated by the methods of nonlinear dynamics are energetically open, they are not generally open to material or informational input or output. Indeed, the widespread use of these methods in their field has been a chief complaint of control theorists. Where classical control theory allowed for the explicit inclusion of many different kinds of input and output, modern control theory based on nonlinear differential equations does not. Eduardo Sontag wrote, "the [classical] control theory formalism—in contrast to dynamical-systems theory, which deals with isolated systems—is not only reasonable, but natural."[70] And in a similar vein, Jan Willems, another control theorist, complained that while the mathematics coming from "planetary motion, the *n*-body problem and Hamiltonian dynamics" has certainly proven fruitful for many kinds of problems, the question arises as to whether "*closed* systems, as flows on manifolds and dx/dt = f(x), form a good mathematical vantage point from which to embark on the study of dynamics."[71] In Willems's view, they do not, for they fail to account for the dynamical interaction of the system with the specific environment in which the system is embedded. "Twenty-five years ago," he wrote,

> it was my hope that system theory, with its emphasis on open systems, would by now have been incorporated and accepted as the new starting point for dynamical systems in mathematics. Better, more general, more natural, more apt for modeling, offering interesting new concepts as controllability, observability, dissipativity, model reduction, and with a rich, well developed, domain as linear system theory. It is disappointing that this didn't happen. What seemed like an intellectual imperative did not even begin to happen. Mathematicians and physicists invariably identify dynamical systems with closed systems.[72]

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[69] See Erica Jen, "Stable or Robust? What's the Difference?" http://www.santafe.edu/~erica/stable.pdf (accessed 15 Oct 2007), for a more extensive discussion of the differences between stability and robustness.

[70] Eduardo Sontag, "Molecular Systems Biology and Control," *European Journal of Control* 11 (2005): 2314–19.

[71] Jan C. Willems, "In Control, almost from the Beginning until the Day after Tomorrow," *European Journal of Control* 13 (2007): 71–81, on 76. Willems's point may be especially relevant to engineering.

[72] Ibid., 76.

Meanings of Complexity: Organized vs. Disorganized Complexity

There remains a final point to be made, and it may be the most important. Analyses of nonlinear dynamical systems clearly demonstrate the ease with which complexity can be generated, but notably lacking from such demonstrations is any account of the properties for which Immanuel Kant originally introduced the term *self-organization*. The patterns that are observed to emerge spontaneously in the systems studied thus far are complex (sometimes extremely so), but they remain patterns without meaning. Stripes, rolls, whirls, eddies are all phenomena indicative of complex, nonlinear dynamics; they are phenomena that can only be found in systems that share with organisms the property of being open, far from equilibrium, dissipative. But they still lack the properties that make organisms so insistently different from physical systems. Most notably, they lack function, agency, and purpose. Perhaps the simplifications assumed in order to render a system tractable (given the tools currently available) are so drastic—typically, these assumptions rob both the interacting elements and their distribution in space of all structure—that they effectively preclude such quintessentially biological properties. In any case, no one has yet succeeded in offering an account of how function, purpose, or agency might emerge from the dynamics of effectively homogeneous systems of simple elements, no matter how complex those dynamics might be. It would seem, and indeed it has been suggested, that these properties require an order of complexity going beyond what can spontaneously emerge out of complex interactions between simple elements—an order of complexity that a number of researchers have come to refer to as *organized complexity.*

Warren Weaver, writing in 1948, may have been the first to call attention to the problem—a problem that he saw as being most clearly evident in, but not restricted to, the life sciences. He lauded nineteenth-century developments in probability theory and statistical mechanics that permitted so great an advance over the science of mechanics that dealt with "simple problems" and allowed us to "deal with what we may call problems of disorganized complexity."[73] But those methods, he continued, leave "a great field untouched"—the field of "organized complexity":

> One is tempted to oversimplify and say that scientific methodology went from one extreme to the other . . . and left untouched a great middle region. The

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[73] Warren Weaver, "Science and Complexity," *American Scientist* 36 (1948): 536–44, on 538.

> importance of this middle region, moreover, does not depend primarily on the fact that the number of variables involved is moderate large compared to two, but small compared to the number of atoms in a pinch of salt. . . . Much more important than the mere number of variables is the fact that these variables are all interrelated. . . . These problems, as contrasted with the disorganized situations with which statistics can cope, show the essential feature of organization. We will therefore refer to this group of problems as those of organized complexity. What makes an evening primrose open when it does? Why does salt water fail to satisfy thirst? . . . What is the description of aging in biochemical terms?[74]

Picking up on Weaver's argument fourteen years later, Nobel laureate Herbert Simon argued that organized complexity was complexity with an architecture. More specifically, the architecture that seems to characterize complex systems in the behavioral and life sciences is one of hierarchical composition (or modularity), whereby a system "is composed of interrelated subsystems, each of the latter being in turn hierarchic in structure until we reach some lowest level of elementary subsystem."[75] Above all, Simon argued (persuasively) that such an architecture allows us to make sense of the rapidity with which biological complexity has evolved.

Today, one of the most articulate proponents of organized complexity is the biologist John Mattick.[76] He too has focused on the question of architecture, arguing that the organization of complexity is mandated by the meaninglessness of the structures generated by the sheer combinatorics of complex interactions: "[B]oth development and evolution have to navigate a course through these possibilities to find those that are sensible and competitive."[77] More specifically, he has claimed that "organized complexity is a function of regulatory information," and accordingly he reads the recently discovered system of

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[74] Ibid., 540.

[75] Herbert A. Simon, "The Architecture of Complexity," *Proceedings of the American Philosophical Society* 106, no. 6 (1962): 467–82, on 468.

[76] "Organized complexity" was at times used to refer to theories of self-organization being developed in the late 1950s, and needs to be sharply distinguished from what is now often called "self-organized complexity." The latter term is used interchangeably with "self-organized criticality" and refers primarily to statistical mechanical models of complex systems, i.e., to what Weaver called "disorganized complexity."

[77] John S. Mattick, "RNA Regulation: A New Genetics?" *Nature Reviews Genetics* 5 (2004): 316–23, on 317.

RNA-based regulation as evidence of a new control architecture that came into being in the Cambrian era and made multi-cellular life possible.[78]

Addressing a question similar to Mattick's, Walter Fontana and Leo Buss have concluded that "the traditional theory of 'dynamical systems' is not equipped for dealing with constructive processes. Indeed, the very notion of 'construction' requires a description that involves the structure of objects. Yet, it was precisely the elimination of objects from the formalism that make dynamical systems approaches so tremendously successful."[79] The main problem, they argue, is that although conventional dynamical systems theory is "well-suited to treat changes in the magnitudes of quantitative properties of fixed object species, [it is] ill-suited to address interactions that change the objects themselves."[80] This, of course, is precisely what characterizes objects of biological systems, crafted by evolution so that they are subject to change not only by external insult, but also by the internal dynamics of the system. As Fontana and Buss put it, "Mutation is to construction like perturbation is to dynamics."[81] They call their own interesting effort to expand the traditional theory to include objects, their internal properties, their construction, and their dynamics *constructive dynamical systems.*

CONCLUSION

To Kant, it seemed "contrary to reason" that "raw material could have originally formed itself according to mechanical laws, that life could have originated from the nature of the lifeless, and that matter could have arranged itself in the form of a self-maintaining purposiveness."[82] Yet he did not rule out the

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[78] Ibid. See also J. M. Carlson and John Doyle, "Complexity and Robustness," *Proceedings of the National Academy of Sciences* 99, suppl. 1 (2002): 2538–45; and John Doyle, "Robustness and Complexity," http://sbs-xnet.sbs.ox.ac.uk/complexity/complexity_PDFs/AbstractProfDoyle.pdf (accessed 30 Oct 2007).

[79] Walter Fontana and Leo Buss, "The Barrier of Objects: From Dynamical Systems to Bounded Organizations," in *Boundaries and Barriers,* ed. John Casti and Anders Karlqvist (Reading, MA: Addison-Wesley, 1996), 56–116, on 59.

[80] Fontana and Buss, "Barrier" (ref. 79), 63.

[81] Ibid.

[82] Immanuel Kant, *Critique of Judgment*, vol. 39 of *Great Books* (Chicago: Encyclopedia Britannica, 1993), 581, §81.

possibility that it might have done so. The "reason" to which he refers, and which he finds so inadequate to the task, is human reason. Perhaps he was right, perhaps the task is just too difficult—too large—for the mind to encompass. Certainly, 200 years of effort have produced a number of challenging proposals, often enormously fruitful even while promising more than could be delivered. Yet it remains the case that no one has succeeded in doing for biology what Newton did for physics: construct a satisfying account either of the origin of life or of its organization, in terms that can be laid out in a few graspable equations.

From cybernetics and dynamical systems theory, we have learned of the importance of feedback and nonlinear interactions. From Herbert Simon and the many computer scientists who followed his lead, we have learned of the importance of composition and hierarchical construction. From recent work in systems biology, a multifaceted interdisciplinary effort triggered by the sequencing of the human genome and forged with the aim of "Putting Humpty Dumpty Together Again,"[83] we have learned of the importance of particularity, of heterogeneity, of architecture, and of large numbers. We have learned that a science of self-organized complexity will have to take into account processes of self-assembly and self-organization in multilevel systems, operating on multiple spatial and temporal scales through multilevel feedback, in which the internal structure and properties of the component elements are themselves responsive to the dynamics of the system. Rather than trying to transcend the particularities of the system through statistical averaging and placing one's confidence in the significant emerging patterns of maximum likelihood, we may find the secrets of biological organization residing precisely in the details that have been washed away. It may be, as suggested by the study of engineered systems, that the most biologically relevant are usable patterns arising from such particularities with only low likelihood. The work of John Doyle, a control theorist who has studied design principles for functional architectures in both living and technological systems, provides crucial support for innovative theorists like Mattick. According to Doyle and his colleagues, close examination of the Internet and other technologies shows that the best-performing topologies are precisely those with low likelihood. Indeed, the authors conclude that the "likely" topologies "have such bad performance as to

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[83] Denis Noble, "The Future: Putting Humpty Dumpty Together Again," *Biochemical Society Transactions* 31, no. 1 (2003): 156–58.

make it completely unrealistic that they could reasonably represent a highly engineered system."[84]

To be sure, Kant's problem still has not been solved. But perhaps the task is now defined with sufficient clarity to support a degree of optimism. There may not be any consensus about the best terms in which to describe the organized complexity exemplified by living organisms, but the challenge has been clearly laid out. Also, it remains uncertain just what kind of explanation the most sophisticated models coming out of systems biology might yield—whether, for instance, such explanations will fall within the range of human reason, graspable by our cognitive capacities, or whether they will require reliance on computers that are so much better at handling complexity than we are. But that is another question altogether. Perhaps, in the end, if and when we succeed in explaining just what it is that is so distinctive about biological entities, Kant will have been proven right—right, that is, about the relation between such accounts and the capacities of human judgment.

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[84] L. Li, D. Alderson, J. C. Doyle, and W. Willinger, "A First-Principles Approach to Understanding the Internet's Router-Level Topology," in *Association for Computing Machinery SIGCOMM, Portland, Oregon*, 30 Aug–2 Sep 2004.