SCIENCE WITHOUT LAWS

MODEL SYSTEMS, CASES, EXEMPLARY NARRATIVES

Edited by Angela N. H. Creager, Elizabeth Lunbeck, and M. Norton Wise

Duke University Press Durham and London 2007

Printed in the United States of
America on acid-free paper ♾
Designed by Amy Ruth Buchanan
Typeset in Minion by Tseng
Information Systems, Inc.
Library of Congress Cataloging-in-
Publication Data appear on the last
printed page of this book.

To the memory of
Clifford Geertz (1926–2006)
colleague and friend

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Introduction

ANGELA N. H. CREAGER, ELIZABETH LUNBECK, AND M. NORTON WISE

At the dawn of the twenty-first century, the face of biology may well be that of a laboratory mouse. Science writers, government agencies, and researchers alike tout the crucial role played by biology's experimental subjects, "model systems" as they are termed, in advancing knowledge. These creatures are not showcased for their appeal—the flies, mice, worms, and microbes that are the mainstay of laboratory science would be regarded as vermin or germs outside their scientific homes—but because they have become the locus of producing knowledge about life and disease. To make the case that improving human health rests on our intimate understanding of a select set of rodents, fish, amphibians, microbes, and even a plant, the National Institutes of Health (NIH) features a Web site titled "Model Organisms for Biomedical Research" (www.nih.gov/science/models). These are the organisms whose genomes were sequenced as part of the Human Genome Project. And as the NIH wants to make clear, they are the creatures that stand in for us humans as laboratory biologists investigate how living processes work—and how they go awry. A special supplement to *The Scientist* titled "Model Organisms" offers feature articles on eight such exemplary forms of life, from the intestinal bacterium *Escherichia coli* to the nematode worm *Caenorahdbitis elegans* (see figure 1). As the editors explain the importance of this "motley collection of creatures":

> Researchers selected this weird and wonderful assortment from tens of millions of possibilities because they have common attributes as well as unique characteristics. They're practical: A model must be cheap and plentiful; be inexpensive to house; be straightforward to propagate; have short gestation periods that produce large numbers of offspring; be easy to manipulate in the lab; and boast a fairly small and (relatively) uncomplicated genome. This type of tractability is a feature of all well-used models.[1]

At one level, the reliance of biomedical researchers on standardized creatures for experimentation is mere practical necessity. Biological materials are, by their nature, variable and complex; life scientists have sought to control the variability they face by selecting out and standardizing particular experimental subjects. Yet these organisms, no matter how standardized they become as laboratory instruments, maintain an independent existence in a contingent world. They are not models in the traditional sense—they are not smaller versions of humans, and they do not exactly replicate our experiences or diseases. Unlike the idealized representational models characteristically featured in the history of the exact sciences, in which the model (e.g., the Bohr atom) has been supposed to mirror a natural system (hydrogen) by embodying the mathematical laws and structure from which the behavior of the system can be deduced, model systems maintain their own autonomy and specificity. That is, model systems do not directly represent humans as models *of* them. Rather, they serve as exemplars or analogues that are probed and manipulated in the search for generic (and genetic) relationships. They serve as models *for* human attributes.[2] The use of standardized organisms in biomedicine is part of a broader model-systems approach in the life sciences that includes the investigation of a far wider range of entities, from specific proteins (e.g., hemoglobin) to particular lakes (e.g., Linsley Pond in Connecticut), and whose utility in producing general knowledge relies on the routine use of analogies to other examples and entities.

These distinctions between representational and representative functions, between models of and models for, have proven quite useful in discussing the characteristics of model systems. We suggest that insofar as similar objects inhabit spaces far beyond biology laboratories, the same distinctions extend to other areas, areas where relations of similarity rather than deduction have grounded claims to generality and where specificity has been a resource rather than a problem. Many fields have developed canonical examples that have played something like the role of model systems, which serve not only as points of reference and as illustrations of general principles or values but also as sites of continued investigation and reinterpretation. What we here call model objects of this sort in this volume include Athenian democracy in political theory, the ritual in anthropology, and the so-called Prisoner's Dilemma in game theory. Through what processes do particular organisms, cases, materials, or texts become foundational to their fields? How do they serve a classificatory function for the organization of knowledge, whether it is in a biology laboratory or an art museum? When does the specificity or idiosyncrasy of an example threaten its utility?

Examining the pursuit of knowledge organized around exemplars rather than around fundamental laws, we aim to reopen the old question of the relation between the human sciences and the natural sciences. In the nineteenth century, the question was cast in terms of the relation between the generalizing,

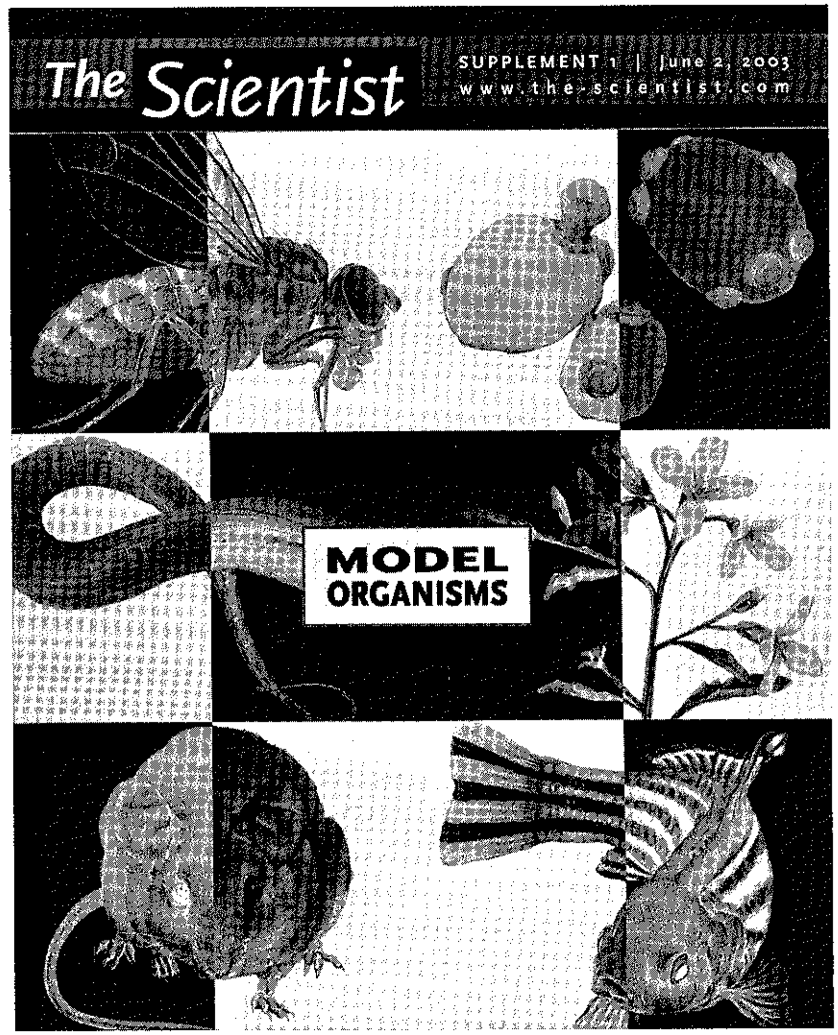

FIGURE 1 Cover Image, "Model Organisms," *The Scientist* 17, supplement 1 (2003). Reprinted with permission of *The Scientist*.

lawlike sciences (*nomothetic*, in the canonical formulation of Wilhelm Windelband) and the particularizing sciences (*idiographic*), where lawlike referred to the universal laws of physics as the ideal of science.[3] It is no longer the case, however, that universal laws either do or can serve as a model for all science, even natural science. This has become most apparent with the emergence of biology in the past thirty years as the so-called science of the future. It is not clear that there are any high-level laws in biology, in the sense of predictive laws that determine the future behavior of a biological system (except perhaps in evolutionary theory); we will not be concerned with whether such laws may emerge. Instead, we want to show how the model-systems approach so pervasive in biology compares with the use of cases, exemplars, and related methods in other fields. Interestingly, it appears that many of these approaches grew up in response to the challenge of producing something like lawlike knowledge in disciplines in which laws seemed incapable of capturing the specificity and complexity of organisms, geological processes, or human productions. If the result has not been laws, it has nevertheless been reliable, systematic knowledge. Thus our title: *Science without Laws*.

Beginning with model systems as understood in recent biology, then, this book compares the scope and function of model objects in domains as diverse as geology and history, attending to differences between fields as well as to epistemological commonalities. What distinguishes this collection of essays from other studies of models in science is both its attention to model systems as concrete autonomous objects and the breadth of disciplines it addresses.[4] Traditional approaches to scientific models derive from the philosophy of science, especially the philosophy of theoretical physics, in which the model is presented as a precise, usually mathematical, representation of the phenomenon in question.[5] But this picture of mathematical models, valorized by physicists and philosophers, is highly idealized. As Nancy Cartwright has argued, the models mobilized by physical scientists to illustrate their theories are far from real-world situations; they are, rather, "nomological machines" that manufacture universal laws of nature by providing the kind of simplified mechanical or mathematical evidence that could not be found in "nature."[6] Not only the character of the model but also the presumption that the exact sciences should provide the basis for understanding general scientific method or rationality has recently come under question. To Ian Hacking's enumeration of six "styles of reasoning" that characterize the sciences, for example, John Forrester has proposed "reasoning by cases" as a seventh scientific method, widely used not only in the human (and biological) sciences but also in law, medicine, and ethics.[7] Case-based reasoning relies on relations of similarity rather than on conventional

reductionism and treats specificity as a resource, not a problem. The essays in this book attend to case-based modes of inquiry usually neglected by historians and philosophers of science, demonstrating that their epistemological practices and patterns extend far beyond the boundaries of "science."

PART I: BIOLOGY

A model system in biology refers to an organism, object, or process selected for intensive research as an exemplar of a widely observed feature of life (or disease). The traditional contrast between laboratory physiology and natural history within nineteenth-century biology provides some background to the emergence of model systems in twentieth-century laboratories. Of the two traditions, it was experimental physiology that most closely emulated the ideals of the physical sciences. Nineteenth-century physiologists used animal models such as frogs because of their accessibility for experimental manipulation, and they aimed to produce universalistic knowledge through reductionist (and often instrument-based) approaches. This mechanistic approach treated the organism itself as a workshop, but there was considerable debate as to whether vital phenomena could be completely reduced to physico-chemical principles or laws. Hermann Helmholtz, Emil Du Bois-Reymond, Carl Ludwig, and others strove to reduce physiology to the physics of atoms and forces in their attempts to show how laws (such as the conservation of energy) might account for processes in both living and nonliving materials. Not all physiologists agreed with the aims of this "organic physics," however. Claude Bernard cautioned that biology might borrow methods and instruments, but not theory, from physics and chemistry. In fact, radical reduction soon went bankrupt and even its most committed adherents had to redirect their energies. Ludwig, who in the early 1850s had asserted that "physiology is nothing other than applied physics," had also at the same time expressed his "hope someday to work with a capable clinician or pathological anatomist . . . and together with him experimentally reproduce the conditions of disease. . . . It ought to be possible to generate innumerable illnesses similar to those found in man." This is the expansive program he took up with Karl Wunderlich when he moved to Leipzig in 1871 to establish the new physiology laboratory there.[8] It is also the program that turned increasingly to animal models rather than physical laws for insight into biological processes.

The turn to model systems in the twentieth century resulted from the conjunction of this experimental tradition with a new industrial infrastructure. Accompanying the increasing mass production of scientific materials and equipment was a narrowing of the number of organisms intensively studied, and also the commodification of many of the laboratory's experimental inhabitants. The ascendance of the fruit fly in genetics research grew out of T. H. Morgan's laboratory in the 1910s and 1920s; during the same time period, maize became a dominant organism for plant genetics, particularly cytogenetics. By midcentury, inbred strains of mice were widely used not only to understand mammalian genetics but also for investigations of cancer and other human afflictions. And the molecular emphasis of postwar biology grew out of the intensive study of a handful of microbes, especially yeast (*Saccharomyces cerevisiae*), bacteria (most notably *E. coli*), and viruses (particularly the bacteriophages and tobacco mosaic virus). Since the 1960s, scientists have domesticated new animals for research, such as the nematode worm and the zebrafish. Each of these model organisms has a unique history in the laboratory, with particular physical features and experimental advantages. Yet each of these model systems has also gained ground by virtue of its historically acquired prominence within a field of study. Indeed, model systems exhibit a self-reinforcing quality: the more the model system is studied, and the greater the number of perspectives from which it is understood, the more it becomes established as a model system. Even for the many biologists who do not study one of the canonical model organisms, these systems tend to serve as benchmarks and methodological guides when they turn to other organisms and objects as researchers.[9]

Historians and philosophers of biology have recently become interested in the consequences of privileging particular model systems for the shape of knowledge. In genetics, as Marcel Weber shows, the collection of mutant *Drosophila* strains that dated to Thomas Hunt Morgan's "fly room" in the early twentieth century were redeployed in the molecular mapping of fly genes in the 1970s. The recombinant revolution in biology thus catalyzed the reworking of a classic model system, but one that was advantaged by the accumulation of decades of knowledge and hands-on experience, not to mention the fly strains themselves. *Drosophila* was especially ascendant in the field of developmental genetics, and there its success cannot be attributed to a simple notion of typicality. The process of development in *Drosophila* is not characteristic of other organisms, especially mammals. Indeed, some biologists argue that the highly canalized development of *Drosophila* that makes its development so easy to study in the laboratory also makes it a relatively poor evolutionary representative of its own phylum.[10] Weber argues that what made *Drosophila* such a powerful research tool were rather the experimental resources associated with it. In the process of its molecularization, he argues, key concepts from classical genetics—not least the concept of the gene itself—were mobilized and recast.

For a time, the combination of accumulated knowledge and resources associated with the fly gave it an edge over other experimental organisms, such as the mouse and the worm, in efforts to adapt cloning techniques from the study of microbes to multicellular organisms.

Not all current model systems are advantaged by such a long history in the laboratory. Rachel Ankeny describes the way in which a newly developed model system, the nematode *Caenorhabditis elegans*, came to figure very prominently in medical research following its initial domestication in the 1960s. Sydney Brenner, already well known for his work in molecular biology, chose this free-living nematode on the basis of its developmental invariance and simplicity in order to facilitate developmental studies of the nervous system. As Ankeny argues, this kind of modeling of a general process on a carefully selected example involves at least two levels of idealization. First, the so-called wild type that serves as a benchmark for genetic comparison may not be especially representative in terms of the variation within a species, but it serves as a biological norm nonetheless. Second, a great deal of scientific work goes into the building up of a descriptive model of the organism in question. In the case of *C. elegans*, this work centered on the articulation of a wiring diagram that represented all of the neurological connections within the worm. Once again, this diagram is itself an idealized version of actual worms, whose properties are abstracted into the diagram. Even so, it provides a crucial point of reference for the analysis of various mutants. As Ankeny demonstrates, a well-studied model organism (or at least its wild-type representative) serves as an index case against which others can be compared and contrasted. In this respect, case-based reasoning underlies the routine use of model systems in biomedical research, particularly when properties of genetic mutants are compared against the wild type.

Analogies also play a crucial role in linking worms and humans, particularly at the level of homologies between genes. It is these analogies that have given nematode worms their purchase in medical research. This process is best described in terms of an unpredictable relevance, as Jane Hubbard puts it, for genetic homologies that can underlie divergent physiological properties. The molecular biology of organisms such as the worm began as unabashedly reductionistic in spirit, yet the specificity and particular features of the model systems have remained experimental resources rather than mere complications. And although model organisms are standardized in order to facilitate highly controlled biological experimentation, their inherent complexity means that the systems are never fully understood and can continue to generate surprising results. Indeed, as models, they are no simpler biologically than the humans they illuminate by analogy.

The career and utility of a model system not only derives from pragmatic experimental considerations but may also reflect broad disciplinary and political changes. Susan Sperling shows that the intensive study of the baboon in field primatology of the 1970s coincided with the declining legitimacy in the anthropological study of "primitive peoples" as stand-ins for our prehistoric ancestors. The structural-functionalist anthropologists in the 1960s were no less universalizing than their disciplinary predecessors, but they turned from anthropology to primatology in their efforts to theorize human evolution. As Sperling observes, "the ubiquitous baboon troop now firmly occupied the position of primitive society in the schemas of nineteenth-century anthropologists." Postulating that the earliest humans behaved like baboons, with their patterns of male dominance and troop control through aggression, primatologists provided a putative biological basis for similar human behaviors. These visions of the human past and present did not go uncontested, as a generation of new researchers, many of them women, found the received view of baboons' gendered division of labor inadequate to describe troop behavior. In addition, a new emphasis on other primate model systems, such as chimpanzees and lemurs, complicated the vision of an ur-primate society based principally on baboon observations.

PART 2: SIMULATIONS

As we leave the domain of biological organisms and contemplate extensions of the concept of a model system into other areas, it is helpful to return again to the mid-nineteenth century. As noted previously, anyone at that point seeking a contrast with the sciences of laws would immediately have turned to natural history in opposition to natural philosophy (or physics, as the field was called after midcentury). Natural historical sciences like botany, zoology, geology, and meteorology typically relied for their claims to knowledge on extensive observation rather than experimentation, and on the classification and ordering of large amounts of quite specific information rather than on subsuming particulars under general laws. The understanding of a generic trait of an organism, for example, depended on knowing what was typical and what was possible within the range of variation for that organism and on a thorough familiarity with the analogues and homologues of the trait in other organisms. Similarly, through extensive fieldwork and mapping projects on rocks and fossils, geologists of the early nineteenth century (geognosists) made great strides in understanding the order and temporal sequence of rock strata.[11] Such descriptive ordering, however, did not yield causal laws. When investigators from the natural historical sciences wanted to understand causes—thereby taking on the usual goal of natural philosophy—they sometimes had recourse to models, but as Naomi Oreskes shows for geology, they were models of a distinctly natural historical sort, based on mimesis rather than on general laws of cause and effect. Such mimetic models may be thought of both as *analogues* of the real system, being like it, and as *analog* simulations of its development in time. This suggests an interesting similarity to present-day digital simulations, with which both Oreskes and Amy Dahan Dalmedico are concerned. Their studies suggest that many forms of computer simulation, in areas in which laws are either inappropriate or imperfectly understood, may be usefully interpreted as continuations of natural historical methods of modeling.[12]

Oreskes makes this argument directly for the use of models in the earth sciences. She describes the development of scale models, from the early nineteenth century to the mid-twentieth century, for investigating such things as the deformation of the earth's crust and mountain building associated with the common assumption that the earth was a cooling ball of originally molten matter. A long line of geologists constructed "boxes" containing model materials for the earth's crust that could be subjected to compression and other forces of deformation in order to imitate, or to run an analog simulation of, geological processes. The difficulty lay in finding materials for the models—lead, sandstone, paraffin, or pancake batter—that would behave in the small-scale box on a short time-scale in a manner similar to the large-scale plasticity of the earth's rocks on a much larger time scale. Characteristic of these models was their heuristic role; they allowed experimental investigation, by analogy, of possible causal stories whose plausibility might thereby be supported. And they allowed the investigator to watch the story developing dynamically, in time.

Interestingly, by the time geologists had developed effective rules for the scaling-up of materials and forces from the box to the landscape, they realized that they could just as well calculate the results as build and run the physical model. With this realization in the 1940s, and with the advent of digital computers, they largely gave up their analog models in favor of running digital simulations. The computer simulations, however, while substituting digital plasticity for the plasticity of model materials, continue in some ways to serve much the same exploratory and explanatory function as the analog models. They map out the possible while seeking understanding in the details of carefully chosen objects whose very specificity constitutes the source of insight. Thus both sorts of models, analog and digital, might well be called the model systems of the earth sciences. The new simulations, however, have taken on a role foreign to geology's natural historical origins, that of predicting future events. Oreskes argues that this dramatic shift in the modeling goals of geologists, from an exclusive concern with explanation to an increasing emphasis on prediction, derived originally from the Cold War problem of predicting the long-term stability of disposal sites for nuclear waste generated by a mammoth weapons program. Long-term prediction using computer simulations presented new sorts of problems for geological modeling, including the validation of results for a distant future not accessible to observation.

More generally, the computer simulations that Oreskes discusses contain a feature of model systems that we have not yet explicitly addressed: their complexity. Like biological model systems, analog and digital simulations are of immense value precisely because they mimic in part the complexity of natural systems, which typically involve multiple processes, nonlinear interactions, feedback loops, and emergent properties. These characteristics make complex systems irreducible in mechanical terms and thus impossible to replicate, except in a similarly complex analogue. It might well be said, in retrospect, that the traditional methods of natural history for producing order out of nature's diversity, including the role of analogues, were already one sort of response to the problem of complexity. Model systems and computer simulations are another. They are extended forms of analogues, but they offer the great advantage of experimental exploration of those aspects that produce complexity. In computer simulations, those aspects are built into the models and modified to match some of the features of nature's own behavior.

Amy Dahan Dalmedico analyzes the practices that have gone into building and modifying meteorological and climatological models since the 1950s, particularly in France. Like Oreskes, she finds that prediction, or forecasting, presents problems different from those of understanding mechanisms and causes. In fact, the inherent tension between predicting and understanding has constantly appeared not only within the models but between modelers from different institutions with different interests. For climate change especially, the tension is greatly enhanced by the interdependence of models of the natural biosphere, models of the effects of various scenarios for socioeconomic development, and models for possible mitigation strategies. A full climate model must integrate these partial models and the feedback between them. The complexity of the full model, Dahan Dalmedico shows, mirrors the complexity of these interactions and the various modeling practices associated with them. As a whole, they constitute the model system of climatology, a science both natural and social.

To illuminate the different modeling practices that must somehow be integrated into a global model, Dahan Dalmedico compares the two main groups in France that model climate change. Météo-France is a national public institution whose approach derives from meteorological forecasting and which uses a large computer simulation called the Arpège model. The staff comprises primarily engineers who become civil servants and are interested in the operational aspects of modeling. The laboratories of the Institut Laplace, in contrast, are supported by the CNRS (National Center of Scientific Research, somewhat like the American National Science Foundation) and are primarily interested in basic research on the components of models. The organization of the work is relatively loose, and individual members work on a variety of topics. One of the laboratories, anxious not to lose their independence to Météo-France and the Arpège model, built their own large climate model, called LMDZ, which is not immediately compatible with Arpège. Neither model, furthermore, takes into account the coupling of the atmosphere with the ocean. For that purpose, a third organization has arisen, Cerfacs, whose coupling model attempts to maintain neutrality between Arpège and LMDZ, taking the results of either as the input for higher-order integration. Yet further levels involve the continents, in both soil properties and biosphere.

The result of all of this integration is a virtual climate so complex that it constitutes an almost autonomous object, a "simulacrum of reality," as one researcher called it, whose self-consistency sometimes seems to compete with reality. This worry reflects again the tension between understanding and predicting. But in their stubborn autonomy, climate models recall a basic characteristic of model systems that we have wanted to stress. They do not represent the climate in the sense of being models *of* it; instead, their properties represent certain aspects (carbon dioxide content, temperature) and are models *for* those aspects. They yield understanding by revealing processes in the model that are closely analogous to those in the atmosphere, although realized in a different medium, the computer simulation. That is just the characteristic, at a completely different level, that seems typical of the carefully selected strains of mice that are used to investigate the genetics of particular disease entities. Of course a mouse is not a computer model, and a mouse cannot be abstracted from its life-support system; still, in both these cases of modeling complex systems, to understand a thing is to understand the workings of a model *for* it rather than a model *of* it. Along these lines, Dahan Dalmedico cites the apt remark of one of the originators of meteorological simulation, John von Neumann, who said that "the sciences do not try to explain . . . they mainly make models."[13]

Von Neumann went on to define a (mathematical) model as "a mathematical construct which, with the addition of certain verbal interpretations, describes observed phenomena."[14] Conscious that these "certain verbal interpretations" played a role, he nevertheless did not make much of them. Mary Morgan takes up the missing link in her discussion of how games derived from the Prisoner's Dilemma have become the *E. coli* of economics. Economists have long sought after a science of laws governing general equilibrium in a perfectly competitive market in which individual actors are utility-maximizing rationalists, but the Prisoner's Dilemma, Morgan shows, has rather insidiously undermined their faith. It became a pervasive means of conceptualizing economic action during the Cold War, after game theory was introduced at the RAND Corporation and the Institute for Advanced Study. The Cold War, in the eyes of analysts, came simply to be a sequence of prisoner's dilemmas.

Morgan wants us to appreciate just how bizarre it is that the wide world of international diplomacy got squeezed into the 2 x 2 matrix of payoffs for the Prisoner's Dilemma. She would have us bring our incredulity to the question of how economists could have developed the same little matrix into a proliferating set of games for reasoning about complicated economic situations. How could such an apparently poverty-stricken tool prove so versatile? Morgan's answer breathes life into von Neumann's "certain verbal phenomena." She argues that it is the narratives accompanying the matrix that adapt it to various situations by spawning new games, for it is the text, not the matrix, that contains both economists' traditional assumptions about rationality and equilibrium and the rules of the game to be played. Thus the narratives produce quite specific types of games as model situations appropriate for exploring particular kinds of economic behavior. All of this particularity in the narratives suggests that they might well be compared to certain modes of analysis common in the human sciences. We take up two of those methods in our concluding section.

PART 3: HUMAN SCIENCES

Focused methodological engagement with the problem of extracting the universal from the particular in the human sciences first occurred in the late nineteenth century, when newly emerging fields like sociology and anthropology sought professional identification and status. The physical sciences themselves had only recently gained credibility in their aim to subject the formerly experimental subjects of electricity, magnetism, and heat to general mathematical laws and to unite all branches of physics under the laws of energy conservation and entropy increase. Evolutionary theory in biology and newly discovered statistical regularities in society promised equivalent generality. To many, the time seemed ripe to attempt something similar for more directly human productions. We are most familiar with the problem in its negative form, articulated by Windelband as the contrast between the methods of the *Geisteswissenschaften* and the *Naturwissenschaften*, and by Wilhelm Dilthey as the contrast between the requirements of understanding (*Verstehen*) and those of explanation (*Erklärung*).[15] Closely bound up with that problematic are two interrelated modes of dealing with it, the case and the exemplary narrative.

Two of our authors have written elsewhere on cases and exemplary narratives in the human sciences. John Forrester, in his 1996 article, "If *p*, Then What? Thinking in Cases,"[16] argued that the scientificity of psychoanalysis—and, for that matter, the human sciences in general—was decidedly nonuniversalistic, organized principally not around the attainment of generalizable laws, but rather on knowledge of what Aristotle called "infinitely various" individuals, or cases. The Aristotelian position holds that there can only be sciences of what is universal and generalizable, but, as Forrester shows, the late nineteenth century saw the proliferation of case-based reports and of sciences—psychiatry, psychology, criminology, and the other clinical disciplines—premised on such reports and their capacity to render individual lives in a form amenable to the conventions of science. Since then—in the human sciences and in law and medicine—"the case" has functioned much like a model system. Cases in these disciplines are valued for their specific material reality, their individuality, and at the same time their typicality. Cases, it is assumed, capture individuals in all their complex uniqueness while at the same time rendering them in a generically analyzable form. The disciplines of psychiatry and psychoanalysis in particular have been constructed over the course of the twentieth century largely from the knowledge of cases, both those published in journals and books and those discussed and passed down as part of the field's oral traditions.

Case thinking found its first formal disciplinary instantiation as a system of instruction, introduced in the 1870s by C. C. Langdell of the Harvard Law School, who promoted it as a means by which aspiring lawyers could master the law's principles and governing doctrines through the study of "the cases in which it [was] embodied."[17] Walter B. Cannon and Richard Cabot soon after imported the case method to Harvard's medical school and teaching hospitals, and, in 1911, the university's newly established business school followed suit. Forrester points out that in law in particular, but in the clinical sciences as well, a strong tradition of reasoning by example, of, in Oliver Wendell Holmes's words, "reasoning from case to case," has profoundly but indeterminately shaped practices and that it has in turn found renewed life in the new casuistic science of bioethics.[18] Yet however durable the science of the particular fact has proven, a tension between an interest in capturing complexity and individuality and the demand for system, for formal principles—often presented as the demand of "science"—has historically run through the case-based disciplines, generating epistemological confusion and practical controversies. Can the case serve both for generating rational principles and for maintaining locality and specificity? What has been the history of this relation? The essays by Forrester and Clifford Geertz explore aspects of the ways in which cases function like model systems in such respects and how certain cases have achieved exemplary status within their disciplines, serving as nodal points around which practice—including teaching, research, and the generation of theory—have been organized.

Forrester's reflections on the consolidation of a new epistemological model that fixed on specificity rather than generality in the late-nineteenth-century human sciences complement the historiographical analyses of Carlo Ginzburg. In his now classic 1983 article, "Clues: Morelli, Freud, and Sherlock Holmes," Ginzburg traced the emergence of what he called the conjectural or semiotic model, counterposed to Galilean science's stress on measurement and experiment, that focused on the particular and on individual cases and that favored "little insights" over grand theory, traces and clues over systematic knowledge. The more a discipline was concerned with individuals, Ginzburg wrote, "the more difficult it became to construct a body of rigorously scientific knowledge."[19] The human sciences, concerned as they were with the individuals and their particularities, perforce accepted the conjectural paradigm, which prefigures Forrester's case thinking, characterized by reasoning "from particulars to particulars." History is a case in point, "irremediably based in the concrete," Ginzburg writes: "Historians cannot refrain from referring back (explicitly or by implication) to comparable series of phenomena; but their strategy for finding things out, as well as their expressive codes, is basically about particular cases, whether concerning individuals, or social groups, or whole societies."[20] They seek the universal through the particular, through the clues of the highly specific and located—the basis of Ginzburg's own microhistorical method. While the medically trained art historian Giovanni Morelli offers the most direct historical source for the method of clues, Sherlock Holmes remains the most colorful representative of both the method and its origins in medical diagnosis (through the training of Sir Arthur Conan Doyle). Holmes cared nothing for abstract laws of nature or for such grand schemes as the Copernican system. Instead he used his profound knowledge of the natural historical aspects of sciences like chemistry and geology, and of mundane things like tobacco ash and mud, to connect one particular case to another by analogy. "I am generally able, by the help of my knowledge of the history of crime, to set them [confused colleagues] straight. There is a strong family resemblance about misdeeds, and if you have all the details of a thousand at your finger ends, it is odd if you can't unravel the thousand and first."[21] Although Holmes did not aim at general principles, his method of clues does not differ as much as one might expect from views sometimes expressed by mathematicians like von Neumann and Stanislav Ulam about the heuristic use of the computer to obtain insights into pure mathematics. Ulam remarked: "By producing examples and by observing the properties of special mathematical objects, one could hope to obtain clues as to the behavior of general statements which have been tested on examples."[22]

Ginzburg points out that Freud found inspiration in the Italian physician Morelli's method, writing that his method of inquiry was "closely related to the technique of psychoanalysis"—divining "secret and concealed things from despised or unnoticed features."[23] In psychoanalysis, as in the other sciences of humans, the interpretation of clues formed the substance of the science. Similarly, Forrester's essay here on Robert Stoller's classic case study, *Sexual Excitement*, invites readers to explore the question of how the case figures in psychoanalysis. But rather than constitute a formal inquiry into the issue, this essay is itself distinctively psychoanalytic, addressing theoretical questions through the mining of one exemplary case. Forrester highlights the distinctive function of the psychoanalytic case, suggesting that it attempts to convey the experience of both analyst and patient, eschewing any claims to disinterested scientificity. He argues that the case, as a genre of writing, replicates the transferential and countertransferential relations that govern the analytic situation; it cannot transcend the conditions of the psychoanalytic encounter it is meant to document. The actual writing of the case thus enacts the very forces it attempts to capture—exhibitionism, in this instance, which figures variously as Stoller's patient's signal symptom, as a charge against which Stoller preemptively defends himself in an endlessly reiterated and deferred way, and, finally, as a charge against which Forrester, in turn, must mount his own defense. But both of these defenses are bound to fail, he suggests, for not only does the narrative force of this specific case prompt one at every turn to assume the position of the voyeur one disavows but the psychoanalytic case as a genre traduces the condition of confidentiality that is at the heart of the analytic transaction, inviting the reader to, in Stoller's words, peek in on forbidden scenes. Exhibitionism is thus a formal characteristic of the case in its written, transmissible form. Readers of the case necessarily participate in the perversion of looking in on forbidden scenes; in transmitting these scenes, Forrester both participates in the voyeurism and attempts, through his exhaustive analysis, to mimic the depletion of transferential eroticism that Stoller informs readers was a condition of publication. Forrester's essay consists in a layered enactment of and reflection on the conditions under which psychoanalytic knowledge is produced and transmitted. Can writers on psychoanalysis transmit psychoanalytic knowledge, or are they fated, like the analyst, to "infect" readers with its terms and frames of reference?

Clifford Geertz's essay on rituals as model systems in anthropology examines another facet of the human sciences. Surveying the deployment of ritual in generations of classic anthropological texts (or case studies), Geertz reconsiders both functionalist and structuralist approaches before reframing the question of what it is rituals model in hermeneutical terms: rituals, he proposes, model the cultural achievement of having confidence in the "reality" of one's world, its depth and substantiality, and, at the same time, the threat of losing that confidence. Rituals—characterized here as "cultural *Drosophila*," like Morgan's Prisoner's Dilemma as the *E. coli* of economics—thus not only model practical action and category change but bring to light modes of being-in-the-world. Geertz traces the historical fate of ritual, from, in anthropology's early days, an object of study to, more recently, an object to study with: "the microscope, not the bug under it." He concludes that ritual might usefully be considered akin to model systems in biology, sharing with them the characteristics of specificity, typicality, materiality, and complexity. Further, he argues, ritual can model just about anything that the anthropologist finds in need of explanation. Assuming the position of observer of moderns' lostness and alienation in the world—pointedly evident in the schizophrenic's apprehension of her surroundings as strange and peculiar, herself as detached and outside—Geertz provocatively concludes that what is missing, a commonsense orientation to reality, is precisely what ritual provides, not only to the individual in a culture but also to the anthropologist studying that culture.

It will be apparent that canonical case studies of ritual, such as Geertz's of the Balinese cockfight, and exemplary psychoanalytic cases, like Freud's of Dora, depend for their effectiveness on the specificity of the narrative that expresses them. In this they share something with the Prisoner's Dilemma, whose applicability depends on a narrative construction. Arguably, the case, even when highly standardized to resemble a laboratory mouse, depends crucially on its accompanying narrative, for it is the narrative that conveys its specificity. This suggestion obtains further support from the article by Josiah Ober on Athenian democracy as an exemplary narrative for normative political theory. Ober faces the alternative charges of historicism and essentialism leveled, respectively, against those (historians) who would prioritize specific instances of democratic organization and against those (political theorists) who would propose transcendent universal principles. He finds a middle ground in which the Athenian case, as a richly articulated narrative of political practice, can serve as the basis

for running thought experiments on such things as rights, citizenship, and civic virtue. It can help to frame the questions and expand the scope of contemporary ethical intuitions while challenging the capacity of any universal laws of democratic development to deal fully with historically specific political formations. Democratic Athens, as a model system, enriches political theory, while political theory enriches the model system of democratic Athens.

We end this volume with Carlo Ginzburg's self-proclaimedly experimental microhistory, in which he directly addresses the question that animates this section: "Can an individual case, if explored in depth, be theoretically relevant?" Answering his question in the affirmative, Ginzburg shows how plumbing the particular is not to abjure generalities, but to open the possibility of unsettling them—in this instance, those that inform Max Weber's ideal-typical rendering of *The Protestant Ethic and the Spirit of Capitalism* and Karl Marx's earlier alternative in *Capital*. Ginzburg is no proponent of the inductive methodology championed by early-twentieth-century theoreticians of the case examined by Forrester. Rather, taking his cue from Weber's argument that ideal types—representing Weber's attempt to capture human universals—must be continuously tested, he uses the obscure but detailed story of an actual Calvinist entrepreneur of the early eighteenth century to critique the missing role of violence and conquest in Weber's thesis and the missing religious agents in Marx. Ginzburg's entrepreneur, Jean-Pierre Purry, turns out to be altogether incompatible with the Weberian ideal type of the ascetic early capitalist forerunner, which forces not so much a revision of the ideal type as a reconsideration of the theoretical utility of such a construct. Ideal types, by Weber's telling, are "conceptual wholes," distillations of characteristics from innumerable individual examples. Cases and exemplary narratives, organized around specificity and particularity, are resistant to the level of generality on which the ideal type is premised; Purry is too idiosyncratic a figure to fit Weber's scheme. But neither is his case to be the occasion for qualifications and tinkering at the margins. Rather, it prompts Ginzburg to lay out the theoretical possibilities of the particular, to question the reflexive relegation of the individual case to the periphery of theory and to mount instead an argument for its centrality to historical writing, its capacity to yield interpretive riches comparable to—even exceeding—those of theory. Ginzburg closes his piece on a Proustian note in which the problematic of this volume is nicely anticipated. "People foolishly imagine that the broad generalities of social phenomena afford an excellent opportunity to penetrate further into the human soul; they ought, on the contrary, to realise that it is by plumbing the depths of a single personality that they might have a chance of understanding those phenomena."[24]

NOTES

This volume had its origins in a seven-session workshop, convened by the editors, "Model Systems, Cases, and Exemplary Narratives," in the Program in History of Science at Princeton University, 1999–2001. We thank the history department and Shelby Cullom Davis Center for supporting these events and graduate students Renée Raphael and Doogab Yi for assistance in preparing the manuscript. Two individuals outside the program faithfully attended nearly all the workshops, keeping a continuous conversation going in addition to contributing their own papers and comments: Mary Morgan and Clifford Geertz. We were deeply saddened that Cliff passed away while the volume was in production, and dedicate this book to his memory.

1. Christine Bahls, Jonathan Weitzman, and Richard Gallagher, "Biology's Models," in "Model Organisms," *The Scientist* 17, supplement 1 (2003), 5.
2. The distinction made here calls upon a more fine-grained understanding of how models function in various different representing roles compared with the description sometimes stressed in philosophy of science that scientists build models *of* something *for* some purpose. For an account that does pay close attention to how models represent within that latter framework, see Evelyn Fox Keller, "Models of and Models for: Theory and Practice in Contemporary Biology," *Philosophy of Science* 67 (2000): S72–S86. We are indebted to Mary Morgan for bringing the model of/model for distinction to our attention, and helping clarify its role in our analysis.
3. Wilhelm Windelband, "Geschichte und Naturwissenschaft," in *Präludien: Aufsätze und Reden zur Philosophie und ihrer Geschichte* (Tübingen, Germany: Mohr, 1915): 145; reprinted as "Rectorial Address, Strasbourg, 1894" (trans. Guy Oakes) in *History and Theory* 19 (1980): 169–85.
4. Two other collections highlight the role of concrete objects in the formation of knowledge: Soraya de Chadarevian and Nick Hopwood, eds., *Models: The Third Dimension of Science* (Stanford: Stanford University Press, 2004), examines the role of three-dimensional models in science, technology, and medicine. Lorraine Daston, ed., *Things That Talk: Object Lessons from Art and Science* (New York: Zone, 2004), considers more broadly how particular made "things" articulate what we come to know.
5. See, for example, Mary B. Hesse, *Models and Analogies in Science* (London: Sheed and Ward, 1963). We have taken inspiration from a recent reconsideration of scientific models: Mary S. Morgan and Margaret Morrison, eds., *Models as Mediators: Perspectives on Natural and Social Science* (Cambridge: Cambridge University Press, 1999). Although the models Morgan and Morrison address are by and large mathematical, not material, their emphasis on the partial autonomy of models connects with our interest in the autonomy of model systems.
6. Nancy Cartwright, "Nomological Machines and the Laws They Produce," in *The Dappled World: A Study of the Boundaries of Science* (Cambridge: Cambridge University Press, 1999), 49–74.

7. John Forrester, "If *p*, Then What? Thinking in Cases," *History of the Human Sciences* 9 (1996): 1–25. Ian Hacking, *The Taming of Chance* (Cambridge: Cambridge University Press, 1990), 6–7; following A. C. Crombie, "Philosophical Presuppositions and Shifting Interpretations of Galileo," in *Theory Change, Ancient Axiomatics, and Galileo's Methodology*, ed. Jaakko Hintikka, David Gruender, and Evandro Agazzi (Dordrecht: Reidel, 1981), 271–86. Hacking developed the notion at more length in "Styles of Reasoning," in *Post-Analytic Philosophy*, ed. John Rajchman and Cornel West (New York: Columbia University Press, 1985), 145–64.
8. Timothy Lenoir, "Science for the Clinic: Science Policy and the Formation of Carl Ludwig's Institute in Leipzig," in *Instituting Science: The Cultural Production of Scientific Disciplines* (Stanford: Stanford University Press, 1997), 113, 127. See also Frederic L. Holmes, "The Old Martyr of Science: The Frog in Experimental Physiology," *Journal of the History of Biology* 26 (1993): 311–28; Frederic L. Holmes, *Claude Bernard and Animal Chemistry: The Emergence of a Scientist* (Cambridge: Harvard University Press, 1974), 1–2; Claude Bernard, *Introduction à l'étude de la médecine expérimentale*, 3d ed., trans. Henry Copley Greene (New York: Dover, 1957), 149–50; Robert M. Brain and M. Norton Wise, "Muscles and Engines: Indicator Diagrams and Helmholtz's Graphical Methods," in *Universalgenie Helmholtz: Rückblick nach 100 Jahren*, ed. Lorenz Krüger (Berlin: Akademie Verlag, 1994), 124–45.
9. Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994); Nathaniel C. Comfort, *The Tangled Field: Barbara McClintock's Search for the Patterns of Genetic Control* (Cambridge: Harvard University Press, 2001); Karen A. Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900–1955* (Princeton: Princeton University Press, 2004); Ilana Löwy and Jean-Paul Gaudillière, "Disciplining Cancer: Mice and the Practice of Genetic Purity," in *The Invisible Industrialist: Manufactures and the Production of Scientific Knowledge*, ed. Gaudillière and Löwy (London: Macmillan, 1998), 209–49; Joshua Lederberg, "*Escherichia coli*," in *Instruments of Science: An Historical Encyclopedia*, ed. Robert Bud and Deborah Jean Warner (New York: Garland, 1998), 230–32; Angela N. H. Creager, *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930–1965* (Chicago: University of Chicago Press, 2002).
10. Jessica A. Bolker, "Model Systems in Developmental Biology," *BioEssays* 17 (1995): 451–54. On the use of *Drosophila* in developmental genetics, see Evelyn Fox Keller, "*Drosophila* Embryos as Transitional Objects: The Work of Donald Poulson and Christiane Nüsslein-Volhard," *Historical Studies in the Physical and Biological Sciences* 26 (1996): 313–46.
11. Martin Rudwick, "Minerals, Strata, and Fossils," in *The Cultures of Natural His-*

tory, ed. N. Jardine, J. A. Secord, and E. C. Spary (Cambridge: Cambridge University Press, 1996), 266–86, offers a concise survey.

12. See also Peter Galison and Alexi Assmus, "Artificial Clouds, Real Particles," in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (New York: Cambridge University Press, 1989), 225–74; and Harro Maas, *William Stanley Jevons and the Making of Modern Economics* (Cambridge: Cambridge University Press, 2005), chap. 4, "Mimetic Experiments."
13. John von Neumann, "Methods in the Physical Sciences," in *Collected Works*, ed. A. H. Taub, 6 vols. (Oxford: Pergamon, 1961–63), 6:491.
14. Ibid.
15. H. P. Rickman, ed., *Meaning in History: W. Dilthey's Thought on History and Society* (London: Allen and Unwin, 1961).
16. Forrester, "If *p*, Then What? Thinking in Cases."
17. C. C. Langdell, *A Selection of Cases on the Law of Contracts* (Cambridge: Harvard University Press, 1871), vii, quoted in Forrester, "If *p*, Then What," 15.
18. Edward H. Levi, *An Introduction to Legal Reasoning* (Chicago: University of Chicago Press, 1949), 1.
19. Carlo Ginzburg, "Morelli, Freud, and Sherlock Holmes: Clues and Scientific Method," in *The Sign of Three: Dupin, Holmes, Peirce*, ed. Umberto Eco and Thomas A. Sebeok (Bloomington: Indiana University Press, 1983), 97.
20. Ibid., 92.
21. Sir Arthur Conan Doyle, *A Study in Scarlet*, 1887, in *Sherlock Holmes: The Complete Novels and Stories*, 2 vols (New York: Bantam, 1986), 1:15.
22. Stanislav Ulam, *Adventures of a Mathematician* (New York: Scribner's, 1976), 201.
23. Sigmund Freud, "The Moses of Michelangelo," in *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, ed. James Strachey (London: Hogarth Press, 1955), 13:222.
24. Marcel Proust, *The Guermantes Way*, vol. 3. of *In Search of Lost Time*, ed. D. J. Enright, trans. C. K. Scott Moncrieff and Terence Kilmartin (New York: Chatto and Windus, 1992), 450.

PART I: BIOLOGY