Four Domains of Complexity

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Abstract

In this short paper, which reflects on one of my contributions to the systems literature in 1992 (*Pluralism and the Legitimation of Systems Science*), I discuss the context at that time. Systems scientists were embroiled in a paradigm war, which threatened to fragment the systems research community. This is relevant, not only to understanding my 1992 contribution, but also because the same paradigms are evident in the complexity science community, and therefore it potentially faces the same risk of fragmentation. Having explained the context, I then go on to discuss my proposed solution to the paradigm war: that there are four domains of complexity, three of which reflect the competing paradigms. The problem comes when researchers say that inquiry into just one of these domains is valid. However, when we recognise all four as part of a new theory of complexity, we can view them as complementary. The four domains are natural world complexity, or "what is" (where the ideal of inquiry is *truth*); social world complexity, or the complexity of "what ought to be" in relation to actual or potential action (where the ideal of inquiry is *rightness*); subjective world complexity, or the complexity of what any individual (the self or another) is thinking, intending or feeling (where the ideal of inquiry is *understanding subjectivity*); and the complexity of *interactions* between elements of the other domains of complexity in the context of research and intervention practice. Following a discussion of the relevance of this theory for complexity scientists, I end the paper with a final critical reflection on my 1992 paper, pointing to some theoretical assumptions and terminology that I would, in retrospect, revise.

Keywords: complexity theory; critical systems thinking; methodological pluralism; ontology; paradigms; systems philosophy.

Introduction

I would like to start by thanking Kurt Richardson, the editor of *Emergence: Complexity and Organization* (E:CO), for asking me to write this introduction to my own paper, *Pluralism and the Legitimation of Systems Science* (Midgley, 1992a). It is very rare to have the opportunity to publically reflect, 25 years later, on one's own older work, and I am honoured that Kurt considered the paper significant enough to be included in E:CO's "classic papers" series.

Below, I will do three things. First, I will explain the context in which I wrote the paper, as the whole framing of it might be puzzling for those reading this in 2016 or later.

Second, I will briefly summarise what I believe is of value in this paper for complexity theorists and practitioners. The paper was not intended as a contribution to the literature on complexity (it was primarily aimed at the systems science research community), but I was aware at the time that, if researchers were to follow up the implications for developing our understanding of complexity, it might lead complexity scientists down a considerably different road than the one that the majority have taken. This is now a chance to start that follow up discussion – better late than never!

The third and final section below explains what I would do differently if I could go back 25 years and write the paper again. Back then I was nearing the end of my doctoral studies, and this paper (Midgley, 1992a) and three others (Midgley, 1992b, 1996a, 2001) together summarize the arguments in my PhD dissertation (Midgley, 1992c). I smile now at the number of theoretical assumptions and uses of terminology that I would revise today, if writing the paper again with the benefit of hindsight.

Having said this, I still believe that much of my 1992a analysis of four types of complexity stands up to scrutiny today, even if I now question some of the social/linguistic theory that I drew upon to develop it. Further, I reckon that the argument has practical implications for understanding how complexity theory can be translated into methodology and practice in new ways.

The Context

In the late 1980s and early 1990s, systems scientists were dealing with what might be termed an "existential crisis", with the clashing of several incommensurable paradigms and a consequent fragmentation of their research community into competing camps. The history of this paradigm war is instructive, not just because it provided the context for my own contribution to the literature at that time, but also because we can see the same divisions in complexity science today (Midgley and Richardson, 2007), and hence the potential is there for the fragmentation of the complexity research community too.

In brief, following the consolidation in the 1940s and 1950s of some important transdisciplinary theories in the fields of systems science (e.g., Angyal, 1941; von Bertalanffy, 1956; Boulding, 1956; Hall and Fagan, 1956), cybernetics (e.g., Wiener, 1948; Ashby, 1956; Bateson, 1967) and complexity science (e.g., Weaver, 1948; Simon, 1962), there was a push in the systems community to embody these theories in methodologies for intervention to support systemic organizational and social change. These methodologies included systems analysis (e.g., Quade and Boucher, 1968; Optner, 1973), systems engineering (e.g., Hall, 1962; Jenkins, 1969), system dynamics (e.g., Forrester, 1961, 1969) and viable system modelling (e.g., Beer, 1959, 1966). The authors of these methodologies

tended to adopt an approach that assumed the need for expert-driven modelling of real world complex systems, and they strove for comprehensiveness in their models while emphasising quantification, prediction and control (Jackson, 1991).

This whole approach ran into problems in the 1960s and 1970s, and strong criticisms were advanced of the assumptions built into it (Hoos, 1972; Lee, 1973; Lilienfeld, 1978; Ackoff, 1979a). As a consequence, the late 1970s and early 1980s saw the emergence of a new paradigm with its own methodologies (e.g., Ackoff, 1981; Checkland, 1981; Mason and Mitroff, 1981; Rosenhead, 1989; Christakis and Bausch, 2006) based on very different assumptions. The expert researcher was replaced by a facilitator, whose role was to include stakeholders in participative, qualitative modelling (Rosenhead, 1989). The meaning of the term "expertise" was thereby democratized to refer to relevant knowledge held by those involved in and affected by a problematic situation (Churchman, 1970). The emphasis was no longer on systems as real world entities, but instead attention was switched to how collaborative groups could develop *better systemic understandings of potential actions*: a "system" became a useful way of viewing the world rather than something that can be assumed to exist objectively (Checkland, 1981). With this shift came recognition of the inevitable lack of comprehensiveness in every analysis (Churchman, 1970; Ulrich, 1983), and hence a relaxation in assumptions about prediction and control, with more of an emphasis on the need for better mutual understanding between stakeholders (Checkland, 1981), dialogue (Ulrich, 1983; Christakis and Bausch, 2006) and learning (Ackoff, 1979b; Checkland, 1985; de Geus, 1994; Sterman, 1994). It was at this time that the terminology of "hard" and "soft" systems was first proposed (Checkland, 1981), with hard systems methodologists being those who wanted experts to quantify analyses of real world systems, and soft systems methodologists being those who wanted facilitators to support dialogue around different ways of seeing systems and possible actions to change them. Thankfully, in more recent years, this divisive language has become largely redundant (Rosenhead and Mingers, 2001).

Of course, the advocates of the first paradigm didn't go away when the second one was proposed, and a paradigm war ensued. By the time we had hit the late 1980s, when I wrote my own first contributions to the literature on systems methodology (Midgley, 1989a,b, 1990a,b), it was evident that the war was tearing the systems research community apart, with advocates of the different paradigms denigrating their opponents and refusing to participate in cross-boundary dialogues. It was in this context that a new paradigm called "critical systems thinking" was proposed (e.g., Flood and Jackson, 1991; Flood and Romm, 1996), and this had two principle aims: first, to deepen our understanding of how power relations can be addressed during intervention (e.g., Ulrich, 1983; Flood, 1990; Midgley, 1992b; Oliga, 1996), given that both previous paradigms were arguably rather naive in their analyses of power; and second, to develop an adequate theory and practice of *methodological pluralism* so we could transcend the paradigm war and draw upon the best from both previous paradigms to create a much more flexible and responsive approach to systemic intervention (e.g., Jackson and Keys, 1984; Jackson 1987a,b, 1991, 2000; Oliga, 1988; Flood, 1990, 1995; Flood and Jackson, 1991; Gregory, 1992, 1996a,b; Midgley, 1992a,c, 1997, 2000; Brocklesby, 1994, 1997; Flood and Romm, 1996; Mingers and Brocklesby, 1996; Mingers and Gill, 1997).

My 1992a paper was a relatively early contribution to critical systems thinking, with a specific focus on developing a theory of complexity to underpin the practice of methodological pluralism, and thereby transcend the paradigm war. My proposal was for the identification of four domains of complexity, and the methods from the various competing methodologies could be aligned with these according to the ideal of inquiry they embodied. More details are provided below. It is my contention that this argument may be of value to complexity theorists and practitioners who may have to deal with paradigmatic divisions in the complexity research community along similar lines to those that were previously encountered and addressed in the systems community.

The Value of the Paper for Complexity Theorists and Practitioners

The four domains (or types) of complexity that I proposed in my 1992a paper were:

1. "Natural world" complexity, or the complexity of "what is". The ideal of inquiry into this form of complexity is *truth* – but note the term "ideal" which, following Popper (1959, 1972), indicates that truth is something we aim for, but we can never know for certain whether it has been achieved.
2. "Social world" complexity, or the complexity of "what ought to be" in relation to actual or potential action. The ideal of inquiry into this form of complexity is *rightness*.
3. "Subjective world" complexity, or the complexity of what any individual (the self or another) is thinking, intending or feeling. The ideal of inquiry into this form of complexity can be called *understanding subjectivity*.
4. We very often have to deal with *interactions* between phenomena in the above three domains of complexity. This means that there is also the *meta-level complexity of these interactions*, which needs to be a focus of inquiry. However, it's important to note that, in the context of intervention (rather than just observation), meta-level analyses may not stay "meta" for long: communication of them, and action upon them, may feed back to change the original pattern of interactions.

In my 1992a paper, I drew upon Habermas's (1976, 1984a,b) linguistic theory of 'three worlds' to underpin the first three of the above domains of complexity (the fourth comes about because the first three interact). In brief, Habermas argues that any sentence intended for communication has three implicit validity claims: a claim that its propositional content is *true*; that it is the *right* thing to say in the context; and that the speaker is *sincere* in saying it. These validity claims refer to three worlds: our external natural world; our normative social world; and my (or your) internal subjective world. According to Habermas, a rational, free and fair dialogue is one where anything that is said can, in principle, be opened up to critique on the basis of truth, rightness or sincerity. When some aspect of potential critique is repressed (for example when a company allows its employees to participate in discussing the means to achieve already-given ends, but those ends are not open for discussion), this produces 'distorted communication'.

In three short, logical steps, we can move from Habermas's linguistic theory to my own proposal for the first three domains of complexity above. First, we can view Habermas's three "worlds" as foci for research and inquiry, and not just rational argumentation in dialogue. Second, this broadening of the focus beyond dialogue means that we have to be concerned with more than just the sincerity of speakers when we consider subjectivity: it is the whole panoply of intentions, thoughts and feelings that come to be of interest in inquiry. Third, why would we need research and inquiry if there were no uncertainties, and hence underlying complexities, to deal with? Seeing Habermas's "worlds" as the first three domains of complexity therefore makes sense – and, as we have seen, the fourth domain of complexity concerns how phenomena in the other domains interact.

Now, Habermas (1984a,b) said his theory was *ontological*, as he claimed that it is the intrinsic properties of language that enable us to distinguish between the natural, social and subjective worlds. In 1992, I therefore labelled the fourth (meta) level of complexity 'ontological complexity' to indicate that *all three* of the other forms of complexity *and* their interactions are essential to consider when dealing with any non-trivial issue requiring systemic action research: ignoring one or more of the domains will result in missing a significant source of complexity, and this will impoverish analysis, understanding and action.

I think the implications of this for complexity *theory* are clear. There are now multiple complexity paradigms represented in the literature (Midgley and Richardson, 2007), and most of these focus on just one of the "worlds" identified in my 1992a paper. Indeed, it is arguably still the case that the majority concentrate on describing the generic characteristics of complex adaptive systems, network interactions, etc., and then either apply their theories to interpret empirical findings, or refine their theories through the analysis of findings. Essentially, they produce theories of "natural world" complexity, guided by the ideal of truth (acknowledging, of course, that their truth judgements are not absolute, as any reasonable scientist would). However, for the most part, they do not inquire into the normative social world of moral judgements concerning actions that people might want to take. The kind of research needed for this would be substantially different: in a particular context, it would involve exploring the complexities and uncertainties around possible human actions that people can envisage, and the perceived moral implications of these (Ulrich, 1983; Friend and Hickling, 2004); or the focus might be on how people's values contribute to the setting of purposes that then limit their framing of issues, and both values and framings can be shifted through action research (Cilliers, 1998; Midgley, 2000). Because the social world is about what "ought to be" in the context of action, exploring these things in action research mode makes sense. It is also worth asking, how much complexity research has a focus on the purely subjective perspectives of individuals? As far as I am aware, there is very little, although Snowden (2010) is a notable exception, advocating the collection of multiple individuals' stories and then looking for patterns across these. There are major opportunities for developing new theories relating to the social and subjective complexity domains, and the biggest challenge of all is arguably to produce theories that *explain repeating patterns in the interactions between the three types of complexity*. We might thereby be able to offer generic

insights into the fourth (meta) domain of complexity, as well as enable bespoke analyses of the interactions that are relevant to particular local and temporary contexts of practice.

For complexity *practice*, the distinction between the four domains could also be valuable. Remember that, in 1992, I advocated the theory of the four domains of complexity to support *methodological pluralism*: drawing upon and mixing methods from across a range of systems (and other) approaches. It would be possible to reinterpret and harmonize the existing complexity paradigms in terms of the first three domains of complexity (concerning the natural, social and subjective worlds), thereby making them complementary. It is then reflecting on the *interactions* between the three types of complexity (i.e., beginning to get to grips with the fourth domain of complexity) in any action research project that can guide the mixing of methods to support inquiry and change in practice.

While the systems research community has placed considerable emphasis on the development of methodologies and methods to support systemic inquiry over the past 60 years (Jackson, 1991, 2000; Midgley, 2000, 2003; Reynolds and Holwell, 2010), I suggest that less emphasis has been placed on this by complexity scientists. Methodology and methods provide a bridge from theory to practice, and more concentration on this by complexity scientists would be useful – not just to inform practice with complexity theories of the natural world, but to support the exploration of normative and subjective complexities too.

Reflections on Theoretical Assumptions and Terminology with the Benefit of Hindsight

Having said earlier that I still believe that much of this theory of the four domains of complexity stands up to scrutiny today, it is perhaps unsurprising that I would now, with 25 years of hindsight, choose to change some assumptions and terminology.

Over the years, I have developed an increasing scepticism of "grand theories": sweeping theories of universals in human nature and society (see Skinner, 1985, for an examination of grand theories in the social sciences). For a start, many theories of supposedly "universal" characteristics of human beings or societies have been found to be culture dependent in light of evidence from comparative anthropology. Perhaps the most famous recent example concerns colour perception, where it now appears that discriminating between colours has a lot to do with expectations of colour distinctions established in linguistic categories that have evolved in particular ecological and social contexts (Roberson et al, 2002). One of the problems with grand theories is that a lot of the particular, unique complexity in social situations is passed over in favour of relatively simple generic observations: almost the reverse of the old adage that "we cannot see the wood for the trees". With grand theories, it's mostly wood and the trees become blurred.

Even when I wrote my 1992a paper (and indeed my 1992c PhD thesis, which I was writing at the same time), I struggled with a dilemma. I was unsure whether or not to accept Habermas's (1976, 1984a,b) ontology, which (in the typical manner of a grand theory) roots categories of inquiry in the universal properties of language. I was advocating

methodological pluralism, and of course different methodologies draw upon different (sometimes incompatible) theories, so would it then be contradictory to say that there is one theory of language that can organize all the methodological diversity?

In the end I found a way through this. As I saw it, simply accepting a diversity of methodological paradigms and saying we can draw upon them pragmatically as we see fit meant also accepting all the incommensurable philosophical assumptions that come with those paradigms. Can we credibly say that our knowledge reflects a real world (the epistemological assumption of much "hard" systems science) and also, at the same time, believe that we only have access to our subjective and inter-subjective understandings, which cannot necessarily be assumed to reflect anything real at all (the epistemology of much "soft" systems thinking)? Surely this leaves us in a philosophical muddle. This is plurality without any theory that explains how and why the various aspects of the plurality are valid or legitimate. I therefore decided, after much reflection, to go for a unifying theory through which the plurality of *methods* (not methodologies, with all their accompanying, potentially incommensurate theories) could be explained. I argued that any unifying ontology had to be 'multi-faceted' in order to have the requisite variety to contextualize methods drawn from different paradigms (Midgley, 1992a, p.169). I therefore moved beyond Habermas's purely linguistic theory, arguing that the external natural world, the normative social world and our internal subjective worlds all exist, and indeed it is possible to show that the existence of each of them is dependent on the existence of the other two (Midgley, 1992a, p.160-161).

The other issue I struggled with at the time of writing my 1992a paper, but I ended up ignoring my first instincts on this, was that judgements concerning *beauty* cannot be reduced to one of the three ideals of inquiry: truth, rightness and understanding subjectivity. Where did aesthetic judgement fit in Habermas's analysis of the inherent validity claims in any sentence intended for communication? I was already aware that Habermas's ontology could be viewed as a "grand theory", which might not be such a good thing, and the fact that beauty wasn't recognised as an ideal of inquiry suggested that his theory could be overly reductive.

Although I set aside these concerns about aesthetics in 1992, it finally dawned on me in the late 1990s that I had been right to be concerned. I set out to write a history of systems thinking in a chapter of a book (Midgley, 2000), and I was discussing how three long-standing traditions had informed the various systems paradigms: pure science (which tried to establish truth claims), applied science (which was also concerned with truth, but with a view to informing right action) and psychoanalysis (which is much more focused on understanding the subjective perspectives of individuals). It suddenly occurred to me that what Habermas might have done when he produced his ontology was to observe the major analytical traditions that mattered to him in society and then reflected the validity claims associated with those traditions in his theory of the universals of language! Although I cannot prove that his logic went in this direction (from the analytical traditions to the ontology, rather than the other way around), it makes some sense of the way in which Habermas (1976, 1984a,b) talks about those traditions; and the absence of beauty as an ideal of inquiry is arguably because

Habermas does not view art and aesthetics as a major analytical tradition in the same sense as the sciences and psychoanalysis.

So today I am left feeling somewhat equivocal about my 1992a argument. On the one hand, I think that the idea of four domains of complexity stands up to scrutiny and could be useful for understanding how complexity science could develop into the future. On the other hand, I am much more critical than I once was of Habermas's (1984a,b) linguistic ontology, although I was already looking for ways to go beyond this in 1992 (such as viewing the natural, social and subjective worlds as real rather than as mere reflections of the universal capacities of language). It might be more appropriate to simply think about historical traditions, such as science (oriented to the ideal of truth), politics (oriented to the ideal of rightness) and therapy (which, when undertaken with individuals, is oriented to the ideal of understanding subjectivity), and we can then argue that these are embodied in the first three complexity domains, with the fourth domain being concerned with their interaction. With this introductory paper, I am putting both interpretations into the literature, and I leave it to the reader to judge which is the most useful and appropriate.

Incidentally, one other thing that I would change about my 1992 paper is discussion of 'ecological harmony' (Midgley, 1992a, page 150). Gunderson and Holling (2002) persuasively argue that ecological systems cycle between periods of conservation (when the complexity of interrelationships grows steadily over time), release (when there is an external perturbation and the complexity of interrelationships makes the ecosystem "brittle" and vulnerable, causing some of these interrelationships to break), reorganization (when fresh interrelationships are formed), exploitation (when new complexity starts to burgeon quickly), and back to conservation again. In the context of this dynamic pattern, Gunderson and Holling argue that the word "harmony" connotes the maintenance of an equilibrium that does not actually exist. In retrospect, I would perhaps have talked about living within sustainable limits, which does not preclude a cycle that, in all its phases, remains within those limits.

Conclusion

In conclusion, I recommend my 1992a paper to you, with the proviso that you read it critically, understanding that it is a product of its time. My hope is that the basic argument for four domains of complexity is still relevant now for researchers asking where complexity theory remains under-developed and could go in the future.

I also hope that the argument for methodological pluralism is just as relevant for complexity practitioners as practitioners in the systems community. Certainly this paper, and many other books and papers on critical systems thinking (see especially Flood and Jackson, 1991; Jackson, 1991, 2000; Flood and Romm, 1996; Mingers and Gill, 1997; and Midgley, 2000), helped us win the argument for methodological pluralism in the systems community. While there are still live debates about the theory underpinning the practice of methodological pluralism (e.g., Zhu, 2011), it is undoubtedly the case that most people now accept it as preferable to both a paradigm war and the limitations on practice that come with believing

that only a narrow range of methods has validity. If complexity scientists need to transcend a paradigm war themselves in coming years, they do not need to start with a clean slate: there is a lot of prior work in the systems literature, including my own 1992a paper, reprinted next.

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