## QUALITATIVE AND QUANTITATIVE RESEARCH ON HIERARCHIES AS A CASE STUDY OF GENERAL SYSTEMS RESEARCH

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#### Abstract and Keywords

This paper presents a diagnosis of the malaise in ISSS general systems research whose clinical symptom is lack of that level of growth and development expected for a 50-year-old adiscipline compared to others that attempt to explore equally complex systems. It suggests that the illness results from the lack of qualitative specificity and quantitative, empirically-based refinement observed in recent work. Two possible prescriptions of several are suggested and explored. First, very specific systems science tenets should be expressed in discrete statements that can be non-trivially challenged and serve as a practical foci for consensusbuilding. Linkage propositions in the areas of hierarchy theory and emergence of systems are presented as specific examples of this strategy. Second, quantitative verification and stepwise refinement of some of these discrete statements must be attempted to move theory building from the purely creative realm to reliable demonstration and application. To illustrate this strategy three commonly cited "laws" of systems science are examined, and data is applied from a range of natural systems to hierarchy theory with results that lead to the new field of systems allometry.

<u>Keywords:</u> Ashby's Law; Conservation of Systems Proportions, design intervention opportunities, Deutsch's Law, Dollo's Law, empirical refinement, general systems theory, hierarchy, linkage propositions, systems allometry, Zipf/Pareto's Law

#### General Systems, Systems Science, & the Sciences of Complexity

General Systems Theory, and its more humble name general systems research, is at least 50 years old. Beginning with the conjectures of Bertalanffy, and continuing with the work of Ashby, Beer, Boulding, Churchman, Klir, Miller, Mead, Pask, Prigogine, Rappoport, vonFoerster, Weiner, and others from a diversity of fields, GST has been touted as the next important intellectual revolution. But outside an initial and aging, small and devoted following, GST simply has not been accepted or adequately developed. Clearly, there is a major difference between the development in GST and other fields such as molecular biology or even for this lack of development comes from the methods used to contribute to the field and the traditions internal to the field. This paper tries to illustrate how more *qualitative detail* and *quantitative rigor* might increase progress in systems theory.

Systems science, although contained in the new name of the ISSS, is actually not yet born. The *intent* of the name is to bring research in systems down to earth; to invent traditions and methodologies that will increase the tempo of development and progress to a degree that would permit wider recognition and more productive effort beyond proselytizing. For systems science to be recognized, its unique, distinct, and detailed knowledge base must be identified, improved, and replicated across at least one generation of students. Further, it must be institutionalized. This paper tries to illustrate how more qualitative detail and quantitative rigor could help rapidly identify and develop a significant knowledge base for systems science, speeding its acceptance and justifying its application.

While GST presents a rather disorganized set of insights and unintegrated systems approaches from different authors in different domains on mostly the theoretical level, systems science *intends* to present a set of demonstrated patterns or processes that have reached the level of consensus and demonstration. But while GST has never proven its theoretical insights, at least there is a general belief that a general theory probably exists among its devotee's. Systems science has not reached such a consensus; in fact, it is not yet fully conscious of itself.

It is not that the subject of study, complex systems, is so daunting. Workers in both the natural (physicists, biologists, mathematicians) and social sciences (economists, psychologists) at places like the Sante Fe Institute have made significant progress and attracted considerable favorable attention in the last decade working on exactly the same problems. Why have they demonstrated impressive progress, while old societies like the ISGSR/ISSS and the IFSR have not?

### Critical Need for Descriptive Depth

There is a dramatic difference between efforts at formulating GST/SS by natural-systems-based authors and social-systems-based authors. The objectives, definitions, methods, recognition of milestones, and exemplars of each of these two domains differ to such a degree as to be actually in opposition. This is unfortunate. A general theory of systems should be able

to transcend the living: non-living as well as the physical: social boundary. Instead, the best systems texts are tomes which hardly relate to one another. Workers such as Klir, Odum, and Miller, who emphasize natural systems approaches, have developed theories that have considerable descriptive depth. However, social scientists find them difficult to follow or make use of, much less apply to human needs. On the other hand, workers such as Ackoff, Churchman, Checkland, Mitroff, Linstone, Banathy and others have produced work which is more readily applied and related to human and organizational affairs, but which does not appear to have descriptive depth or sufficient empirical verification in the judgment of many natural scientists.

Is there a way to define adequacy of descriptive depth that is independent of the domain of application or study? One way might be to measure the number of discrete, identifiable propositions (relations), or processes identified by each theory. Identification is not as powerful a criterion as verification, or even better, consensus about verification. But to impose this criterion would be to favor the natural sciences. Even in these sciences it is difficult at present to provide verification and consensus about a systems process. We are left with the practical goal of ever more detailed identification of systems mechanisms. But it is not within the tradition of the social science texts to identify clearly a set of propositions as much as to discuss insights in a prose context. Texts have many words. Words are notorious chameleons. They change with the intent and background of each user. Unless there is a consensus about the meaning of the words in a proposition or process, little is accomplished. In fact, I have often suggested to my students that one definition of science is "the ever more precise use of words" -- a very anthropomorphic definition. Yet, if our GST/SS papers do not at least try to clearly demark the mechanisms and patterns they present, there is no chance for the reader to come away with usable information other than a dispersed sense that the "systems approach" must be important, and that on faith. It is exactly this vagueness inherent in some of the systems literature that has earned it the disrespect of the academy and of industry and has resulted in the demise of systems education programs.

Using hierarchy theory as a case study, this paper tries to present an "image" of a methodology that would distill the vast number of "words" about hierarchy into a series of discrete "propositions." These propositions are then characterized as conjectures that may deserve further refinement by "empirical refinement." Because of their discrete nature, these propositions : (i) are more easily taught; (ii) encourage and require additional research, perhaps over decades; (iii) render GST/SS much less vague; (iv) contribute to faster formation of a consensus by providing very clear items to debate; (v) enable clearer presentation of the tenets of GST/SS to those making decisions about it; (vi) enable more specific "connections" between various processes and mechanisms of systems (a much needed meta-level of descriptive depth); and (vii) provide for better application and demonstration of systems content.

## Needed: Empirical Refinement as a "Selection Mechanism"

In the conventional natural sciences theoretical research often is regarded as a second class endeavor compared to empirical research. This is generally because many alternative theories, most of them wrong, can be formulated simply by creative human thought. Theorizing is relatively easy. Only with a great deal of effort and patience can empirical research test each and discard those theories that are incorrect. Most empiricists toil virtually without reward all of their lives to add just a bit of hard data to the grand pile that either supports or refutes some past, grand unifying theory. Whoever manages to have first created the grand, unifying theory receives a great deal of credit, but it is the unsung empiricist that brings value and rectitude to the theory. It is the empiricist that demonstrates that an hypothesis or conjecture is correct in its model of some real system. The vigor and rigor of the most respected sciences comes exactly from this high value places on the menial, but necessary task of testing. Just as in evolution the mechanism of natural selection is essential if there is to be an increase in adaptation and fitness in the next generation, so also in systems science some selection mechanism is necessary to separate good from poor theories if there is to be any progress.

In the conventional social sciences the opposite is the case. Although some have advocated using empirical approaches to social theories, the main body of workers prefer to remain theorists. There has even been a very strong backlash against empiricists who are decried as beancounters in a specialty where the methods of the natural sciences are deemed impossible or inappropriate. For some reason natural scientists seem to insist that their empirical testing methods be simply adopted to the social domain which is clearly impossible. Yet the social scientists have not succeeded in creating new empirical refinement methods adequate for their domains. The experience of the new "sciences of complexity" are instructive here. Investigations of chaos, fractals, neural networks, artificial life and other complex systems suggest that new approaches may make social systems more tractable.

Systems scientists are rarely trained as such. They come from these other natural or social sciences with much of the baggage of their earlier training, purposes, methods, and expectations. As a result there has been a rather sterile battle between extreme approaches to systems science. Instead of reaching consensus, the empirically oriented have just deserted general systems oriented societies which in turn have been inundated with theories that are not required to be sufficiently specific, applications that cannot provide much real evidence of their utility, and endless reiterations of the same basic ideas with no improvement. So in addition to specifying the special "knowledge bits" of systems science, there is a need to distinguish between those "bits" were are accurate expressions of scaleinvariant patterns, and those which are not.

# Needed: Systems Mechanisms, Not Descriptions, Not "Approaches"

One way out of this sterile tradition is to focus on the search for scale invariant mechanisms that can be specified and ever so gradually elucidated in finer and finer detail. Some will argue that this is just reductionism, but *it is not if the mechanisms sought are scale invariant*, which is to say transdisciplinary. Focusing on "mechanisms" and "processes" that occur "out there" in real systems forces humans to be less anthropomorphic, that great enemy of most knowledge accumulation in the human race. While there is a very significant role for systems design and applications as well as systems methods, these three must be informed by a strong foundation of knowledge on how systems work or malfunction. And this foundation is best laid by patient and consistent work on detailing the fundamental processes and mechanisms of systems and their interactions.

Consider an analogy of medicine in the middle ages. There was no less a need for health care then than now. We could be outraged at the misery of the commoner and the king during those days. But until medicine improved its knowledge of how the real human body worked it could not perform the comparative miracles of cure of today. For some systems workers to say that we must apply our knowledge now or we our morally bankrupt is to say that physicians of old who applied leeches to the sick were doing more good than those who were trying to understand the basis of the disease. The preferred tradition for our systems-based societies is neither to condemn theoretical empirical research, nor design and application, but for each of these groups to respect and encourage each other, and very carefully study each others output as each domain informs the other in unique and necessary ways.

### Needed: Conjectures, Not Laws

That GST/SS studies "messy" systems is no excuse for "messy" thinking. As an exercise in debunking messy thinking we might consider the standing of several so-called systems LAWS. In virtually every case, it can be shown that there was no basis for them to be labeled "laws." At best, they might be called conjectures in the mathematical sense. A regularity, relation, or theorem that has just enough basis from past work to warrant more work, but which is not yet proven. Just this simple change to a tradition of labeling our propositions as "conjectures" not "laws" would go a long way to making GST/SS more rigorous.

Ashby's Law (Requisite Variety): Roughly states that the system doing the controlling must possess a range of variety exceeding the controlled system. This is widely stated as a law in the cybernetic community. I have attended meetings where this was virtually the central idea discussed, and always with a disconcerting idolatrous air. Yet workers such as Ackoff claim that graduate students under their direction have disproved the relation. Why is this kind of negative information not cited? In the conventional sciences negative evidence has a time-honored role. You ignore it at your peril. You are actually supposed to SEEK negative evidence more that positive. Note the exact reversal of roles. Could this be why systems science is stillborn? It is not the purpose of this paper to refute the Law of Requisite Variety at all, but instead to raise the question of the role of negative evidence in this field, and to raise the embarrassing spectre of our teaching and repeating "laws" that have not been sufficiently tested. Ashby is widely considered one of our founders and served as ISSS President. I doubt that he would have raised his general insight to the level of "law" in the face of any significant challenge. He meant only to focus attention on a specific aspect of systems. He was too much an intellect to raise it to the level it has been raised by his disciples.

**Deutsch's Law** (Dollo's Law): Roughly states that the "N + 1" level emerges not from what we might expect, the "N" level, but rather from the "N-1" level. In my first year as Managing Director of the then ISGSR I sat next to Karl Deutsch, our incoming President and dinner speaker at the head table in my duties as emcee. I had been studying GST literature with an interdisciplinary team for a contract at my home Institute and one of our elders had told me about "Deutsch's Law" from the early days of the society. I asked him about it and his story changed my conception of GS research from that day on. The reason that younger members had not known about it, he said, is that he had never published anything on the statement. He explained thus. At another, past head table he was sitting

next to Ashby and as you can imagine they had an animated conversation in which Karl had described the above stated insight and discussed how it often seemed to be the case for succession of political systems. Whereupon Ashby, who was the dinner speaker, included the statement in his talk and dubbed it "Deutsch's Law" which it has been known as ever since. How easy it is to produce a law in systems science! How envious the other sciences must be of us! In reality, we miss part of the point if we do not enter into the spirit of the early society. There were several excellent, deep thinking, creative minds gathered together to explore how disciplines might be compared for similarities rather than differences. They exulted in new insights. They played with the idea's. Neither Ashby nor Deutsch intended to sanctify these "propositional" insights as much as those who began to quote them. Deutsch is a hard working and careful scholar. I have never met anyone who could name so many names and "things you just must look into" from sheer memory. He was debunking his own law by telling me this story. The moral of the story is that we must begin a strict tradition of naming these types of insights "conjectures", not "laws."

Zipf/Pareto's Law: Roughly states that the size of objects in the inverse of the frequency of their occurrence. Of all the above this one has the most real evidence behind it. In astronomical systems, Wilson has demonstrated the relation to be true of entities at the scale of clusters of galaxies and also at the scale of stars. On the chemical level, Winiwarter demonstrated that it is true of elements and also of corporations. In biological systems, Yule demonstrated that it is true of organisms and species. On the human level, Auerbach has shown that the relation holds true of nation-states. In the original version it was shown to be true of "words in text" by Zipf and monetary units as well as salaries by Pareto, so that it holds for information systems and artifical systems. But while it has much empirical verification compared to the others, this "law" illustrates another dilemma. How much evidence is needed to "prove" a transdisciplinary proposition? Like the most rigorous of the physical sciences, empirical studies never prove a theory or relation so much as fail to refute it. When a significant amount of empirical evidence accumulates that agrees with the relation, and none that contradicts it, a consensus is tentatively reached about its verity. But for a systems science relation to reach this status, it must be challenged on all scalar levels. Otherwise it is not scale-invariant. Must we now check Zipf-Pareto on the sub-atomic level? The sub-sub-atomic level? All languages? Therein lies the dilemma. But at least a tradition of pursuing stepwise empirical refinement of a putative systems law would provide an immense amount of new detail about many systems and at least tell us over what range of systems or scales it might apply and perhaps even why it occurs at all. This latter would help us immensely in

the design of new systems and the correction of malfunctioning systems. Which brings us to the next step in forming a stronger inferential [] tradition in the systems sciences.....the need for a much stronger scholarship of studying the available literature.

# Trends in Hierarchy Theory Literature As A Measure

We cannot hope to cover the literature of some 80 possible systems mechanisms recognized at our Institute [6] in one paper. Perhaps a case study of just the literature on hierarchy theory, not counting the numerous books in the area [1, 2, 3, 4, 5, 18], would give us a taste of the extent of available knowledge on any one putative systems isomorphy. The point will be that to adequately specify that knowledge in discrete statements, and to adequately empirically challenge those statements, the entire literature must be studied carefully. I contend that this is not the tradition in systems science to date. And yet it is the hallmark of adequate work in every other discipline, hard or soft. Simply look at the references cited section to prove the point. A review on a very, very specific piece of the knowledge base of cell biology often cites 100's of papers. Do we? And often our papers are targeted at a much larger piece of knowledge with less than ten references to cover it.

One way to characterize the extent of the literature on Hierarchy Theory as a case study of systems research in general would be to measure the total number of articles produced per year on this one topic across the disciplines. I first tried this in the mid-eighties by searching for usage of the term "hierarch\*" in refereed journals in MEDLINE, BIOSIS, INSPEC, and SCISEARCH over a fifteen year period from 1966-80. We retrieved 2,658 research articles. Analysis of the titles and institutions indicated that hierarchy research was conducted in 32 disciplines and in 27 countries by hundreds of investigators. A relational data base listing of the specifics is available. For this paper, I extended this search to use of the term "hierarch\*" in titles in refereed journals in BIOSIS, MEDLINE, COMPENDEX, INSPEC, SCISEARCH, and SOCIAL SCISEARCH from 1985 to the first third of 1992. We retrieved a total of 9,684 in this seven year period. It would appear that reports of hierarchy research had increased by over 350 % in a period covering half the years in the last decade not correcting for redundant citations or journal coverage. If citations of "hierarch\*" in the title or abstract is included in the search routine, the total is 15,630. While inclusion of the term in the title indicates an article truly focused on hierarchy, inclusion in the abstract suggests the article contains useful info.

BIOSIS covers biological science journals, but not medicine. MEDLINE covers some biology journals and all medical journals. COMPENDEX covers the engineering disciplines. INSPEC covers physics, computer science, and CS-related electrical engineering, SCISEARCH covers the hard science based Current Contents, and SOCIAL SCISEARCH covers the social science based CURRENT CONTENTS. There is some redundancy so total numbers would be somewhat smaller.

What is fascinating, though, is the total. In an approximately twenty year period, which corresponds to about half of a professional working lifetime, a rigorous generalist who aspired to covering hierarchy research thoroughly and in a transdisciplinary way would have to examine from 12,000 to 18,000 research articles, or 75 per month. This is not impossible, but unlikely simply because of the number of different disciplines involved. Yet if one is serious about contributing to the field of scaleinvariant research on hierarchies, it is simply necessary. Clearly, no one is doing this level of general systems research. Now if we consider the incredible amount of detail we could add to our understanding of the role of hierarchical structure and process in general systems if we did analyze what is already available, then it becomes clear very quickly that a method in needed to encode this detail in a way that encourages and enables communication, education, testing, and consensus-building. One way devised at our Institute to accomplish this is to encode the results in a consistent format called "linkage propositions."

### Qualitative Depth: Linkage Propositions on Hierarchy Theory

Linkage propositions have been described in a series of previous papers [6, 8, 11, 12, 14, 17]. Here I would just like to illustrate how the method of linkage propositions could be used to add a needed dimension of increased specificity or needed qualitative depth to systems knowledge and its empirical refinement.

At our Institute we are trying to organize and present each systems mechanism in as much detail as possible for our students. That, after all, is our primary task. The following list shows the 20 different categories of knowledge we try to present on each systems process and where linkage propositions fit in relative to the others.

- Identifying Characteristics or Criteria (Qualitative, Descriptive)
- Comparative Definitions
- Intriguing Examples in Real Systems (Exemplar vs Case Study)
- Role or Function in Systems Life Cycle

• Discinyms

• Linkage Propositions on and between...

- Types and Taxonomies
- Formal Development (Computer Representation & Simulation)
- Formal Development (Mathematical)
- Tests For Transdisciplinarity
- · Analysis of Requirements and Pre-requisites
- Special Techniques
- Relationship to Systems Analytical Methods
- Role in Known Pathologies of Systems
- Design Intervention Opportunities (DIO's)
- · Discovery and History
- Data to Date
- · Graphics, Sound, Animation, and Slide Inventory
- Evaluation of Current Status: Future Questions
- Literature Data Base
- · Institutions and Workers

Now given the above analysis of the hierarchy literature, it is clear that putative linkage propositions (LP's) could potentially be found in a wide range of disciplines. It is essential that the following criteria be used to formulate these as we study the various domain literatures. LP's must: (i) be expressed in generic, not disciplinary-based terms or scale restricted processes; (ii) express one discrete, directional, or mutual influence between two and only two systems mechanisms; (iii) follow the form "systems process A" (underlined) "influence phrase" "systems process B" (underlined); (iv) select "influence" terminology form the Table of Influences for consistent terminology; (v) select "systems process or mechanism" from the List of Isomorphies for consistent terminology; (vi) add either isomorphies, mechanisms, processes, or influences to the Lists and Table by consensus agreement with LPTM-GENSYS participants; (vii) break up chaining or pathways of influences described in the literature into dyadic units; (viii) follow LP with last name of source to credit workers in the field; (ix) place on GENSYS as soon as possible; (x) document all literature sources and institutions which provide evidence for the LP according to the formats of GENSYS.

The few examples below illustrate how the voluminous words of the hierarchy literature could be captured in a few discrete phrases of the LP's. Samples of LP's are included from various disciplinary domains to give a flavor of how the literature can be mined for details on systems mechanisms and processes. These discrete LP's can then be recorded and communicated to a wider audience and future generations for examination and cross-comparison with other domains on the way to proving transdisciplinarity or scale invariance, or to increase dramatically the empirical refinement of each LP or LP set (next section):

Some linkage propositions on hierarchy theory come from recent studies in theoretical physics, for example:

- Uncoupling of Dualities is a partial cause of Symmetry Breaks
- Symmetry Breaks are a partial cause of Hierarchical Structure
- <u>Non-Equilibrium Thermodynamics</u> is a necessary condition for Diffusion Limited Aggregation
- Diffusion Limited Aggregation is a partial cause of Hierarchy Structure

Some linkage propositions on hierarchy theory come from recent studies in theoretical ecology and evolution, (see Allen)[1] for example:

- Hierarchical Structure insulates and excludes levels from Perturbations
- Too many Hierarchical Couplings/Linkages reduce Equilibrium
- Too few Hierarchical Couplings/Linkages reduce Stability
- Too few <u>Hierarchical Couplings/Linkages</u> reduce <u>Cooperative</u> <u>Mechanisms</u>
- Too few Hierarchical Couplings/Linkages reduce Feedback Control

Some linkage propositions on hierarchy theory come from the past, established literature of general systems theory itself, for example: • Hierarchical Structures are Decomposable

- <u>Hierarchical Structures</u> are <u>Decomposable</u>
- Hierarchical Process is a partial cause of the Exclusion Principle
- <u>Allometries in Hierarchies</u> result from comparison of magnitudes of <u>Boundary Conditions</u>
- Transgressive Equilibrium is a partial cause of Hierarchical Levels

Some linkage propositions on hierarchy theory come from studies in mathematics and computer science, for example:

- Some <u>Hierarchies</u> possess a <u>Fractal Structure</u>
- <u>Non-Equilibrium Thermodynamics</u> is a necessary condition for <u>Fractal</u> <u>Structure</u>

• <u>Diffusion Limited Aggregation</u> is a partial cause of <u>Fractal Structure</u> Note how these LP's relate to the first four in the first group and all connect with hierarchical form and process.

Each of the systems processes could serve as nodes that are visualized in graphics with LP's as connecting them. It is easy to see that many LP's will connect the many nodes resulting in a very rich network of very specific interactions as shown in Figure One. It is this level of specificity that we will need to increase respect for the detail and rigor of

systems science. It is also a computerized version of this level of detail that will be needed to truly use systems theory to help design new systems, or diagnose the problems of malfunctioning systems.

### Empirical Selection: Systems Allometry and Hierarchy Theory

It is necessary, but not sufficient that numerous LP's be formed on hierarchy theory or any of the other 80+ systems processes. The next essential step is that they be empirically refined [13]. Until some evidence of their verity, and their limitations or range of utility are demonstrated, LP's are much less useful. Long-term studies should be able to increase dramatically the resolution of detail on any LP or set of LP's further increasing their utility and applicability. How can you propose to design better systems unless you have a toolbox of proven design elements and how they best fit together in particular circumstances?

As a case study of how elements of a systems mechanism or its resulting structure can be empirically refined consider the application of data to some of the most fundamental questions in hierarchy theory [9]. The quantitative and empirical testing of simple assumptions underlying hierarchy theory has been a long-term task at our Institute. We have attempted to use the data on 15 Newtonian parameters and several information parameters contained in the refereed literature to statistically test for clustering of natural systems into recognizable levels. Then we attempted to ask if there was some regularity to the gaps between levels, to the minimum and maximum size of entities on each level and between levels, and whether or not there were any regular patterns that are maintained across levels. A series of papers describe the surprising results of these attempts [7, 12, 15, 16, 17]. For example, Figures Two to Four show that many clearly different entities at widely different scales in nature that arose by different coupling mechanisms at remarkably different times still all follow statistically significant proportions that can be expressed in allometric formulae. We call this Conservation of General Systems Proportions [16]. It is truly a general systems finding that could only emerge from the transdisciplinary work characteristic of the systems science knowledge base. It is too soon to describe it as proven, but data support is accumulating. It is this type of qualitative depth and quantitative testing and empirical verification that we advocate for general systems research and the eventual emergence and acceptance of systems science.

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