SD762 – S99

Readings and Reflections on Systems Thinking

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Introduction

Goals

This paper summarizes, in multiple ways, my recent research activities in the field of systems. The primary goal of my research was to develop some appreciation for the issues facing the systems movement. As an enabling step, I first had to develop some notion of what constitutes said movement. In neither case did I intend to find answers or to develop complete and consistent definitions. Instead, I intended to frame the information I found within my personal context. This context, which is based on my ambitions, is described in further detail below.

Moving towards more mundane purposes, the research and this paper were also intended to help develop my scholarly skills. In particular, I was interested in practicing scholarly communication and analysis. To meet these goals this document contains a wide variety of information and presentation styles. Of particular importance are the sections that include reflection. Reflection, in the sense of examining ones own assumptions, questions, and beliefs, seems to be one of the few ways to manage within a complex world. However its personal focus has excluded reflection from much serious scholarship. In my experience very few scholarly publications include reflection, informal or otherwise, and it is generally not taught as part of a university education. I am trying to rectify this oversight, in the context of my own education, by including reflection in this paper.

The reader is likely to notice two fairly distinct writing styles. The first is a more academic style. In this instance 'academic' refers to such practices as speaking in the third person, not addressing the reader, and to a more formal use of language and syntax. This style is associated with the analytical sections of this paper. The second style is more relaxed and narrative in nature. This style was chosen because it is how I personally tend to think and present myself and is usually associated with reflections and synthesis.

Ambitions

My primary research goal, as described above, was to investigate the issues facing the systems movement. However I was not interested in developing a complete enumeration of these issues. In the longer term, my ambition is to teach systems concepts to engineering students. I currently feel that the most appropriate way to introduce these concepts is to present "big picture" issues first. Application areas and domain-specific concerns should be presented later. Accordingly, I am primarily interested in topics such as:

- What is ‘systems’?
- Where did ‘systems’ come from?
- What are the philosophies, ontologies, epistemologies, models, and methodologies that underlie ‘systems’?
- Where is ‘systems’ going?
- Who is teaching systems and how are they doing so?

Having discussed my areas of interest, I should mention those areas that I am currently identifying but not actively pursuing. These areas include:

- Discussions of particular methodologies;
- Applications of particular methodologies to particular areas; and,
- Case studies.

My current belief is that while specific examples are important, in that they can provide context and grounding to a discussion, reflecting on underlying concepts is what separates the novice from the expert. My understanding of the original goals of the Doctor of Philosophy degree supports this assertion. Having completed a number of courses that discussed specific systems methodologies and provided case studies describing their use, my interests have turned towards these deeper issues.

Organization

This paper is organized into three sections. The first section both summarizes and synthesizes a selection of journal articles that I found relevant to my interests. The second section contains my reflections on systems concepts and thinking. The final section lists many of the more interesting sources that I encountered during my research. This section is not intended to be exhaustive or complete. Instead it demonstrates which areas of the systems literature I focused on and, hopefully, provides the reader with avenues for future investigation.

Readings

Detailed Discussions

This section discusses in detail a selection of the papers that captured my interest. The papers all discuss broad or fundamental issues relevant to the systems community. The format for the detailed discussions consist of a summary of the paper, an analysis of the paper's structure, and concludes with a synthesis of the paper in a wider context. The definitions of the discussion format provided below are heuristics. The boundaries between the three sub-sections are porous, and some mixing of content is inevitable.

The summary provides sufficient detail that the reader should not feel that they have to read the original paper. The summary style is a sequential walk through the source paper that identifies key elements and arguments. Ideally these items were identified from the structure and format used by the original author. If that was not possible, these items were chosen based on my interpretation of what information was key and what was supporting evidence or an aside. Whenever possible succinct quotes from the source papers were used in the summary as opposed to a rephrasing of the source material.

Some of the papers that discuss broad issues provide their own summaries. These summaries tend to follow a particular path through the historical development of a portion of the systems field. Where possible this paper uses tables to provide a "summary of a summary" so as to avoid undue length. For source papers that contain summaries the reader is encouraged to examine the original work.

The second sub-section is a critical analysis of the source paper. "Critical analysis" in this instance refers to examining the paper for such characteristics as:

- logical consistency;
- the use of references;
- the explicit acknowledgement of assumptions; and,
- the use of consistent definitions.

This form of criticality could be called "academic criticality", as the items listed above are stereotypical of quality academic papers in the modern scientific community.

Synthesis, the final sub-section, addresses the implications of the source paper on the wider systems community and on my research. Where the other two sub-sections discuss the papers in isolation, this sub-section focuses on the paper's relationships. This part is also significantly more subjective, as it ascribes motives to the authors and proposes wider implications of their work.

This chosen format affects the readability and ease of comprehension of this paper. It is likely that the reader will repeatedly cross-reference between the summary and the other two sections. While this cross-referencing may disrupt the reader's flow, I feel that this disadvantage is offset by two of the format's positive characteristics. Separating the sections also separates my positions and thoughts from those of the source author. Many times when I read a journal paper for research purposes I get annoyed at not being able to easily make this separation. I would prefer not to have to tease original thoughts from a mass of quotations and references, and have structured this paper accordingly. There is a place for the integrated narrative in academia but that place is not here. The second advantage of the chosen format is that is allows me, as researcher, to practice the scholarly skills of summarizing, analyzing, and synthesizing. A side effect of my practicing these skills is that it demonstrates to the reader that a rigorous academic process was followed in the production of this work[1].

[1] Of course this only works so long as the reader accepts a similar definition of rigour.

Systems and Scholarship: The Need to do Better

Author: Peter Checkland
Journal: Journal of the Operational Research Society
Reference: 1992, Vol. 43, No. 11, pp. 1023-1030

Summary

In this article Checkland discusses what he considers to be the state of the systems field. His analytical tool is a notion of 'scholarship', and the theme he presents is "that our field [systems] is intellectually in an extremely primitive state compared with the intellectual state of established scholarly fields."

Checkland immediately moves to address the excuse that systems is an extremely young intellectual phenomenon. He identifies the field's starting points as the seminars presented by von Bertalanffy in 1948. He compares systems' 40+ years of development with the development time of the Western intellectual tradition, which he traces to the pre-Socratics in the 6th century BC. Checkland accepts that obvious difference in development time between the two traditions should be reflected in differing maturities. To justify continuing his critique he appends to his original theme by stating that "my feeling is that our scholarship is more primitive than it ought to be *at this stage of its development*[2]."

Checkland investigates three areas to support his theme. These areas are the exposition of ideas, discussions of epistemology and ontology, and style and tone.

With regards to the primitiveness is the exposition of systems ideas, Checkland's primary evidence comes from the numerous differing accounts of the fundamental systems ideas that are found in the systems literature. He claims that "the way in which [authors] present these most fundamental systems ideas, and the order in which they do so, [is] a considerable jumble." Checkland presents excerpts from three systems authors to support his assertion.

Turning to the issue of primitiveness in epistemology and ontology, Checkland begins by stating that "we have not yet been very successful in...making clear the relation between the epistemology of the subject area and the ontology of the perceived world with which it is concerned." He provides a more

[2] The emphasis is mine, not Checkland's.

accessible interpretation of this statement by further stating that "the difference between *is* and *may be taken to be*, is very important."

Checkland's evidence for this assertion comes from his experience as a teacher, and from similar experiences of teachers working at the Open University. "Unfortunately for the students, however, nowhere in the material of these courses was acknowledged the problem of using the word 'system' as a label for something in the world, for example, and air-traffic control system, and also using exactly the same word as an epistemological device with which we may try and make sense of the observed behaviour of real world air-traffic control systems."

Checkland contrasts the state of ontological affairs in systems with that of History, which he considers to be a more established discipline. He cites from a number of different sources that discuss the Renaissance. "Real historians" take extreme care to differentiate between actual historical events and the labels, in this case 'Renaissance', which they use. Conversely textbooks, which presumably Checkland feels do not represent advanced scholarship, describe the Renaissance as something that actually happened. The explicit differentiation between epistemological and ontological statements by the "real historians" is representative of Checkland's view of advanced scholarship.

The section of the critique concludes with a proposed direction for future systems research. In Checkland's opinion, "we should be trying to deliver findings from that process [of mapping holons onto perceived real-world happenings] that are in some sense testable, though not usually with the strength of the tests in empirical natural science."

Checkland's final critique of the state of systems scholarship addresses the field's primitiveness in style and tone. He feels that ideological statements, such as "the Systems idea is intrinsically emancipatory", are not appropriate. As evidence he proposes that systems ideas could be used by any regime, for example the Nazis, for non-emancipatory purposes. A related concern is that members of the systems community are self-proclaiming the importance of their work to the field. He feels that decisions on the merit and importance of a scholarly work should be made by the community, not by the authors themselves.

In his conclusion Checkland states that "what we need is more hard-thought scrutiny of our ideas. We need to be more intellectually disciplined than we have been in this field. We need to pay more attention to mapping our particular epistemology onto the real world. We need rather less running for cover under the ideological umbrellas..."

In addition to the three core areas that comprise his critique of scholarship in systems Checkland includes a number of interesting asides and common threads. He presents a partial solution to his critique of the exposition of systems ideas, provides a recurring definition of the work systems thinkers should be pursuing, discusses the concentration of systems departments in Britain, and declares the original goals of general systems theory to be untenable.

To "escape the confusion" of different expositions of systems ideas, Checkland uses an excerpt from Checkland (1981). He declares that underlying all of the various lists of systems ideas is the notion of systems as adaptive wholes. Further, he agrees with Koestler's use of the term 'holon', "conceding the word 'system' to everyday language where it is now so shop-soiled that we shall never recover it as a useful technical term."

He continues from this definition to present a recursive structure that he feels coherently expresses systems ideas. The structure is excerpted from Checkland (1991). Based on the recursive nature of the structure he concludes that "there are no absolutes in our epistemology; as systems thinkers we are virtually driven to a process view of the world."

Throughout the paper Checkland reiterates a particular notion of the role of the systems thinker. This role is described in the following quotes:

- "Finding out whether, or how, real world systems map this concept is one sharp definition of the work systems thinkers should do."
- "what we ought to be doing, as would-be scholars in the systems field, is establishing the evidence for mapping the perceived world and its systems with particular 'holons'"
- "This is the failure to be scholarly in the sense of devoting our best attention to the processes of trying to map the epistemology onto the happenings in the real world."

Checkland mentions in passing that there seems to be a higher concentration of departments of systems in Britain than elsewhere in the world. While he acknowledges that there are a number of similar departments in the United States, he feels that those departments are more independent than those in Britain, which have grouped themselves together. As he discusses the history of the British presence in systems, Checkland comments on an apparent "drift away from the pure notion of a Department of systems as such." He continues by stating that "This is not surprising because we can now see that the expectations of the original pioneers of systems thinking, in the shape of a general systems theory, have not been met." His evidence for this statement is the apparent trade-off between generality and quality. He concludes this aside by saying that "the systems meta-language is too abstract to bring together the specific idiosyncratic features of problems in different disciplines."

Analysis

In this paper, Checkland uses scholarship as his primary analytical tool. Curiously he does not present an explicit definition of scholarship. The reader has to deduce Checkland's definition from the criticisms he makes of the systems field. For Checkland, scholarship seems to be defined as an academic practice that:

- makes use of a small set of rigorous core definitions;
- has developed a common progression of facts to introduce new practitioners to the field;
- clearly separates its models from that which is being modeled;
- devotes its energies to demonstrating the applicability of its models to real world happenings;
- is divorced from ideology;
- demonstrates intellectual rigour; and,
- evaluates its ideas using hard-thought scrutiny.

Based on this implicit definition of scholarship, Checkland seems justified in his critique of the systems field. However Checkland has biased his paper by forcing the reader to deduce the definition. Good scholarship is, by this definition, everything that systems is not doing.

Checkland's definition of scholarship is very similar to the contemporary definition of science. The similarities include having a notion of a separate model and reality, the focus on applicability, the removal of ideology, and the use of terms such as 'rigour' and 'scrutiny'. While Checkland is willing to accept testing criteria that are not as 'strong' as those in the natural sciences, he still feels that testability is of key importance. The implication in the paper is that the definitions of 'rigour', 'scrutiny', and 'strong' used by the systems field should be similar to those of the Western sciences. Based on these definitions, practitioners of systems should adopt mathematics, Aristotelian logic, and empiricism as their primary tools.

Checkland does not consider alternative notions of scholarship that may be applicable to the systems field, such as those used in the arts or the humanities. He also neglects to discuss findings from disciplines such as philosophy of science, sociology of science, and post-normal science that directly challenge some of his notions of scholarship. These disciplines question whether it is possible to separate the model from the modeled, whether ideology can be divorced from human activities, and whether scientific rigour actually exists. While many of these finding have come to the fore after Checkland wrote this paper, they were being discussed in the literature at the time this paper was written and are deserving of comment.

Given that he mentions explicitly the "groundless of systems thinking" and the importance of perception in the field, Checkland's desire for uniformity in definition and education seems contradictory. He provides one possible expression of the systems epistemology based on his perceptions, but does not acknowledge that there are other equally valid definitions. Given that Checkland developed the Soft Systems Methodology, where the notion of having multiple valid perspectives is pervasive, it is difficult to understand why he seeks uniformity.

Ultimately, Checkland seems to be interested in being "taken seriously by scholars in other fields." This desire may explain some of the contradictions identified earlier. It is likely that the scholars in these other fields will only take systems seriously if it adheres to the same standards that they are used to. Although he only uses the term science once, Checkland seems to see systems as a type of science. To be accepted "by the family" systems therefore has to exhibit the scholarly characteristics of science.

When he discusses the notion of a general systems theory Checkland adopts another implicit model. This model treats generality and content as conservative duals. Checkland is quite correct, using this model, to say that "you pay for generality with lack of content", and to conclude that general systems theory cannot meet the expectation of its pioneers. Checkland does not question whether his model is universal, or applicable to the systems disciplines. Hierarchy theory might suggest that this model mixes concepts that exist at different hierarchical levels. General system theories exist at one level of abstraction, while disciplines that focus on particular phenomena exist at another. By definition they should not be compared directly, and the critique of the general theories is therefore unfounded.

In terms of the evidence Checkland provides for his overall critique, his references are somewhat dated. They span the period from 1963 to 1972 and end 20 years before his paper was written. Based on other more recent works in the field, for example Flood and Carson (1988), the trends he identifies in the areas of core definition and introductory sequencing have continued. It appears that the lack of contemporary references does not weaken his positions.

Synthesis

Peter Checkland has been associated with the systems field for a long time and has a distinguished academic pedigree. His opinions carry a lot of weight, especially among those who read and produce systems literature. While his statements on systems' scholarship are important, what is more important is that he has publicly stated that systems should uniformly adopt an established definition of scholarship. Using terminology from post-normal science, Checkland has stated that systems

should move from being a sympoetic field to being an autopoetic one. Instead of allowing, and possibly even promoting, a free-for-all of ideas and interpretations, Checkland believes that the field should decide on a set of fixed notions and should build a body of work that supports or refutes them.

Checkland identifies two apparent tradeoffs in his paper. Explicitly he trades off between theoretical generality and practical utility. Implicitly he trades off between efficiency and adaptability. In both cases he does not question whether the tradeoff can be avoided. These particular tradeoffs are discussed so often and are so prevalent, both in systems and in popular culture, that they are almost truisms. As mentioned earlier hierarchy theory offers an alternative perspective on these issues. Unfortunately there does not seem to be much investigation into such alternate perspectives or into finding counter-examples the refute the tradeoffs. In complex systems, and especially social systems such as the research community, exceptions to rules that seem absolute can generally be found. In some cases it is possible to replicate these exceptions, in essence changing the rules. If researchers can find examples of organizations that exhibit both adaptability and efficiency, and I believe that some must exist, there is a chance that similar organizations could be designed. I agree with Brown & Dunguid (2000) that transplanting information between organizations is extremely difficult. However I also agree that once people are shown that an impossible standard can be attained, that standard soon becomes the norm.[3]

Accepting for the sake of argument Checkland's position on the utility of general system theories, the question arises as to whether they should be studied as independent subjects. An alternative approach would be to incorporate studies of such general theories into different disciplines. Each discipline would then be able to customize the theories to suit their applications. While this allows for greater relevance, it removes the possibility for the valuable study of isomorphisms. Many of the applied theoretical disciplines have adopted a hybrid approach which may better suit the general system theories. Statistics, along with calculus and probability, are taught in many disciplines and sub-disciplines. Civil engineering students learn statistics different than do students of management science or of philosophy. At the same time there are groups devoted to the study of statistics without reference to particular applications. These groups are generally seen as sub-disciplines, in this case of applied mathematics, not as full disciplines. Nevertheless value is still perceived in their study, as their results can in many cases be applied across the entire spectrum of applications. The experiences in the United Kingdom seem to have demonstrated that dedicated institutes do not work in the case of general system theories. It remains to be seen as to whether a hybrid approach would be more

[3] Examples of this phenomenon include the four minute mile and much of the computing industry, especially in the area of computer graphics.

effective. One problem that would have to be addressed up front is what discipline would act as parent? By definition a general system theory fits at the top of any disciplinary hierarchy that is based on generality.

Looking at the current activities in the systems field reveals that Checkland's call for consistency and the use of established definitions of scholarship has not been heeded by systems practitioners. The major systems journals continue to cover a wide breadth of topics, and organizations such as the ISSS continue to struggle to develop definitions along the lines of those suggested by Checkland. It is unclear as to whether this is because the field continues to expand or because of a desire among practitioners to remain flexible and eschew static traditions.

In the specific area of SSM a number of Checkland's points have been addressed. Checkland has published a number of books and papers that expand on his core definition of systems thinking. SSM is also moving out of the research lab and into practice and wider education. What is interesting is that SSM is a difficult methodology to validate. Because it generally stops before the design and implementation stages of problem solving, its deliverables are usually changes in the perceptions of the participants. This kind of deliverable cannot easily be tested according to the dictums of scientific scholarship, which presumably would be of concern to Checkland.

By focusing on traditional scholarship Checkland ignores much of the research being done within the systems umbrella on alternative definitions of science and scientific legitimacy. Many of those who support critical and multi-participant approaches would likely take exception to having their work judged on the basis of Checkland's definitions. Cultural Theory and Post-Normal Science are two examples of such approaches. In both approaches issues such as the nature of peer review, the explicit incorporation of values and beliefs, and the acceptance of multiple valid perspectives are raised. These issues do not fit under classical scholarship's umbrella. Checkland is in the interesting position of supporting a perspective that his own discipline increasingly sees as overly restrictive and of questionable value.

Given my personal focus on education, Checkland's article is inspiring. I feel that many of his criticisms could be addressed if the systems community devoted some time and effort to education. This is not to say that I agree with the direction Checkland proposes for the discipline. Instead, I feel that the issues he raises are important and that practitioners of systems must be aware of them. Many practitioners are not exposed to discussions of the philosophical, ontological, and epistemological premises that underlie systems. Similarly, I believe that very few systems thinkers reflect on why the different definitions of systems exist and what their existence implies about the

discipline. Different kinds of courses are required to support these activities. People need courses that cover the complete spectrum from definition through method, methodology, and philosophy. In the broader sense education is about providing people with the opportunity to choose their own paths. Education should describe existing paths and beliefs so that informed decisions can be made.

A System of Systems Approaches

Author: Heiner Müller-Merbach
Journal: Interfaces
Reference: July-August 1994, Vol. 24, No. 4, pp. 16-25

Summary

In this article Müller-Merbach presents a typology of four systems approaches. The approaches are presented in a series of fictional dialogues patterned after those stereotypically associated with Asian philosophy. Müller-Merbach's stated purpose for the article "is to provide an understanding of the broad variety of systems approaches as a basis for a methodological pluralism."

The article begins by describing the presence of the systems approach among various disciplines. Müller-Merbach states that "the system approach seems to be a constituent property of modern science, contemporary humanities, design methodology, and meditative contemplation.", but questions whether we have entered "the systems age."

He then provides some basic background information and a description of the systems approach. The philosophical roots of the approach are described as coming from the Greeks (Plato, Aristotle) and the Asians (Lao-tse). He gives the bible is given as an example of an important work containing "systems-thinking wisdom." For Müller-Merbach the systems approach "focuses the consideration of wholes and of their relations to their parts." As descriptive adjectives he uses terms such as 'comprehensive', 'holistic', and 'interdisciplinary'.

The paper now turns to a discussion of the different types of competing systems approaches and to different typologies that have been used in other discussions. Müller-Merbach explicitly mentions the systems approaches used by Churchman (social system design) and Simon (sciences of the artificial) and implies that there are a significant number of other authors with alternative views. He then cites the categorization schemes of two particular authors, Bunge and Bahm. Their schemes are summarized in Table 1.

Table 1 - Categorizations of the Systems Approaches

Bunge	Bahm
• atomistic • holistic • systemic	• atomism • emergentism • organicism • emergentism • holism

Müller-Merbach rejects these categorizations. In their place, he presents a shallow[4] hierarchical typology that distinguishes at the highest level between the 'systematic' and the 'systemic'. The systematic approaches are those that are closely connected to stereotypical Westerns science whereas the systemic approaches are connected to Eastern traditions. For Müller-Merbach the primary difference between the two is the intended use of the results. Western results are intended to be shared interpersonally whereas Eastern results are intended solely for the individual. He does acknowledge that "this hierarchical typology is as artificial as any typology", but feels that by being artificial it "casts light on the differences between and the characteristics properties of large, identifiable groups of systems approaches."

The paper then proceeds to Müller-Merbach's hierarchical typology of systems approaches. The typology consists of four categories: introspection, extraspection, construction, and contemplation. Each of the four categories are presented in identically structured dialogues between a student and a master. The dialogues are broken down into the following sections:

- a basic description of the approach;
- a discussion of the application of the approach to four examples (a written text, a plant, a person, and a social organization);
- an explanation of the role of the researcher; and,
- a discussion of the requirement for interdisciplinarity.

While the approaches differ, the descriptive dialogues all share many common traits. For example each approach applies equally well to all of the examples and each requires an interdisciplinary focus. Müller-Merbach summarizes the differences between the approaches in a table that is reproduced in Table 2.

[4] In the organizational, not intellectual sense.

Table 2 – Morphology of the four types of systems approaches

	Systems Approach			
Morphology	**Introspection**	**Extraspection**	**Construction**	**Contemplation**
Enlightenment reduced to	Knowledge	Insight	Acquaintance	Understanding
Orientation	Causality	Finality	Pragmatism	Internalization
Principle	Analysis	Synthesis	Creation	Holism
Method	Reduction	Integration	Design	Meditation
Subjectivity	Rational distance	Individual relation	Responsibility	Identification

The following four tables provide additional details on each approach that are not included in Müller-Merbach's summary.

Table 3 – Characteristics of Introspection

Patterned after	Natural science
Technique	Recursive division, ending in comprehension
Keywords	Reduction, analysis, introspection
Interdisciplinary?	Each subdivision requires different knowledge to understand, therefore a interdisciplinary group is required.

Table 4 – Characteristics of Extraspection

Patterned after	Humanities
Technique	Successive insertion into context, ending with a complete picture
Keywords	integration, synthesis, extraspection
Interdisciplinary?	Each context is represented by a different discipline, therefore a interdisciplinary group is required.

Table 5 – Characteristics of Construction

Patterned after	Engineering and management science
Technique	Simultaneous division and insertion ending with a feasible design
Keywords	design, creation, construction
Interdisciplinary?	Yes, since it is a combination of intro- and extraspection, which are both interdisciplinary.

Table 6 – Characteristics of Contemplation

Patterned after	Eastern philosophy
Technique	Identify with
Keywords	holism, meditation, contemplation
Interdisciplinary?	On an individual level, since all parts of the researcher are involved. Also, there are no disciplines as a discipline is a division of knowledge.

Having completed his descriptions, Müller-Merbach discusses some of the ramifications of his typology and of his choice of presentation. He explicitly cautions readers not to assume that any one approach is superior to any other. Each approach "has its own rationality, its own justification, its own strengths, its own fascination, its own appeal, and its own community." Identical, self-contained dialogues were used in the descriptions to ensure that no one approach seemed more valid. As Müller-Merbach says, "how can one not agree with any of the four masters?" However the style also demonstrates each approaches limitations. In each dialogue no other approaches were mentioned, as it was assumed that the approach in question was sufficient. In contrast, Müller-Merbach states unequivocally that "professionalism in the systems approach requires mastery in all of them."

The paper concludes with a brief discussion of, and plea for, methodological pluralism. Müller-Merbach feels that most systems literature discusses only a single methodology. On occasion authors may identify alternative approaches, but they do so in order to demonstrate the superiority of their chosen approach. Given the breadth and complexity of the problems being addressed using systems approaches, He feels that "…quite a few problems may justify a combination of different systems approaches."

Analysis

In this paper the object of discussion is alternatively the "systems approach" or "systems approaches." In the introductory section of the paper the object of discussion is the singular "approach". After touching on its origins, the systems approach is defined simply as an approach that "focuses on the consideration of wholes and their relations to their parts." No explicit mention is made of the approach's intended purpose, which could conceivably range from understanding, to acceptance, aesthetic appreciation or personal enlightenment.

Interestingly, the origins of the approaches are cast as philosophies, not as methods. Later on in the paper the discussion turns to methodological, not philosophical issues. While philosophy and methodology are intertwined, the paper does not make explicit which aspect is being discussed and

why the focus changes. Further, the paper does not include explicit definitions of terms such as method, methodology, or philosophy, that form the basis of the discussion. Overall there is little evidence that Müller-Merbach has reflected on the relationships between his terms and this lack of reflection makes it difficult for the reader to follow the flow of his arguments.

In saying "there are several types of systems approaches around", Müller-Merbach is stating an assumption. However he does not challenge this assumption by investigating whether or not the approaches are actually different. His evidence for their being different is expressed only in the sentence "But still other authors [then Churchman] have quite a different understanding of the systems approach." It is possible that instead of there being multiple approaches that there are different understandings of a single approach. Müller-Merbach does not pursue this avenue of investigation. In keeping with the philosophical focus of this section of the paper the different understandings of the systems approach are presented as, for example, the "philosophy of the sciences of the artificial" and "the philosophy of social systems design."

As the paper turns to the categories of systems approaches Müller-Merbach starts to blur the philosophical and methodological distinction. One of the typologies he presents discusses approaches (e.g. "atomistic approach) whereas the other discusses philosophies (e.g. "atomism"). He does not discuss the distinction between the typologies, nor does he discuss their utility. Instead he dismisses both of them, saying only that "I doubt that the different types of systems approaches can appropriately be classified in one dimension." He does not explain what motivates his doubt, nor does he provide any explicit direction as to how his doubts could be alleviated. The reader must assume that an increase in the number of discriminatory dimensions would be sufficient to remove Müller-Merbach's doubts.

In suggesting a hierarchical typology, Müller-Merbach is presenting one method of increasing the number of dimensions of the typology. Another straightforward method would be to create a higher-dimensional space and to situate the approaches within that space. No justification for choosing a hierarchy is given. The choice of a hierarchy is interesting because, as presented, it is composed of two stacked, uni-dimensional typologies. The first discriminates between 'systematic' and 'systemic', the second between 'division' and 'insertion'. While the number of dimensions is increased, at any given time only one dimension is in focus. This raises the question of whether the hierarchy is substantially different from the typologies it is intended to replace. If Müller-Merbach's hierarchy is collapsed, which would remove some relational information but would retain the categorizations, it is strikingly similar to one of the typologies he dismisses.

The paragraph that discusses the artificial nature of any typology is an extremely important part of this paper. Müller-Merbach claims to believe in pluralism and presenting his typology as 'correct' would contradict this belief. Instead, he reflects on his approach and manages to turn a possible weakness, artificiality, into a strength, in that it provides a context for future discussion. He also manages to recast his use of coarse descriptions as a strength. The sweeping divisions he makes provide a context for future discussions that could focus on smaller or more subtle differences. While he does engage in some reflection, there remains room for more. For example Müller-Merbach could have discussed his own background and what he was focusing on when he created his typology. The reader is left to guess why he chose the distinctions he did.

As with many of the discussions of systems concepts, rigorous definitions of key terms are not always provided. While he offers a simple definition of the systems approach, he does not define the two terms that make up the first level of his hierarchy, namely 'systematic' and 'systemic'. The reader is left to define the terms based on the characteristics of the methods that embody them. One possible set of resulting definitions is found in Table 7.

Table 7 – Possible Definitions of 'Systematic' and 'Systemic'

	Systems Approach	
Characteristic	**Systematic**	**Systemic**
Basis	Western science	Eastern cognition
Results	Interpersonal	Individual

The four dialogues, while caricatures, are both internally consistent and easy to understand. The example systems provide sufficient variety that some of the practical implications of using the approaches are discussed. There is an appropriate mix of the methodological, in the form of the examples, and the philosophical, in the form of the questions regarding the role of the researcher. Again, the switches between these two foci are implicit, not explicit.

A serious weakness of the paper is its lack of supporting evidence regarding the four approaches. The only references Müller-Merbach cites are used in the introductory sections when he makes his case that there are multiple approaches. In the narratives he presents the caricatured approaches without providing any references for further information. Usually papers that present categorizations begin each category with a list of references. These references provide extra information to the experienced reader, who can reflect on their own interpretation of the references. Novices can use the references as avenues for further study. By not linking his categorizations to the work of other academics, Müller-Merbach's paper becomes nothing more than the unsubstantiated opinions of its author. Similarly terms such as "Western tradition" and "Eastern philosophy" are used with neither

definition or reference. While using stereotyped descriptions of the systems approaches comes across as a reasonable simplification, presenting unsubstantiated cultural stereotypes does not.

The use of an identical narrative structure makes it easier for the reader to compare and contrast between the different approaches. The format seems reasonable given Müller-Merbach's goal of providing a context for discussion. However the use of a constraining structure seems at odds with the notion of pluralism. Given that each approach embodies a different philosophy or perspective on systems, it would have been instructive to see how each approach discusses itself. Müller-Merbach's has implicitly chosen the introspective systems approach to discuss the different systems approaches. This choice forces each approach be shoehorned, to greater or lesser degrees, to fit within that structure. For example the premise of each narrative is that of gaining enlightenment, but the constructive approach focuses on the ability to design. Similarly, the question of interdisciplinarity is not appropriate to the contemplative approach which denies the existence of disciplines.

In his summary of the four methods, Müller-Merbach engages in further reflective thought. He acknowledges that he purposefully structured the narratives to be one-sided and equally persuasive. He does not provide an explicit rationale as to why he chose to completely isolate each approach. Given that he describes the systems approach as focusing on the relations between things, his isolationist approach seems contradictory. The only relationships between the approaches that he discusses is that the intro- and extraspective approaches can be seen as opposites.

In his conclusion, Müller-Merbach makes some sweeping statements regarding pluralism and professionalism. As with many of the other terms he uses that may be contentious, he provides neither an explicit definition nor substantiative references. Professionalism seems to be defined only as pluralistic behaviour. Similarly, pluralistic behaviour seems to consist of knowing about multiple approaches. By using the term 'professional' in this context Müller-Merbach is establishing a hierarchy of ability based solely on the number of approaches an individual can apply. Is a systems practitioner familiar with three approaches therefore more professional than one familiar with two? The paper implies that the answer is yes, without taking into account other factors such as experience or depth of understanding.

Müller-Merbach's final assertion is that solving some problems may require the use of one or more of the various systems approaches. The reader is led to conclude that the systems approaches are in fact problem-solving approaches, which is not necessarily the case. One common alternative description is that systems approaches, in particular Soft Systems Methodology, are about sense-making not

problem-solving. Assuming that a problem is to solved, how and how many approaches should be chosen? Müller-Merbach offers no advice to aid in making these decisions.

Synthesis

The journal "Interfaces" is a management science journal and this paper is indicative of a recent trend in this field towards discussing pluralism. This paper is typical of in that it basically says "pluralism is good" and "pluralism is knowing about other ideas." Coincidentally the papers that critique pluralism generally follow the model is "pluralism is bad" and "pluralism is just people crying sour grapes that their method didn't work." While this paper was written in 1994, the sophistication of the debate, except in exceptional cases, has remained roughly at this level.

The general notion of pluralism seems to be solid, in that it allows for using as many, or as few approaches as required. In this sense it is a complete superset of exiting practices. There is however little evidence that in practice practitioners have ever adhered solely to a single approach. In the abstract it is impossible for this to have happened, given that each practitioner brings with them their own inescapable experiences and beliefs. I believe that what differentiates pluralism from simply using multiple approaches is the amount of reflection undertaken by the practitioner. A pluralistic approach is one where the practitioner is aware of their options and has made a conscious choice to adopt or reject an option as they saw fit. That having been said, it is likely that there is some minimum level of understanding required before an option can be chosen or dismissed. In this paper Müller-Merbach attempts to describe both the practice and the philosophy of each approach. I feel that a knowledge of both aspects is required for an informed choice to be made.

The use of the terms 'systematic' and 'systemic' was interesting given that the shift to these terms has occurred recently and across a wide spectrum of the systems field. At the ISSS 1999 conference, there were very few mentions of the "systems sciences." Instead the participants referred to the "systemic sciences." Even a large systems journal changed its name from "Systems Practice and Action Research" to "Systemic Practice and Action Research." I have yet to determine why this shift in name and terminology has taken place. The term 'systematic' is also extremely interesting in that it has an extremely negative connotation within the systems community. Again using the ISSS 1999 conference as an example, a paper describing a 'systematic' method seemed to be considered less highly by the participants than a paper that described a 'systemic' approach. In choosing to classify many of the current systems approaches as being systematic, Müller-Merbach has rhetorically insulted many systems practitioners.

By linking 'professionalism' with 'pluralism' Müller-Merbach has implicitly stated that a sympoetic state of affairs is to be preferred over an autopoetic one. Interestingly, this is the opposite approach to Checkland who seems to favour an autopoetic discipline. I am not sure where I stand on this issue. It is difficult to say that an entire discipline, or in Müller-Merbach's case those practitioners of a discipline who are more elevated in some way, should follow a single approach. There is a place for those open to multiple perspectives and new information and a place for those who have chosen to adopt unreservedly a single perspective and to manipulate existing information. If both attitudes cannot be found in the same individual, and I question whether this is indeed the case, then it seems sensible to ensure that both attitudes can be found in the larger community. The appropriate division is open for debate, and will likely change over time. Nevertheless positive connotations should be established for both ways of viewing the world. Müller-Merbach's choice to emphasize one perspective over the other is misguided if taken to any extremes.

Beyond the dialogues and the brief discussion of pluralism, this paper's only other interesting contribution is its brief discussion of the history of the systems approach. Where Checkland gives systems a 40+ year history, Müller-Merbach traces systems thinking back to the Greeks. Coincidentally this is the same origin Checkland gives for Western science. The contradiction between the two papers is interesting because it speaks to a lack of reflection on the origins and meanings of systems thinking. This is not to say that there is one right answer. Instead, I continue to believe that attention must be paid to broad systems education so that practitioners can make their own comparisons and inferences about the origins of the field. Personally I think that systems-oriented thinking pre-dates Western science. For example the polytheism of India and Greece, that explains many natural phenomena in terms of the relationships between various deities, can be seen as an example of systems thinking.

This paper, if it were backed up with examples of the various approaches, would be a good introductory read. The dialogues are simple and engaging, the reflective sections sufficient to provoke discussion, and the typology provides a reasonable context for further discussion. The largest concern I have is that it is difficult for many students, myself included, to avoid to temptation to pigeon-hole. If this paper were presented to students before they were exposed to the breadth of systems approaches they might be tempted to blindly accept these categories. Conversely if this paper was presented at a later stage it might easily be seen as irrelevant. Many experienced students of systems will have developed their own categorizations and may dismiss Müller-Merbach's. So long as students are encouraged, for example through strong teaching or developed skills in critical thinking, to question the categories presented in this paper it is a good addition to an introductory systems course.

As a final thought, the term pluralism is generally found as part of the phrase "methodological pluralism." I have yet to encounter a paper that calls for or describes "epistemological pluralism" or "philosophical pluralism." While this paper does touch on philosophical issues, when discussing pluralism Müller-Merbach refers only to methodology. Given how closely intertwined philosophy, epistemology, and practice seem to be, restricting pluralism to methodology seems overly limiting.

Towards coherent pluralism in management science

Author: MC Jackson
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Summary

In this article, Jackson presents a coherent account of pluralism in management science and proposes an agenda for further research in this area. His goal is to "contribute to the orchestration of coherent pluralism in management science." In the abstract to this paper Jackson defines pluralism as "the use of different methodologies in combination."

Jackson begins by tracing what he feels are some of the historical events that have led to pluralism becoming a "topic of considerable interest in the applied disciplines these days." He starts with the statement that "the development of pluralism in systems thinking is inseparable from the rise of critical systems thinking." This rise of 'soft' approaches in the 1980s, in particular Checkland's Soft Systems Methodology (SSM), is seen to have started the questioning of the established approaches. However these new approaches were quickly critiqued, for example when Jackson claimed that "the assumptions made by soft systems thinkers…constrained the ability of their methodologies to intervene, in the manner intended." Given that the two major orthodoxies of hard and soft systems were both seen as limited, Jackson argues that pluralism became an attractive option.

Evidence is also cited from the fields of Operations Research (OR), Organization Theory (OT), and Information Systems (IS). In each case Jackson first ties the field in question to the systems movement. For example IS uses SSM in some of its design processes. The implication is that any field that uses systems techniques will be affected by systems' interest in pluralism. He also references the emergence of particular methodologies in each field that meet his definition of pluralism. His examples include "mutlimethodology" in OR, "multidimensional evaluation" in OT and "Multiview" in IS.

Jackson concludes his introduction by presenting three possible reasons for the current pluralism-oriented climate. "One is the critique that has taken place, in many of these disciplines, of traditional approaches." A second is "the prevailing fashion for 'relativism'." Finally, "pluralism seems to be necessary."

The paper now turns to a discussion of two contributions that Jackson feels can help frame the continuing discussion of pluralism, both in his paper and in the wider community. The first is a paper he wrote in 1987 that proposed four categories of pluralism. These categories, and their salient characteristics are summarized in Table 8.

Table 8 – Four Types of Pluralism

Category	Characteristic(s)
Isolationism	• A single approach is sufficient • Nothing can be learned from other perspectives
Imperialism	• One approach dominates • Other perspectives may be used if the strengthen the dominant approach
Pragmatism	• Toolkits of methods are used • Artificial theoretical distinctions are ignored
Pluralism	• Respect the differences of the approaches • Development of a Meta-methodology to guide practitioners to the best approaches

The categories were originally developed as possible directions for the evolution of the management science discipline. Jackson provides a short critique of each category, except for pluralism which he sees as being the best avenue to pursue. His critique of pragmatism is the most interesting, as it does not relate to the practical effectiveness of the approach. Instead he claims that "theory, which pragmatists eschew, is necessary…so that we can pass on knowledge to future generations."

The second contribution Jackson cites is that of Mingers and Brocklesby from 1996. They also provided a categorization of pluralistic approaches. Jackson uses a portion of their results to further sub-define his 'pluralism' category. Their distinctions within this category are summarized in Table 9.

Table 9 – Additional Categories Within Pluralism

Category	Characteristic(s)
Methodology selection	• Whole methodologies are used • Selection is based on the best fit of models, methods, and techniques
Whole methodology management	• Whole methodologies are used • Multiple methodologies are used in the same intervention
Multiparadigm multi-methodology	• Portions of a methodology are used • Easily relapses into imperialism or pragmatism

Jackson now turns to a historical discussion of pluralism in the domain of management science. He identifies four landmark events that he feels support the emergence of pluralism in this field. The first event so identified is the publication in 1984 of *Multiple Perspectives for Decision Making* by Linstone and "Towards a System of Systems Methodologies" by Jackson and Keys. Jackson feels that prior to these publication the discussions of combining methods or methodologies were based on the imperialist model. Interestingly, his one critique of Linstone's work is that it too is imperialist, although at the level of paradigm, not methodology. When discussing his work with Keys, Jackson notes that it explicitly "suggested that pluralism needed to be based on methodologies which were developed from more than one paradigm." However the paper is critiqued for failing to distinguish between 'methodology' and 'method', and for promoting only methodology selection.

The second landmark Jackson identifies is the development of "Total Systems Intervention" (TSI) by Flood and Jackson in 1991. It is described as a meta-methodology that consists of three stages: 'creativity', 'choice', and 'implementation'. 'Creativity' refers to making sense of the situation, after which a 'choice', drawing from the system of systems methodologies, is made as to which methodology or methodologies to use. 'Implementation' is simply the use of the methodology to tackle the problem situation. Jackson feels that "the contribution of TSI is to postulate a meta-methodology for using methodologies adhering to different paradigms in the same intervention on the same problem situation."

Jackson identifies three flaws present in TSI. The first is that TSI purports to stand above the various systems paradigms. However if TSI is seen as a paradigm on its own, then it is in fact not a pluralist paradigm, as it will be present and unchanged in all interventions. The second flaw is that TSI "which demands multi-methodological competence and various ethical commitments, clearly asks a great deal from would be users…" Jackson presents evidence that individual practitioners have significant

cognitive difficulties when they are asked to shift their methodological allegiance. TSI's final flaw is that in practice it mandates the use of whole methodologies. While Jackson claims that TSI supports the use of a selection of methods from different paradigms, he acknowledges that in practice this does not happen.

Jackson's third landmark is the gradual acceptance in management science of mixing methods and tools from multiple methodologies. He notes that this acceptance is most advanced in OR, and cites a number of examples from 'soft' OR practitioners. Based on these examples, Jackson criticizes this approach, noting that "they [the different methods] are all managed under the 'imperialism' of the interpretive [soft] paradigm."

The final landmark in the establishment of pluralism in management science is the alignment of pluralism with postmodernism. He cites a number of systems authors, including Checkland, who link their approaches to postmodernism. Postmodernism, in Jackson's estimation, is "opposed to the totalising endeavours of the 'grand narratives' and committed to promoting 'difference' in a world which…we can no longer represent with the certainty provided by the old paradigms…" However in practice Jackson feels that postmodern practices are "associated with an almost inevitable relapse into pragmatism" as the practitioners tend to focus on method, not on methodology or paradigm.

Jackson now sets out to define what he feels are the future requirements for pluralism in management science. He begins by amending his earlier, practice-focused definition of pluralism. "The point of pluralist thinking and practice is to make the best use of the tools and methodologies [developed by management scientists] by using them in a way that allows us to continuously improve them (do research) and, at the same time, improve our ability to tackle diverse and difficult problem situations." Having broadened his definition of pluralism to include theory and study, Jackson presents three requirements for its adoption.

The first requirement is flexibility. In Jackson's words, "The pluralism needed, therefore, is one that recognizes…that methodologies can be decomposed and that the link between the traditional host methodology and the methods, tools and techniques usually associated with it, need not necessarily be a close one." In keeping with his focus on theory he does caution that "We have to be careful however to resist relapse into pragmatism." He also espouses the belief that testing a method requires that it be used under the guidance of a single paradigm, although that paradigm does not have to be the one which originally developed the method.

The second requirement is that pluralism be present at all stages of an intervention. "…methodologies owing allegiance to different paradigms should be employed in the same intervention and at all stages of that intervention, unless good reasons are given for temporary relapse into imperialism." Jackson feels that pluralistic activities tended to cluster together during an intervention, for example during the implementation phase, as opposed to being distributed throughout. Jackson ties this requirement into theoretical knowledge by stating that "If such theoretical understanding is neglected then…methodologies owing the allegiance to the same paradigm could be employed together in the mistaken belief that 'genuine' pluralism was being observed."

The final requirement for pluralism is that "pluralists must learn to live with and manage some degree of paradigm incompatibility." He rejects the notion that a meta-paradigm containing all other paradigms is a reasonable solutions. In his words, "One paradigm pluralism is simply not pluralism." However he is comfortable with the notion of a meta-methodology, so long as it does not constrain diversity.

Having presented his requirements for pluralism, Jackson sketches out his vision of a pluralistic practice. At the level of tools and techniques, his vision is one where practitioners have the freedom to choose. However in keeping with his emphasis on theory, "they [the tools] should always be employed according to a methodology serving a particular paradigm." With regards to methodologies Jackson's position is similar. In this case he is concerned that the methodologies be linked to particular paradigms, so as to ensure paradigmatic diversity. Finally, with regard to meta-methodologies, Jackson recasts TSI as "critical systems practice." This practice is expected to promote diversity, accept difference, and ensure that "no paradigm is allowed to escape unquestioned." He attempts to avoid having the practice become a meta-paradigm by having it "seek to manage the paradigms not by aspiring to meta-paradigmatic status, but by mediating between the paradigms."

This paper concludes with a concise description of the conditions under which coherent pluralism would be valuable. "Management science will get the greatest benefit from pluralism as an approach to managing complex problems when it employs a meta-methodology to take maximum advantage of the benefits to be gained from using methodologies premised upon alternative paradigms together, and also encourages the combined use of diverse methods, models, tools and techniques, in a theoretically informed way, the ensure maximum flexibility in an intervention." Jackson realizes that what he is proposing is a fundamental shift in the practice of management science and identifies what he believes are the crucial factors required for success. "Crucial to the success of critical systems

practice…will be challenging the 'cognitive' and 'cultural' constraints…educational and training programmes will have to be made available."

Analysis

In this paper Jackson presents what he sees as an obvious progression of interest and activity in pluralism. This mode of presentation is common in many scientific and historical texts. It suffers from the weakness in that it does not present a balanced view, as other paths and explored dead-ends are never discussed. In this paper the reader is led to assume that pluralism has been steadily and implacably graining momentum. Given the number of references to his own work, Jackson comes across as someone who is furthering their own research agenda and who has interpreted history to suit his own motives.

That having been said, Jackson remains a prominent systems thinker. He has already had significant influence on the systems movement. However his attempt to extend his view to the large context of the "applied disciplines" is a stretch. Pluralism is not a common topic in engineering, which can be seen as an applied discipline, nor in medicine. This is not to say that pluralism does not apply to these areas, only that there is little evidence that it is being investigated. Taken within the context of management science, Jackson's position and assertions may be both accurate and relevant.

The three reasons Jackson gives for the rise of pluralism do not uniquely support his conclusion. In academia critiques are always leveled against prevailing orthodoxy. He provides no evidence that the current critiques are more in number or in quality than those that came before. Similarly, there is always some degree of incommensurability between problem solving techniques and the problems being solved. Again, he provides no evidence that the situation is different than at any other point in the history of management science. Using Kuhn's model of scientific progress, there is no evidence that a crisis point has been reached in management science that requires the emergence of a new paradigm.

The third reason Jackson cites deserves special attention. His claim is that the current fashion for 'relativism', linked in some undefined way with postmodernism, promotes pluralism. There are two assumptions being made in this sentence. First, that there is a link between relativism and pluralism. Unfortunately, Jackson does not define relativism, saying only that it is related to postmodernism and "against 'totalising' discourses that claim to know the truth about things." Postmodernism itself is a singular paradigm, in that it rejects all notions of truth, be they absolute or merely sufficient to meet a need. Claiming that such a singular paradigm promotes pluralism, given his later critique of TSI for similar reasons, seems to be an important contradiction. Jackson's second assumption is that

the "current fashion" is either important enough, or possesses enough staying power, to warrant a reshaping of management science in its image. This is a powerful assumption, especially given the transient connotations of the term 'fashion'. Jackson also uses statements such as "to be in tune with the times we must…", which embody similar sentiments. His only justification for promoting change at this instant is that he feels that many applied practitioners, which he distinguishes from theoreticians, are already pursuing some degree of pluralism. Given that he feels that theory and practice must be linked, the choices seem to be to rein in the practitioners or to advance the theory to meet the practice. Jackson offers many indications that he prefers the latter solution.

Pluralism is one possible reaction to these three observations. However it is not the only one. Management scientists could choose to address these issues by trying to develop even more complex and comprehensive methodologies. If a core concern is that current methodologies aren't providing useful answers, then a new methodology should be developed that does. Similarly, if certain problems aren't being solved by management science methodologies, then perhaps they should be dealt with by practitioners from other disciplines. Constraining the domain of applicability of existing methodologies is another way of ensuring that management scientists meet with success. Jackson does not address these or other possible responses to his observations, preferring instead to advocate for his chosen response, namely the adoption of pluralism.

The next section of the paper is intended to provide some basic definitions and vocabulary for the continuing discussion of pluralism. Jackson seems to prefer to partition things into fairly rigid categories. This is evidenced by both the format of this paper and by the nature of his "Towards a System of Systems Methodologies" paper. In both cases he creates rigorous typologies. Unlike some authors, such as Müller-Merbach, he does not caution the reader that his categories are heuristics and should not be accepted blindly. Given his stated belief in postmodern and pluralist thinking it is surprising that he does not include such warnings.

The categories of pluralism that Jackson presents, besides being isolationist, provide a reasonably complete coverage of the term. His critiques of the different categories however include some unacknowledged assumptions which he presents as unsupported assertions. Imperialism is dismissed "because methodologies and methods developed in the service of one theoretical position would be 'denatured' if used under the auspices of another…" Similarly, pragmatism is dismissed because it 'eschews' theory. Later, Jackson states that theory is required for both successful teaching and successful research. He provides no evidence to support any of these assertions, presumably relying on the fact that such views might be prominent among academics. His repeated distinction between consultant and researcher provides some support this explanation. The supposition that an

understanding of theory is required for research and teaching seems false, given the number of counter-examples that can be found. An excellent example comes from engineering, in the form of the theory of operators[5].

Engineers who were using differential equations to model physical phenomena 'discovered' that they could manipulate differentiation and integration in the same way that they manipulated multiplication and division. These 'operator techniques' were taught to many classes of engineers who were told that the technique worked, in that they provided correct answers, but that there was no underlying theory. After the techniques had become popular mathematicians began to investigate their function. Eventually they discovered a mathematics, based on the Fourier transform, that provided an explanation. Even today many engineers are not told why operators work, only that they do. While a formal operator theory has helped mathematicians develop other tools for the engineer, and as such the theory can be seen as valuable, from the engineer's perspective the theory is unimportant. For many practicing management scientists it is likely that similar opinions hold true.

Jackson's categories imply a particular relationship between paradigms, methodologies, tools, and methods. He does not explicitly allow for methodologies that are informed by multiple paradigms, nor does he allow for tools that are informed by multiple methodologies. He hints that it may be possible to research how well a tool from one methodology fares under another, for example the use of Rich Pictures in a functionalist intervention, but his categories do not support this activity. It may be that a single methodology cannot be informed by multiple paradigms, but there is no evidence for or against this assertion. Jackson's categories promote a rigid tree of relationships as described in Figure 1.

[5] This story comes from a classroom discussion with Professor Froese in a Systems Design Engineering calculus class.

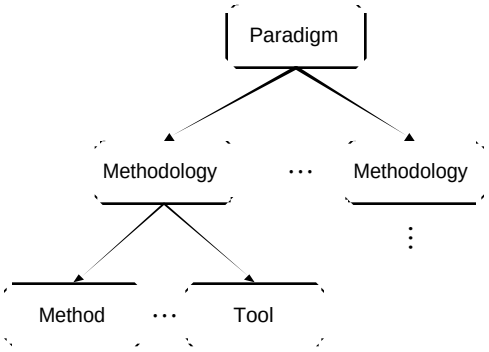

Figure 1 – Jackson's Relationship Tree Between Paradigms, Methodologies, and Tools

The account of the history of pluralism in management science draws heavily on Jackson's categories. As mentioned earlier, he presents a linear view of history that proceeds from one important event to another. It is impossible to critique his interpretation of the importance of these events without a more comprehensive historical knowledge. A few interesting points can however be drawn from the discussion. Jackson cites his own work as being two of the four major historical events, which seems biased in his favour. He also continues to demonstrate his fascination with categorization, in the form of his earlier paper on the "System of Systems Methodologies." His criticisms tend to be similar in all cases, namely that theoretical rigour is lacking. In Jackson's history, pragmatism has reigned supreme.

When he asserts that pluralism and postmodernism are linked, Jackson provides no evidence to support the assertion. He does cite a number of authors, including Checkland, who claim that their methodologies are informed by postmodernism. However the extension to pluralism is unsupported. He also fails to provide a definition of postmodernism, except to say that it opposes singular explanations. Jackson alludes to supporting Lyotard's approach to postmodernism and presumably he also supports the definitions that underlie that approach. Again, Jackson presents only the interpretations that support his goals as opposed to providing a balanced perspective and allowing the reader to choose for themselves.

The discussion of Jackson's requirements for pluralism follow logically from his definitions and categories. His vision is of a practice where different ideas are followed by different people throughout a particular intervention. As in many cases throughout the paper he provides a vision that seems completely desirable, if you accept that core notion that pluralism is a good thing. He does not justify why pluralism is required throughout an intervention, although based on his other arguments it is likely that he would claim that imperialism is the only possible result if this is not the case.

Jackson delves into cognitive issues twice in this paper. Both times he raises important issues that need further discussion and further research. When he discusses the problems with TSI he mentions that it may be difficult for an individual to deal with multiple methodologies and paradigms. He cites one report that discusses this issue. Later, when he describes how pluralist practitioners must accept that sometimes conflicting paradigms will be required in the same intervention, he does not discuss whether this acceptance is possible. Given his focus on education it is interesting that nowhere in the paper does he discuss the need for further research into these cognitive issues and into ways of training practitioners to be able to cope with the ramifications of pluralism.

In his discussion of future research Jackson favours extending TSI, although he recasts it as "critical systems practice." Although TSI is not the focus of this paper, the direction he suggests deals only with its meta-methodological aspects. As he describes it TSI is in many ways a simplified design process. There is significant research on design methodology, some of which may be relevant to the extension of TSI. The extension he does suggest, that TSI should mediate between paradigms, is interesting and deserves further study. He does not discuss the ramifications or feasibility of this approach. Given the ease that pluralism seems to become pragmatism, and given that TSI has already been criticized for attempting to become a meta-paradigm, further research into this area will be extremely important. Changing the methodology's name to "critical systems practice" is unlikely to address any of these deeper issues. The use mediation as a solution also continues Jackson's indirect references to the cognitive and social aspects of management science practice.

The discussion of TSI's future role highlights one of the areas where Jackson does not adopt sufficient rigour in his use of terminology. Throughout most of the paper he is careful to define his terms, for example pluralism, and adhere to these definitions. In this section the distinction between a methodology and a paradigm becomes confused. The confusion becomes even more apparent in the appendix, which for some researchers can be seen as a hidden treasure. It defines a high-level categorization that separates methodologies into functionalist, interpretive, and radical families. Given the apparent prevalence of functional interpretations in management science and engineering, the other two categories open up new avenues of investigation for interested researchers. Unfortunately Jackson does not make clear whether these categories represent paradigms or not, nor does he provide any references to guide investigation.

This paper seems to be intended for management science academics who want a return to theoretical rigour. Given that the genie of pragmatism has been released by management science consultants, Jackson chooses to accept that pluralism is inescapable and offers a compelling, if at times

unsubstantiated vision for the future of the discipline. By providing a context, in the form of his definitions and history, he has established an environment in which both the academics and the consultants can be satisfied and the field enriched.

Synthesis

This paper compares well with the previous one by Müller-Merbach. While the papers were published five years apart they have similar underlying themes and objectives. Five years seems to be a long time, given that Checkland feels that the systems movement is only 40+ years old. One would expect that something would have changed in the intervening years. Instead, the need for pluralism remains. Two reasons for this jump to mind. First, the systems field may have entered a stage of relative equilibrium where existing ideas are seen as sufficient. Pluralism will not be adopted if single-perspective ideas are felt to be sufficient. Second, perhaps pluralism is not the direction that should be taken. Neither Müller-Merbach nor Jackson present any compelling reasons to assume that pluralism is an inevitable result. Given that it has been more than five years since pluralism was first proposed as a desirable result and that it has not materialized, perhaps it is time to revisit their assumptions.

A third reason for the failure of pluralism to gain widespread acceptance may be the appeal of pragmatism. Time and again in this paper Jackson says, in effect, "Yes, we have pragmatism. But that isn't good enough!" Assuming that Jackson's categories and categorizations are even somewhat valid, for many people pragmatism seems to be a perfectly reasonable state of affairs. I am torn between believing that this is the case and believing that true pluralism, in which theoretical considerations are as important as practical results, should be the end goal. My reservations come from the connotations associated with theory. To many, especially in the applied disciplines, theory is something to be tolerated, not embraced. There seems to be a belief that practice informed by theory is no more useful than practice alone. While I accept that this may be the case in the short term, I can only believe that in the longer term theory has a positive impact. Software design is an example where this is the case. Good designs tend to be developed by those who apply or borrow existing techniques. The best designs are developed by those who understand underlying theories. That having been said, as things stand the market does not seem to demand the best designs. Given that the pragmatic solutions are generally cheaper, again in the short term, the market tends to choose them. It is rare that the design informed by theory, which can costs more up front but generally less over time, is chosen. The effectiveness of the free market as decision maker is predicated on having an informed free market. Given that few people are exposed to systems thinking, or other belief systems that take into account a wider scope of information and perspective, it is not reasonable to presume that the market will make an informed decision. Again,

in keeping with my chosen orientation, I feel that widespread education in systems thinking and similar concepts is one possible solution to this problem.

Much like Checkland, Jackson seems to be trying to legitimize management science and systems thinking. Where Checkland is looking to scholarship for legitimacy, Jackson seems to be looking elsewhere. The systems field seems to suffer from an inferiority complex caused by its failure to take the intellectual and scholarly worlds by storm. This complex results in appeals, such as these two, for the field to recast itself into either a traditional or popular image. While I cannot substantiate this belief, I feel that these appeals are coming from those systems practitioners who are nearing the end of their careers and who want to feel that they have made a difference. Because systems is still not seen as completely legitimate these individuals are concerned that their contributions will be cast aside as intellectual fashion changes. Their solution is for systems seek to legitimacy. Expecting systems thinking, which requires philosophical changes in it practitioners, to propagate rapidly is, in my opinion, unfair. If systems thinking was nothing more than a methodology then this goal might be attainable. Given that it involves a different ontology, epistemology, philosophy, and set of methodologies, I think that it will take a more than 40 years for it to catch on.

Interestingly, for someone promoting pluralism Jackson's suggestions for enhancing legitimacy are pragmatic in nature. His appeals to postmodernism and relativism are based not on their theoretical foundations, but on their popularity. Following intellectual trends is unlikely to produce a coherent, critically reflected discipline. Instead the discipline risks becoming fragmented, difficult to learn, and difficult to teach. This is not to say that postmodern arguments and methods have no place in systems thinking, only that they should be examined as both theoretical and practical entities.

A common critique of pluralism is that in many cases it is itself a single perspective. There is the feeling that pluralism is in fact paradoxical. I agree that pluralism may be paradoxical, but question whether the presence of paradox is in fact a flaw. Bower (2000) presents the notion that the ability to accept paradox may depend on cultural upbringing. Stereotypically, members of Asian cultures see paradox as something to be enjoyed and accepted. In contrast, members of Western cultures tend to see paradoxes as things to be teased apart and resolved. Since both cultures have managed to thrive it may be possible to train someone so that they can successfully embrace paradoxical ideas without suffering angst or cognitive difficulties.

A reality of modern academia that Jackson, among others, completely ignores is the difficulty in obtaining multidisciplinary funding. For example in Canada it is very difficult to obtain funding for research that does not align well with NSERC's internal divisions. In recent years NSERC has

attempted to promote interdisciplinary or multidisciplinary research. To date those projects that have been accepted do not measure up to the pluralistic ideal described in Jackson's paper. Individual researchers might be funded to perform pluralistic research within a particular field, but it is unlikely that multidisciplinary research will be supported. Interestingly, all of Jackson's examples of pluralism in management science involve techniques that originate from within that discipline. There is no mention of investigating links between management science and, for example, physics or fine art. Pluralism for Jackson seems to be restricted to explorations within a single discipline which may allow for government funding. True pluralism, in the sense of crossing disciplinary boundaries, is not likely to receive government research grants.

Jackson deserves credit for explicitly discussing the implications of pluralism, and implicitly systems thinking in general, on the practitioner. His is the first paper that I have read that discusses the difficulties many people, including myself, have when they try and adopt a pluralist perspective. I have heard that a single individual is likely to be able to survive two paradigm shifts without losing cognitive effectiveness. This sentiment is much like that of the saying "you can't teach an old dog new tricks." Unfortunately Jackson doesn't offer any advice to practitioners for overcoming these difficulties. My opinion is that it is possible to teach people to cope with shifting perspectives. As with many of the problems identified in these papers, I feel that that education is a solution.

Unlike many systems authors Jackson discusses education explicitly. In his mind successful education requires theoretical knowledge. As I discussed earlier, much education occurs without any reference to theory. An interesting question is whether such education is in some way lessened by not including theory. Based on my earlier comments, I would have to say that it is. In my vision education provides a means for those who have been educated to make their own critically reflected decisions. However my comments implicitly assumed that critical reflection requires theoretical understanding. I have no evidence to support this belief. Neither can I support my contention that it is possible to teach an individual to be able to consciously shift their perceptions or their epistemological and ontological beliefs. My preliminary investigations have not revealed any research that investigates these assertions. At this stage I can only say that I have faith that they are true.

The Case for "Holon"

Author: Peter Checkland
Journal: Systems Practice
Reference: 1988, Vol. 1, No. 3, pp. 235-238

Summary

In this article, Checkland discusses the use of the term 'system' in the systems discipline. His thesis is that the term 'system' was, and remains, a hindrance to the wider understanding of the concepts that underlie the discipline.

Checkland begins by reminiscing about his early work as a physical chemist. Based on his experiences he feels that natural scientists "cannot fail to be aware of two fundamental considerations." These considerations are that:

- Professional discourse uses words that have precise meanings and careful definitions; and,
- These words refer to models, not to reality.

He continues to say that "it is easy for natural scientists to remember that they [models] are only constructions, even when they are casually used as if they were describing (rather than in fact being *relevant to describing*) physical reality."

For comparison, Checkland describes the state of affairs in the applied social sciences. In passing, he mentions, but does not pursue, the problematic nature of the term "social science." In Checkland's opinion social scientists "struggle to use as technical terms the words which are all too casually used in everyday language." Further, "such words are so shop-soiled from use in casual everyday talk that they probably cannot now be purchased as technical term." Unfortunately, Checkland also believes that the simple solution, creating new terms, is not feasible. "If the social scientist resorts to making up new words, then severe accusations of 'jargon' will be made, accusations intended to cripple." With regards to the issue of models, Checkland feels that the use of casual terminology reinforces the notion that the term used, such as 'role' or 'norm', refer to real things.

The paper now turns to the systems field and its terminology. Checkland feels that "where ecologists can use the word [system] as a technical term without causing too much confusion, people trying to study human situations are in extreme difficulty because of the casual way the system is used in everyday chat." He briefly discusses what he thinks the term system means. He feels that a system is an intellectual conception of "an entity having properties as a whole, a certain kind of layered

structure and processes which enable it to adapt in the face of environmental pressures." This section concludes by saying that using the term system is likely to "both mix up everyday language and the language of professional discourse *and* confuse a possibly plausible description of perceived reality with perceived reality itself."

Turning to the systems literature Checkland asserts that it exhibits similar characteristics to that of the social sciences. He suggests that now (1988) is the time to switch terms in order to reduce confusion. He quotes from some of Bertalanffy's original work, substituting the term 'telon' for 'system', and suggests that the substitution significantly enhances understanding. Staying within the systems literature he identifies a number of alternative terms including 'org', 'integron', and 'holon'. Of these Checkland feels that 'holon' is the term that "has come closest to catching on."

After a brief description of Arthur Koestler, the originator of the term holon, he provides some definitions. Checkland links holons to the notion of layers or hierarchy and cites Koestler's original definition: "Every holon has the dual tendency to preserve and assert its individuality as a quasi-autonomous whole; and to function as an integrated part of an (existing or evolving) larger whole." To demonstrate the point that a holon is an abstract concept Checkland quotes from Tim Allen and Thomas Starr. Their quote, which in Checkland's mind "puts the authors on the side of the angels", includes the sentence "What a holon shall contain is determined be the observer." As another throw-away comment on the nature of modeling, Checkland points out the similarity and distinction between "are" and "may be regarded as."

The final section of this paper is best summarized by quoting it verbatim.

> "Forty years after Bertalanffy's semantic disaster we could begin to undo some of the confusion it has caused. We could improve the clarity of systems thinking at a stroke by conceding the word system to everyday language and using holon whenever we refer to the abstract concept of a whole or build a model of a holon (models being always descriptions of holons which might or might not map onto some bit of real-world complexity). Shall we do it? Have we got the nerve?"

Analysis

This article is both short and is a guest editorial. Because of these two characteristics it might be acceptable for Checkland to relax his scholarly rigour. There is very little referencing in this paper and a similar a lack of rigorous definitions. The style and use of language is also very informal. While these characteristics make the paper very readable they also obfuscate some of Checkland's points.

For examine in much of the paper he discusses epistemological and ontological issues without identifying them as such using these terms. In general Checkland has traded rigour for accessibility.

Checkland's description of the state of intellectual affairs in the natural sciences is presented without any references. The notions he presents are discussed at length and in detail in the disciplines of the philosophy and sociology of science. He also paints natural scientists with a very broad and idealistic brush. In my experience many natural scientists do fail to be aware of their terminology and of the nature of their models. When discussing their use of terminology Checkland does not differentiate between scientists who are discussing amongst themselves and scientists who are interacting with the wider community. Many scientists, especially when instructing, tend to use metaphors and common language to explain their concepts. By not providing the context for his assertions Checkland comes across as idealistic.

When discussing social scientists Checkland does provide evidence to support some of his assertions. He provides examples of terms whose definitions are problematic because of their common use. He does not however provide evidence that new terms cannot be created. He claims that there is a backlash against creating jargon but ignores the presence of new terms that commonly appear in social science literature. Finally, Checkland does not justify his belief that 'norms' and 'roles' are not real things. To the physicist this may seem nonsensical, but to the social scientist norms can be real. By not providing definitions of terms such as 'real', Checkland implicitly adopts a naïve realist perspective.

The discussion of the term 'system' begins with an expansive and unsupported assertion. Checkland claims that ecologists have developed a unified definition of the term 'system'. Based on discussions with people studying ecology, this does not seem to be the case. Again, Checkland seems to have an idealized notion of the state of intellectual rigour in disciplines other than his own. When he begins to discuss the use of the term 'system' in the systems field he restricts his context to social systems literature. In doing so he ignores the 'hard' systems literature. While he has defined his context he does not discuss the ramifications of his later suggestions for terminology change on the wider systems fields. In this case the appropriateness of his context must be questioned as the ramifications of his proposed changes extend beyond the context's boundaries.

To demonstrate the need for a new term to replace 'systems', Checkland rewrites a passage from Bertalanffy using the term 'telon'. The fact that in Checkland's opinion the rewritten passage remains useful is seen as justification for a change in terminology. As someone familiar with the systems field, Checkland is not an unbiased observer. It is not at all clear that someone unfamiliar

with the discipline would find the new passage more understandable. For better or for worse the term system does exist in common use. While the definition of system in this context is different than the definition in Checkland's context, it does have some basic similarities. These similarities allow the novice to glean some information from the original passage. 'Telon', which to use Checkland's description is jargon, does not provide any useful information to the novice.

Checkland asserts that the term 'holon' seems to hold the most promise as an alternative to 'system'. His primary evidence for this assertion seems to be that it is the only alternative term used by someone other than its originator. He also supports the use of the term because its definition makes explicit mention of the fact that it is an abstraction. In doing so Checkland continues to assert the importance of naïve realism.

In his conclusion Checkland proposes that the time (1988) is ripe for a change in terminology. He does not explain why he feels this way. Given that he has already mentioned four other terms that have failed to catch on, there is some evidence that the systems field is willing to accept the limitations of its terminology. Given the turmoil that a change in terminology would cause, turmoil that Checkland does not discuss other than to acknowledge its existence, more evidence of positive results is required. The only advantage to changing terminology that he discusses is that he feels the change would improve the clarity of systems thinking. Beyond appealing to naïve realism his paper has not provided any evidence that this would be the case. He has also not discussed any of the possible positive effects of having unclear terminology. A case could be made that rigid terminology leads to intellectual stagnation and a lack of reflection.

Synthesis

Guest editorials are interesting because they provide a pulpit for people who are considered of some importance to discuss the issues that are important to them. From a research point of view these editorials can provide insights into the more philosophical issues that most journal articles tend to avoid. The fact that Checkland raises the issue of terminology in this forum gives his discussion more weight than it would have had in article form. However the weakness of using an editorial to present an opinion, as has already been pointed out, is that editorials are generally not held to the same scholarly standards as articles. Although he wrote his article on scholarly rigour after this editorial, I still feel that these is some inconsistency in Checkland's position. His choice not to use appropriate formal terminology, while simultaneously demanding that the discipline as a whole switch to a more rigorously defined term, weakens his position.

Checkland seems unable to escape, or even to acknowledge, the biases of his own experiences. His early training as a physicist seems to have biased him against many notions that someone from the social sciences might accept. A 'norm' is just as real as the law of gravity, in the proper context. Checkland does not acknowledge that such a context is possible. Similarly his idealistic notion of the nature of thought in the natural science seems to be based more on an idealized sense of personal history than on the evidence of researchers who study these notions for a living.

Since Checkland wrote this paper in 1988 a major shift in terminology has occurred. Beginning around 1996 the systems movement began to use the term 'systemic'. In 1998 the term attained relative prominence when the journal *Systems Practice* changed its name to *Systemic Practice and Action Research*. Similarly, at the 1999 conference of the ISSS the term system was almost never used. In its place the participants used the term 'systemic'. I have yet to find any documented rationale for the change. My suspicion is that is occurred after the publication of Senge (1994). The popularity of this work introduced many people to the term "systems thinking." Unfortunately[6], Senge chose to use this broader term as opposed to the more technically correct "system dynamics." It is now very difficult to use electronic search tools to find information on systems thinking that does not refer to Senge's definition. My supposition is that the systems community chose to change terminology in order to distinguish their work from that of Senge. What they chose 'systemic' as opposed to 'holonistic' remains unknown.

I am also concerned with the relative focus within the systems community on soft or social systems. Hard systems, and I use the term loosely, remains an important part of the history and practice of the discipline as a whole. Unfortunately there seems to have been an almost absolute schism between the two areas in the literature. My supposition is that much of the hard systems research has been assumed by engineers and computer scientists. There are a large number of references to systems concepts in the journals of the IEEE and other engineering organizations. While they may have absorbed the methodologies of hard systems theory they do not seem to have absorbed the field's history and philosophical underpinnings. This is likely a cause of the apparent schism between systems scientists and those studying hard systems theories.

Turning to my personal agenda, again I again feel that education is one way to address many of Checkland's concerns. In my own experience I was not taught the theory of modeling. This field, along with the study of epistemology, discusses questions such as the difference between "are" and "may be regarded as" and when a scientific hypothesis becomes a law. Many people seem to feel that

[6] Or fortunately, depending on your perspective.

these issues are quibbles. As Checkland demonstrates in this paper, and I agree with this position, these quibbles can have significant ramifications on practice and theoretical understanding. Education is, I believe, one of the best ways to promote reflection on the nature of modeling and understanding.

Finally, Checkland is the first person I have encountered who links reflection and professional behaviour. He does so when he mentions, in passing, the difference between casual conversation and "reflective professional discourse." I agree wholeheartedly that a primary characteristic of professional behaviour is reflection. This perspective is not mentioned in many of the texts that are used to introduce student engineers to their profession. Neither is it mentioned in the descriptions of the engineering, medical, or legal professions. Professionals can be seen as a response to an increasingly complex world where it is difficult to be knowledgeable in a wide variety of areas. As I mentioned earlier reflection seems to offer a lot of promise in addressing complexity. The link between professional behaviour and reflection therefore seems straightforward.

Brief Discussions

Imagine Complexity: The past, present and future potential of complex thinking

Authors: Simon Shackley, Brian Wynne and Claire Waterton
Journal: Futures
Reference: 1996, Vol. 28, No. 3, pp. 201-225

In this article, Shackley, Wynne, and Waterton reexamine some of the founding premises of complexity theory and discuss some of the field's future directions. They propose that the current emergence of complexity does not imply that the world used to be simple. Their premise is that perceived complexity is a function of social relationships and that the dominant relationships in modern culture have driven a simplified world view. They assert that the modern sciences of complexity risk being subverted by the existing social relationships and conclude that institutional development is required for the successful application and acceptance of complex thinking.

The article begins with a brief discussion of the emergence of the notion of complexity in planning. The emergence of the notion is linked to the development of new tools that are described as "less reductionist than the economic cost-benefit family of methods." 'Post-normal science' and 'cultural theory' are given as examples of these new. The authors feel that the term 'tool' raises issues and that "our main purpose here is to ask how policy imagination and scope may be limited by this conceptualization…" While they acknowledge that many of the issues they raise have already been discussed by other social scientists and policy analysts, the authors feel that the certain consequences of the issues have not been explored. In particular, the authors feel that many of the discussions have used 'naïve realist' arguments and have accordingly not examined complexity from a relativist viewpoint. The introduction concludes with a statement of vision: "the challenge becomes the refashioning of our institutions, and of exploring new ways to represent and acknowledge their 'messiness' – more so in fact than the elaboration of new, more sophisticated, methodologies to capture complexity for the purposes of better management and control."

As an aside, the authors briefly touch on the issue of whether institutions can accept complexity. "It is probably inevitable that institutions require simplifying heuristics and reifications of the more complex and dynamic in order to generate sufficient stability to sustain a coherent set of institutional identities and roles." Their belief is that even if institutions require such simplifications, discussions

of complexity can help choose which simplifications to adopt and can discuss the ramifications of their adoption.

The authors now turn to a background discussion of the theoretical notions of simplicity and complexity. Simplicity is assumed to involve "linear cause-effect processes, often unidimensional and deterministic." Gleick, Waldrop, and Lewin, who typify the label "popularizer of complexity", are described as arguing that "many aspects of the physical, material world cannot be defined in these terms because of their inherent complexity." Similar claims about the social world are made by practitioners of philosophy of science, cultural studies, and sociology of risk. A further argument made by these practitioners is that "the traditional distinction between the political/social and natural is itself an *a priori* assumption in need of being complexified."

The argument for complexity based on critical realism is now analyzed. This argument is based on the notion that new conceptual tools are required to understand certain systems, for example a dripping tap or an ecosystem. The need to develop models that can be measured against the physical behaviour of these systems drives the development of the new tools of complexity. The authors take exception to this argument as "[the realist account] risks sidelining the finding from the history and sociology of science that scientific knowledge also comes to reflect, and is shaped by its institutional, cultural, and political contexts…" These same sociologists of science argue that perceived simplicity is a result of a choice of epistemology and that the new focus on complexity is a result of "attention now being paid to previously neglected systems."

The authors now focus on post-normal science (PNS) as an example of a policy tool for dealing with complexity. PNS is presented as being based on a realist position. The argument that PNS focuses on areas of higher uncertainty is seen as an outgrowth of the inherent complexity of the phenomena under investigation and of the "improvement in scientific understanding and methodologies which permit their exploration." PNS is also described as having a social dimension, in that the social and political decision stakes are higher in PNS. This presence of this dimension is explained through the claim that the 'old certainties' of modernism are eroding. The authors question this explanation, citing the emergence of new forms of simplification and integration in the social sphere.

This section concludes by stating that that PNS and cultural theory are based primarily on "critical realist" arguments. The authors express concern that many practitioners are not aware of the arguments that underlie these tools and have not been exposed to other forms of argument. Their concern is that "if the source of the new complexity is identifies as being located primarily in real changes in nature and society…or in our epistemology, then the issue can all too easily be translated

into the need to develop new, more sophisticated methods to better capture the new understanding of reality…complexity may come to imply no more than the substitution of one set of methods, now seen as inadequate, with another more sophisticated set, with no consequent changes in existing practices and institutions."

The authors now turn to a discussion of the study of the production of scientific knowledge. They summarize a number of the major findings, focusing on the relationships between individual practitioners and wider networks. Bruno Latour's work forms the basis of this discussion. Two aspects of the discussion deserve special mention. The first is the notion of social validation. The authors quote from Latour, who says that "the 'truth' of a knowledge claim depends on what is taken up and used as if it were true by the wider network of actors enrolled in multiple ways in the technoscientific programme." This assertion is seen as applying even more heavily in policy science than in the natural sciences. The second aspect of this discussion that deserves mention is the notion that even simple tools generally require complex application or justification. The authors use this notion to suggest that the practice of science was, and remains, complex.

The next section of the discussion of the origins of simplicity and complexity deals with the boundary between the two. The authors describe how the separation of the complex from the simple may have important rhetorical effects. For example, "such rhetoric justifies the redirection of research funding and provides a ready explanation for the apparent failures of past science for policy." The boundary between simplicity and complexity is itself seen as fluid and context-dependent. The authors do not argue that a single fixed boundary is required, but the do feel that it is important for practitioners of complexity to be aware of its effects. Perhaps the most interesting part of this discussion is a throw-away comment made by the authors: "Paradoxically, the construction of complexity theory, as itself largely novel, may be self-contradictory in that it oversimplifies the context dependence of boundary work.

The final discussion section describes the mutual co-construction of science and society. The fundamental premise of this discussion is that science and society are not only described in relation to each other, but that they also continuously create and recreate each other. "It is their mutual reinforcement which solidifies them into apparently independent wholes." An implication of this belief is that "the characteristics of scientific knowledge…embody unstated assumptions about the needs, capabilities, and structures of the policy world." The authors believe that at some point in the past society desired a simplified understanding of social problems and that concurrently science desired simple scientific tools. The results was a co-construction in which simplicity supported

simplicity. They describe how this mutual support has allowed for the notion of policy management: "Indeterminacies are translated through such mutual construction into deterministic uncertainties which allow for the promise of being controlled tomorrow, if we continue to apply more science, within the same basic institutional relationships." The authors feel that this co-construction has allowed the paradigm of simplicity to survive in the face of obvious inadequacies. They also feel that the emergence of complexity may be a reflection not only of critical realism, but also of the instability of the relationship between science and society. The solution, according to the authors, is to turn to reflexivity. "However it is induced, reflexivity is about opening up scientific and policy institutions to articulation and open debate of these pre-analytic implicit assumption-commitments, thus introducing the possibility not only of more intelligence and imagination, but correspondingly also of more social resilience." Their concern is that "if the newly promoted paradigm [of complexity] just incorporates the same ambition to management and control, then it risks redeterminizing science, though more comprehensively."

The article now turn to a re-evaluation of apparently simple problems. The goal is to demonstrate how the mutual co-construction of science and society, coupled with the desire for simplicity, constrained the investigations of the problems. The quotes in the following list represent some of the more salient and interesting outcomes of this re-evaluation.

- "Complexity has always been reduced in this 'experimental' way (in the sense that the inquiry is modeled according to the concept of controlling agents in space and time, as in a laboratory) by many institutions…which have not been able to acknowledge and negotiate their way through less deterministic and controlled terrain."
- "The behaviour [of the subjects] did not fall into one of their [scientist's] categories and therefore the rule of ignoring spoilt ballots was put into operation. In this case, rule following concealed the shaky, and not at all universally held, hence highly conditional assumptions of the policy tool."
- "In other words, the policy-related development of contingent valuation relies on a double interpretation by different social actors: the formal account as a sound tool for policy, accepted by many end-users of the knowledge, and the informal version recognizing the indeterminacies and its limitations for policy, entertained by many policy makers and economists."
- "As Taylor put it, ecological models '[were] not simply representing the nature of the system, but rather [were] a science of representing in relation to someone's conception of possible interventions'."

Having complete their examination of hidden complexity, the authors discuss the consequences of sustaining simplicity. The section begins with an assertion that simplicity requires complexity to exist. "Ironically, simplicity in the construction of knowledge…rests on highly complex sets of relations, with many interpenetrating layers of trust, identity-change, disciplining, dependency, ambivalence, resistance, and so on." The notion presented earlier, that a tool has a separate identity for its practitioners and those who use its results, is seen as supporting this belief. Cultural theory is also described as operating in this fashion. "[Cultural theorists] sometimes write as if their theory is about understanding for the purposes of prediction…yet at other times describe it as coherent set of hypothesis which aim to understand variability in perceptions, beliefs…" The authors are concerned that this dual nature of many complexity theories, couples with the dominant social desire for simplicity, will result in the methodologizing of complexity. "The methodological elaboration [of contingent valuation] acted as a direct substitute for institutional development and reflexivity."

The article now turns to future directions in policy research. In keeping with their focus on co-construction, and given the emergence of sciences of complexity, their fundamental question to address is "are appropriate institutional forms available in which relational aspects, and contextualities, emerge and can develop in constructive ways." Again, they turn to cultural theory to discuss their vision. The theory underlying cultural theory encourages new forms of social mediation, but there is little evidence that this happens in practice. They feel that at present (1996) simplifications and reifications are being made to support policy modeling exercises. Their concern is that "if CT [cultural theory], or some other theory in the social sciences, appears to 'solve' the problem of culture for policy makers, modelers and other researchers, there is a danger that the only new mediation which occurs is that between rather narrow communities of natural and social scientists, rather than this being the prelude to the exploration of new fora for participation, interaction, mediation, and so forth."

The conclusion to this paper calls for reflexive behaviour on a number of levels. The authors believe that all social science theories are heuristics, and as such that their use requires continual reflection on the part of the practitioner. As well, "any social theory, or 'tool', embodying theoretical assumptions, such as cost-benefit analysis, CT, or post-normal science, must also be evaluated for the extent to which it is consistent with its own tenets." Since indeterminacy is an integral part of complexity, any tool that addresses complexity must itself contain indeterminacy and effort should be expended to ensure that the tool does not degenerate into orthodoxy. The overall goal is to move from talking about complexity towards embodying complexity.

From one perspective this paper is nothing more than an examination of the notions of complexity using the tools of the sociology of science. It is a well written paper that includes excellent referencing and cogent arguments. Unfortunately, as this summary mirrors, the authors tend to use jargon and somewhat convoluted arguments. As with Checkland's papers, it include a number of relatively controversial throw-away comments that deserve further investigation. For example the engineer in me was offended by the notion that putting a man on the moon was a simple problem, but the systems scientist agreed wholeheartedly. Nevertheless it doesn't offer much in the way of new information to the informed systems practitioner.

From another perspective this paper should be required reading for students of systems. Many academics are not aware of the research that has been done in areas such as epistemology and the sociology of science. While many practitioners are aware of some of these issues, as they deal with them on a regular basis, in my experience few have investigated any of the formal research. By providing a well-written work applying such concepts to complexity and systems thinking the authors have produced an excellent teaching tool.

Experiences in Teaching Systems Thinking

Authors: Johan Strümpfer and Tom Ryan
Institution: University of Cape Town, South Africa
URL: http://www.ee.uct.ac.za/~psm/SPPAPER.htm

This paper describes the design and results of a course that teaches systems thinking to MBA students. The authors feel that the reflexive use of soft systems methodology (SSM) as part of the course design provided them with an increased understanding of the course context and allowed them to develop better criteria to judge their results.

Pedagogically the authors based their course design on Kolb's 1975 model of learning. This rather dated, yet still common, model uses the following progression:

Concrete experience → Observations and reflections →
Abstract concepts and generalizations → Implication testing

I personally believe that this approach is too grounded in perception and observation. I would like to see an approach that is based on epistemological and ontological positions that are defined before perception is undertaken. In this way the learner will develop experience in consciously adjusting and manipulating their own perceptions and will not be bound to, and restricted by, their experiences. Quantum physicists anecdotally have significant amounts of unlearning to perform, as quantum phenomena are significantly different from any concrete experiences.

The authors chose to operationalize Kolb's model using the suggestions of Mumford and Honey. These authors proposed that each stage of Kolb's model has an associated learning style and that individuals each have different preferred styles. In the course the authors also chose to focus on small groups which seems to be a hallmark of education in applied disciplines. Their finding that group selection, which was done according to learning style, was an important success factor is interesting. My belief, informed by my experiences in undergraduate engineering, is that group composition is less important than experience, a willingness to work together, and an introduction to the theory of small group dynamics.

As part of their design the authors developed a set of critical course messages. This set can be seen as the author's perceptions of the important aspects of systems thinking, mediated by their context as

teachers of a particular course. I find this list interesting because it represents, to paraphrase Latour, one view of the essential truths of systems thinking. The authors' list is as follows:

- Problem solving is a collective, social process of inquiry, aimed at building up an appropriately rich and shared model of reality;
- Systems thinking is pervaded by the idea and explicit use of multiple perspectives; and,
- Systems thinking addresses the poverty of general ideas of causality and uses a rich, including a non-linear and circular, concept of causality in complex systems design.

Later on in the paper the authors describe some of the fundamental ideas of systems thinking as being relationships, emergence, systems hierarchies, structure, function, purpose, process, regulation, multiple perspectives, causality, analysis, syntheses, and flow tracing.

The authors feel that of these messages the first is the hardest for most students to accept. This is an interesting contrast to engineering where students tend to form problem-solving groups instinctively. It may also be indicative of the general trend in primary and secondary education towards individual, not group performance.

In the area of systems methodologies the authors chose to teach SSM and system dynamics modeling (SDM). They teach SSM through a brief introduction that is followed by practice in a controlled context. One of their primary concerns is that students not get hung up on the situation being studied. They would prefer that students learn to use the information that they are provided with according to the dictums of SSM. The authors feel that the early stages of SSM can be quite lengthy and that they cannot afford to have students expend too much time on them. I disagree with this approach as it insulates the students from having to choose when to stop a particular activity. This choice is one of the most important aspects of SSM. While I accept that the authors do not want to see their students do poorly in their course, I still feel that the experience would be enhanced by allowing them to make, and recover from, mistakes. With regards to teaching SDM the authors follow a fairly standard progression. I am extremely worried about their linking of SSM and SDM as they do so exclusively at a methodological level. Lane and Oliva (1998) mention this problem explicitly in their discussion of synthesizing SSM and SDM.

The authors use an experiential teaching method where the students apply the various techniques they are taught. The students are expected to pick up theory on their own and through classroom interactions with each other. The learning activities used include concept mapping, critiquing, papers, and presentations. The goal is for the students to create their own syntheses of the material as

opposed to being given a synthesis by the instructors. A final statement of individual learning is used at the end of the course to promote reflective behaviour.

In terms of student experiences, the authors present the following observations:

- Self-study is not common in management teaching and as such the students both have difficulty with, and are required to, work on their own.
- Successful group work is vital to gaining an understanding of the course material, but in general the groups are not self-managing and require instructor intervention to ensure smooth functioning.
- There can be significant time lags before understanding is reached, and the material has a tendency to be understood at significantly different rates by different people. This leads to intra-student conflict and appeal to the instructor for a "right answer."
- The class size has to be small given the nature of the material and the desire to give the students ongoing feedback.
- The students tend to want tools and methods, not different ways of thinking.
- The students tend to assume that the 80-20 rule holds in systems design, in that the first 80 percent of a project is the most important and the most simple. The instructors' experience is that the last 20 percent is the most important and causes 80 percent of future problems.

In their reflections on teaching the course the authors focus on the difficulties associated with teaching a mindset. They imply that the Kolb model and its interpretations may not be appropriate if the subject matter being taught is not information. They also touch on probably the most important problem with teaching systems thinking which is that such thinking is the exception, not the rule, in education. A mindset must be continuously nurtured and broadly accepted. Although the authors feel isolated they continue to teach their material out of a sense of pride that some of their graduates have used the techniques taught in the course to effect real change in real organizations.

The Necessity of Being "Un-disciplined" and "Out of Control": Design Action and Systems Thinking

Author: Harold G. Nelson
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This paper discusses and links design and systems thinking in the context of organizational change. The author feels that the complexity of the modern world, coupled with the need to go beyond analysis and into creation, implies that a synthesis of design and systems thinking is necessary to meet modern organizational challenges.

Nelson begins with a discussion of the pressing need for dramatic change in our social institutions. As examples he gives businesses, schools, hospitals, and government agencies. He also mentions buzzwords such as 're-engineering' and 're-structuring' as examples of responses to this need for change. It is difficult to argue with this introduction given the similar reports in the media. Nelson then goes on to claim that similar changes are needed across the globe. I have to disagree with this assertion. While the need for change may be global, the specific changes depend on local context.

The paper now presents a very concise and elegant discussion of complexity. He begins by discussing the difficulty in establishing clearly defined problems and stable problem solving contexts. Stability is an especially important aspect, as without it solutions may not have sufficient time to take effect. As an engineering graduate I agree with this notion. While I accept the need to revisit a problem definition to ensure that it still describes the situation, at some point, to use a colloquialism, the practitioner has to decide whether to fish or cut bait. Continual reflection can easily degenerate into paralysis through excessive analysis. Nelson also touches on the need from pluralism, both in perspective and in methodology.

In Nelson's opinion the consequence of complexity is that "we either feel paralyzed or are only willing to take small, incremental actions that have little consequence." He likens this reaction to rearranging deck chairs on the Titanic. Nelson believes that we need to feel able to make changes that are transformational, not incremental. While this argument resonates with me, it contradicts with my understanding of complex systems. In a complex system it is difficult to know in advance what the effects of a small action will be. They may also be small or they may balloon through mechanisms such as positive feedback. In making this statement Nelson falls victim to a linear model where small results in small. I do not believe that this model holds in many complex systems.

Nelson cites Herbert Simon's "science of the artificial" as an example of an appropriate perspective for promoting fundamental changes. He sees Simon's work as bridging between science, which promotes simplicity and description, and design. Nelson describes design as "a strategy for facilitating change." He feels that design encompasses the science of the artificial in that it deals with both physical and social systems. He speaks of design in fairly glowing terms and includes ontological statements such as "Design is both process and artifact." Unfortunately he provides no references in this section to justify his assertions. Reference material would be useful in this instance given that his definition of design is very different from that used in engineering. Nelson explicitly differentiates design from planning, management, or problem solving. Again, it is unfortunate that he does not include references and the differences between these terms are very subtle.

The paper continues to define design by describing four alternative perspectives on the meaning of the term. The first perspective is that design is a strategy for change. He discusses this perspective through the use of two scenarios. The first is when an organization proactively designs a way to attain a desired state. Design in the basis for this activity. The second is when an organization reacts to move away from an undesired state. Nelson feels that there is a lot of experience with the second scenario and very little with the first. The second perspective on design is that it is a perspective on the nature of problems. Organizational change is presented as a wicked problem. Design, as opposed to problem solving, is seen as addressing such problems. The third perspective on design is that it is an application of judgement. In Nelson's opinion, "Design decisions are not made through formal deliberations dealing with true or false answers to questions of fact or logic. Design decisions are made as a matter of formal design judgement." He continues to say that "[Judgement] is a form of deliberation that leads to understandings that cannot be reached using critical thinking skills that more successfully obtain answers of truth in internally consistent problematic contexts." Nelson's final point is that unlike science or art, which he sees as being self-expressive, design is about creating new things for someone else. He feels that the work of a designer cannot be separated from the work of the person for whom the designer is creating. Terms such as client or stakeholder are not seen as capturing the depth of this relationship.

Nelson's definitions of design are unlike any I have encountered in the engineering literature. Notions of wicked problems have been discussed elsewhere, but the remaining points are unique. I tend to agree with Nelson's description, which may be because my own research is intimately related to linking design and systems thinking. Although he claims to be discussing only design in this part of the paper, his framing of the discussion is oriented along systems lines. While this will make it easier for him to meet his end goal of linking the two notions, it does make his conclusions slightly suspect.

Two of Nelson's point deserve special mention. His assertion that critical thinking skills are used to elicit truth in simple contexts is a very different perspective on this issue. Critical thinking is generally seen, for example by Jackson, as a tool that allows for deeper understanding in a wide variety of contexts. Unfortunately Nelson does not provide references to back up has assertion. Given that I believe critical reflection, which is related to critical thinking, is an essential skill in a number of areas I find this assertion troubling. A second interesting notion is captured in the statement that during design "individuals must give individual control of the outcome over to a process of emergent quality rather than to a linearly managed process." When I have helped teach design to undergraduate engineers we have emphasized half of this assertion. The point of a design process is not what exact process you followed but that you had some process at all. We do not take the next step and emphasize that the process does not have to be managed in a linear way. While we emphasize the notion of iteration, the iterations always follow the same set of steps. Nelson is proposing a nonlinear process of design. Again, I would have preferred that he provide references, as this is a unique notion that is presented in a vacuum.

The paper now turns to a discussion of systems thinking. Nelson describes the systems approach as "an integrated, 'un-disciplined' approach to understanding human endeavours." He believes that this notion of being 'un-disciplined', which seems to be the same as being multidisciplinary, is a requirement when dealing with social systems. This section continues to describe how systems terms, such as 'wholism' and 'interconnection', are becoming more prevalent in everyday settings. He contrasts the modern use of these terms with their use in systems engineering, which he dates to the 1960s and 1970s. He feels that this older discipline relied on being systematic, not systemic, and that modern techniques have moved beyond this limitation. While his use of terminology differs from the norm in the systems literature, Nelson's description of systems thinking is reasonable if the reader is already familiar with the discipline. Given that he is writing in a journal to focuses on performance improvement in organizations, many of the people reading this paper will likely not be familiar with systems ideas. For these people Nelson's description can easily be seen as making a mountain out of a molehill. He ascribes significant advantages to what seems like common sense. By not even touching on the deep ontological and epistemological changes that systems thinking entails Nelson is in some senses cheapening the discipline.

Nelson concludes by claiming, and rightly so in my opinion, that "it becomes difficult to distinguish between [design and systems thinking] when they are brought into intellectual proximity of one another." His explanation of their similarity is elegantly described when he says that "Systems theory describes patterns of relationships; design prescribes unique patterns." I agree with his

sentiments. This paper, if was expanded to include further references and discussion, describes what I believe to be a key direction that systems research should pursue.

Reflections

On Systems Papers

Many of the papers that discussed the deeper issues that interest me tended to be written for academics. By this I mean that the nature of the arguments was such that an academic would tend to arrive at the same conclusions as the author. I have tried to show in my analyses that other interpretations are possible if the reader is not already immersed in the academic discipline of systems thinking. The authors seem to assume that suggestions that benefit academics should be adopted without stopping to consider the ramifications on those people who are simply users of systems concepts or who are being introduced to these concepts for the first time. As an aspiring educator this assumption worries me as it affects teaching directly. I would like to see the systems authors consider how best to serve individuals who are both inside and outside the discipline. As things stand systems seems to be looking inward, not outward. There is a chance that by looking inward the discipline may become more accessible. I do not hold much faith that this will actually happen.

Those papers that included historical sections tended to only discuss the history that supported their premises. I call this tendency "Connections syndrome." Connections is a television program that traces the historical development of modern devices. The histories presented on the show make it seem that all devices are the result of a sequence of events that, while not necessarily planned, all led inexorably to the modern product. Those who study the sociology of science have demonstrated on many occasions that the history of science is filled with backtracking, incorrect assumptions, and trails that ended in dead ends. In some cases it is more interesting to see what did not work as the reasons for failure may have been removed by further developments. The case of blue light emitting diodes (LEDs) is a recent example of this phenomenon. A particular path towards their development had been dropped in the 1960s because of technical difficulties in working with some of the materials. In the mid-1990s a researcher looking into the problem rediscovered this research, which was not included in his education, and realized that the difficulties could now be overcome. He soon demonstrated a working prototype. While I realize that most journal articles are too short to provide complete histories I still feel that they should include some references, possibly in an appendix, to alternative histories or to events that they have chosen not to discuss. The advantage to the discipline of providing a richer history is that it can be mined for information that can be made relevant in the modern context.

A final observation is that systems articles have a tendency to use references that were published within five or ten years of the article. Those readers who have investigated the history of the discipline soon become aware that there is a tendency for researchers to "discover" ideas that have been known about for a long time. While some of these authors provide a different context for the original notion, and as such provide added value, many do not. The result is that the systems movement does not, to use a coined phrase, always stand on the shoulders of its giants. I will agree that this practice is not restricted to practitioners of systems. The same tendency can be found in articles on engineering education or design. What concerns me is that opportunities for innovation are being wasted on rediscovering the past as opposed to developing the future. A greater concern is that the increasing use of the World Wide Web in research may exacerbate this tendency. Students in engineering are relying more and more on web sites as references than they are on printed books or journal articles. While I accept that the web is a valuable tool, as I mentioned earlier it is a transient medium. I can envision a future where innovative scholarship has ground to a halt because everyone is busy reinventing information that they can no longer locate.

On Systems as a Discipline

What is 'systems'?

In this paper I refer to systems as a field, a discipline, a practice, and a belief. These terms all mean different things and a scholarly analysis of this paper would say that I have not rigorously defined my terms. The reason for the use of the different terms is that I believe that systems encompasses all of these things. In this I disagree with Checkland who feels that terms should be rigorously defined. Having a variety of definitions promotes discussion, dialogue, and reflection. I see these activities as valuable, not to mention stimulating. If there is a need for more rigorous definitions they should be developed in the moment and seen as transient. If, as Checkland believes, commonly accepted definitions are required for education then these definitions should be prefaced with an admonition that they are heuristics. Although I support their use at times I have difficulty dealing with ambiguous terminology. I attribute this to a lack of training in accepting paradoxes and uncertainties. I have faith that both future students and I can be taught to overcome this difficulty.

What is 'understanding'?

The systems literature is peppered with terms like 'understand', 'control', 'predict', 'tendency', and 'intuition'. Unfortunately these terms are rarely defined. In general all someone reading the literature

knows is that prediction and control are unattainable goals if the system understudy is complex. I have grave concerns about the use of this terminology in the context of complexity[7].

In my opinion any activity that manipulates a system in any way to restrict the range of possible responses requires prediction. Even if the results are expressed probabilistically, for example "this intervention will reduce the likelihood of result A by around one third", prediction is still involved. If a system is truly complex, in that there is no way to know what its future behaviour will be, any assumptions other than "anything is possible" seem untenable.

Many systems methodologies talk about mapping particular constructs onto the perceived world to enhance understanding. Some of these methodologies continue to say that these constructs can then be used to influence the world, for example by de-coupling feedback loops. Why is the systems dynamics model, as an example, and more or less likely to succeed in an intervention than a linear model? Both kinds of model lack the variety, to use Ashby's terminology, to represent complex systems in a way that supports intervention.

I acknowledge that my position on this issue it fatalistic. A double pendulum is a system in which the position of the pendulums over time cannot be predicted. However I can bound the positions by a circle with a radius equal to the sum of the pendulum lengths. Is this a prediction? Over time, assuming that friction and gravity are present, the pendulums will both point down. Is this a prediction? There is a chance that a physical double pendulum may come lose from its mounting and fly across the room. The chances of this happening are very small, but they are non-zero. In complex systems, anything non-zero may, for example through positive feedback, become a dominant force. Does this mean that I cannot ignore even the smallest probability? In an infinite universe over infinite time, anything becomes possible and prediction impossible.

I do not require exact answers to these questions. Arbitrarily saying that a probability of 1 in 100,000 is small enough to be discounted is not a solution, as the boundary between relevant and irrelevant change depending on context. The answer seems to be that those who might be affected by an outcome decide on relevance. This raises the omnipresent questions of who should be involved and how decisions should be made. Any answers I give or suggestions I make to address these questions can always be met with cases where my notions fail. Similarly I can always find cases to critique the notions of others. How can we avoid being paralyzed by indecision? Is paralysis really so bad?

[7] A useful definition, or definitions, of complexity would also be useful, but that's a topic for another paper.

Is Theory Really That Important?

Theory and rigour are both aspects of traditional scholarship. In common language rigour has a positive connotations while theory's connotations are mixed. Questions are being raised, for example in post-normal science and studies of the sociology of science, as to whether either notion should be pursued. Jackson and Checkland both believe that theory is an important part of scholarship and is vital to the success of the systems discipline. Consultants and popularizers tend to ignore theory and take a more pragmatic stance. I am no longer clear where I stand on this issue.

I have earlier described how a technique can be taught without knowing its theoretical underpinnings. The question remains as to whether theory is required to enhance a technique. In engineering the practice of design without theory is called crafting. It is distinguished from "real" engineering where theory and science are used to develop new ideas. The connotation is that theory is a prerequisite for engineering work. Again, there are many examples support this contention and its opposite. A similar separation is made in the university context between training an education. Here the connotations are reversed. Training, where theory comes second to application, is considered less valuable than the more theoretically oriented education.

In Canada, and especially in Ontario, governments are focusing on the applied disciplines. In practice this seems to translate into a desire for more people trained in technique. My experience, having worked with individuals trained in this way, is that they are unsurpassed as solving familiar problems and deficient when it comes to dealing with the novel. Theoretical knowledge, at least in the field of computer programming, is immensely valuable when developing innovate solutions.

At one point I read Gribbon (1984), a popular book that discussed quantum theory. This book was interesting because it proposed the notion of a "quantum cookbook." The author believed, and I concur, that very few people need to know why devices based on quantum theory actually work. Lasers, an example of such a device, can now be purchased at electronic stores in the form of a simple diode than can be incorporated into any DC circuit. The result of an extremely complex theory is now a commodity, thanks in the author's opinion, to the presence of an entry for "laser" in the quantum cookbook.

A concern that has been voiced in many science fiction novels is that we are evolving a "technological priesthood." This priesthood contains those who have gone beyond using devices and actually understand the underlying theory. In many of these books this priesthood has become the ruling class in a technocracy. This belief is not limited to science fiction. Veblen (1953), described as

a seminal work of economic thought, includes a discussion of how engineers will become the dominant political force in the United States through their control of technology. As my engineering education has progressed I have become more and more aware of a striking lack of theoretical education. Knowledge of theory is being compressed into a smaller and smaller segment of the population. I have no doubt that this evolution will have serious implication implications on future research.

An interesting argument that I have not seen raised is that practice carries with it theory, whether it is discussed explicitly or not. Apparently there is a formal theory that describes how artifacts embody the context and assumptions that guided their creation. This is attractive notion, as it can be used to justify not teaching theory explicitly. Why teach theory if much of it is included for free when teaching practice? Without having investigated this notion, I feel somewhat skeptical that it would apply in this case. In an early engineering exercise students are asked to enumerate the possible uses of a brick. Under this theory of embedded context, the students should be biased towards construction. Instead the students developed over forty different uses ranging from jewelry to cooking aid. I imagine that the formal theory is more subtle and involved that I am giving it credit for, but I remain skeptical about its utility in this context.

"But I don't want to play!"

There seems to be a belief that systems ideas promote dialogue and respect among systems practitioners. While Checkland challenges this notion, as it is an ideological statement, many systems practitioners seem to accept it. Post-normal science accepts this notion wholeheartedly in the form of the wider peer community. At the 1999 ISSS conference many of the presenters stressed notions of community and dialogue in their presentations. Some went so far as to say that systems is inherently feminist, as feminism is believed to be about promoting equality and respect.

While I do not agree with the more radical interpretations, I do agree that pluralism and other aspects of systems thinking do imply a need to communicate and collaborate. That having been said there is an issue that concerns me and that I have yet to hear addressed. What should happen if a participant decides that they no longer want to participate?

One justification for promoting dialogue is so that the group adopts the solutions that is best for it as a whole. This is a laudable goal. I believe that this goal is predicated on the notion that in many cases the entire group is needed for a successful solution to be manifest. In some situations this may be the case. However I can think of many cases, especially in the environmental arena, where single individuals could choose to act alone and still have a large impact. In these situations imbalances in

power allow individuals to opt out of the collective process and make individual choices with significant repercussions. While the decision may not be the best for the group, or possibly even for the individual, it is difficult to ensure that it is not taken. For example a discussion group I am a part of is currently discussing a plan for the Grand River. Ideally this discussion would involve all of those parties interested in the river's future. Even if the vast majority of the community was involved there is noting stopping a single individual from dumping sufficient chemicals to destroy all life in the river in an afternoon. This one individual has managed to disrupt the process for the entire community and there doesn't seem to be anything that could have been done to stop them. In this example one could argue that environmental protection laws act as a deterrent. If the example is changed to have a rail car filled with sulfuric acid derail into the river the results are the same and the laws have little or no effect.

Technology, power imbalances, and human will make it possible for single individuals to effect immense changes. How can practitioners of systems ensure that lone wolves do not choose to opt out of their cherished dialogues and act on their own behalf? If systems is truly an open and pluralistic practice, such individuals cannot be curtailed without jeopardizing the very foundations of the discipline.

Does Systems Want (or Need) Popularizers?

Popularizers are those people who manage to introduce particular concepts into popular culture. Some of the popularizers of systems thinking are Senge (1994) Gleick (1987), Laszlo (1972b), and Casti (1989). On Canadian television David Suzuki, through the medium of the television program "The Nature of Things", can also be seen as a popularizer.

Painting them with a broad brush, popularizers seem to be a double-edged sword. On the one hand they help expand the community of interest and promote the acceptance of new ideas. On the other hand they tend to describe their discipline without resorting to any form of rigour. Gleick (1987) merges a number of different disciplines under the blanket term 'chaos'. Senge (1994) uses the tools of systems dynamics without presenting them in the larger context of the systems field. To use Jackson's terminology, popularizers seem to be inveterate pragmatists. They bring together a variety of ideas that seem to perform well together, without going into the theoretical rigour academics would prefer.

An important question is whether the advantages to a discipline of being well known are worth the disadvantage of being known incorrectly. Funding is easier to come by when researching a "hot topic", and popularizers have a tendency of creating such topics. The problem is that the topics they

create, and the research that is supported, are sometimes very different from those that academics see as being important. Speaking from experience[8], it can be morally difficult to justify using research funds intended to investigate a popular notion to support researching a more rigorous interpretation.

If I am to believe sociologists of science such as Latour my concerns may be groundless. Latour believes that the underlying truth of an idea is that which is accepted as truth by the wider community. If popularizers are treated an expression of the public's perceptions of truth, then who can say that more scholarly interpretations are in any way better? Post-normal science, a cutting-edge systems theory, supports the notion of a wider peer community. The discipline of systems itself seems to be arguing that the popularizers are right. Who am I to disagree?

On Writing this Paper

This paper four times larger, in terms of word count, than any other document I have produced. It has taken me over 10 months to complete. Of those 10 months approximately three weeks worth of effort has gone into generating the final text. The remaining time was spent researching, reading, and agonizing over the writing process. It is this last activity that I will focus on in this reflection.

This is the first term paper that I have written as part of my Master's degree. As part of my agonizing over its production I have discovered that I have a different conception what of a Master's degree represents than many of the people I have talked to. In my mind a Master's degree should demonstrate that the recipient has attained a complete understanding of the existing material in their area. They should, at the drop of a hat, be able to discuss all of the foundation literature in their field as well as the information specific to their area of interest. In short, a Master's student should be as complete an encyclopedia of knowledge as possible. Further, they should have undertaken a complete analysis of this information. Under this model a Master's thesis should be a summary and analysis of existing information.

By comparison, in my model being awarded a Ph.D. means that the student has gone beyond analysis to synthesis. Where the Master's student can describe what is the Ph.D. student can create what has never been. An implication of this distinction is that writing a Ph.D. thesis can take very little time,

[8] My NSERC funding comes from the Industrial Engineering group to research as there is no group supporting research into systems thinking. I justify redirecting research finds in this way by believing that enhancing the teaching of systems thinking will in some way benefit future industrial engineers.

if the student has an original idea in their head. It also implies that some students will never, regardless of the effort they expend, be able to earn their Doctorate.

This second point does not, in my opinion, hold for the Master's degree. Summary and analysis are generally seen as being the easy part of scholarship, as they do not involve much creative thought. I have spoken to many people who feel that making changes to a document is easy. For these people going from a blank piece of paper to a draft is the challenge. In my case I felt up front that my skills were not adequate to meet my goals for a Master's degree. I simultaneously did not feel that I had the time to waste with draft after draft, iterating towards a quality product. The amount of information that had to be covered made such iteration a waste of time. The result, paradoxical though it may sound, was that for a long time I did nothing. Rather than waste time by iterating, I chose to waste time by not starting on work that would require iteration.

On occasion I would attempt to summarize a paper that I thought was interesting. The resulting efforts, none of which were ever completed, were disheartening. While summarizing seemed simple in the abstract, it was extremely difficult in practice. My undergraduate degree in engineering did not provide me with opportunities to practice the skill, nor did my high school education, at least as far as I can remember. It is likely that if I had put my mind to it I could have spent this entire paper, or more, summarizing any one of the articles I found. I am still not sure how much is enough. Summarizing without editorializing and judging requires significant restraint on the part of the reviewer. There is a tendency to either quote large sections of the source article, negating the value of the summary, or to rephrase the source article within a different context. Neither of these tendencies represent good scholarship. The skill comes from being able to immerse ones self into the context of the paper so that only the important quotes are extracted and so that any rephrasing or paraphrasing remains true to the original author's intent. The ability to consciously shift contexts in this manner is extremely valuable when interacting with a complex world.

Maintaining rigour in analysis and synthesis is also a difficult task. While I mentioned earlier that these divisions are heuristics, I also mentioned how annoyed I get when an author combines the three haphazardly. I feel that scholarship should require that an individual be able to discuss a piece within the context of logic and rigour, in the case of analysis, or within the context of other pieces and whole disciplines, in the case of synthesis. Again, the defining aspect of the skill is the ability to shift contexts while discussing the same work. By the end of this paper I began to feel that I could accomplish this shifting.

Interestingly, the sections I had anticipated would be the hardest were in fact the easiest. Reflection is not taught in undergraduate engineering and requires significant emotional commitment on the part of the participant. Academic papers do not usually include emotion or personal commentary, so again I had little opportunity to practice. When it came time to reflect I had significantly less problem generating what I felt to be quality content. While I know that I still have more things to say, and I am certain that I have not thought my reflections through fully, I am confident that they add significant value to this paper.

As things stand this paper does not meet my expectations[9]. I know that there is relevant information, both in my files and that I do not even know exists, that is not included. The analysis and synthesis portions of the discussions omit large quantities of discussion. I know that I have had thoughts as I walked to school, read a novel, or talked to a mentor, that never made it into this paper. The sentence structure and style, while in most cases acceptable, still contain flaws[10]. By the end of writing this paper I was more concerned with getting my thoughts recorded than with making sure that I avoided passive or run-on sentences.

The work you have just finished reading is flawed in many ways. It is however completed. I have come to realize many things in getting it to this stage. The most important thing is that it is always worth trying. In a relativist world such as ours, it is impossible to produce a work that will be relevant, topical, or even stylish, when read after its production. This holds equally for the work written ten minutes ago and for the work written a hundred years ago. The one thing that I now believe is that it's better to have written something, however flawed, than to have written nothing at all. I got a lot out of writing this paper. I hope that you got something out of reading it.

Thanks for taking the time to read it through.

Jason Andrew Foster

[9] Based on my discussion with professionals in this area, it may be that no paper could.

[10] However thanks to the miracle of the spell-checker it is unlikely to contain and spelling mistakes.

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Sources

This sections contains summarizes lists of the references that I encountered during the research for this paper. The lists were informed by the following sources:

- University of Waterloo Electronic Library Indices;
- course notes for "SD761 – The Epistemology of Systems Thinking" (Winter 1998);
- the Principia Cybernetica Web Site; and,
- the International Society for the Systems Sciences (ISSS) Web Site

For this paper my primary sources were journal articles. Compared to books and online sources, journal articles have two advantages. First, they are topical. Many of the classic works in systems theory were first published some time ago. While there are some newer books, for example Flood (1999), Gharajedaghi (1999), and Brown & Dunguid (2000), these books tend to be either application specific or to popularize systems ideas. Given my areas of interest such books were not appropriate source material. The second advantage of journal articles is that unlike online material the same journal article can usually be found twice. Online sources suffer from the transient nature of their medium. It is very likely that many of the online sources referred to by an online index will be missing or will have been altered when a researcher pursues them. This severely disrupts the continuity of research and, in my opinion, is a significant enough problem that online material should not be a part of published academic works. That having been said, the search capabilities of online services are virtually indispensable to the modern researcher.

It must be emphasized that these lists have been filtered multiple times. As with all aspects of this research my interests were the primary filters. Additional filters were any annotations made by the sources as to which items were 'recommended' or 'required'. Those items shown in **bold** are those that, based on a brief perusal or on corroborative evidence, I feel are most likely to be interesting to an individual who shares my interests.

Journal Indices

This list is the complete subset of the electronic journal indices available at the University of Waterloo that returned results for the search strings "`system* thinking`" and "`systems* design`" as of January, 2000.

- CBCA
- CBCA Fulltext
- Canadian Research Index
- Criminal Justice Abstracts
- EconLit
- GeoRef
- Geography
- Humanities Abstracts Fulltext
- INSPEC
- MLA Bibliography
- PsycINFO
- Social Sciences Abstracts
- Wilson Business Abstracts

Journals

My Searches

Note: That full list numbers over 241 different journals. It includes journals where, in the limit, a single paper was published containing the appropriate search terms. The lists below represents those journals that, in my estimation, have some level of ongoing commitment to the systems sciences or that demonstrate a breadth of interest in systems concepts.

Journals I Expected to Encounter

- Alternatives
- Behavioral Science
- Cybernetics and Systems
- Ecological Economics
- European Journal of Operational Research
- Futures
- Interfaces
- International Institute for Advanced Systems Research & Cybernetics
- International Journal of General Systems
- Journal of Systems Science and Systems Engineering
- Management Science
- Operations Research
- Systemic Practice and Action Research
- Systemist
- Systems Research and Behavioral Science
- Systems Science

Journals it Makes Sense to Have Encountered

- American Philosophical Quarterly
- Canadian Business Review
- Harvard Business Review
- Information Technology Management and Organizational Innovations
- Innovation
- **Philosophy of Social Sciences**
- **Philosophy of Science**

Journals I had no Idea I Would Encounter

- Business Ethics Quarterly
- Canadian Review of Comparative Literature
- Catholic New Times
- Entrepreneurship, Innovation and Change
- **European Journal of Engineering Education**
- Geomorphology
- Journal of Creative Behavior
- Journal of Grey System
- New Statesman and Society
- Poetics Today
- **Research in Engineering Design**
- The Journal of Sex Research

Principia Cybernetica

- Adaptive Behaviour, MIT Press
- Artificial Life, MIT Press
- Behavioral and Brain Sciences, Cambridge University Press
- Behavioral Science, ISSS
- Biological Cybernetics, Springer
- Cognitive Systems
- Complexity
- Complexity International
- Complex Systems, Complex Systems Inc.
- Cybernetica
- Cybernetics & Human Knowing
- Cybernetics and Systems Analysis, Plenum
- Cybernetics and Systems, Taylor & Francis
- Emergence: A Journal of Complexity Issues in Organizations and Management, New England Complex Systems Institute
- European Journal of Economic and Social Systems, EDP Sciences
- General Systems Yearbook, ISSS
- IFSR Newsletter, International Federation for Systems Research
- **International Journal of General Systems, Gordon and Breach**
- **International Journal of Systems Science, Taylor & Francis**
- **Journal of Applied Systems Analysis, University of Lancaster**
- **Journal of Applied Systems Studies, Cambridge International Science Publishing**
- Journal of Complexity, Academic Press
- Journal of Complex Systems, Hermes
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- **Kybernetes, MCB University Press**
- Open Systems & Information Dynamics, Kluwer
- Sistemica, IAS
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