

Using complexity science in organization studies

A case for loose application

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Abstract

Some authors claim that attempts to apply complexity science to organization can only be successful if loyalty is paid to original meanings: only when students of organization accept complexity science as indivisible and operationalize complexity concepts rigorously can faddism be forestalled. In this article it is argued that loose application of complexity theory is not only inevitable, but that meaningful use of complexity theory in the field of organization and management actually depends on flexible application and translation of complexity concepts. The example of the 'career' of the anthropological concept of culture in the field of organization is used to support the argument that fitting complexity concepts into their new habitat does not leave them meaningless, but is instead the *conditio sine qua non* of successful application.

Introduction

According to some authors, using concepts like 'self-organization', 'nonlinearity' or 'emergence' in the field of organization is not without danger. In 1999 Bill McKelvey signalled that complexity science applied to management had all the earmarks of becoming a management consulting fad (McKelvey, 1999). Therefore, together with Steve Maguire, McKelvey made an attempt to differentiate between sound attempts to apply complexity theory on the one hand and reckless ones on the other. As guest editors to a special issue of *Emergence: A Journal of Complexity Issues and Management*, Maguire and McKelvey aimed "to give readers a broad overview of the general quality of complexity applications to CEO problems and to test how vulnerable to faddism they are" (Maguire & McKelvey, 1999: 19). This special issue contained fifty reviews of over thirty books on the relationship between complexity and management.

Like the reviewers whose findings they summarized, Maguire and McKelvey were troubled by the lax treatment of complexity science. "Not unexpectedly, the complexity gurus are most upset with how complexity science terms are loosely, if not metaphorically, defined and tossed in managerial discourse – one goes as far as to suggest that the book offers many insights for managers, but one should simply black out all references to complexity science... [O]ther reviewers worry about 'loose definition' and applications, 'oversimplification', 'incorrect use of concepts', 'superficial' treatments, lack of research, and missing the computational modeling underlay of complexity science" (p. 55). According to Maguire and McKelvey, sloppy applications make dead ends. "From what the reviews indicate, as well as our own reading of the trade books, the *New Science* is well on its way toward shortlived faddism unless serious research shows that there is more than metaphor to chaos theory and complexity science applications" (p. 57). Maguire and McKelvey are not alone here. Mathews, *et al.* (1999: 20), for instance, write that "it is our belief that the complexity sciences offer an approach to the understanding of social change that has more than metaphorical or analogical benefits." And like Johnson and Burton (1994: 320), who believe that the metaphorical use of complexity science is "problematic", Fuller and Moran assert that "moving beyond metaphor" is of critical importance because when "there is no grounding of these analogies in [the world of small firms], there is no evidence that complexity has validity in describing or explaining empirical observation" (1999: 50).

The reason why Maguire and McKelvey believe that the relationship between organizations and complexity theory is not just metaphorical becomes clear when we think through the authors' reference to "the particular complex systems that are human organizations" (1999: 26). When organizations are seen as a particular kind of complex system, a theory of complex systems simply is a theory of organizations. Logically then the ideas and concepts from the science of complexity apply to organization. According to Maguire and McKelvey complexity science thus gets to the bottom of organizations and allows us to know these phenomena as they really are, in and by themselves. By understanding the relationship between complexity theory and organizations this way, Maguire and McKelvey firmly locate themselves in the 'complexity school' that says that with complex systems, differences between allegedly different phenomena are skin-deep only: if you scratch the surface, you will find that phenomena as diverse as ecologies and brains work in the same sort of way. The reason for this is as simple as it is important: different phenomena "work in the same sort of way because they are the same sort of system" as Byrne (1998: 53) phrases it. As far as Meiss (2000) is concerned the idea that a single science of complexity could account for the behavior of seemingly unrelated phenomena is one of its "most exciting aspects." Complexity science "has applications to a wide variety of fields, from mathematics, physics, biology, and chemistry, to engineering, economics, and medicine" and thus "brings researchers from many disciplines together with a common language" (Meiss, 2000). Complexity scientists are not the first group of researchers to look for a shared scientific language however.

The ideal of a common language

Young (1990) points out that several attempts have been made to reduce the number of academic discourses and find a single language to discuss both social and natural phenomena. "Comtethought that sociology could find the laws of social dynamics. Laplace held that modern science was powerful enough to predict social phenomena with precision were all initial states and all laws of interaction known; he assumed linearity. Pareto said: 'My wish is to construct a system of sociology on the model of celestial mechanics, physics, and chemistry'." Probably the most rigorous and systematic attempt to arrive at a scientific *lingua franca* was made during the Macy conferences, held shortly after World War II. The idea behind the Macy conferences was to "bring together a group of researchers working on the forefront of their fields to forge a new interdisciplinary paradigm that became known, retrospectively, as cybernetics. Christened by Norbert Wiener, cybernetics was conceived as a science that would develop a common explanatory framework to talk about animals, machines, and humans" (Hayles, 2000: 246). According to Bale, the "introduction of cybernetics as an interdisciplinary field led to considerable enthusiasm among the scientists who attended the Macy conferences. Many ... believed the ideas offered were sufficiently deep, yet acceptably overarching, that out of them might come a vocabulary suitable as a unifying conceptual framework for the biological and social sciences" (Bale, 1995). The hope of finding a single discourse for a wide range of phenomena rested on the assumption that deep down these various phenomena were essentially all "information processors that encoded and decoded messages, exacerbated or corrected their actions through feedback loops, and demonstrated causality" (Hayles, 2000: 246). There are interesting parallels between cybernetics and complexity science as introduced above.

Cybernetics was based on the idea that machines and living individuals are in essence information processors. Complexity theorists on their turn believe that when you look carefully you will find that earthquakes, ecologies, brains, cultures, economies, languages or organizations and so on are really all complex systems. This assumption allows for the following situation to be imagined. A student of the particular complex system that is an ecosystem carries out research and 'uploads' his findings into a database where knowledge about complex systems is stored. That piece of knowledge then becomes available to other students of complex systems, for instance to the student of the particular complex system that is the brain. If the latter researcher is not so much interested in the superficial 'braininess' of her research object as she is in its underlying, more fundamental 'complex systemness', she can make use of the knowledge that the student of the particular complex systems that are ecosystems is willing to share with his fellow complex systems researchers. Under what I will call the Grand Complexity Project, complexity scientists working in seemingly different (academic) areas are free to exchange research findings. According to Maguire and McKelvey this includes students of complex systems who work in the faculty of Management and Organization. "Yes, we said 'exchange'. As that field within the social sciences that devotes itself to the study of 'organization' and 'organizing', it would be surprising if management and organization studies had nothing to contribute to complexity scholars as they turn their attention to systems composed of human agents" (1999: 24). Byrne also foresees an interdisciplinary exchanging of ideas and he too maintains that all complexity scientists are created equal. He insists that even though complexity research as we know it has its roots in the hard sciences, the insights offered by social scientists are no less valuable: "in no sense whatsoever is the project of applying the ideas of complexity theory to the social driven by any sort of physics envy... It is true that chaos/complexity emerges from the experimental mathematics and thermodynamics, and has been particularly developed in physical chemistry and evolutionary biology. But, and it is a big but, once the social sciences get going, then other fields of inquiry will have a lot to learn from them" (Byrne, 1998: 17).

There are important conditions that need to be met before students of complex systems can actually profit from each other's research. For one thing, the exchangeability of research findings requires that complexity researchers mind their language. For the results of research on the particular complex system that is a human organization to make any sense to the student of the particular complex system that is an ecosystem, it is necessary that these research findings do not contain references to the form in which the phenomenon expresses itself in daily life. That is, if a student of human organization were to include in the research findings to be uploaded concepts like strategy, culture, leadership, technological developments or budgets the data

would be useless to ecologists. Typical features of 'organizationess' must be removed before knowledge can be offered to the community of complexity researchers. Under the Grand Complexity Project it is assumed this can be done. As argued above, the presumption that underlies this Project is that the differences that normally encourage us to distinguish between organizations, economies, languages or coral reefs can be ignored in favour of a focus on the deeper complex systemness. But if not by means of the language they normally talk about organizations (strategy, culture, leadership, technological developments, budgets) how are students of organization to communicate their research findings instead? Here, too, there are important parallels between complexity science and cybernetics. Norbert Wiener left no room for misunderstanding as to what he thought was the appropriate unifying conceptual framework for discussing various sorts of information processors: "cybernetics is mathematics or it is nothing" (Wiener quoted in Hayles, 2000: 148-9). Participants of the Grand Complexity Project share this preference for the language of the hard sciences. As Byrne (1998: 55) sees it, complexity science can only deal with "mathematically formalized accounts of reality." Mathematics offers 'lean', abstract descriptions and as such it has a natural advantage over any language that offers 'thick' descriptions that do depend on references to the earthly appearance of organizations. Knowledge that is embedded in spatio-temporal specifics does not lend itself to being processed by computers, and according to Maguire and McKelvey this poses a real problem: "complexity science and computational modeling go hand in hand" (1999: 56). To overcome the problem of descriptive thickness and to do away with context, Maguire and McKelvey propose alternative concepts to describe organizational life. As they see it organizational concepts such as "conversation elements, people, departments" should be redefined as "stochastically idiosyncratic agents" because the "platform assumptions [of such agents] are more easily tolerated in computational models" (1999: 56). The advantages of abstracted concepts to the Grand Complexity Project are obvious: whereas concepts like 'staff meetings', 'managers' or 'human resource management department' mean nothing to the student of the particular complex system that is the brain, she can make use of the work of the student of organization who publishes on the behavior of 'stochastically idiosyncratic agents'.

The war on faddism

Having explored the main assumptions underlying the Grand Complexity Project we can now turn to the question of how complexity science research is to be conducted. According to Maguire and McKelvey the fact that complexity science is still a "nascent" field has significant implications for researchers who seek to make sense of organizations using the science of complexity. "If complexity researchers themselves have not yet reached a consensus on how to integrate and synthesize all the ideas and concepts they bring together from diverse disciplines it would appear unwise for organizational researchers to charge ahead with the building of models or entire organization theories and management philosophies, drawing a few concepts from one discipline and a few from another based on whatever is convenient or has a nice metaphorical ring to it" (1999: 56, 25). While there is no reason to assume that such mass consensus among complexity scientists could ever be reached – like any other field of research, the science of complexity consists of large numbers of schools that 'fight' one another over issues as fundamental as what complexity really is, over the way in which the complex is to be studied, over how complexity concepts should be interpreted, and so on (see e.g., Van Uden, 2004: 56-64) – Maguire and McKelvey maintain that in the absence of such consensus, attempts to apply complexity to management are bound to fail: "the record is clear over the past several decades – management ideas that do not become legitimized by resting on a foundation of quality research are quickly replaced by the next fad coming down the pike" (1999: 19). Below I will argue there is no compelling evidence to support Maguire and McKelvey's claim that only a rigorous approach can make complexity applied to management and organization 'work'.

According to Maguire and McKelvey one of the main characteristics of quality research is the avoidance of loose or metaphorical application of concepts. In their eyes faddism is inevitable unless we commit ourselves to "the tough work of rigorous definition and operationalization" (1999: 25). This claim is fully consistent with how the authors see the relationship between organization and complexity science, namely that organizations are complex systems. Within this view it makes perfect sense to maintain that when we apply concepts like 'emergence', 'nonlinearity', 'strange attractors' or 'fitness landscapes' to organization, we do not use those words metaphorically: when used correctly, when we are loyal to the original meaning of these concepts, they mirror the true nature of organizations. If, on the other hand, we use these concepts incorrectly, if we simplify their meaning or treat them superficially, these concepts no longer correspond with the reality of the particular complex systems that are human organizations. In the latter case 'the word' no longer matches 'the world' which means the complexity concepts have lost all value.

I see no reason to accept this scenario where loose application necessarily leads to meaninglessness. There is nothing that indicates that students of organization tend to take great interest in the original meaning of the concepts they borrow from new sources, let alone in applying them rigorously. But rather than this leading to poor research quality it can be argued that because students of organization apply new concepts loosely rather than rigorously that such applications turn out to be successful. An analysis of the role metaphor plays in scientific inquiry makes clear that only 'tailor-made' ideas and concepts – ideas and concepts that have been borrowed and reshaped to fit their new environment – stand a good chance of making a valuable contribution in the realm of organization.

Metaphors gone literal

The warning that applications of complexity science must go 'beyond metaphor' bears testimony to a distinct philosophical position in

which metaphorical descriptions are categorically separated from literal descriptions that represent a phenomenon as is. According to this line of reason, you either grab the subject matter by its very essence, in which case 'you tell it like it is', or you discuss it in an inappropriate language, which makes you a poet or liar. Other philosophical positions lead to different appreciations of metaphor. Gareth Morgan (1980, 1983, 1986) thinks of metaphor as a lens and argues that with every other metaphor we 'look through', we disclose more and more of the same organization. Others have radicalized Morgan's conclusion that the difference between literal and metaphorical is problematic and claim that a fundamental difference between the two simply does not exist (see e.g., Letiche & Van Uden, 1998). These authors deny that the world is already 'out there', that language has the capacity to objectively describe that world and that we, as 'researchers' or 'discoverers' are thus in the business of finding the particular language that 'represents' that world accurately. Instead, language is said to 'produce what it denotes'. Chia, for instance, writes that "language and words are not wrappings in which things are packed for the commerce of those who write and speak. Social entities, events and things do not first pre-exist and then suffer descriptive distortion through language. Instead language actively configures such entities and events in the very act of representing... [W]e do not just 'write about' our objects/subjects of analysis. Rather we bring these objects into existence through representational acts of writing" (Chia, 1996: 72). This has far-reaching consequences for how metaphor is valued. Instead of accepting an ontological distinction between right (literal) and wrong (metaphorical) descriptions, it is now assumed that if there is a line that separates between different kinds of descriptions, it is the line that separates standard, old, embedded, ingrained, normal descriptions on the one hand from new, speculative, surprising, odd ones on the other. As Nietzsche (1872: 50-1) describes the situation, "the only intrinsic difference here is the difference between custom and novelty, frequency and rarity." If the difference between metaphorical and literal is a difference of degree rather than of kind, it follows that one can only discriminate between routine and new understandings of organization. To illustrate this point, I will show that the development of the concept of organizational culture is best characterized as a process in which metaphors slowly developed into literal descriptions. The process that underlies the 'career' of organizational culture will then be applied to complexity science and it will be argued that here also success will ultimately be determined by the extent to which new ideas and concepts contribute to existing organizational research programmes.

The Culture Example

According to Edgar Schein (1992: 490) “the fields of organizational psychology and sociology have developed a variety of useful concepts for understanding individual behavior in organizations and the ways in which organizations structure themselves. But the dynamic of why and how they grow, change, sometimes fail, and – perhaps most important of all – do things that don’t seem to make any sense continues to elude us”. Here is where Schein inserts culture: “the concept of organizational culture holds promise for illuminating this difficult area. I will try to show that a deeper understanding of cultural issues in organizations is necessary not only to decipher what goes on in them but, even more important, to identify what may be priority issues for leaders and leadership”. According to Shafritz and Ott (1992: 481-3), the organizational culture school accused the dominant research groups of that time of “using the wrong tools (or ‘lenses’) to look at the organizational elements to understand and predict organizational behavior” and therefore proposed new concepts to make sense of organizational life. Today these ‘odd’ culture concepts (myths, heroes, rituals, sagas, etc.) are part of the ‘official’ language of organizations and a satisfactory answer to the question “can you tell me about the culture of your organization?” is considered by most as real and valid a description of that organization as is the answer to a question like “can you tell me about the structure of your organization?” Slowly the speculative concept of culture has become a concept we believe says something meaningful about organizations. What is interesting to observe is that in this process the dependency on the field in which the concept of culture was originally developed is diminished. Initially, when organizational culture was still an underdeveloped concept, its theorization heavily relied on the literature that inspired the pioneers of organizational culture in the first place. Schein for instance aimed to “provide a clear, workable definition of organizational culture that takes into account the accumulated insights of anthropologists, sociologists, and psychologists” (1992: 491). Armed with the insights of these anthropologists, sociologists, and psychologists, authors like Schein faced the challenge of using the culture discourse in a way that made sense. What if these students of organization would have committed themselves to Maguire and McKelvey’s guidelines for quality research? Let us presume for the sake of the argument that at some point all anthropologists, sociologists, and psychologists reached overall consensus on the meaning of culture. Even in this imaginary situation it is not clear why students of organization would be interested in such a widely agreed upon definition of culture. What is important for them is that the concept of culture ‘works’ in the realm of organization and above all this requires that concepts like ‘culture’, ‘heroes’, ‘sagas’, ‘rituals’, ‘artefacts’ or ‘norms’ bear relevance to existing work on organizations. Schein valued culture for the ways in which this concept contributed to his research on leadership in organizations. Other researchers have appreciated the notion of culture for different reasons; as an example, for its capacity to say something useful about dysfunctional bureaucracies. Or because knowledge of myths adds something to our understanding of inter-departmental communication. Or because insight into cultural norms and values makes clear why people are reluctant to file a sexual harassment complaint against a colleague. The point being made is here is that students of organization value the concept of culture first and foremost for what it has to add to standing research. As a result of the process of ‘subjecting’ culture to existing organizational research projects the link between the meaning of culture in an organizational context and its ‘original’ meaning has grown weaker over the years.

Much in accordance with how anthropologists, sociologists, and psychologists used the concept, Schein understood culture as a ‘conceptual tool’ to describe certain phenomena. Here culture was still seen as descriptive in its intent: “a dynamic model of culture will be especially useful in improving our un-

derstanding of how human systems evolve over time” (Schein, 1992: 491). But Jeffcutt shows that in the course of time the meaning of organizational culture diversified. He argues that “a number of contradictory discourses have become constructed and embellished in order to give coherence to the differing positions in the interpretation of organizational culture.” One of those discourses he describes as “culture as a corporate (managerial) possession, (a.k.a. ‘Corporate Culture’).” This management-centric twist instigated a departure from the descriptive nature of the concept of culture. “A remarkable turnaround or critical inversion appears to have taken place [in organization studies] in which the manipulation of the non-rational has become the latest tool of managerial control” (Jeffcutt, 1993: 25-6; 1989: 23). This collapse of the ‘true’ meaning of culture was accompanied by a growing disinterest in the original writings on culture by anthropologists, sociologists and psychologists. As the concept of culture nestled itself in its new habitat, and ‘organizational culture’ became a phenomenon in its own right, organizational researchers – and this can be concluded from the literature references they make – turned to books and articles that dealt specifically with organizational culture. Over the years organizational culture thus emancipated itself from ‘normal’ culture. The case of culture is not unique in this respect. Students of organization have borrowed the concept of ‘strategy’ from war studies the concept of strategy. In the field of organization studies strategy, like culture, has come to be (re-)interpreted in many different ways (see Mintzberg, *et al.*, 1998). And in the very same way that students of organizational culture ‘got the idea’ at some point and lost interest in the work of anthropologists, students of organizational strategy eventually decided to disregard new theories of strategy developed back in the field of war studies. As a result the military associations of the concept of strategy were washed out, and strategy became an organizational phenomenon in its own right: business strategy.

The process Maguire and McKelvey (1999: 25) refer to as the “importing and translating” of ideas and concepts can be summarized as follows. On an ongoing basis students of organization explore other disciplines, borrow ideas and concepts they believe can make a contribution of some kind, and try to fit these newly adopted ideas and concepts into existing organizational research programmes. Concepts that prove successful in making the contribution to organizational research become part of normal organizational discourse and lose association with the sources from which they were derived. There is no reason to expect that students of organization will approach the science of complexity in a way that is fundamentally different from how they welcomed culture studies. In the same way that researchers linked novel cultural concepts to (already) normal(ized) organizational phenomena such as leadership or structure, complexity concepts are, and will continue to be, tested on their

capacity to fit into research on organizations as we know them. Surprisingly enough Maguire and McKelvey actually seem to support this practice. In their eyes, complexity science will end up a meaningless hype unless “serious research ... shows that CEOs using the New Science produce more competitively advanced firms than CEOs who do not” (1999: 57). Maguire and McKelvey’s norm for successful “importing and translating” is clearly not of an ‘intra-complexity science’ nature but is instead defined in very specific managerial terms: does complexity science help us build competitively advanced firms or does it not? Other students of organization value complexity for very different reasons. Letiche (2000: 555), for example, hopes to find that complexity theory can “provide us with ethnographies of emergent action” while Irvin uses insights from the science of chaos and complexity to scrutinize “one of the main features of the conventional credo of organizational management: the ‘ethic of self-preservation’” (2002: 359). Svyantek and Brown have reasons to believe that the representation methods associated with complex adaptive systems studies, and in particular its theories of the way in which agents interact with each other so as to form coherent, self-reinforcing clusters, may be useful in developing a better understanding of the US automobile industry (2001: 42-3). And having first pointed out that theories of complexity have already been used in studies on ‘organizational transformation’, on ‘corporate strategy’, and on ‘organization design’, Styhre, Ingelgard and Roth set out to examine “the notion of knowledge from the nonlinear, non- reductionistic perspective put forth by complexity theory” (2000: 51). So far from success depending on rigorous application, it is in these very specific organizational terms that complexity science must live up to the expectations.

Applying complexity theory to organization: Concluding remarks

In this article the argument has been developed that the discourse of organization cannot be replaced with the supposedly ‘deeper’ discourse of

complexity. From the claim that rigorous application is not the key to successful use of complexity science in the field of organization, it should not be concluded that complexity science has no ‘intrinsic’ value however. The reason why students of organization turn to ideas, words or even entire models developed in other disciplines is because they expect that these ideas, words and models have something to add to existing research projects. A borrowed concept is deemed valuable because it offers something that was not already available to the student of organization prior to the ‘appropriation’. There are many ways in which a new concept can make a contribution to existing research. A new concept may enable us to call into existence a phenomenon that was inexpressible in our pre-existing language, for instance. Alternatively, we may borrow a concept to undermine the foundation of a dominant theory we seek to destroy. Or we hope to find that the concept combines existing organizational themes in a way that was inconceivable in the absence of it. In any of these cases the concept must connect to an existing research project. At the same time, it is important that the concept is not fully consumed by that project. The value that the borrowed concepts add is that they make a difference, one way or the other. Given that we turn to the use of new concepts because they add something to an existing discussion, it makes no sense to kill its potential by sterilizing it – by arguing for example that the concept we take from complexity science is really but a synonym for an already existing organizational concept. The successful import and translation of ‘alien’ concepts thus boils down to a balancing act: for the concept to be relevant, it needs to fit into existing research programmes. At the same time, if we want to preserve the potential we appreciated it for in the first place we must be careful not to let the concept be fully assimilated into and taken over by existing body of knowledge.

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