CHAPTER 1

Introduction

Historical Socionatural Systems and Models

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How should archaeologists and other social scientists tackle the big and little questions of change in socionatural systems? Most nonpractitioners think that the answer is obvious—fieldwork! That is certainly the place to start, and we know very few archaeologists who were not first drawn to their profession because of a love of fieldwork and discovery.

Still, it does not take long before we realize that the things we discover, in and of themselves, are not immediately helpful in answering the questions that usually intrigue us most—those involving explanations for change. This realization might start with little questions: why is one raw material for making stone tools prevalent at this site, while right next door, another was more important? But before long, bigger questions surface: how and why do humans cooperate so successfully in large, unrelated groups, and how and why do human societies move from egalitarian to more hierarchical organizations?

Every time we ask one of these troublesome "how" or "why" questions, we build a model to try to answer it. This model may be very informal and even a little vague ("maybe the best source of stone was depleted, and later people had to settle for second best"). Thinking out loud about the possibilities shows that to understand anything, we make a mental model of it: a model here is just a candidate explanation. This is the first meaning of our title: in this volume are presented many models for how societies work and how they change. These models are partial, provisional, and subject to elaboration and revision.

Cognitive scientists have a view of models that explains their foundational character in our thought processes. We operate in the world, in their view, by constantly making representations of it. Cognitive scientists do not completely agree on the nature of those representations or on how they are constructed and retained; indeed, it seems likely that our minds use several methods to represent knowledge (Markman 1999:277–300). These mental representations, or models, are quite often incomplete, not infrequently wrong, and almost always qualitative (Markman 1999:248–76).

Beginning no later than the early Upper Paleolithic with artifacts such as the Blanchard Plaque (which seems to provide simultaneously a calendric calculator and a kind of topographic model for the placement on the horizon of the moon at sunset as it waxes and wanes through two-and-a-quarter months [Marshack 1985]), we humans have increasingly turned to tangible external models to supplement the operation of our cognition and to store and share the fruits of those cognitive labors. The creativity with which this can be done, even within the limited domain of graphic models, is wonderful (for example, Tufte 1990). This book is about new developments in applying dynamic models for understanding relatively small-scale human systems that are deeply embedded within, and studied as a part of, the environments they inhabit and alter.

Rethinking Archaeology as a Model-Based Science

But underlying this is a subtext. Taken together, the chapters in this volume constitute an argument for a new way of thinking about how archaeology is (and should be) conducted. Most archaeologists of a certain age will remember the call to re-create archaeology as a logical positivist or logical empiricist science (for example, Watson, LeBlanc, and Redman 1971), with its hope that generalizations, or "covering laws," could be found that would explain those phenomena that could be subsumed under them, with other trial explanations rejected in a Popperian fashion:

> If we agree that explanation means subsumption of the particular events and processes [to be explained] under appropriate general or covering laws, then we must agree on the source of these laws. Do the necessary confirmed laws already exist, or must we formulate and test them? If the former, what are they? If the latter, how do we go about it? Can we use the archaeological record to help us formulate and to test hypothetical laws about particular events in human prehistory and processual aspects of human behavior, and about major aspects of culture and cultural processes? Yes, of course, to the extent that archaeology is pursued as a science.… Logically speaking…scientific archaeology is a viable discipline whose practitioners are primarily con-

> cerned with explanation of past events and processes, and also with the use of those particular events and processes to help formulate and test culture processual laws. (Watson, LeBlanc, and Redman 1971:171–72)

Progress in cumulating these laws, however, has been slow at best, as even advocates admit (for example, LaMotta and Schiffer 2001:47; for a demographer's perspective, see Burch 2006). Already in the 1970s this project seems to have been honored most frequently in the breach. Instead, we believe that archaeologists—especially those committed to explanation—have drifted *in practice* towards what philosophers of science call a "model-based" (Giere 1999) or "semantic" (Lloyd 1988; Suppe 1977b:221–30) approach to the task of explaining what happened, and why, in prehistory, and away from variants of either covering-law models or broader hypotheticodeductive, positivist approaches in the tradition advocated by many New Archaeologists.[1]

As described by Suppe for science more generally, when a new theory is undergoing development and is widely believed to be inadequate in some respects (which we take to be an accurate description of existing theory in archaeology), trying either to refute or confirm it is pointless:

> What *is* to the point is to use observation and experiment to discover shortcomings in the theory, to determine how to improve the theory, and to discover how to eliminate known artificialities, distortions, oversimplifications, and errors in the descriptions, explanations, and predictions of reality that theory affords.… Except in primitive sciences…one finds little concern with refutation or inductive confirmations of theories in actual scientific practice. Rather the focus is on the use of reason, observation, and experiment to develop a promising theory. (Suppe 1977a:706–07)

In what respects, then, does a model-based archaeology (we offer the uncapitalized acronym *mba* to emphasize the pluralism of this approach) differ from the scientific approaches to archaeology offered by the New Archaeologists? A critical difference is that, in mba, models are not true or false in the manner of hypotheses, though to be useful, they must be clear and internally consistent. A good model is not a universal scientific truth but fits some portion of the real world reasonably well, in certain respects and for some specific purpose (Burch 2006; Giere 1999:5–6, 73). Degree of fit is determined through empirical research, but a model that does not fit one case may be useful for another—as opposed to a candidate generalization or covering law that can be fully discredited by one contrary observation. The recognition that a model might be useful for one *purpose* but not for another recalls Hodder's (2001:5) observation that archaeological theories are always "of something" rather than completely general.

What is a model, in the approach we favor? It is an imaginary system, represented in language, mathematics, computer code, or some other symbolic medium, that has useful similarities to aspects of a target system in the real world. It is often highly simplified, omitting details that are thought to be noncritical to the aspects of the target system being explored. It might be viewed as an abstraction, a simplification, an idealization, or a conceptual device.

Why not try to understand a target system directly, without the use of models? Cognitive scientists would argue that this is impossible: we always interpose models between the real world and ourselves in any attempt to understand the world. Those who believe that they are following a nonmodel strategy have simply not made their models explicit.

If that is the case, then why not try to build an abstract model of a target system directly, rather than interpose some model possibly first constructed for some other use? The mba approach offers the possibility of making progress on two fronts simultaneously: the advancement of theory (How well and in what respects does this model fit this situation? How could the fit be improved?) and the application of theory (Is this target system a member of the class of systems described by this model? If not, why not?). It legitimizes development of toolkits of models that might be useful for different purposes (for example, to explain processes as viewed at various spatial and temporal scales). Model-based archaeology need not be limited to theories with measurable parameters, although it is true that theories with nonmeasurable parameters cannot be viewed as structures within phase spaces having configurations "imposed on them by the laws of the theory" (Suppe 1977b:227)—a foundational definition of theory in semantic approaches. Above all, the mba approach encourages a sense of flexibility in model choice and in joint exploration of the model and its target system, which is in contrast to the more rigid prescriptions of the covering-law model yet is more guided, disciplined, and theory-driven than simple exploratory data analysis.

This, then, is the second meaning of our title; we recognize what we are doing in this volume as a model-based approach to archaeology. Here, we use *model-based* to mean a way of doing science, which, in our case, involves use of specific, generally quantitative models that provide partial descriptions of socionatural systems of interest that are then examined against those systems.

Whether the reader agrees with us that this model-based view of science describes how archaeology *should be* conducted, within the past forty years models clearly have received much attention in archaeology. This is, in part, due to the fact that they promise to help us do things we have difficulty doing with our brains, for example, representing complex sets of interacting dynamics and "exercising" them by watching how they play out through time. This interest was triggered in the 1960s and 1970s by the introduction of computers that seemed able to extend our capacity to deal with such complex dynamics, coupled with a sense that the principles of general systems theory might be useful for understanding processes of change (for example, Flannery 1968). We soon found out, however, that these computers could not help us much in cases in which the dynamics are really complex. Moreover, many archaeologists (and commentators on archaeology, such as Salmon [1978:181]) began to suspect that the "systems" that archaeologists deal with are "too complex" to yield to these techniques.

Dealing with Complexity

After a lull, we now (since 1995) seem to have access to sufficiently powerful computers and to sufficiently sophisticated software to have another go at it. But more importantly, we have at our disposal a theoretical approach that differs in many ways from the relatively simplistic systems models of the 1970s. McGlade and van der Leeuw (1997) argue the relevance of these new ideas for archaeology. Contrary to earlier systems approaches, this "complex systems" approach acknowledges that all systems, including social ones, are in open interaction with their environment, exchanging matter and energy with it. Moreover, such system dynamics are now considered to be nonlinear, multiscalar, and irreversible, in the sense that they exhibit path dependence or history. The following chapters, we believe, show that the implications of this reconceptualization of systems dynamics are profound and far-reaching.

While definitions for complex systems, and for complexity itself, are elusive, we are satisfied to follow Boccara (2004), who suggests that such systems exhibit at least the following common characteristics:

> 1. They consist of large numbers of interacting agents.
> 2. They exhibit *emergence*; that is, a self-organizing, collective behavior difficult to anticipate from knowledge of the agents' behavior.
> 3. Their emergent behavior does not result from the existence of a central controller. (Boccara 2004:3)

We argue that the approach constructed on this basis does contribute significantly to a better understanding of (1) the dynamics responsible for long-term human (social and biological) evolution and (2) the ways human biological and social evolution have slowly transformed our environment and made it, in many cases, dependent on its interaction with society. We base this claim on the fact that the "complexity perspective" has shown itself to be capable of the following:

- Conceptualizing problems in both natural and social dynamics in a language independent of specific disciplines. Concepts used for genetic networks (Kauffman 1993), for example, have been shown to apply helpfully to networks of economic exchange in small-scale societies (Kohler, Van Pelt, and Yap 2000).
- Conceptualizing the interaction between phenomena at different spatiotemporal scales by viewing large, stable phenomena as the result of unstable interactions between smaller entities.
- Reformulating natural dynamics from an irreversible temporal perspective by introducing the notion that similar causes can have different results and different causes, similar results. A typical strategy is to characterize the various possible outcomes of a set of interacting processes in terms of their probabilities in the space of the parameters examined and in terms of their stability under perturbation (Skyrms 1996).
- Rethinking issues of cause and effect in the social sciences, in which a common research tactic has been the evaluation of causal hypotheses with statistical analysis of

data on system behavior. In the nonlinear dynamics that are apparently so pervasive in nature, however, the effect of a change in a state variable depends to a very great extent on the state of the entire system at that moment (Wagner 1999).

- Describing in one approach both continuity and change, tradition and innovation, by relating the one to the other and thus moving away from our traditional emphasis on stability and our focus on investigating change. This is seen, for example, in the emphasis on the trajectories of systems in complexity approaches.

But theory alone is never enough. Keeping track of the actions of many agents, individually and simultaneously, who inhabit a spatially defined world and exhibit mostly local interactions was made immensely easier by the proliferation of object-oriented programming languages beginning in the 1990s and, following on those, various simulation platforms that built on those languages. Such approaches enable us to examine the possibility of the emergence of new structures (for example, institutions, alliances, and communities) out of the basal units and their interactions. But as we shall see, although these technical improvements have been important in making progress in studying some kinds of complexity, other kinds are more resistant.

Models can be characterized along a number of dimensions. The distinction among mental, verbal, physical (such as maps or three-dimensional models of buildings or landscapes), mathematical, and simulation models concentrates on the medium in which the representation is made; the last two together can be considered members of the class of "formal" models. Or we can focus on the degree of aggregation within the model, as in the distinction between systems (or systems-dynamic) models and agent-based (or individual-based) models. All the models collected here are, or are striving to become, formal models of some sets of processes that focus on explaining change in some human (usually prehistoric) system and involve both social and natural components in that explanation.

We suggest that, in developing multidisciplinary research of the kind in this volume, formal models offer advantages that might not be readily apparent (see also van der Leeuw 2004):

- Such models can be used to express phenomena and ideas in precise, unambiguous ways that typically involve economic logic and also can be understood by practitioners of all the disciplines involved in our kind of research.
- In theory, the domain of application of formal models is unlimited (but see below). They may be applied to all aspects of the social sciences, as well as the natural, earth, and life sciences, and are eminently suited to the study of the dynamics between society and the environment.
- Formal models are sufficiently abstract not to be confounded with reality and can be sufficiently detailed, rigorous, and "realistic" to force people with different backgrounds to focus on the same relational and behavioral issues.
- No less important in our context is the fact that formal models use a language (math-

ematics or computer algorithms) that differs from any natural language. This facilitates abstract thinking that links dynamic patterns in different domains.

- Certain kinds of formal models are able to describe changes occurring in complex sets of relationships. Hence, formal modeling is very suitable for constructing dynamic theories about complex phenomena (in the sense of Boccara, above), which can then be compared with the observations on which they are based. As Wilkinson and his colleagues argue in chapter 9, "if early state societies were truly complex, then it is necessary to tackle the full range of complexity that exists. If we are to do this with any degree of analytical rigor, we must build up large, complex models that incorporate a wide array of data sources and incorporate a range of interacting processes: social, economic, environmental, and political."
- Formal dynamic models may allow us to experiment relatively cheaply with different scenarios to explain certain sequences of cause and effect. This is particularly important in domains where real-life experiments are impossible.
- Certain classes of formal models enable us to study how interactions between individual, nonidentical entities at a lower level result in patterns at a higher level. This property is particularly relevant to the study of many collective phenomena that are the subject of the social sciences: the interactions between individuals create the society (and its culture), which, in turn, affect the behavior of the individuals or groups that constitute it.

A venerable but useful distinction that characterizes the focus of models was proposed by ecologist Richard Levins (1966). Levins argued that in models of complex systems it is, practically speaking, impossible to maximize the generality, realism, and precision of our models simultaneously: we can, at best, hope to maximize only two of these qualities (but see Sober and Orzack 1993). The most general of the models collected here, in the sense that the processes modeled might be applicable in a wide range of real-world systems, is certainly the contribution by Smith and Choi to understanding the emergence of social inequality (chapter 5). In part, this is due to the fact that it is also the least realistic, because there has been no effort to instantiate the model to fit a particular case. Most of the models here, in contrast, make an effort towards realism and precision that—inevitably, it seems—sacrifices generality to achieve goodness-of-fit to the situations at hand.

Models, then, enable us to explore concepts and theories either in relatively general (but perhaps never universal) contexts or in a specific context. Therefore, it is essential to consider the relationship among concepts, theories, and empirical research (for example, Dugatkin 2001:xii–vi). We understand concepts as deeper constructs that underlie theories in the way that the concepts of variation and differential proliferation (along with divergence or speciation) underlie Darwin's theory of evolution by natural selection. What is concept and what is theory, of course, is not always clear. Dugatkin (2001:xiii) considers Hamilton's "theory" of inclusive fitness (the notion that an individual's total fitness takes into account both her own offspring and, with discount determined by relatedness, those of her relatives) to be a fundamental conceptual advance stimulating much recent empirical research in behavioral ecology. Zahavi's (1975) "handicap principle" has been extremely fruitful in the same manner, leading in anthropology to the current widespread interest in costly signaling theory. In general, concepts give rise to theory that makes predictions about real-world phenomena. As theories (construed as classes of models) become more complicated, it becomes useful or even mandatory to have a computer derive the predictions of models derived from that theory: this is the process of simulation.

Do shared concepts underlie the chapters that follow? We suspect that there are several, though they are not articulated in most cases.

First, these researchers seem to share a view about the complexity of the world they are trying to understand. Auyang (1998:10) distinguishes three kinds of increasingly complex systems. First are systems regarded as being composed of many bodies but of only a few kinds and having only a few kinds of relations. Many physical systems, such as spin glasses and solar systems, fall into this category, and their complexity is fully described by Boccara's characterization, above.

Next in increasing complexity are organic systems, also composed of many bodies that, in this case, are highly specialized and integrated, as are the tissues and organs in an organism. The functional characteristics to which such organization gives rise complicate modeling considerably.

More complex still are what Auyang calls "cybernetic" systems, which, as in the case of people in societies, have all the complexity of the first two kinds of systems but add intentionality. Auyang suggests pessimistically, but realistically (in our view), that current theory can achieve a full understanding for only the first type of system and that we necessarily make radical simplifications when we study the second and third types of systems in order to try to understand them as many-body systems (often using concepts, such as optimization, developed for understanding physical systems). Modeling systems of these last two kinds thus requires considerable "coarse-graining" (a filtering out of insignificant details and an attendance to aggregate, large-scale behavior), successive approximations with models that apply only to specific regions (taken spatially, temporally, or with respect to a problem) of the phenomena of interest, and a willingness to move back and forth between models and historical narrative. This view of the way our target systems are organized and operate makes model-based archaeology attractive.

We suspect that the researchers assembled here would agree with this characterization but, at the same time, would push hard against these limitations, by trying to build theory that is appropriate to organic and cybernetic systems (see again chapter 5 by Smith and Choi) and by applying simulation methods that give us the possibility of creating specialized, variable, and tightly integrated agents whose behaviors can exhibit—if not intentionality—at least evolution through time (for example, see the use of cultural algorithms in chapter 4 by Kohler et al.). Also notable are the attempts by Jordan and Shennan to understand how the processes of culture change—though rooted in the processes of biological evolution—take on a life of their own, building on concepts pioneered by Cavalli-Sforza and Feldman (1981) and Boyd and Richerson (1985).

Beyond some shared notion of the complexities of the system they are dealing with, as well as the difficulties that this entails, several chapters rely on concepts such as trade-offs, or optimality, to give their argument structure and direction. This is most explicit in the case of chapter 8 by Janssen and Anderies and chapter 4 by Kohler and colleagues. Both chapters use these concepts as nonarbitrary points of entry into systems that, the authors realize, are affected by many forces in addition to possible tendencies towards optimization. These concepts are also implicit elsewhere, for example, in the arguments by Wilkinson and colleagues (chapter 9) that the differential costs of transport in southern and northern Mesopotamia structured many other differences realized in their divergent evolutionary trajectories.

In this volume, Cleuziou in chapter 10 and Berger and colleagues in chapter 3 seem the closest to developing concepts (or perhaps just theories) that are uniquely social in their referents. In part, this is a consequence of the fact that their questions are different from those asked in the other chapters. They study socioenvironmental change over very long time periods (millennia) in which different cultures and different societies have had different impacts on the environments concerned. They are therefore less interested in the detailed dynamics of human-environmental interaction than in the way in which such dynamics change over long-term time under the impact of *different* cultural regimes. Hence, they adopt the position that at the millennial-scale coarse-graining at which they are attempting to make sense of the available information, the societal dynamics are the drivers of change.

But, ultimately, that difference in focus is also a result of a difference in intellectual tradition. Both chapters are by archaeologists who have been trained in an inductivist tradition that finds its roots in the archaeology of historical periods, rather than in the anthropological archaeology prevalent in the United States. That approach owes much to history and, in particular, to the Annales school of economic and social history that was developed in France between the 1930s and the 1980s. It has therefore more of an eye for the societal dynamics but is also less willing to forego detail for the kinds of simplicity that accompany the more abstract types of models that find their origins in the natural and life sciences.

These French authors consequently have the opposite point of departure from that of authors such as Jordan (chapter 2), Shennan (chapter 7), and Janssen and Anderies (chapter 8). The latter begin their intellectual trajectory in the realm of concepts, often transposed (mutatis mutandis) from other disciplines (life sciences or economics, for example) and work from there towards the design of theories. The former are predominantly (but not completely) generalizing from observed data, building theories before they can reach the realm of concepts. In this they resemble more closely the work of those US and UK scholars trained in historical archaeology, who use both archaeological and historical data to design a very detailed and specific model of the socioeconomic systems operating over relatively short time spans. It might be said that the regional approach informed by training in geography that infuses the contribution by Wilkinson and his colleagues combines elements of both these approaches to very good effect.

No Apology for Neologisms

The editors of this volume are not enthusiastic about neologisms, but we acquiesce if an approach is significantly new and promises to grow to the point where a compact description is handy. This is the case for three words encountered throughout this volume, though they are not in common use in archaeology more generally.

The term *socionatural* is not entirely novel; the geographer Harold Miller used it, for example, to describe the complex of factors surrounding the founding of New Albany, Indiana, in a specific location and the resultant waxing and waning of its fortunes (Miller 1938). Reference to socionatural systems implies that neither social nor environmental factors are automatically accorded pride of place in explanation. Instead, emphasis is given to understanding how the dynamics of these two systems, traditionally considered from different perspectives, at different levels in a hierarchy, and analyzed by different means, might coincide to facilitate or inhibit change. Emphasis is also given to identifying the points of coupling between the systems (for more discussion, see McGlade [1995], van der Leeuw and Redman [2002], and Berger et al., chapter 3, this volume).

Ecodynamics is a term that, though in use earlier, was surely made famous by Kenneth Boulding in his 1978 book *Ecodynamics: A New Theory of Societal Evolution*. Boulding was a broad and provocative thinker whose work deserves to be remembered, but our specific use of this term owes more to McGlade (1995). He took up the term in archaeology to argue that "there is no 'environment,' there is no 'ecosystem,' there are only socio-natural systems." In particular, he reminded us that the concepts "environment" and "ecosystem" are objectified and reified cultural constructions embedded in contemporary attitudes and value systems. He called the study of the (essentially irreducible) natural and social phenomena and their interaction "human ecodynamics" and contrasted it with the then prevalent "human ecology," which in his view applies the more traditional perspective of societies as adapting to their environments.

Finally, this book owes much to the concept of "biocomplexity." Rita Colwell, director of the National Science Foundation (NSF) from 1998 to 2004, made biocomplexity a key initiative and was able to gain substantial funding increases for NSF, in part, to support increased biocomplexity-related research.

Biocomplexity research is designed to be both multidisciplinary and anti-reductionist, in that it is not supposed to study some parts of ecosystems in isolation from other parts. It is also designed from the outset to integrate social and behavioral sciences into studies of the ecosystem. In an interview with *Science* shortly after being named to NSF's top position, Colwell called biocomplexity research

> an attempt at understanding all the interrelationships between cells and organisms and between an organism and its environment.… We're taking all we know and utilizing it to build the type of models that we thought about 25 years ago that turned out to be so riddled with black boxes that we couldn't get the simulation we needed. But now, with the vastly increased power of computing and data mining, we can infuse a very strong science underpinning into environmental studies and make some dramatic gains in knowledge. (Mervis 1998)

To many anthropologists, the territory staked out by the juxtaposition of these three words should not seem alien—many elements of this approach can be found in our intellectual progenitors. From Julian Steward we can derive an interest in the great importance of the interface between societies and their environments that simultaneously acknowledges the importance of history, the "tools and knowledge" that people bring to bear on that interaction, and the "patterns of work necessary to bring the technology to bear upon the resources" (Murphy 1977:22). From Leslie White we can see an interest in what might be termed the "metabolic" basis of society—energy flows—which invites us to think about societies and ecosystems in terms of a common currency. Although one might object that Colwell's quotation asserts a greater role for modeling and simulation than has been traditional in anthropology, calls for this have been with us for some time now. Lewis Binford (1981:25–30) on numerous occasions emphasized that we must link our observations of the archaeological record to an understanding of systems dynamics to make reliable inferences about the past.

On with the Show

We invite you readers to view these chapters as exercises in coping with complexity in socionatural systems through successive approximations by models of various sorts. More specifically, we invite you to engage them on two levels: for what the application of these techniques can tell us about processes of change in some part of the world, or at some sociopolitical scale, and for what these authors have to say about the techniques themselves, as trial paths for approaching classes of problems. Here, the reader becomes an agent in the world of research:

> In problems of complication, as decision makers…agents look for ways to frame the situation that faces them. They try to associate temporary internal models or patterns or hypotheses to frame the situation. And they work with these. They may single out one such pattern or model and carry out simplified deductions on it, if they seek guidance for action. As further evidence from the environment comes in, they may strengthen or weaken their beliefs in their current models or hypotheses. They may also discard some when they cease to perform, and replace them as needed with new ones. (Arthur 2005:296)

Use these models and learn from them, build on them, make them better, assess them against new data, and let this process assist you in making complexity a little more tractable.

Acknowledgments

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The chapters are arranged, more or less, according to the scale of the society under consideration (Bodley 2003), which has the happy effect of putting papers side by side that are quite different in approach. We hope that this emphasizes the range of possible approaches and gives some sense of the advantages of each. Given the funding by NSF's Biocomplexity competition, we have encouraged the three projects with Biocomplexity funding (Kirch et al., Kohler et al., and Wilkinson et al.) to give somewhat fuller accounts of their activities.

Note

1. The term *model-based science* can be associated with rather different programs (Godfrey-Smith 2003:186–89, 238). One earlier meaning employs a logician's concept of model and denotes an attempt to find general similarities of structure in all scientific theories. We are more interested in a second use, apparently more common now, that considers *model-based science* to denote a way of doing science that involves a two-step process of building an imaginary model system, for example, with a set of coupled differential equations, and then making arguments of resemblance between that model and aspects of a target system in the real world.