A CASE FOR SYSTEM-SPECIFIC MODELING

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ABSTRACT

The field of systems grew out of, and then parallel to, science in many ways. In continuing its focus on general principles and the unification of science, though, through a search for isomorphies, much of the value of work in systems may have been missed. Systems are formed around patterns of organization, and the ability to affect specific systems ultimately lies in the ability to recognize and affect those patterns. Despite the early rejection of reductionism, most models and many principles used in systems still rely on characteristics applicable to physics. Softer forms of design and modeling have tended to be vague and non-specific. This paper recommends a next advance in systems, focusing on basic principles of organization at the level of unique systems.

Keywords: system-specific modeling, systems research, systems science, self-organization

INTRODUCTION

Since the introduction of formal systems theories, there have been questions about the relationship between systems and science, as well as other fields of study and methods for research and description. Some the questions are deep and philosophical. Is there one historic reality for the universe, and can we as humans come to understand that? If we discover the origins of the universe, will that explain all that has happened since then, including our own existence?

Other questions are more applied and practical. For instance, how accurately can we describe a given phenomenon, and what are the best tools or methods for doing so? Does accurate modeling require mathematical descriptions? Is electronic computation (e.g. computers) the ultimate answer to the limitations of human cognition and complexity? Are prediction and control the ultimate goals, and are models and engineering the means for achieving those? Do issues such as justice and ethics play a role in science or systems, and if so, what?

One of the great dilemmas for both science and systems has been the search for universal principles. In science this has been in terms of first principles, or natural laws. In systems it has been in terms of a general theory, unifying the principles in varying fields of science by way of *isomorphies*. The value of such principles is explained by Miller (1978):

> Perhaps the most valuable service of general theory… is suggesting how to make new observations or to conduct experiments on a wide range of phenomena in order to extend our grasp of the basic principles underlying them. Without such theory the scientist does not know how to decide which of an overwhelming number of possible observations are worth considering. A general theoretical structure also provides common measurement units that make research at different levels comparable in a way they are not when each field has its own idiosyncratic measures (p. 5).

While the potential value of general principles and theories is not disputed, this paper will propose that a distinguishing characteristic of systems work is the ability to recognize and describe specific patterns of organization. The ability to identify varying levels of organization for a given phenomena is a matter of boundary distinctions. A primary value of systems work may be found through what will be explained as *systems-specific modeling,* a process for describing essential characteristics of phenomena at given levels of functioning, and affecting those systems through their own processes of organization.

SCIENCE AND SYSTEMS

The field of systems began in science. The International Society for the Systems Sciences (ISSS), (initially, the Society for General Systems Research) for instance, was founded as a part of the American Association for the Advancement of Science (AAAS).

The introduction of modern science was meant to move human understanding beyond the vagaries of mysticism and groundless conjecture. In doing so, science has been intimately tied to methods of research. In order to understand why a particular phenomenon appeared the way that it did, or functioned in a certain way, it was not enough simply to speculate. A fundamental expectation of science has been the ability to explain phenomena in such a way as to make their future courses of action predictable, even to the point that we as humans could affect (or control) that future action.

At a more fundamental level of research, traditional science has been advanced on the basis of hypotheses which were subject to testing and verification by other scientists. As described by Hempel (1966):

> Scientific objectivity is safeguarded by the principle that while hypotheses and theories may be freely invented and *proposed* in science, they can be *accepted* into the body of scientific knowledge only if they pass critical scrutiny, which includes in particular the checking of suitable test implications by careful observation or experiment (p. 16).

System-Specific Modeling

The founders of the systems movement were also scientists, and espoused many of the same fundamental assumptions. As expressed by Miller and Miller (1992):

> Any scientific theory derives its credibility and eventual validation from its correspondence to the real phenomena to which it is applied, its usefulness in solving problems and answering questions, and the degree to which it contributes to science in general. It is, therefore, critical that research be undertaken on testable hypotheses derived from it (par. 24).

Over the last century there have been challenges to, and developments in, both science and systems. Questions have been raised about objectivity, the role of language, and the limitations of human understanding. Studies of complexity and non-linear dynamics have been incorporated into both arenas as well.

An early and critical distinction between systems science and traditional science was the understanding about how various theories fit together. Essentially, the Laws of Science as described by Newton, and then built upon by others, were thought to be universal. All other theories were expected to eventually be connected through *reductionism* – equating them with principles of physics.

For Bertalanffy and other systems scientists, such an approach was not feasible. First, physics was not considered to be adequate for explaining biology. Second, if science could be unified, it was not through physics. While general system theory did seek a unification of scientific principles, it was not through reductionism but through the discovery of isomorphies which could bridge the specialized disciplines.

Bertalanffy (1968) suggested a list of general principles which should be investigated as isomorphies applying to many different fields, including "wholeness, sum, centralization, differentiation, leading part, closed and open system, finality, equifinality, growth in time, relative growth, [and] competition" (p. 84). Troncale (1985) proposed 75 principal systems concepts (p. 63). Francois (2004) has provided over 4000 keywords and articles regarding systems and cybernetics. As yet, though, no specific list of principals or isomorphies has been produced and agreed upon by systems researchers.

In terms of methods for studying systems, Bertalanffy (1968) cites Ashby's work, differentiating empirico-intuitive approaches from deductive. Troncale (1985) proposes 60 different techniques for systems analysis. Miller and Miller (1992) list what they consider to be appropriate methods for systems research:

> Catastrophe theory, set theory, group theory, topology, fuzzy set theory, bifurcation theory, stability theory, and hierarchy theory are all used in systems models. Other mathematical approaches useful in analyzing data from this type of system or representing system processes include information theory, game theory, queuing theory, statistical decision theory, conceptual structure theory, inventory theory, factor analysis, and cluster analysis (Miller & Miller, 1992, Primer, para. 4-5)

While these methods are applicable to systems work, none are unique to it and therefore are not distinguishing factors of systems. There are of course a number of systems-specific models and methods, including Living Systems Theory, Viable System Model, Soft Systems Methodology, System Dynamics, Idealized Design, Interactive Management, Total Systems Intervention, as well as other variations on cybernetics, action research, and so on. Each chooses different systemic properties and concepts on which to focus, but none have achieved preeminent acceptance within the field of systems, or developed a critical base of research on which to advance.

In terms of appropriate methods for modeling systems, Bertlanffy (1968) describes his ultimate vision:

> General system theory, in its developed form, would replace what is known as "theory of categories"…by an exact system of logico-mathematical laws. General notions as yet expressed in the vernacular would acquire the unambiguous and exact expression possible only in mathematical language (pp. 85-86)

This contrasts of course with other statements about the very abstract nature of general system theory, in which mathematical models were not necessary, or possibly even appropriate. As Bertalanffy (1968) states elsewhere:

> One may, of course, limit it to the "technical" meaning in the sense of mathematical theory (as is frequently done), but this appears unadvisable in view of the fact that there are many "system" problems asking for "theory" which latter is not at present available in mathematical terms. So the name "general system theory" is here used broadly, similar to our speaking of the "theory of evolution," which comprises about everything between fossil digging, anatomy and the mathematical theory of selection; or "behavior theory" extending from bird watching to sophisticated neurophysiological theories. It is the introduction of a new paradigm that matters (p. xix)

Rosen (1985) describes not methodologies *per se*, but rather the nature of models. Models, as he explains, are formal systems. As such, they are representations of natural systems (essentially, things in the real world.) The more accurately the formal system replicates the key aspects of the natural system, the better it explains it, and predicts its future behavior. As he cautions, though, "A formal system…is entirely a creation of the mind, possessing no properties beyond those which enter into its definition and their implications" (p. 73). The most accurate approach to modeling is assumed to be mathematical, since it eliminates the vagaries of language. A model, though, can be entirely self-consistent – a perfect mathematical representation – without actually representing the system which it was intended to describe.

Some 30 years after the founding of what is now the ISSS, and 60 years after Bertalanffy's early writings, Troncale (1985) provided what he described as "an evaluative and prescriptive overview of the young field of systems science" (p. 43). In his article he cites “thirty-three specific obstacles inhibiting current research in systems science” (p. 43), including a need for precise definitions, a need to overcome internal conflicts within the field, a need for increased rigor in the research, and “a need for an operational taxonomy of isomorphies, systems, types and tools” (p. 64).

In the same journal issue, Boulding (1985) concludes that:

> The general systems movement itself developed in part because of a feeling among a slightly eccentric group of scholars that the disciplinary organization of the scientific and scholarly community had neglected or indeed violated the principle of basic structural unity in the systems of the universe (p. 10).

But as he continues:

> Even within the general systems movement itself, however, I worry about a certain potential for trouble. I have said, only partly in jest, that general systems divided into two parts: general general systems, which is at the more philosophical end of the scale; and special general systems, which is mathematical and computer modeling. Both, it seems to me, are desirable and useful, but I would worry if special general systems moved too much in the direction of deterministic modeling and projection (p. 11).

THE BREAK WITH SCIENCE

The science-based approach to systems is referred to by Midgley (2000) as a first wave of systems thinking. It is most often associated with systems engineering. By the late 1960s, and through the early 1980s, this first wave of systems thinking came under scrutiny and a second wave began forming. According to Midgley (2000), “In this new wave, ‘systems’ were no longer seen as real world entities, but as constructs to aid understanding. The emphasis was on dialogue, mutual appreciation and the inter-subjective construction of realities” (p. 193).

For the sake of simplicity, it easiest to trace the start of this change in systems through the influence of one very prominent systems practitioner, C. West Churchman. Churchman’s work heavily influenced that of Russell Ackoff , Peter Checkland, Bela H. Banathy, and many other systems theorists. It created a philosophical break which Banathy (1996) describes as that between hard and soft systems, or as Midgley (2000) describes as the first and second waves of systems.

From within the realm of systems work questions distinctions, including human choice and perception, have most often been addressed in terms of boundaries. Boundaries distinguish that which is part of a given system from that which is not. As described by Midgley (2000):

> Prior to the work of Churchman, many people (most notably general systems theorists) assumed that the boundaries of a system are ‘given’ by the structure of

> reality. In contrast, Churchman made it clear that boundaries are social or personal constructs that define the limits of the knowledge that is to be taken as pertinent in analysis… Thus, setting boundaries defines both the knowledge to be considered pertinent *and* the people who generate that knowledge (and who also have a stake in the results of any attempt to improve the system) (p. 35)

Midgley (2000) cites Hegel as an influence on Churchman, in terms of his call for self-reflection about theoretical assumptions, and his use of a dialectical approach for refining ideas. Most directly, though, it was the work of E. A. Singer which influenced Churchman (Britton & McCallion, 1994).

Singer had focused on the relationship between laws of science and facts that were learned. In doing so, he proposed three major philosophies of science: *rationalism* (where all facts depend on laws), *empiricism* (where all laws depend on facts, and most but not all facts depend on laws), and *criticism* (where some laws are assumed prior to any experience and others are generated as a result of experience.) He found each of these to be incomplete, though, "because there is no unique starting set of ideas or facts; there are no simple truths from which one could develop further truths" (Britton & McCallion, 1994, p. 490). Singer then defined a new philosophy of science which he called *experimentalism*. In experimentalism, "all knowledge of law implies knowledge of fact; all knowledge of fact implies knowledge of law… Facts and laws are inextricably intertwined, they cannot be separated. Truth is not the starting point of inquiry; it is the end point" (p. 490).

Science, then, becomes a progression of learning in which neither facts nor laws are sacred. This is why the *sweep-in process* (adopted by Churchman from Singer) is necessary. The scientist begins with an image of nature and then adjusts that in relation to continued observations.

Later, Churchman broke even from the experimentalist position. According to Britton & McCallion (1994), "Churchman (1968, 1971, 1979) challenged the fundamental presuppositions of the scientific viewpoint" (p. 497), which in turn challenged experimentalist philosophy. He argued that while science included human thinking, sensations and intuition, it excluded feelings. In addition, there were limitations to what could be known from a scientific viewpoint, and therefore science had no more authority for pursuing ideals than did other strategies, such as artistic, moral, or political. In essence, he reportedly believed, "science needs a guarantor to prove that it is actually bettering humankind, but there is no rational way for it to prove the existence of such a guarantor. Thus the whole scientific philosophy and strategy has to be accepted on faith" (p. 498). His solution was that "one should formulate the optimal approach from a scientific point of view and then subject it to critique from other viewpoints [i.e. *enemies*]" (p. 498).

By the late 1970s the second wave of systems was already under attack, "primarily on the grounds that the participative methodologies that characterized this wave did not account sufficiently for power relationships within interventions, and / or conflicts built into the structure of society" (Midgley, 2000, p. 203)

According to Midgley (2000), Ulrich utilized much of the work from Churchman in his development of Critical Systems Heuristics (CSH). Ulrich apparently agreed with the theoretical concept of sweeping-in of ideas, but challenged the practical reality of doing so in any comprehensive way. In order to address questions about the choice of boundaries (which in turn defined what ideas were swept in) he drew on the ideas of Jürgen Habermas and his *ideal speech situation*. In its own way, though, this is also impractical (and utopian). As explained by Midgley (2000),

> For all viewpoints to be heard, the ideal speech situation would have to extend debate to every citizen of the world, both present and future. This is quite simply impossible. Ulrich sees his task as the *pragmatisation* of the ideal speech situation, and a marriage between 'critical' and 'systems' thinking is the means by which this can be achieved (p. 139)

Ulrich's CSH was the foundation for the development of Critical Systems Thinking (CST) (Flood & Jackson, 1991). Most importantly, this relied on theories from Habermas in order to add *emancipatory* interests to the second wave of systems thinking. As Midgley (2000) explains, though, there were conflicts in the CST movement even before Flood and Jackson published the first collection of readings.

Midgley (2000) continues his own development of systems to a position of theoretical pluralism. This change indicates not just a difference in orientation or approach, but a fundamental break from traditional science. As he explains:

> In order to accept theoretical pluralism, we must (like Kuhn, 1962) give up the common assumption made in traditional scientific circles (e.g., by Popper, 1959) that knowledge is cumulative; in other words, that scientists are developing a 'body of knowledge' that is moving inexorably closer and closer to the 'truth' about reality (p. 160).

Midgley (2000) does not argue against an actual, material world, just against the idea that humans (or any sentient beings) can know the world in any absolute way. Any search for knowledge or attempt at change has implications both about the people involved, and their frames of understanding for interpretation.

Instead of a scientific approach of objective observation, Midgley (2000) proposes intervention. In fact, he suggests that "*Scientific observation has to be seen as a form of intervention*. Observation is undertaken *purposefully*, by an *agent*, to *create change* (in knowledge and / or practice)" (p. 128)

These developments in systems might be interpreted as meaning that the field of systems has completely divorced itself from science. That would be a mistake on a number of fronts.

As noted earlier, the first wave of systems was most closely associated with systems engineering. The International Council on Systems Engineering was only formed in 1990, and as of 2008 had over 6700 members (http://www.incose.org/about/index.aspx). The Systems Engineering Society of China was established in 1980, and today counts 11,000 individual members and 95 corporate members (http://www.amss.ac.cn/sesc/). It seems safe to say that technical, first-wave approaches to systems still far outweigh less technical, second-wave approaches in terms both of prevalence and number of practitioners. (By contrast, specific efforts in Japan have started incorporating Soft Systems Modeling as a way of advancing technical systems, and moving towards service science. See for example Chujo & Kijima, 2006.)

Just as importantly, at the level of national policy and funding for research the expectations of traditional science still predominate. As an example, this author was recently involved in a series of discussions with senior administrative personnel at the National Institutes of Health (NIH) in the US. Despite the fact that the US healthcare system continues in disarray most medical research funding continues to be directed towards more expensive treatment technologies. (According to the World Health Organization, "The U.S. health system spends a higher portion of its gross domestic product than any other country but ranks 37 out of 191 countries according to its performance, http://www.who.int/whr/2000/media_centre/press_release/en/). While trying to determine the kind of proposal that might be appropriate in response to an NIH announcement for systems science research, the following correspondence was received:

> Starting out [as an NIH researcher] is at first an uphill trudge. It means having to think about problems from a different perspective -- like a university researcher (hypotheses, models, theories, statistics) because they do the vetting. An additional burden is that you are advocating a paradigm shift – Complexity Theory repudiates Newtonian notions and the reductionism that has dominated medical science. Thus, you have to educate and convince reviewers, rationalize your approach, do something clever that promotes the public health, and in the end advances science (personal communication from NIH senior officer, September, 2008)

In a nutshell, this summarizes something of the first-wave, traditional science view. If you are interested in NIH funding, you must do research which will be acceptable to the existing professional community (the reviewers), which in some way fits their paradigm of science, and also advance science in a universal way. To be fair, NIH has expressed specific interests in three modeling processes familiar to systems: agent-based modeling, system dynamics, and network analysis. These are apparently only acceptable, though, if the research design is done in terms of traditional science.

It would be possible, then, to read all of the trends in the opposite way and assume that science has only continued to overcome the objections of those who are too ignorant to understand it. In his most recent book, however, Kaufmann (2008), an author and researcher who rose to prominence through his association with the Santa Fe Institute and their work in complexity science, sounds as though he is restating early works systems scientists:

> Part of my goal is to discuss newly discovered limitations to the reductionism that has dominated Western science at least since Galileo and Newton but leaves us in a meaningless world of facts devoid of values… The reductionism derived from Galileo and his successors ultimately views reality as particles (or strings) in motion in space… Societies are to be explained by laws about people, they in turn by laws about organs, then about cells, then about biochemistry, chemistry, and finally physics and particle physics… But organisms, whose evolution of organization of structures and processes, such as the human heart, cannot be deduced from physics, have causal powers of their own, and therefore are emergent real entities in the universe. So, too, are the biosphere, the human economy, human culture, human action… (pp. 2-3)

And then, only to complicate things further, an article by Bejan and Marden (2008) was recently circulated, proposing a reunification of physics and biology; that "the occurrence of design features in biological systems is similar to and can be reasoned based on the same principle as the occurrence of design features in geophysical systems" (p. 2) Then more explicitly:

> The objective of our article is to explore this broader view, the world of animate and inanimate flows together as physics, and to show that the progress that physics has made in explaining design in biology is in fact fundamental and applicable across the board. Our physics objective is similar to Perlovsky's…in his unification of the modeling of the mind based on first principles of physics (p. 3).

It would appear that we continue to be caught in an ever-expanding chasm between the complexity of the world and our needs for stability and predictability; between ways of understanding with certainty and the *messes* with which we are faced. Snow (1959) described the earlier split between literary intellectuals and scientists. For Schön (1983) it was the distinction between *rigor* and *relevance*.

The terminology of systems and complexity is finding its way into the public domain on a daily basis, in relation to reports and debates about economic crises, climate change, health pandemics, war and terrorism, energy, political stability, and so on.

On one side of the chasm lie many scientists and engineers with models of accuracy. They are the models that fit our existing computer programs and the capacity of our databases. Importantly, though, they are almost universally models based on principles of physics. That is, whatever they may describe of the living and symbolic realms of existence on Earth, the models describe first and foremost the dynamics of matter and energy.

System-Specific Modeling

On the other side of the chasm lie the researchers and writers in specifically human realms (e.g. sociology, management, etc.), as well as the policy-makers, artists, business owners and employees, farmers, and general citizens of the world. For them, the concern is not so much the accuracy of the model as the applicability to the real-world issues with which the model is supposed to help.

The chasm does not necessarily represent a lack of awareness. As described by Mainzer (2009), "Even in modern engineering science self-organizing systems are developed to manage complex networks and processes. It is now recognized that many of our ecological, social, economic, and political problems are also of a global, complex, and nonlinear nature… Systemic crises need systemic answers" (p. 53).

The goal for science and engineering, though, continues to focus on control. For example, according to Mainzer (2009), financial markets can be explained (to some degree) using models of fluid turbulence. "*Dynamical systems* and their *phase transitions* deliver a successful formalism to model the *emergence of order in nature*… The question is how to select, interpret and quantify the appropriate variables of dynamic models" (p. 58). As he concludes, "We need a *balance* between *self-organization* and an appropriate *degree of control*… In engineering science, we should aim at self-organizing systems with *controlled emergence* of new appropriate features" (p. 69).

The problem is not our lack of capabilities. It is our approach.

According to Simon (1990):

> Forty years of experience in modeling systems on computers, which every year have grown larger and faster, have taught us that brute force does not carry us along a royal road to understanding [complex] systems. Nature is capable of building, on the scale of microcosms or macrocosms or any scale between, systems whose complexity lies far beyond the reach of our computers or supercomputers, present or prospective (p. 7)

At present, the work that seems most encouraging in terms of bridging the chasm of science lies in the realm of ecosystems. As Kay (1999) explains, this only takes us back to the beginning of work in systems: "Complex systems thinking has its origins in von Bertalanffy's general systems theory" (p. 14). But as it is now applied: "A new understanding of complex systems, and in particular ecosystems, is emerging… The hierarchical nature of these systems requires that they be studied from different types of perspectives and at different scales of examination. There is no correct perspective" (p. 135).

This kind of approach changes the traditional nature of modeling, which as Simon (1990) explained was intended for prediction (in science) and prescription (in policy-making.) As Kay (1999) further elaborates:

> This approach is different from the ‘traditional’ ecosystem approaches which are interdisciplinary in nature but focus on forecasting and a single type of entity… Rather this approach is in the mode of post normal science… In this approach scientists take on the role of narrators… [who then] inform the decision makers, through the narratives, about the ecological options, the tradeoffs and uncertainties involved, and various strategies for influencing what happens on a landscape” (pp. 135-136).

Ultimately, though, it is our fundamental approach to the description of systems that needs to be reconsidered. As Simon (1990) concludes:

> Human beings are great users of natural language, and much less frequent users of numbers. It is not at all obvious that the best way to model human behavior is to describe it numerically. Of course, if we are modeling an economic system, prices and quantities of goods and services are quite natural symbols to use. But what about a political system? What are the units of power? Of persuasion? Of patriotism and nationalistic fervor? (p. 13).

SYSTEM-SPECIFIC MODELS

What seems to have been lost in striving for a unification of science is the focus on basic principles of organization. In fact, what Bertalanffy (1968) offered in contrast to reductionism was what he called *perspectivism*. Through such an approach, he proposes, we might “find constructs and possibly laws within the individual levels” (p. 49).

This takes us back to the question of boundaries. Referring again to Bertalanffy (1968), “Any system as an entity which can be investigated in its own right must have boundaries, either spatial or dynamic. Strictly speaking, spatial boundaries exist only in naïve observation, and all boundaries are ultimately dynamic” (p. 215).

So we are faced again with the question of the reality of systems. Do systems exist *out there*, in the *real world*, or are they only mental constructs? A more useful way to pose the question is actually, are systems and boundaries arbitrary? Can you simply draw a circle around any random set of elements and call it a system?

It seems critical to the study of systems that they be something other than random and arbitrary. More importantly, we as humans appear to operate in relation to specific forms of organization (systems) as if they were real.

Following from this we are drawn back to Simon’s (1990) question; what are the units, or the elements, or the properties of a given system? What is it that makes a particular system *this* rather than *that*? In terms of properties of *self-organization*, around what principles does a given organism exist and evolve? (Alternately, can a specific system change its properties of organization and continue to exist as the same system?)

The most direct example with which to begin is that of an individual person. In that case, there is a strong societal expectation that each person have an individual identity. It is a very early developmental step for children to be able to distinguish themselves from their surroundings, and from other people. Individuals who lose that sense of identity, through accident or injury resulting in amnesia, for instance, are considered to be extremely impaired, and typically experience a great deal of distress over the confusion. Disorders of dementia often cause severe disturbances in relationships, resulting from the perception that the individual is *no longer who they were*. At the same time, there is an expectation that individuals will grow and learn over time as they develop, and that development continues in different ways through the lifespan.

One the one hand, then, there is a clear expectation of a central, ongoing pattern of organization for each individual person which we tend to capture in terms of an identity. On the other hand, that pattern is dynamic rather than static. In fact, in Buddhism, *anatta* represents a concept of *not-self*; the idea that there is no absolute, unchanging identity, and that trying to cling to such a notion is one of the reasons for human suffering.

There are obviously many, many ways to describe and classify characteristics of individuals: genetic code, physical characteristics, psychological traits, familial relations, social roles, nationality, culture and ethnicity, etc. and etc. All of them have some relevance. If the desire, though, is to understand or appreciate an individual, it is the essential characteristics rather than the generalities that make a difference.

At the level of human collectives, questions of system properties become crucial. It is a basic tenant of work in systems that elements or components do not *add up* to systems (the issue of *emergence*.) The question then, is what are the *properties* of a family, or an organization, or a culture, or a society? There are many, many characteristics of each, and many factors that affect them. But what properties create the identifying characteristics of a given system?

As noted already, this is not an arbitrary matter. To act as a part of a family requires acting within specific constraints which perpetuate the patterns of organization of *that* family. There are many more commonalities between human families than differences, around the world (roles, functions, daily activities, etc.) but acting generically will not make one a part of a particular family.

In work with formal organizations (companies, etc.) the same issues hold true, but the properties are different. (Companies are not families, even in the case of a family business. If there are no distinctions then there are no differences – in which case you have only one actual entity.) In terms of effecting change, this is why a new approach is crucial. Most organizational work has attempted to apply general principles which have been assumed to be characteristic of organizations, and which have often been only analogies – not actual systemic principles – at best. In many cases, work has simply happened at the wrong level, assuming that *fixing* individual employees through training, etc., would change the patterns of a company, for instance. Generally, though, there is nothing that identifies the specific characteristics of a given organization – the actual patterns of organization – which need to be affected in order to change that particular system.

At the level of economics, specifics and generalities also apply. Since descriptions of globalization first appeared there have discussions and debates about the nature of the economies around the world. One way to think about economies is as systems of exchange. Is there now, though, only one global economy, or are they separate but more closely interdependent? For the purpose of theoretical debates, different views and definitions simply reflect different perspectives. When it comes to efforts to affect economies, though, their actual functionality makes a difference. If we simply create disturbances in the environment, systems are prone to react and re-stabilize. In order to change the functioning of the system (i.e. to create a new type of economic system) we would need to create new patterns of organization. In order to endure, the new patterns would have to be supported, or reinforced – actually co-created – interdependently with the environments of which they were a part.

Similarly, issues of climate change are extremely complex in terms of the magnitude of potential factors involved. Efforts to affect one or more of the systems involved, though, will obviously have to target specific factors thought to be of particular importance – and believed to actually make a difference in systemic functioning.

None of what has been said here negates the importance of ethics or values or morality or justice. There are conscious choices involved in what and how we choose to model. Each of those choices has implications for the outcomes involved, and the effects on people and the larger environment. Once the choices are enacted, though, they will have real consequences – or they may simply prove to be ineffective. Good intentions are laudable, but by themselves they do not create improvements.

A paper was recently circulated amongst members of the systems community, claiming that social democracy is unequivocally the best form of government (Mercieca, n.d.) According to the author, this is because, "In accordance with the Natural Law, human beings have a sacrosanct right to receive all basic help needed to survive and to become eventually an asset to society" (p. 1) The basic premise is likely to receive support from many stakeholders. An important question, though, is what actually happens when you create such a system and put it into place? Does it become sustainable, or self-organizing, by virtue of its relationship to the larger environment, or does it become parasitic over time, requiring inordinate amounts of resources in order to keep it operating?

Alternately, capitalism has been criticized on many fronts related to greed, waste, injustice, and so on. There should be no justification for continuing the destructive aspects, if we as humans can envision and create a better alternative. There are important reasons, though, to address not just *what should be*, but also *what is*. Some historic path brought capitalism into being, and to its current place in the economic systems of the world. It is worth understanding that process, because whatever factors came together to shape the current economic state of the world are still likely to be relevant to whatever will come next.

There are also crucial distinctions and clarifications that need to be kept in mind about systems, in order to avoid confusion. One of these is directly related to choices about models and boundaries. Models are always artificial (they are only limited representations), but they can also be hypothetical. For instance, a virtual reality computer program might attempt to replicate the actual experience of flying a plane (i.e. a flight simulator) or it might allow the characters themselves to fly (as in Second Life.)

There are many actions and activities that take place in the world on any given day. There is some threshold, though, at which the repetition in the patterns of organization become stable enough to create a functioning system.

This relates to a second issue as well, which is that systems do not necessarily comprise all the characteristics of the elements. It is assumed, for instance, that when different molecules combine, everything about each element is involved. When people organize into systems, however, the characteristics of the resulting system may be simpler than the combined characteristics of the individuals involved. Complexity is not additive. As individuals, we participate as parts of many types of social systems: families, groups, economies, etc. Each type of system is based on different properties, but none is simply an aggregate of all of the individual characteristics of the people involved.

Third, proposing that there are specific, essential properties of systems does not mean that they occur in isolation, or that they are simplistic. A final example from the realm of ecological systems may be helpful at this point.

Allen, Giampietro and Little (2003) describe distinctions between environmental engineering and ecological engineering. While environmental engineering involves “the purpose-driven design of structures,” ecological engineering is comprised of “the natural process of self-organization characteristic of life” (p. 389). Environmental engineering is not simply about the construction of man-made structures such as roads, bridges or buildings. As the authors explain, biological organisms ranging from yeast to draft animals can be used mechanically, with the proper constraints. The ecological engineer, by contrast, “co-opts a creative process that is intrinsic to the emergent biological structure” (p. 391). Agriculture is actually an example of ecological engineering.

The relationship between the system and its environment is critical. An organism, for example, is a more stable configuration (pattern) than an ecosystem, but both share some characteristics. As Allen, et al. (2003) explain it, “A mature organism is a relatively fixed realization of a type (associated with a given context), translated through DNA into a concrete structure” (p. 397). On the other hand, “ecosystem identity is a matter of becoming in time, so it is necessary for the process of realization to keep going. Realization is never complete… An ecosystem is a becoming, not a being” (p. 397).

The realization of a system, then, is an ongoing act of co-creation. At any given time, a system has a specific pattern of organization with some degree of stability, lasting moments or millennia. This co-creation may involve, or be affected by, human behavior directed through conscious intention, or it may operate well outside of our involvement or awareness (as most do.) Our next advance in systems might well be through focusing on specifics rather than generalities.

REFERENCES

Allen, T., Giampietro, M. and Little, A. (2003). Distinguishing Ecological Engineering from Environmental Engineering, *Ecological Engineering*. (20):389-407.

Banathy, B. (1996). *Designing Social Systems in a Changing World*. Plenum, New York.

Bejan, A. and Marden, J. (2008). The constructal unification of biological and geophysical design [Electronic version]. *Physics of Life Reviews*. http://www.constructal.org/en/art/PLREV_60.pdf

Bertalanffy, L. (1968). *General System Theory: Foundations, development, applications* (Rev. ed.). George Braziller, New York.

Boulding, K. (1985). Systems Research and the Hierarchy of World Systems: General systems in special chaos, *Systems Research*. 2(1):7-11.

Britton, G. and McCallion, H. (1994). An Overview of the Singer/Churchman/Ackoff School of Thought, *Systems Practice*. (7), 5:487-521.

Chujo, H. and Kijima, K. (2006). Soft Systems Approach to Project-based Education and its Practice in a Japanese University, *Systems Research and Behavioral Science*. (23)1:89-105.

Flood, R. and Jackson, M. (1991). *Critical Systems Thinking: Directed readings*. John Wiley & Sons, New York.

François, C. (2004). *International Encyclopedia of Systems and Cybernetics* (2nd ed.). K. G. Sauer, Munich.

Hempel, C. (1966). *Philosophy of Natural Science*. Prentice Hall, Upper Saddle River, NJ.

Kay, J. (1999). Ecosystems as Self-organizing Holarchic Open Systems: Narratives and the second law of thermodynamics [Electronic version], In S. V. Jorgensen and F. Müller (Eds.) *Handbook of Ecosystem Theories and Management*, (pp. 135-160). http://www.redfish.com/papers/Kay_SelforgEcosystem.pdf

Kauffman, S. (2008). *Reinventing the Sacred: A new view of science, reason, and religion*. Basic Books, New York.

Mainzer, K. (2009). Challenges of Complexity in the 21st Century. In *Proceedings of the 5th Symposium of the 21st Century COE Program, The Creation of Agent-Based Social Systems Sciences*, Tokyo Institute of Technology, 53-70.

Mercieca , C. (n.d.) *Best Form of Government: Social Democracy* (Received by personal communication.)

Midgley, G. (2000). *Systemic Intervention: Philosophy, methodology, and practice*. Kluwer Academic / Plenum, New York.

Miller, J. (1978). *Living Systems*. McGraw-Hill, New York.

Miller, J. G. & Miller, J. L. (1992). International Society for the Systems Sciences website, Primer Project, *Applications of Living Systems Theory*, Adapted from *Analysis of Dynamic Psychological Systems, Volume 2: Methods and Applications*, edited by Ralph L. Levine and Hiram E. Fitzgerald. Plenum Press, New York, 1992, Retrieved April 14, 2009 from, http://www.newciv.org/ISSS_Primer/asem05jm.html

Rosen, R. (1985). *Anticipatory systems: Philosophical, mathematical and methodological foundations*. Pergamon Press, New York.

Schön, D. (1983). *The Reflective Practitioner: How professionals think in action*. Basic Books, New York.

Simon, H. (1990). Prediction and Prescription in Systems Modeling, *Operations Research*, (38)1:7-14.

Snow, C. (1959). *The Two Cultures and the Scientific Revolution*. Cambridge University Press, New York.

Troncale, L. (1985). The Future of General Systems Research: Obstacles, potentials, case studies, *Systems Research*. 2 (1):43-84.