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Special Editors' Introduction: What Is Complexity Science? A View from Different Directions

The "field" of complexity science is a popular stream of thought that brings together a diverse range of apparently disparate disciplines within contemporary science. It is currently all the rage in management science. Although a number of conferences have been held to explore the potential of complexity science within the realms of management and organization science, there still appears to be little agreement on what complexity science actually *is*. A study of the "complexity" literature confirms that a number of schools of thought are developing, and that they differ substantially. It is the aim of this special issue to investigate this state of affairs. Since this problem is a complex one in itself, it is probably not possible to come up with a final solution—and here we may already be at odds with some complexity scientists! We hope at least to stimulate the debate around the usefulness and limits of the study of complexity.

REDUCTIONISTIC COMPLEXITY SCIENCE

There are at least three themes, or communities, that characterize the research effort directed to the investigation of complex systems: a hard one, a soft one, and something in between. The first is strongly allied to the quest for a theory of everything (TOE) in physics, i.e., an acontextual explanation for the existence of everything. This community seeks to uncover the general principles of complex systems, likened to the fundamental field equations of physics. Any such TOE, however, may well be of limited value; it certainly will not provide the answers to all our questions. If, indeed, such fundamental principles do exist, they will likely be so abstract as to render them practically useless in the world of organizational management—a decision maker might need a PhD in physics in order to make the simplest of decisions. We might refer to this version as *reductionistic complexity science*, as it mimics the aim of the physical sciences in trying to reduce the wide richness of reality to a handful of powerful, all-embracing algebraic expressions. The theory of bureaucracy was once regarded as *the* theory of management. Modern times have adequately demonstrated the considerable flaws in this theory, but complexity management scientists (of the "reductionistic" variety) still pin their hopes on the discovery of another such all-embracing theory.

This school of complexity science seems to be based on a seductive syllogism (Horgan, 1995):

> Premise 1: There are simple sets of mathematical rules that when followed by a computer give rise to extremely complicated patterns.
> Premise 2: The world also contains many extremely complicated patterns.
> Conclusion: Simple rules underlie many extremely complicated phenomena in the world, and with the help of powerful computers, scientists can root those rules out.

Although this syllogism was brilliantly refuted in a paper by philosopher Naomi Oreskes and her colleagues, in which she warned that "verification and validation of numerical models of natural systems is impossible," (Oreskes *et al.*, 1994) the reductionist position is still common in complexity science.

Soft complexity science

Within the organizational science community, complexity has not only been seen as a route to a possible theory of organization, but also as a powerful metaphorical tool. According to this school, complexity thought, with its associated language, provides a powerful lens through which to "see" organizations. Concepts such as connectivity, edge of chaos, far from equilibrium, dissipative structures, emergence, epi-static coupling, co-evolving landscapes, etc. facilitate organizational scientists and the practicing manager in "seeing" the complexity inherent in sociotechnical organizations. The underlying belief is that the social world is intrinsically different from the natural world, being constituted through language and meaning—the antithesis of the naturalist view. As such, the theories of complexity, which have been developed through the examination of primarily natural systems, are not directly applicable to social systems (at least, not to the practical administration of such systems), although their language may trigger some relevant insights into the behavior of the social world. We might refer to this version as *soft complexity science*. This line of thinking seems to support the popular use of metaphor within the managerial sciences. As Phelan laments in his article "What is complexity science, *really*?", this has led to an increased use of metaphor for metaphor's sake while forgoing the rigorous scientific process that is believed to ensure validity. Phelan calls this "pseudo-science": it sounds like science but it isn't. For example, the application of quantum theory to organizations is rather popular (a result of "physics envy" being taken to the extreme). Far be it for us to suggest that there is no value in the import of quantum mechanics to organization science, but supporters of such a position might bear in mind that when the infamous physicist Erwin Schrödinger developed his famous "cat" thought experiment (Schrödinger, 1935), it was to demonstrate the absurdity of applying quantum thinking to the macroscopic human world (Kaku, 1994).

Given that metaphor, and language in general, does indeed shape our perceptions of the world and therefore our culture, it seems reasonable to assume that the introduction and use of a new complexity-sensitive language would facilitate cultural change. This is a rather naïve view of language, however. To derive meaning(s) from a particular word, a context or frame within which to make sense is necessary. A new language with an old context will not work, although a new language should facilitate a review of existing contextual boundaries.

Complexity Thinking

The two previous conceptions of complexity science, particularly the reductionist school, promise neat packages of knowledge and a universal language that can apparently be easily transferred between any context (the belief in such transferability is why managers run to consultants for answers so often). A quite different line of thinking focuses on the epistemological consequences of assuming the ubiquity of complexity. This view considers the limits of our knowledge in the light of complexity, limits that are often trivialized by contemporary scientific thinking. We might refer to this school simply as *complexity thinking*. Of the three themes mentioned, this is possibly the least well represented in the widespread complexity literature. It involves a shift in philosophical attitude that might well put off practicing managers. According to this school, if one assumes that organizations are indeed complex systems (as the wealth of popular management literature would suggest), a fundamental shift in the way sense is made of our surroundings is necessary: the limited and provisional nature of all understanding has to be recognized.

Is there an agreed definition of complexity?

The identification of three different, though intertwined, schools of thought in "complexity" conveys a neatness that is really rather illusory. Let us consider the question that this special issue attempts to answer: "What is complexity science?" This may seem to be a straightforward question requiring a straightforward answer, but if we examine the definitions of the words that form the question, we find that it is a very muddled question indeed. We will assume that most people share a similar reading of the words "what" and "is," even if you find them hard to define. When we come to the word "complexity," matters are much more tricky. Even if we turn to science, where words are carefully defined and have very precise meanings, we find that an agreed definition of "complexity" is not available. John Horgan, in his essay "From complexity to perplexity" (1995), reports that according to a (now out-of-date) list compiled by the physicist Seth Lloyd, there are at least 31 proposed definitions of complexity, a sample of which includes descriptive, generative, computational, constitutional, taxonomical, organizational, hierarchical, operational, nomic, gnosiological, semiotic, semantic, algorithmic, informational, combinatorial, logical, functional, structural, entropic, cyclomatic, topologic, harmonic, syntactic, perceptional, and diagnostic.

It is not the place of this special issue to present definitions of these different conceptions of complexity; a quick internet search will result in an adequate definition in most cases. Most of the concepts involve some association with other equally slippery terms, such as entropy, randomness, and information. The most useful definition that we personally have found refers to the definition of a complex system, rather than complexity metrics, which is simply given as *a system that is comprised of a large number of entities that display a high level of nonlinear interactivity*. By examining such systems, through what we have called the "reductionistic complexity science" lens, a number of important "laws" have been uncovered. Possibly the most important "law" associated with such systems, in light of the project to develop the Theory of Complexity, is that these systems are incompressible, i.e., any description claiming completeness must be as complex as the system itself. This seems to fly in the face of the process of doing science, possibly undermining the search for such complete descriptions altogether. Whether or not the observed phenomena of complex systems prevent a complete scientific explanation depends, of course, on what one defines as "scientific." Also, given that the legitimacy of much knowledge is determined by how scientific it appears to be, such a definition seems to be all the more important. For example, in an editor's note to a short review article by Corning (1998) the following statement is made: "Until the 'complexity science' researchers can develop a formal notation in symbols and syntax, while at the same time respecting its subjective nature [sic], it will not really be a 'science.'"

An agreed definition of "science" is as elusive as an agreed definition of what is "complex." The complete (20 volume) *Oxford English Dictionary* offers this: "In modern use, often treated as synonymous with 'Natural and Physical Science', and thus restricted to those branches of study that relate to the phenomena of the material universe and their laws, sometimes with implied exclusion of pure mathematics. This is now the dominant sense in ordinary use." Not very helpful. In Phelan's article, a sort of definition of science is presented based on Thagard's demarcation criteria between science and pseudo-science. Science is characterized by the following:

- Uses correlation thinking (e.g., *A* regularly follows *B* in controlled experiments).
- Seeks empirical confirmations and disconfirmations.
- Practitioners care about evaluating theories in relation to alternative theories.
- Uses highly consilient (i.e., explains many facts) and simple theories.
- Progresses over time: develops new theories that explain new facts.

It is not too difficult to show that complex systems can resist such an approach to science. We will not delve too deeply into the depths of attempting to define science. For the layperson, a naïve description of science might be: science is the activity performed by scientists who endeavor to find absolute "truths" about the world around us, extending to cover the entire universe. Scientists believe that there is an absolute reality, a reality that exists regardless of the existence of observers such as ourselves (which is rather interesting, given that the Copenhagen

Interpretation of quantum mechanics, science's jewel in the crown, leads to a set of astonishing contradictions—usually presented as *paradoxes*, as that term seems less absurd—which lead to the negation of both causality and realism[1]). Scientists also believe that "laws of nature" exist (laws that are usually expressed mathematically) and that these can be discovered through the use of the scientific method(s). These methods are supposed to allow scientists to escape the subjective nature of human experience and open up the world of objective analysis. The governing principle of science is reductionism: the belief that parts of the whole can be isolated and examined in order to understand their behavior, and that the whole can then be understood by summing the parts—"the whole is equal to the sum of its parts," if you like.

In terms of the question of this special issue, a science of complexity of this kind would comprise of a body of knowledge, in the form of mathematical expressions that absolutely describe the behaviors displayed by such complex systems. And, given the widespread belief that just about everything in the natural world can be generalized to a complex system, a "science of complexity" would provide us with the knowledge to control just about everything. There is increasing evidence that if such a strict and formal definition of science is maintained, there will be no "science" of complexity. Maybe our growing knowledge of complex systems undermines such a strict understanding of science. Phelan goes to lengths to discern science from pseudo-science, implying that reductionistic complexity science is real science and soft complexity science (the widespread use of metaphor) is pseudo-science. The term "pseudo-science" suggests amateurishness and sloppy research; to suggest that much of the interesting work on complexity might be classified as such is a strong move. One could perhaps develop an understanding of science that is neither so hard as to be brittle, nor so soft that it is pseudo.

What counts as scientific?

One of the motivations for asking the question "What is complexity science?" was, therefore, to prompt further reflection on this problem concerning the scientific status of work done on complexity. There are those who believe that the prevailing scientific dogmas should be upheld and that, if they are not, the resulting body of understanding cannot be regarded as scientific and is therefore of little value. Supporters of this perspective would undoubtedly argue that much of the work performed under the umbrella of complexity studies (particularly in the field of organization science) is merely metaphor, not science at all. On the other hand, there are those who warn that a too traditional scientific approach will prevent us from coming to grips with complexity. The problem of determining what is science and what is not has been dealt with in different ways by numerous authors.

One way of approaching the problem is to work with the modern/postmodern distinction. Byrne, for example, is critical of the work done by Allen and his group because he thinks they are looking for the "master equation" (read "single coherent meta-narrative" and you have Lyotard's classical definition of the modernist quest). Phelan and Morçöl, on the other hand, argue that the postmodern position cannot produce a proper epistemology and therefore cannot defend itself against the accusation of relativism. In a way, the modern/postmodern distinction has run its course and perhaps does not serve us well any more. One of the problems is the many meanings of the notion "postmodern." The term is used as equivalents for relativism, postpositivism, poststructuralism, as an insult, as a compliment, as an escape, as a marketing gimmick, and often to denote something as antiscientific. There is no way to clear up this confusion, since the term has (quite naturally) been appropriated in different ways by different groups. This was perhaps the one thing proven by the Sokal affair (the other being that there is much academic boorishness on all sides). The long and the short of it is that we cannot use the notion "postmodern" to stand for something specific. To equate, for example, postmodern and poststructural (as Morçöl does) is just too sweeping to be useful.

Dropping the notion "postmodern," however, cannot mean that we should return to a traditional (positivist) understanding of what it is to be scientific. That would return us, among other things, to the battered old debate of the scientific status of the human as opposed to the natural sciences. It is one of the real virtues of complexity science that it bridges the old divide between the two worlds without privileging the one above the other. Thus, we can have a discussion on complexity in the social sciences (like that by Medd) without dropping scientific rigor (something Phelan feels strongly about). Such an approach is one of the real virtues of the work done by Peter Allen and the Cranfield group, for example. Insights from the human sciences on the one hand, and natural sciences on the other, should not be set against each other, nor should they be assimilated too easily. They should be used to *challenge* each other. We should not be allowed the comfort of merely sitting within the well-known domain of our own disciplines, we should be forced to transform them. This means that we should also transform the definition of what counts as being scientific. (And let us not forget that transdisciplinary work is *really* difficult, and rarely done properly.)

How does complexity science contribute to this transformation? This still has to be worked out in detail, but for the moment we can suggest two such contributions. The first follows from the general acceptance that complex matters demand a methodological pluralism. This acceptance is not the result of metaphysical commitments, but merely that of trying to understand complex issues with limited (finite) means. If we allow different methods, we should allow them without granting a higher status to some of them. Thus, we need both mathematical equations and narrative descriptions. Perhaps one is more appropriate than the other under certain circumstances, but one should not be seen as more *scientific* than the other. As long as science has to do with understanding, we will need both word and symbols, both simulations and descriptions. This does not mean that any old story is science; the trusted methods for getting rid of charlatans should be used more than ever. It merely means that a well-constructed philosophical argument furthers our scientific understanding in a similar way that a beautiful equation does.

The second contribution made by complexity science to our conception of what it is to be scientific also follows from the fact of our finitude. Since the nonlinear nature of the interactions in complex systems renders them incompressible, we cannot have perfect descriptions of complex systems that are simpler than the systems themselves. Our descriptions (and our scientific models) reduce the complexity in order to generate understanding. Because of the incompleteness of these descriptions, the emphasis shifts from developing an exact understanding (and *therefore* a scientific understanding) to developing an understanding of the *limits* of our knowledge. This issue is investigated by Allen here and will certainly have to receive much more attention in future. The acceptance of a limited position as the best one possible is, of course, also more humble. This introduces an ethical dimension into the discussion, a subject taken up by Abraham, but perhaps one warranting a special edition on its own.

The articles

Our first article dives head first into this area of knowledge limits. In his paper "What is complexity science? Knowledge of the limits of knowledge," Peter Allen systematically explores the types of knowledge that can be derived from different modeling types. He begins by identifying the five key assumptions that are used in reducing an unmanageably complex reality to a manageable approximation.[2] These assumptions, when made in different combinations, lead to quite different representations, ranging from simple models of equilibrium, allowing complete prediction, to adaptive evolutionary models, which prevent complete knowledge of the future but allow for an incomplete appreciation of the possible futures of the modeled system. This incomplete appreciation must be updated on an on-going basis. Even this limited knowledge is dependent on the existence of relatively stable boundaries: boundaries that appear fixed for a useful period of time. However, in some instances stability is so fleeting that there are no discernible forms delimited by stable boundaries, that the creation of knowledge is impossible. The creation of knowledge depends on the stability of the subject matter; but determining such stability is inherently problematic.

For Allen, the fact that reality is complex means that our knowledge of reality is inherently limited and strongly context dependent. It is suggested that:

> Although this sounds tragic, it is in fact our salvation. It is this very "ignorance" or multiple misunderstandings that generates micro diversity, and leads therefore to exploration and (imperfect) learning.

So, although we can never have complete knowledge of a complex system, we can take advantage of the various alternative representations (including our own subjective models) and synthesize more robust approximations allowing more informed decision making. This robustness is, however, incomplete; knowledge of complex systems is inherently fallible. Nevertheless, the errors made through our "ignorance" or "multiple misunderstandings" fuel the learning process and the creativity required to develop limited (in both space and time) but situation-specific understanding. Knowledge of complex systems is not of the "what is" or "what will be" variety, it is "what might be." This is quite different from the conception of knowledge apparent in the positivistic (mechanical-like) view of reality.

In the next article, "What is complexity science? Toward an 'ecology of ignorance'" contributed by Will Medd, the themes apparent in Allen's article of incomplete knowledge, and the implied pluralism to which this leads, continue. Medd considers the question of what complexity science might be from a social science perspective, viewing it as "emergent and self-organized phenomena" rather than a body of coherent knowledge regarding complex systems; he views complexity science as "a reality that is *done* and *enacted* rather than observed." In discussing this "performance" of complexity science, a distinction is made between two different methodological approaches: the study of complex social orders, and the study of complex social order*ings*. The method of complex social order intends to observe "the forms of social structures in relation to parameters which are characteristic of the system as a whole" (Byrne, 1997: 3). This is a kind of top-down approach to social analysis in which the relationships that emerge between perceived key variables are explored. On the other hand, the study of complex social orderings "emphasizes the interacting relationships between agents," a bottom-up approach synonymous with the agent-based modeling so popular in the "reductionistic complexity science" domain.

Both of these quantitative approaches, Medd suggests, are limited in their potential because neither deals with emergence satisfactorily: "The problem is, therefore, that modeling through quantified variables may leave us ignorant of the emergent dynamics of 'complexity science,' and yet emergence is a central aspect of complex systems, according to the complexity sciences." These limitations are a result of the "hidden determinism" inherent in the two methods discussed, in which a "rather reductionist approach to 'emergence' is developed." This problem illustrates the difference of opinion between what we referred to earlier as the "reductionistic complexity science" community and the "complexity thinking" community, which itself is an exemplary illustration of the different world views of human and physical scientists.

The article concludes with a discussion on the inescapable ignorance associated with claims made about the world. This ignorance results from the impossibility of building a model of the world as complex as itself. A practical conception of complexity science, therefore, should have something to say about how ignorance is dealt with; some "complexologists" implicitly deny the systemic routes of such ignorance, while others place it at the center of their thinking.

David Byrne, in his article "What is complexity science? Thinking as a realist about measurement and cities and arguing for natural history," immediately sets himself outside of the dominant "reductionistic" community by asserting that "complexity science is not an activity directed toward the production of any set of master equations, even of nonlinear master equations, which can describe the essential workings of any complex system." He goes as far as to say that "complexity science has to be decidedly skeptical about simulation."

When most of us use the term "science," we are generally referring to organized knowledge:

> with a specific privileged location of that knowledge in the domain of the physical sciences, a general specification of the appropriate form for obtaining that knowledge through experimental method, and a belief that the optimal form of the presentation of that knowledge is through mathematical forms that describe "laws of nature."

Byrne, however, prefers to consider science as *nauk*, a Slavic word referring to "all systematically organized knowledge regardless of its disciplinary location, the method through which it was obtained, or the form of representation." This is quite different from the traditional positivistic notion of science, but it does accommodate the sort of provisional, limited knowledge indicated in the previous two articles.

Byrne begins his assault on the positivistic (reductionistic) complexity science with a cry of "Death to the Variable!". In this, he is not only denigrating the Platonic view that privileges variables with some assumed ontological status, but also highlights the importance of acknowledging the problematic dimension to constructing appropriate variables given the irreducible nature of complex systems. In taking this position Byrne says:

> The abandonment of the idea that there are bits in a complex system that we can analyze out as variables does not imply that there are no subsets of a complex system, change in which changes the system as a whole. Any prospect of agency in relation to systems depends absolutely on the possibility of first the existence and then the understanding of control parameter subsystems. The big question for any complexity science is can we get at these subsystems—can we in any useful way understand them?

The article then moves on to the development of the integrative method, which, it is suggested, provides the toolkit for a practical "inductive complexity science." The four elemental processes of this method are exploring, classifying, interpreting, and ordering. These processes together facilitate the making sense of complex systems, given the strongly problematic nature of such an analysis. They support such aspects of analysis as the boundary recognition and allocation process, the documenting of qualitative changes in the system's behavior, the problematic nature of the interpretation of both quantitative and qualitative "data," etc. The integrative method "involves an engagement with human subjects as individuals and as collectivities whose agendas and decision spaces must be understood if the nature of potential future trajectories is to be established and if the products of scientific research are to play a part in the determination of those future trajectories." Byrne is careful to point out that his unwillingness to privilege quantity-based analysis of complex systems, such as agent-based simulation, is not to dismiss quantification out of hand, but to regard it as simply another input (possibly a dominant input depending on the situation of interest) into the process of understanding rather than the "ultimate representation of understanding." This is an important point. It is all too easy to polarize the complexity science debate between the natural and social sciences. The integrative method explicated by Byrne in his contribution is a practical synthesis of both natural and social methodologies.

The article concludes with a brief discussion of the consequences of employing such an integrative method for urban studies, which, of course, diminishes the role of computer simulation. "Complexity science is inductive, integrative, engaged, and different"—for David Byrne, anyhow.

The common themes of pluralism and on-going learning that are developing in our first three articles are no less apparent in the article from Ray Cooksey, "What is complexity science? A contextually grounded tapestry of systemic dynamism, paradigm diversity, theoretical eclecticism, and organizational learning." He brings together four different but related general principles in an attempt to produce a coherent complexity science of organizations. The four principles are, as is obvious from the title, systemic dynamism, paradigm diversity, theoretical eclecticism, and individual and organizational learning. It is argued that complexity science is a meta-science that legitimates the inclusion of each of the four listed domains, as well as offering guidance, rather than prescription, in how one goes about tying these four domains together in a given context.

Systemic dynamism acknowledges the perpetual change that complex systems exhibit. It includes those elements that are common to all complex systems, i.e., the conspiracy between stabilizing (negative feedback) and destabilizing (positive feedback) mechanisms to produce a diverse range of qualitatively different futures; the potential for small perturbations to grow causing large-scale qualitative reorganization as well as the potential for the system to absorb large perturbations; the changing relationships between entities as well as the changing characteristics of the entities themselves; the appearance and growth of different populations; the inherently transitory nature of all boundaries; the problematization of context recognition and the need for context specific action; the changing nature of perceived context, and so on.

Given the on-going change exhibited by these systems and the on-going "renegotiation" of boundaries and context, it has been argued that pluralism in whatever form is essential when trying to develop context-relevant understanding of a complex system. It is this to which *paradigm diversity* and its related partner, *theoretical eclecticism*, respond. Together, they encourage the transcendence of dogmatic representations and recognize the value, however little, of understanding regarding the behavior of complex systems from any particular perspective.[3] The underlying presumption is that through such pluralism we can develop a clearer picture of what the context of our actions should be, allowing us to develop relevant "knowledge." Pluralism is a way of mitigating the effects of having only partial knowledge—we cannot have *the* explanation, but we might develop the *best* explanation given the nature of the system of interest *and* our limited exploratory resources.

Even if we are capable of developing context-specific understanding, the context is always changing and so must our understanding. As a result of complexity, however, it is not a trivial matter to move understanding/knowledge from one context to another, however apparently similar the two contexts might appear. This problematizes the learning process. Single-loop learning, in which we simply compare what we expected to happen with what actually happened, is quite challenging given the sometimes invisible (to us) causal processes underlying events. So, principle four, *organizational learning*, focuses on double- and triple-loop as well as single-loop learning. In these multi-loop learning modes the "learner" continually re-evaluates the context within which their interpretation of events is bounded.

After having introduced the reader to these four principles, Cooksey continues to explain how they might be sewn together in a "contextually grounded complexity science tapestry for organizations." In concluding, he expresses some of the same concerns regarding the nature of complexity science as those both implicit and explicit in the previous three articles:

> There are no easy answers or simple solutions, because complexity science, by design, does not pretend to offer them. Instead, it offers diverse avenues for discovering what may end up being a multiplicity of answers that are differentially sensitive to and grounded in specific circumstances, conditions, people, times, and places. Therein lies the real promise of the complexity science tapestry.

The next contribution is quite different in nature from the previous four. It is a discussion piece that considers the philosophy underpinning much of the complexity science research published thus far. Göktuğ Morçöl's article, "What is complexity science? Postmodernist or postpositivist?", responds to the claims of Young and Cilliers that complexity science is a postmodern science. He begins with the assertion that (skeptical) postmodern philosophy rejects scientific, empirical, positivistic methods altogether,[4] but it by no means follows that complexity science is positivistic. According to Morçöl:

> Complexity theory renders the Newtonian notion of universal laws questionable, but it still offers generalizations about natural and social phenomena. Postmodernism abhors totalizations…

Postpositivism is offered as a compromise between the extremes of positivism and radical postmodernism, in which the contextuality of (local) knowledge is recognized as well as the existence of universal principles (scientific knowledge); within the postmodern school itself a compromise also exists that might be referred to as *affirmative* postmodernism. The argument for a postpositivist complexity science is developed in a three-part framework. Morçöl begins with an ontological, epistemological, and methodological examination of both positivist and postmodern philosophy and then draws a comparison with that of complexity science.

In discussing complexity science, Morçöl acknowledges the lack of agreement on the definition of the term "complexity" as well as the basic principles of a theory of complexity. We shall see in Phelan's article that this characteristic of the field of complexity has been used to defend arguments that suggest that complexity science is not a science, but rather a proto-science, or even nonscience. A postpositivist view of complexity science broadly assumes a realist ontology, i.e., that universal laws exist "out there," independent of the mind. A postpositivist view also problematizes the subject/object distinction that is presumed within positivism and denied by extreme postmodernism—this essentially justifies the need for both positivistic and interpretive approaches to understanding complex systems, and allows for limited generalizations.

It is important to acknowledge that complexity science undermines the positivist notion of science, as it allows for a broader analysis of such systems other than those molded by positivist tendencies. Much of the complexity science literature thus far has acknowledged the break from traditional Newtonian/mechanistic thinking, but has persisted with determinedly positivistic methods focusing on universals rather than the idiosyncrasies of different complex systems.

Steven Phelan's paper, "What is complexity science, *really*?", stands in contrast to Morçöl's postpositivist account of complexity science. Rather than look at the available philosophies and match the one that best describes the complexity literature, as Morçöl does, Phelan seeks to clarify what science is and then use this to whittle down the complexity literature to the subgroup that might be safely referred to as "science"—the implication being that the rest of the literature is either pseudo-science or nonsense. Given that science is widely regarded, rightly or wrongly, as the "supreme arbiter of truth, objectivity, and rationality," it is important to understand what differentiates scientific knowledge from other types of knowledge, since if some particular knowledge can be labeled "scientific," the tendency is for "customers" to hold that knowledge in higher regard than "other" knowledge. Misguided or not, this is often the way knowledge is sorted between what is legitimate and what is not. This privileged position of science can sometimes lead to fraudulent use of the term "science." Phelan suggests that

> there is always a temptation to claim "scientific status" to inflate the credibility of one's work. While Park (2000) suggests that pseudo-science practices often arise from genuine misunderstandings or self-delusion, he also recognizes a slippery slope from self-delusion to fraud.

On the other hand, organizational managers, for example, will have a more pragmatic approach to sorting the sense from the nonsense: if it works, it is right.

In contrast to the other articles discussed thus far, Phelan does not question whether or not the prevailing naturalistic conception of what science is might be revised in light of "complexity studies." In maintaining a contemporary definition of science, it is implicitly argued that what we previously labeled "reductionistic complexity science" is real science, while "soft complexity science" (or "resemblance thinking" to use Phelan's term, particularly the use of metaphor) and "complexity thinking" are pseudo-sciences. This distinction again illustrates the division between the physical and social sciences. Many social scientists would argue that all of science is resemblance thinking and there is only metaphor in an absolute sense, and that such pluralism is an inherent feature of complexity science (implying that a reductionistic conception of complexity science is fundamentally flawed and self-contradictory). A few complexologists have themselves concluded much the same with very scientific arguments. What if "science" itself were suffering from self-delusion? It is clear that complex systems analysis can be interpreted in a number of ways and that the distinction between what is and what is not science is still an open question. In the meantime, Phelan's warning that we should not allow nonsense merely because it refers to complexity should be well heeded.

In "What is complexity science? It is really order-creation science," Bill McKelvey argues that what is completely missing from the complexity science literature is a theory about "the engine of order-creation [or self-organization] at any of the several levels of order (atoms to social systems), or across the levels." He is concerned that the nature of the connectivity between system elements (what McKelvey refers to as "entanglement pools") from which order emerges is often ignored. What he is asking for is a theory of how we might associate microscopic diversity with macroscopic ordering, i.e., a theory of emergence. One reason that the literature may be deficient in this area is that it is commonly believed that predictions of macroscopic behavior from microscopic characteristics is to most intents and purposes impossible. One of the few exceptions is in statistical mechanics, in which all the laws of thermodynamics (which describe the state of a particular thermodynamic system as a whole, i.e., they are equations of state) can be calculated by summing all of the possible microstates. Even in simple cellular automata experiments, it is still impossible to predict beforehand what the qualitative system structure will be. It is said to be incomputable.

An important insight of complexity science is that order is ubiquitous, sometimes against all the odds—the world is not really chaotic. For example, why, given all the combinatorial possibilities (which is an unimaginably large number), are there only a few types of organizational structure, i.e., why do so few qualitatively different attractors exist? Social scientists might argue that social orderings result from an intersubjectively agreed (through increasing unconscious conformity) convergence to a preferred model. Even if we cannot have a theory of emergence that might allow us to predict macroscopic behavior from microscopic behavior, we may be able to identify the order parameters that fuel the drift toward order. Following a review of how order in matter emerges (as a result of what Gell-Mann calls coarse-graining), McKelvey considers biological systems, dissipative structures, the Bénard convection system (which has become a classical example of a self-organizing system), and order creation in organizations. His analysis "leads to the inescapable conclusion that complexity science is really order-creation science mistakenly characterized by a relatively extreme end state, complexity."

The article concludes that there are two key elements when considering order creation in organizations. First, pairs of agents within the organization are entangled, i.e., their individual histories are correlated in some way; they are dependent on each other. Second, in addition to the Darwinian selection process and human (bounded) rationality, the Bénard process is "the main engine of order creation so far discovered that applies across the hierarchy of phenomena."

In maintaining the diversity of opinion represented in this issue, the next paper from John Luhman and David Boje, "What is complexity science? A possible answer from narrative research," explores the concept of a complexity science from a narrative methodological approach. By illustrating the co-existence of multiple coherent discourses within a particular organization, they argue that complexity science can be seen as a "narrative move."

The article begins with a brief account of the nature of narrative research, and ends with a discussion of complexity science from the perspective of narrative research. Narratives are basically incomplete, evolving stories that are used to make sense of organizational life by ordering events into narrative plots. They contain histories of events, and provide a guide to what is important, as well as expectations of what might be. The same realist/nominalist divide that Phelan discusses in his philosophy of science also persists in narrative research—some believe that the narrative knowledge is only legitimate if it "tells an accurate story about 'reality'," while others take a social constructionist position and regard narratives as fictional tales about possible realities rather than "real realities." Luhman and Boje support the latter position. A particular narrative can be regarded as an organizational "attractor basin," as it effectively delimits what is relevant and what is not, which in turn limits the possible organizational dynamic; individuals located in basins of attraction are in some sense "captured" and "constrained" by them, and so "follow" them, but they can also effect a qualitative change in the narrative and consequent widespread reorganization. In summary, the authors write:

> As humans we tell our stories, we attempt to make our narratives meaningful to the listener, to help them see connections and participate. In each telling, the narrative may change as we respond to the reactions of participants. We may draw on other stories as comparisons, embellishments, to situate our narrative in a broader discursive space, or orient the

> listener by linking our story to theirs. In other words, our narratives are on-going linguistic formulations, composed in the moment, and responsive to the circumstances of a particular time and space. This is not necessarily a linear or a cyclical process, but a responsive one.

Thus, complexity science, from a narrative methodological approach, "is an understanding of an organization's contextualized and emergent discourse as members interpret, reinterpret, and negotiate discourse within a spatial/temporal intersection."

In his article subtitled "Toward the end of ethics and law parading as justice," Matthew Abraham answers the question "What is complexity science?" in an oblique way. For him, complexity science is not something dealing with issues of scientific knowledge in the first place; rather, it is something that helps us in coming to terms with the ethical problems associated with complex (social) systems. His main concerns are the notions of law and justice, complex concepts in themselves. He does not explicitly develop an "ethics of complexity." Instead, he argues that the work of Lyotard and Derrida (following Benjamin) can be supported with insights from complexity. These so-called postmodern positions, he argues, are not excuses for relativism, but a call for assuming ethical responsibility when dealing with complex systems.

This analysis of ethical issues perhaps provides a fitting conclusion to this issue, exactly because ethical issues do not lead to closure. The articles collected here certainly do not exhaust the question "What is complexity science?" Rather, they serve to demonstrate the divergent views at stake. This divergence should, we feel, not be seen as a problem to be overcome, but a resource to be utilized. In that spirit, we hope that the discussion around the scientific status of complexity science will continue to be lively, even controversial.

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Special Editors

NOTES

1 Realism is the belief that universals, or ideas about reality, exist in reality outside the mind.

2 These five key assumptions are discussed at length in Allen, 2000. The paper presented herein and the paper published in the previous issue of *Emergence* should be read together.

3 Remember that even an apparently useless perspective has value, because to understand why it is useless one must construct explanations in support of such a conclusion. Through this process a more informed opinion of the system of interest will develop.

4 This might be seen as a little unfair to some, as postmodernism can be seen to reject the central claims to truth that science makes, rather than a complete rejection of science altogether.

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