Complexity theory in the study of space and place

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Abstract: Researchers across disciplines apply complexity theory to issues ranging from economic development to earthquake prediction. The breadth of applications speaks to the promise of complexity theory, but there remain a number of challenges to be met, particularly those related to its ontological and epistemological dimensions. We identify a number of key issues by asking three questions. Does complexity theory operate at too general a level to enhance understanding? What are the ontological and epistemological implications of complexity? What are the challenges in modeling complexity? In answering these questions, we argue that while complexity offers much to the study of place and space, research in these areas has a number of strengths that enhance complexity research.

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1 Introduction

Complexity theory is an exciting yet contested concept. It finds growing acceptance, as evidenced by recent coverage in the journal *Science* (1999) and lay scientific books (e.g., Kauffman 1995; Wolfram 2002). At the same time, detractors are rightly wary of hyperbole, seeing complexity alternately as a harmless fad (Sardar and Ravetz 1994), as an interesting but over-reaching field of research (Horgan 1995), or as an inappropriate transfer of physical science concepts into the social realm (Lo Presti 1996).

In addressing both human and environmental systems in an interdisciplinary setting, complexity presents researchers of space and place with challenging new opportunities that are nevertheless familiar. It has been suggested that complexity is “preternaturally spatial” (Thrift 1999: 32). Furthermore, relative to more narrowly focused fields, the interdisciplinary nature of space and place-based research allows for greater triangulation and comparison across theories, approaches, and areas of application. We use therefore use the term *space and place-based studies* to encompass a broad yet intrinsically related group of disciplines that find a home in *Environment and Planning A*. These include, but are not limited to, geography, anthropology, environmental science, urban planning, regional science, and architecture. Importantly, while much of the focus on complexity is found in the more ‘spatial scientific’ branches of disciplines, there is also interest found in the more ethnographic, grounded, often fieldwork-based, approaches. The resulting potential for applications across these disciplines to draw on and contribute to complexity theory is borne out by the growth in spatial complexity applications.

To clarify the role of complexity in space and place-based studies (and for spatial sciences in complexity) we must define what ‘complexity’ means. There is no single identifiable complexity theory, but instead an array of concepts applicable to complex systems, dispersed across fields as wide-ranging as new age spirituality and prosaic business practice (Thrift 1999). The word ‘complexity’ attaches to research in three major streams (Manson 2001). Least important for the current discussion, is **algorithmic complexity** from *mathematical complexity theory* and *information theory*, which contends that the complexity of a system resides in the difficulty of describing system characteristics **Deterministic complexity** attempts to simplify some classes of dynamical systems using *chaos theory*, and to a lesser extent, *catastrophe theory*. Finally, complexity research is increasingly identified with **aggregate complexity**, which is the study of how individual elements working in concert create complex systems with internal structure relative to a surrounding environment, and which may also exhibit learning and emergence. Given the fluid nature of complexity concepts, it is important to note that this schema is just one of many, and subject to debate and change (e.g., Thrift 1999; Mikulecky 2001; Stewart 2001; Reitsma 2002; Manson 2003). It is not our intention here to contribute further to attempts to define complexity (see Lissack 2001).

A loose definition of complexity is only to be expected from an endeavor implicated in so many disciplines. Along with Jonathan Phillips (1999, see esp. x-xi) we would argue that while the potential for communication between disciplines offered by the complexity perspective is exciting, it is also important to focus on how the perspective is applicable in the spatial sciences. Again, the interdisciplinary focus of place and space-based studies is fertile ground for complexity concepts, and represents an opportunity for the varied disciplines with a shared focus on place and space to provide direction in the complexity field.

With these ideas in mind, we turn to a number of open questions designed to refine the concept and practice of complexity science, and go some way towards either dismissing or confirming the significance of complexity for spatial sciences. We ask, and consider possible answers to, the following three questions:

1) Does complexity theory operate at too general a level to enhance understanding?
2) What the ontological and epistemological implications of complexity?
3) What are the challenges in modeling complexity?

2 Does complexity theory operate at too general a level to enhance understanding?

Scientists constantly grapple with the need to generalize while not losing sight of the particular. Most of the social sciences over the last three decades have seen disagreement over the extent to which *any* grand 'theory of everything' can claim broad generality faced with far-reaching critiques arising from post-modernism, feminism, post-colonialism, and other positions on the cultural turn (Barnes 2001; Hamnett 2003; Johnston, Hepple et al. 2003). In some respects, complexity is the grand theory to end all grand theories, and so we must question its viability from this perspective at the same time as we assess the utility of drawing on space and place-based research for its familiarity with scale concepts and chaining data, models, and theory with spatial and temporal characteristics.

2.1 Complexity and generalization

Complexity theory proposes a number of high-level generalizations about the behaviour of collections of interacting elements that constitute complex systems. It is therefore about generality, where a diverse range of phenomena is seen as sharing fundamental structural and dynamic properties. For example, Stuart Kauffman (1995) suggests that grammar strings (small collections of structured data) may be considered "models of molecules, models of goods and services in an economy, perhaps even models of cultural memes such as fashions, roles, and ideas" (p. 257). The term model in complexity research applies to abstract conceptualizations through to mathematical measures of complexity. Particularly important are a set of conceptual models that act as generalized templates of complexity that are considered as underlying causes or patterns that are common to different systems. Self-organized criticality, for example, is a single process model with claimed applicability to a host of diverse phenomena (Bak 1996). Zipf's rank size law provides perhaps the best pattern example, with its claimed applicability to domains as diverse as city-size distributions (Krugman 1996), word lengths (Zipf 1949), and e-commerce (Adamic and Huberman 2000).

The sweeping (some would say over-reaching) generality of complexity theory is due in part to its having roots spanning from the 'philosophy of organism' (Whitehead 1925) to cybernetics (Wiener 1961). It also draws from general systems theory, sharing a focus on system interconnectedness (Phelan 1999). Seen from this perspective, complexity is the latest in a long line of cross-disciplinary efforts at scientific generalization and a key critique of general systems theory—that it is too abstract and general (Johnston 1994)—may apply equally to complexity theory. If complexity theory is equally applicable to so many manifestly different phenomena, what useful things can it possibly have to say about anything?

The use of abstract, high-level models in complexity theory is widespread and runs the risk of conflating fundamentally different phenomena. There are questions, for example, concerning the extent to which concepts and practices, particularly from the physical sciences, should be used in other disciplines in a manner that relies heavily on deduction from analogy and pays scant attention to accepted theory and empirical observation. Some models of physical processes such as earthquakes or avalanches, for example, draw on self organization and scaling laws without recourse to any description of the underlying physical principles involved

(Malanson 1999). Similarly, physicists have applied complexity models to species extinction and evolution in a manner that paleobiologists find naïve (Monastersky 2001).

Still more caution may be called for in examining aspects of human endeavor—such as lived experience, culture, or meaning—in order to avoid treating them as facile algorithmic expressions (Stewart 2001). For example, when examining complexity-based models of ethnicity and space—with foci ranging from residential segregation to genocide—one must be necessarily cautious in accepting that useful explanations for such difficult, multifaceted, and long-standing problems are to be found on two-dimensional grids ‘peopled’ by simple-minded households acting according to just a few simple rules (e.g., Schelling 1978; Benenson 1998; Srbljinovic, Penzar et al. 2003). As we discuss below, validation of models against careful observation of real world settings is key to broader acceptance and applicability of complex models of social process.

The issues raised by generalization and simplification are not new to the spatial sciences (e.g., Wilson 1969; Couclelis 1984). Responses to these issues in place and space-based research include greater attention paid to representation, a focus on empirical grounding, and accommodation of interdisciplinary perspectives. By virtue of grappling with space and time in addition to a host of substantive areas, space and place researchers are familiar with the problems of representing processes that vary in several dimensions and thereby call for nuanced specificity combined with generalization. Critics of spatial modeling question the extent to which phenomena can be represented computationally, for example, which has arguably led to a greater focus on issues of computational representation and ontology (Pickles 1997; Wright, Goodchild et al. 1997). Over-generalization can also be addressed by research that pays close attention to how the processes under study work, linking field-based observation to the careful representation of the entities and relations involved. While close attention to such issues is commonplace in the grounded research that typifies much place and space-based work, it is less usual in the modeling work that dominates much complexity science.

A related concern is that while complexity is touted as an *interdisciplinary* movement, in some respects it is actually *supradisciplinary* such that complexity generalizations from one field are applied to others sometimes appropriately, but often not. Space and place-based research tends to be simultaneously *supradisciplinary* and *interdisciplinary*, as seen in the large array of topics and subfields marshaled under the banner of space and place-based research and the interdisciplinary makeup of much of this research. Nigel Thrift (2002) notes that a number of disciplines are discovering the importance of space, which is leading to purposely interdisciplinary research. While this breadth can be seen as a weakness with respect to disciplinary coherence and depth of analysis, it can also ease the necessary task of finding the generalities that define complexity research while not losing sight of the particular. This dynamic is seen in how multiple disciplinary perspectives are combined in both spatial and complexity research in the context of human-environment relationships (Janssen 2003).

2.2 Complexity and specificity

Having leveled an accusation of over-generalization, we acknowledge that complexity research is also often highly specific, often in the name of giving ‘insight’ into real world processes at the cost of general conclusions (Judson 1994). Complexity-based models may focus on particularity and contingency to the point where they cannot provide estimates of result robustness without recourse to Monte Carlo simulation or other means to systematically vary initial conditions (Bankes 1993). Agent-based models, for example, can suffer from the ‘the curse of dimensionality' (Anderies 2003); as the number of entities in a simulation grows, or their abilities increase, there are an exponentially larger number of possible system trajectories

Complexity models are often more oriented towards answering 'what if' questions, and less towards 'what is best' prescriptive questions or 'what happens in general' theoretical questions (Kwasnicki 1999). This terrain is not limited to complexity; it is familiar to researchers working with Spatial Decision Support Systems (SDSSs) (Longley and Batty 1996), where a preference for 'what-if', scenario generation attests to the difficulty of moving from the particular properties of a specific study to general theoretical conclusions. It also points to how complexity models, like SDSSs, are increasingly used to address semi-structured problems whose nature prevents the application of simpler methods.

Geographers have long puzzled over the conundrum of generalizing from particular instances to broader theories or hypotheses (Harvey 1969: 101). In partial answer to our question "Does complexity theory operate at too general a level to enhance understanding?", we would suggest that complexity—when well grounded in substantive space and place-based research—provides a new way to address the problem by focusing attention on the importance of scale to generalization and specialization. Instead of defending the primacy of a particular scale, complexity offers new ways to approach the geographical project of understanding scale, both as a framework for analysis and as a theoretical focus in its own right. Spatial science has long used scale concepts such as rank-size rules or stream-ordering, but complexity science brings new explanations to their interpretation.

In return, studies of space and place can ground complexity science by providing scale concepts and a diverse range of substantive topics for research. While it is important to develop abstract models with which to explore the implications of space (e.g., Sheppard and Barnes

1990), the larger claims of complexity science are very large indeed, and free-floating generalities such as emergence and path dependence are likely to be more useful when brought (literally) down to earth in the spatial sciences. Research on earth surface systems, for example, has shed light on the importance of geographic scale to using complexity concepts (Phillips 1999). Related to this movement across scales is the need to ground theory and model development in detailed place-based studies of people, institutions, and events. However difficult it may be to generalize from the particular, anticipating the particular from the general is harder still. No amount of abstract theorizing can replace well-founded empirical investigation of phenomena in real world settings.

3 What are ontological and epistemological implications of complexity?

Aggregate complexity, with which we are principally concerned, makes few restrictive assumptions about how the world is (in simple terms, its ontology), at the same time as it assumes a great deal about how we can learn about the world (also in simple terms, its epistemology). Underlying aggregate complexity is a limited ontology of entities and the relations among them. While the ontological claims of complexity science are limited, its implicit epistemological claims are grandiose (O'Sullivan 2004). In particular, widespread reliance on simulation modeling as a means of understanding is problematic, an issue that we also consider in this section.

3.1 Complex ontology

Complexity does not posit an all encompassing ontology in that it focuses on entities and the relations among them, a premise that directs attention onto the kinds and strengths of relationships in a system, a focus shared with such divergent schools of thought as actor-network theory (Law 1992) and critical realism (Harvey and Reed 1996). This breadth of perspective in complexity allows the application of ideas from complexity science across the continuum of realist to constructivist approaches to science. Although few complexity theorists pursue a purely constructivist approach, there is growing interest in, for example, complexity-based “postmodern interpretivism” in action research (Reason and Goodwin 1999: 1). Such research takes place against a developing backdrop of ties between complexity theory and constructivist precepts (Cilliers 1998; Henrickson and McKelvey 2002). Complexity as such is therefore not antithetical to either realism or constructionism, since collections of atomic elements may be either ontologically ‘real’ or not.

Place and space-based researchers are equally wide-ranging when seen from an ontological perspective, and have long probed ontological and epistemological movements, anchoring them in concrete examples. The pages of this journal and its sister journals alone highlight how space and place are vehicles for the ontological and epistemological imperatives of structuralism (Storper 1987), feminism (Pratt and Hanson 1988), postmodernism (Dear 1986), discursive and constructionist practices (Philo 1992), and the ontological dimensions of geographic information science (Flowerdew 1998).

Complexity science is broad-minded (permissive, even) in the sense that the entities in question might be anything (including aggregates of other things), and so might the relations be of almost any kind. This said, for any given area of study, certain kinds of relationships are more common, important, or necessary than others. As noted above, complexity ‘templates’ are often used to forward explanations of phenomenon that seemingly ignore well-understood mechanisms. The general aspects of complexity do not relieve the need to address specifics.

Another way in which specificity matters is the key claim that aggregate complexity ontology—defined by entities and their relationships—is holistic, in contrast to reductionist science. However, researchers walk a fine line between holism and reductionism because the claim to holism relies heavily on the notion of emergence, which is invoked to account for the fact that the capacities of a complex system are greater than the sum of its constituent parts. Given that a complex system is defined in part as "one whose properties are not fully explained by an understanding of its components" (Gallagher and Appenzeller 1999: 78), emergence clearly occupies a central place in complexity science. Emergence results from synergism, whereby system-wide characteristics are not a simple superposition of the additive effects of system components. For example, economic phenomena such as stock bubbles and investor herd behavior commonly seen as irrational may be intrinsic and emergent results of rational, local, interactions (Andreoni and Miller 1995).

Having to explain holistic emergence, however, forces the complexity researcher to define lower level entities and their behavior, which can be seen as a reductionist practice. Reflexivity further complicates the concept of emergence, particularly in the social realm. Although social norms, for instance, may be modeled as emerging from agent interaction (Ostrom 1999) the reverse is also true, and emergent norms affect agents. Definitions of emergence typically hold that lower level elements are unaware of their role in emergent phenomena (Forrest 1990) or that emergence is not analytically predictable from the attributes of internal components or apparent at lower levels (Crutchfield 1994). However, this picture is inadequate for human-systems possessing reflexivity because "people are routinely capable of detecting, reasoning about and acting on the macro-level properties (the emergent features) of the societies of which they form part." (Gilbert 1995: 151). Agent-based modeling of resource institutions, for example, sheds light on how small-scale, local interactions scale up to larger-scale artifacts and how larger-scale phenomena feed back onto the local, as seen in how common-property institutions emerge from local interactions and then in turn constrain these interactions (Bousquet, Bakam et al. 1998).

An understanding of the status of emergence as a concept is vital to theoretical progress in applying ideas from complexity science, particularly to the social sciences. Writing about the individualism implicit in agent-based social simulation, Keith Sawyer argues that an exploration of emergent entities, as more than aggregate patterns in the states of lower-level entities, is critical. Elsewhere, noting that “emergence is a slippery concept” (2002: 579), he suggests that supervenience is a more useful one, less laden by the multiple competing characteristics granted to a loosely defined emergence. Supervenience is a more workaday notion than emergence, simply claiming that higher level states of a system are supervenient on collections of micro-states of the elements that constitute the system, so that there can be no changes in the properties of a system without there being changes in its underlying components. Further, the various collections of micro-states that combine to produce the same or similar macro-states may be very different from one another (a concept labeled ‘wild disjunction’). This makes for an approach to the relationship between micro- and macro- system states that depends less on an implied (and rather mysterious) ‘moment’ of emergence.

3.2 Complex epistemology

It is necessary to consider the epistemology of complexity to understand better the relationships between complexity ontology, emergence, and the balance between holism and reductionism. As noted above, complexity has a relatively open ontology that supports a strand of constructivist work drawing on complexity chiefly as a source of metaphor or analogy. The bulk of complexity research, however, relies on computational modeling that entails a worldview more in keeping with realism. Complexity research may be therefore considered through the philosophical lens of the "semantic conception" whereby models intermediate the relationship between reality and theory (Henrickson and McKelvey 2002). Real world variables and experience are criteria against which models may be measured (the realist view) but not at the cost of denying the power of personal interpretation and social construction (tending towards the constructivist/relativist view).

'Modeling' in the context of complexity science refers to many methods. In the realm of aggregate complexity—the focus of this paper—modeling methods are commonplace as a means of understanding interactions among large numbers of entities. Among the methods most closely associated with complexity are evolutionary methods (e.g., genetic algorithms or classifier systems), biological analogs (e.g., artificial neural nets or artificial life), cellular models) such as random Markov fields or cellular automata), and agent-based models. More detailed discussion of these methods is beyond the scope of this paper. In geography, many of these methods have been applied under the heading of 'geocomputation' (Longley, Brooks et al. 1998).

Computational modeling is the dominant epistemology of complexity science largely because of the need to focus on entities and their relationships, especially with respect to emergence. Brian Arthur (1994) argues that use of such models is essential because they allow researchers to understand how emergent phenomena result from the interactions of many entities. This contrasts with methods that simplify systems by reducing them to weakly interacting aggregated components, or 'variables' (e.g., via statistical assumptions). Principles of superposition, equilibrium, and linearity are useful because they simplify problems by providing tractable models and clear indications of causality necessary for theory building. These principles have a drawback however, in that the implicit assumptions of seemingly prosaic methods, such as the enforced choice of independent and dependent variables in regression, actively guide the kinds of questions that can be asked.

In contrast, the explicit model-focused epistemology of complexity theory relies on a heightened awareness of the role of models and of the possibility of a variety of modeling approaches. Guided by an underlying ontology of entities and their relationships, complexity research turns us away, for example, from reductionist aggregation and simplification of the characteristics or behavior of entities within a system and towards representing individual entities and their relationships with few *a priori* assumptions of how these should be represented. Seen in this way, computational models are an increasingly attractive alternative to simply reasoning about the implications of the nature of relationship between theoretical and observable entities (compare Andrew Sayer's (1995: 29) suggestion of the use of thought experiments). In a computational model, we can represent and simulate the numerous, often heterogeneous, discrete entities and their relationships that define aggregate complex systems. A strong focus of economic complexity work, for example, is how modeled system behavior changes when a population of entities is considered as a system of interacting diverse agents instead of a statistical aggregation of broadly similar agents (Tesfatsion 2001). While such work is not (typically) intended to supplant orthodox economic theory-based in equilibrium mathematics, it does attempt to challenge the assumptions of economic orthodoxy to identify contexts in which interactions among individual actors cannot be assumed away. It similarly opens up to scrutiny the corollaries of perfect rationality, such as perfect information or optimizing, and examines alternatives such as bounded rationality that are contingent on local interactions among entities.

The importance of computational models highlights how complexity-based research is rife with assumptions and simplifications that are essentially reductionist. The tension between holism and reductionism, particularly in the context of emergence, can be avoided by owning up to the necessarily reductionist preliminaries in any computational modeling effort. Two points can be made here. First, there is nothing wrong with determining the scope of a model, while at the same time deciding which elements in the real-world setting will be represented and in what way. This is exactly what modeling or any approach to theorizing is! David Rapport (1991) notes that defining the boundaries and components of a system is a perennial challenge, especially when dealing with models It is hard to see how we could proceed otherwise, since modeling at the level of quarks or whatever is hardly desirable (Goldenfeld and Kadanoff 1999). Second, where complexity-based models improve on many earlier efforts is in the open-ended attempt to see what happens when the atomic, and higher-level entities identified as relevant to the model interact.

Place and space-based research presents ample opportunity to fold space and time into substantive areas in a manner that frees research from being trapped at a given level of analysis. This is particularly useful when addressing emergence, which tends to act at multiple spatial, temporal, and societal scales. This is not to say that place and space is necessary to this research, but it simultaneously challenges accepted scale levels and opens new ones to analysis. Agent-based models and cellular automata models of land use have been the subject of research since the 1980s, for example (Couclelis 1985), and this work explicitly addresses the spatial, temporal, and institutional scales that define complexity in land use (Agarwal, Green et al. 2002).

4 What are the challenges in modeling complexity?

Computational modeling is an essential epistemological component of complexity science given the need to represent the entities and relationships that define complex systems and outcomes such as emergence. Computational simulation techniques are to a large extent driving complexity research through “exploratory simulation” of a host of complex systems (Conte and Gilbert 1995: 4). Complexity-based simulation is increasingly accepted as a way to assess possible system outcomes that does not force preordained or deterministic system behaviors (Thrift 1999). While complexity researchers have chosen computational modeling as a tool of choice, in some ways the models created complexity research; the rise of complexity theory is due in no small part to broad availability of increasingly powerful computers, programming languages, and frameworks, such as object-oriented modeling (O'Sullivan 2004).

Complexity modeling faces a number of challenges related to the sheer number of complexity-based modeling methods and the fact that they have only recently been accepted (or at least acknowledged) on a broad scale in most disciplines. Just as an examination of complexity modeling is beyond the scope of this paper, it is not our intent here to address the bulk of these challenges given that they are often particular to given methods. Two issues, the linkage of pattern to process, and model verification and validation, are important to most complexity applications and are germane to the discussions above on generalization and epistemology.

4.1 Pattern and process

As has been noted, generalization is essential to complexity research, particularly through use of archetypical templates that focus on complex patterns—such as fractals, scale invariance, strange attractors—and complex processes such as unpredictability, emergence, self organization. Many of these general patterns and processes are ill defined, however, or more charitably, broadly defined to preserve their generality. This breadth is problematic given the importance of computational modeling to complexity's epistemological underpinnings. This combination of generality and modeling can result in a tendency to "watch the pot boil" (Holland 1992: 185) when running a computational model, sifting through patterns in model results without *a priori* expectations as to what they may be. Such a 'let's see what happens' approach is clearly at odds with theory-led deductive science, although not necessarily at odds with a more inductive orientation.

More importantly, sifting through patterns tends to privilege pattern over process, so that patterns become the chief object of study at the expense of probing underlying processes. The concept of scale invariance, for example, is applied throughout complexity research, whereby a single process or set of processes operates to produce patterns that are identical or similar across spatial or temporal scales. Identifying scale invariant phenomena involves applying some measure—frequency spectra, power laws, fractal dimension—across a series of observational scales in order to trace regularities in patterns across scales. There are myriad examples in place and space-based research. Measures such as fractal dimension, rank-size, or Zipf distributions are invoked to describe scale invariance in natural and social systems ranging from coastlines to urban agglomerations (Mandelbrot 1982; White and Engelen 1993; Batty and Longley 1994; Axtell 2001; Stanley, Amaral et al. 2002; Turcotte and Rundle 2002). If one measure uncovers similar patterns over a range of scales, it may be assumed that the processes giving rise to the patterns are operating across scales, so that understanding the processes at one scale is equivalent to understanding them at other scales.

The key difficulty related to this conflation of pattern and process is that archetypical hallmarks of complexity arise from different processes. Fractals, for instance, are associated with both self-organization and deterministic chaos, which are very different manifestations of complexity. Urban land use is seen as having a fractal pattern but convincingly linking this observation to processes that account for the vagaries of socioeconomic and cultural forces remains a challenge (White and Engelen 1993; Batty and Longley 1994). Even if the type of process at work is known in general terms, the domain-specific particulars of the entities and relationships within the system may remain unknown. This is the equifinality problem, where many processes (or models) can give rise to the same (or very similar) patterns, so that the relationship from process to pattern is many to one. In fact, many patterns can arise from any sufficiently complex process, so the situation is worse even than this, and there is no unique relationship *in general* between patterns and processes. At the same time, a strength of complexity is the ability to link generalized patterns and processes.

These issues speak to those raised above about over-generalization and the willingness with which researchers apply generalized complexity concepts to specific cases while ignoring knowledge about the specific case. As above, the answers to the challenges of pattern and process in computational modeling of complexity are suggested by the nature of place and space-based research. While the common language of complexity science may enable analogies and insight across disciplinary boundaries, considerable caution is required. Jonathan Phillips (1999) explicitly calls for a reinvention and re-application of appropriate ideas from non-linear science in physical geography, and we would make the same call in the realm of human and human-environment interaction in spatial science. A fuller understanding of the relationship between pattern and process is required, and seems most likely to be arrived at by relatively abstract modeling of spatial systems combined with considerable empirical grounding.

Place and space-based research supports a host of theoretical and methodological approaches with a growing number of empirical research opportunities, especially those offered by combining qualitative place-based research and quantitative geospatial technologies such as remote sensing, global positioning systems, and geographic information science. Modeling the complex, multifaceted nature of land use increasingly requires intentional shuttling between theory and empirical data that ranges from remotely sensed imagery to qualitative in-depth interviews (e.g., Bousquet, Bakam et al. 1998). The wealth of data and theories pertaining to land use in various settings has helped spatial scientists drive the complexity research agenda by addressing issues such as in linking pattern to process and defining emergence. At the same time, complexity theory has informed much of the land-use research in the last decade, as seen in the wholesale inclusion of complexity concepts such as self organization and methods such as cellular automata (Parker, Manson et al. 2003).

4.2 Truth and validation

Linking pattern to process is an important aspect of model validation. The difficulties of validating complex spatial models are manifold, and include the spatiotemporal nature of complex models of space and place and the amount of data produced. Expressly complexity-based models also carry with them the expectation that they should exhibit behavior such as emergence, sensitivity to initial conditions, and self-organized criticality (Manson 2003). These behaviors are at odds with many standard methods, requiring the construction of new validation tools (Miller 1998).

Consider the case of validating emergent outcomes. Researchers find themselves with a knotty problem: is it only unanticipated emergent phenomena that may be considered properly emergent, or may we also deem as emergent phenomena that we anticipated? The reverse may also be asked: should truly unexpected outcomes be dismissed as artifacts because they are not accommodated by the theoretical framework being used? On James Gleick's account of Edward Lorenz's work, for example, this is how chaos was 'discovered'—a slow realization that model 'artifacts' were real in some sense (1987). These questions also relate to combination of generality and modeling and the importance of *a priori* expectations of model results—how do we expect the unexpected?

The answer to these questions seems to be validation through triangulation, the application of several viewpoints and shuttling between theory and empirical observations. As noted above, place and space-based researchers anchor a range of theories with examples across multiple spatial and temporal scales. Studies of space and place also host a diversity of approaches that may prove useful to strengthening the interdisciplinary nature of complexity. Craig Nicolson and others (2002) note that one of the chief subjects of interdisciplinary research is complex human-environment systems, and these are also arguably a key focus of place and space-based research. In this vein, models more attentive to the detail derived from fieldwork can demand attention from a broader community of scholars and users. In this regard the suggestion that it is appropriate to validate models in more narrative-oriented and qualitative ways is relevant and in keeping with the use of these qualitative methods for tying pattern to process.

Also necessary to validating emergent outcomes is a frame of mind that actively pursues 'imaginable surprise'—behavior or outcomes that on the surface are novel or unexpected, but which, in retrospect, are within the bounds of the knowable. Broadening the scope of the imaginable requires pushing interdisciplinarity to the point of conducting synthetic research on the edges of current domains and across scales of inquiry. This broadened focus is driven by the realization that surprise can result from not appreciating the connectivity and complexity of a given system—sometimes it requires perspectives from outside of an accepted epistemic framework. Global environmental change research, for example, offers a good deal of commentary on the nature of complexity, its relationship to uncertainty and surprise, and the need for multiple perspectives (Schneider, Turner et al. 1998).

Despite the availability of various strategies for addressing the challenges of validating complex models, Naomi Oreskes and others (1994) argue strongly that model validation in the sense of establishing the truth of a model as a representation of reality, is impossible. In a later paper, Oreskes argues that only systems that are observable and measurable, that have constant structure over time, whose behavior is constant as unparameterized conditions vary, and about which sufficient data can be gathered could ever conceivably be validated in this sense (Oreskes 1998). In the context of complex spatial models, we would add a requirement that systems be invariant across space, so that results for one location might be extrapolated to others. She goes on to argue that such conditions are never satisfied by complex systems—this is arguably what makes them complex!—so that the very idea of a model that can be validated is untenable. The much more modest aim of model *evaluation* is presented as an alternative, obliging the modeler, particularly when speaking in the policy domain, to find accessible ways of expressing the capabilities and limitations of models.

In sum, models are integral to the current practice of complexity science and important to its epistemological underpinnings. It remains, however, to meet the challenges of linking pattern and process and of developing approaches to validating complexity-based models. It is apparent in considering these issues that complexity research must be open to ‘other ways of knowing,’ whereby evaluation and validation is as likely to be narrative-based and political in nature as it is to be technical and quantitative. Again, the global environmental change research community provides a prototype for use of models in complexity science. This community has moved from seeing global change as an exercise in modeling chemistry and physics towards a broader endeavor where ‘trans-scientific’ aspects intervene. These range from dealing with high risk uncertainties in outcomes by involving a broader range of stakeholders earlier in the scientific process to confronting political and institutional dimensions of knowledge production (Jasanoff and Wynne 1998).

5 Conclusion

Our consideration of some fundamental questions draws attention to connections between features of the interdisciplinary terrain of complexity science and longstanding concerns in the spatial sciences. In some areas, the spatial sciences can give the lead to complexity studies more broadly; in others, spatial scientists have much to learn from an encounter with complexity studies. There are points of contact in all three areas considered here—whether complexity theory is too specific or general, its ontological and epistemological implications, and its relationship with computational modeling.

First, we have identified an urgent need to address the question of appropriate levels of generalization and specificity in complexity-based research. This need arises from the tendency of such research to be pitched at one or the other extreme, at the cost of offering generalized explanations belied by closer examination on the one hand, or of focusing on the specifics of a single case study in a way that inhibits broader explanation on the other. Here, space and place-based studies are exemplary because they highlight the importance of connecting generalized models more closely to detailed field-based research. This calls for concerted efforts to relate diverse methodological approaches, a requirement familiar in place and space-based research, which is nothing if not methodologically varied. As noted for the case of land-use modeling, field-based research can assist the development of models and other abstract conceptual tools by tying abstractions to concrete examples. Furthermore, questions of generalization and specificity are intimately related to spatial, temporal, and institutional scale and the need, so often identified in place and space-based research, to articulate the role of scale in analysis.

Second, our examination of the tensions in the ontology and epistemology of complexity science reminds us of important issues for geography and spatial science. A permissive ontology of ‘things’ and ‘relations’ emphasizes the importance of concepts and models well grounded in both theory and observation. An emphasis on emergence is appropriate, provided that it focuses attention on an epistemology of simulation as a means of ‘seeing what happens’ when theoretical entities and relationships interact, rather than on a mystical moment in the evolution of a system. Accounting for emergence raises questions of reductionism versus holism, which is also in keeping with a concern in spatial science to understand how local phenomena scale up to regional and global scales. On the other hand, the spatial sciences offer to complexity science more broadly a more consistent focus on *downward* effects from larger scale global forces and effects back on to local elements to augment the general focus on bottom-up constitution of larger scale entities.

Third, the importance of computational modeling to complexity highlights the need to address issues of conflating pattern with process and questions about validating computational models. These issues are related to those of generalization and specialization and the challenges of complex ontology and epistemology. Not surprisingly, then, similar solutions exist for these issues. The problems encountered in linking pattern and process can be dealt with by tying general complexity concepts to specific cases and, importantly, attending to current research in a given field. The same remedy applies to issues raised by validating complex models, with the additional caveat that an expressly interdisciplinary perspective may be required to enable triangulation among methods. At the same time, less abstract models that were more attentive to the detail derived from fieldwork would demand attention from a broader community of scholars and users. In this regard, the suggestion that it is appropriate to verify models in more narrative-oriented and qualitative ways becomes relevant.

Stepping back, our questions have taken us from whether complexity theory is too specific or general, through some ontological and epistemological implications, and on to its relationships with computational modeling. While we only examine a few issues directly related to modeling per se, these provide a useful point of departure for our final thoughts. The widespread use of models in complexity science has renewed debates in science more broadly about the appropriate use of models for developing knowledge and making predictions. The imaginative notion of simulation models as alternative or “would-be” worlds (Casti 1997) where we can watch the implications of a theoretical idea unfold is a stimulating one. Simulation models are used for more than developing ideas and furthering knowledge of how the world works, however: they are entangled in the messy worlds of politics and policy development. The lesson we draw from this is the urgent necessity for ways of describing, explaining, and learning from models that are more open about the limitations of simulations, and more accessible to a broader audience of stakeholders in the spatial decisions that models ‘support’. This will require model developers to focus less on obscure technical minutiae, and to pay more attention to broader societal concerns.

Place and space-based research has long spoken to these concerns through an ongoing engagement with action research, planning, and policy debate that dovetails with the larger theoretical debates that ultimately govern the nature of research and its eventual application on the ground. The sense of responsibility that ultimately drives this engagement augments those other strengths of place and space-based research that we have identified, which include introducing to complexity research a greater willingness to grapple with myriad ontological and epistemological threads, a desire to tie together theory, models, and data across substantive areas, an emphasis on interdisciplinary research, and the need to incorporate place, space, and time as components of complex systems.

In sum, this is an exciting and critical time to contribute to the ongoing engagement between complexity and place and space-based research. Complexity is beginning to move beyond its initial, almost starry-eyed, exuberance, and towards established practice and principles. This transition is one where place and space-based research can provide an example. While space and place-based research is not a single coherent body of work, this is a field that recognizes the legitimacy of both quantitative spatial science and qualitative and sociotheoretic approaches. The robustness of complexity and place and space-based research gives both the latitude to risk pushing methodological, theoretical, and disciplinary boundaries, so that novel, relevant, and intellectually exciting approaches can be developed and new knowledge uncovered.

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