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1.2 MANAGING DIRECTOR'S EDITORIAL

WHAT WOULD A GENERAL SYSTEMS THEORY LOOK LIKE IF I BUMPED INTO IT?

The Guest Editorial by Albert G. Wilson for this issue of the Bulletin recommends preservation of and a wide diversity of approaches to a general systems theory. Examination of our current literature indicates that this goal may have already been accomplished. One of the major criticisms of General Systems Theory stems from outside observers' inability to pin down the goals of the Society in terms of a cogent description of a GST much less its actual accomplishments toward realizing that goal. What follows is an attempt to sketch out some of these goals; namely a tentative description of the characteristics or qualities one would expect of a General Systems Theory. These statements are not in any way SGSR Policy but rather the editorial opinions of one worker in the field. They are intended to be provocative and critical in order to stimulate continued debate in our new Letters To the Editor Section; a debate that hopefully will be carried on in discussions during the annual meetings.

I begin by stating unequivocally that there is no current general theory of systems. No candidate theory, indeed, if many of these can be identified at all, has all the characteristics necessary for a general systems theory. We must not speak (even to each other) as though a theory already exists. The forefathers of the movement merely pointed out a direction; they did not reach the end of the journey, nor even describe the major signposts along the way. A number of currently prominent workers in the field state categorically that no single general systems theory could, in fact, exist. The definition of systems allows for a great diversity of systems types. These different systems range all across natural as well as man-made phenomena and so share some commonalities as well as many distinct and unique emerging qualities that differ from one another. No one theory of isomorphies could then encompass all systems types. Does this opinion of some experienced workers in the field negate the very tenents upon which the Society for General Systems Research is formed? Must there be one monolithic general systems theory for our work to be significant? Some level-headed investigators would maintain that rather than refuting the purpose for which we join together in a society, this recognition of the polymorphic nature of systems theory would open up our discussions, and render our attempts more rational and unpretentious in the eyes of outside observers. It is the constant call for synthesis exaggerated to the extreme of stating or at least assuming that a single general systems theory exists that gives the general systems approach as an aura of megalomania. Consequently one characteristic of efforts toward formulation of a general theory of systems would be the widespread assumption that a cluster of putative theories is the goal and not a single monolithic giant. In fact, we should never let ourselves even use the term "general systems theory" which implies that there is a "general system" to model. We should always say "a general theory of systems" which implies multiplicity.

Another criticism often leveled at our field is its vagueness. Ironically, this is the tailside of the same coin just mentioned. If the head of the coin is the statement that a number of partial systems theories must indeed exist, then the consequence of that is . . . no singular consensus can ever be found. To specialty fields, this amounts to an admission of inherent vagueness which defies the construction of consensus over time which is what a "discipline" is after all. A recent book review described the goals of a university as "the sharing of a common purpose: to transmit, by teaching, the orthodoxy of the discipline and to generate, by research, a rigorously controlled system of dissent from the orthodoxy". To the disciplinary sciences, the emphasis at the present is much in favor of the orthodoxy and the dissent is only admitted when evidence is piled up so high that it can no longer be ignored ("note" plate tectonics in geology, or clustering in astronomy). Our field has no orthodoxy or consensus to present because it is in its early formative stage and so appears inexcusably vague and chaotic to the discipline trained mind. Part of this vagueness is not indigenous to the field, but rather a consequence of our currently sloppy and immature methodology. Perhaps the greatest disadvantage of this vagueness besides its generation of considerable amounts of well-placed criticism is that it leads many of the very most talented minds to avoid participation in and improvement of our attempts.

But there are also advantages to vagueness. Whenever, in the past, attempts have been made to make our deliberations, our journals, our proceedings, our meetings more rigorous through peer criticism and review, the counter-argument has been that the establishment of consensus and orthodoxy stifles dissent. If this were so, of course, none of the sciences would have progressed, and we see the opposite is the case. Creativity in the development of alternatives must be matched with a considerable amount of empirical effort arrived at eliminating alternatives before any orthodox could be convinced of a new orthodox. Vagueness may allow the expression of creativity of all types, but it also leaves one without the tools to distinguish between nonsense and true contribution. Although we might rate vagueness high in stimulating the field and keeping it open,

both quite desirable aims, it also can serve to stifle progress in the field. Consequently, we would not count vagueness as a quality or characteristic of a putative general systems theory. Precisely the opposite would be the case. A general systems theory should be highly specified, while allowing for the "perspectivism" Dr. Wilson describes in his editorial. And if this sounds paradoxical, it is. Workers in GST should seek paradox, not seek to avoid or resolve paradox as the disciplinary scientists do. That is a central characteristic of GST.

Another premiere characteristic would have to be an adequate level of abstraction. Virtually all fields in science abstract and formulate models from the events of nature. To this degree, all are searching for some level of isomorphies to simplify the myriad details of existence. The quality that distinguishes general systems theory from these other attempts would be the comparative degree of abstraction. While disciplines abstract from the immediate phenomena of nature, a general theory of systems would abstract from abstractions produced by various disciplines. Since the definition of system is fairly isomorphic, for example, between physical and living systems, the working concepts in a general systems theory should span such normal distinctions. Not only would general systems concepts be true across disciplinary lines, they would be true even across major clusters of disciplines even so far as merging such things as living and non-living systems. Few attempts at general systems theory have been as encyclopedic as James G. Miller's opus Living Systems and the theory of systems that emerges from it. However, one criticism of Miller's Living Systems is its concentration on the various levels of biological systems without exploring which of those isomorphies are true of physical systems in addition. Many of the cross-level hypotheses he cites would not transcend the living/non-living demarcation. This supports the earlier described necessity for there to be a cluster of systems theories rather than one monolithic candidate. But we are much closer to our goal if the cross-level hypotheses are true of all levels of living systems (We then have something detailed to compare to physical systems). But these hypotheses would not be candidate isomorphies for a GST and could not be described as such. Surely there will be debate on what is an adequate level of abstraction and the issue will not be resolved for a long time to come. But if the issue of "level of abstraction" is to be properly addressed and resolved at all, each of us must select & communicate specific criteria for appropriate levels of abstraction and to defend those criteria in debates with each other.

While levels of abstraction are one characteristic of candidate systems theories, there is simultaneously the issue of "breadth". A theory which clearly and successfully utilizes two or three isomorphies of considerable depth of abstraction (such that no one could challenge their appropriateness as general systems axioms) could still be wanting in the quality of "scope breadth". Successful candidate theories will have to possess an adequate *number of isomorphies.* For example candidate theories which rest solely on knowledge of feedback and control would be wanting in their descriptions of autopoietic, developmental, evolutionary, stabilizing, or hierarchical features — all of which are perhaps included in parts of other systems theories. This list is merely suggestive, no one consensus exists on what proportion of conglomeration of any of the several dozens of suggested isomorphies are necessary to form the full set. But it is clear that a true GST must *prove* it utilizes the *minimal full set.* Again, only future debate can resolve this issue. But the issue will never be resolved unless it is clearly stated over and over again, and consistent debate on the topic occurs. In most of the literature in our society, neither the issue of "appropriate level of abstraction" or the issue of "appropriate or minimal breadth and scope" of isomorphies is even addressed. I think they must be — soon and vigorously.

A useful set of qualities for a general theory of systems would emerge from the well-established Kuhnian definition of a paradigm. Historians and philosophers analyzing the progress of bodies of knowledge throughout modern civilization have noted that several characteristics have emerged that constitute orthodoxy in a discipline. The seven most-recognized are (1) concepts, (2) symbolic generalizations, (3) methodology/techniques/tools, (4) guiding questions, (5) exemplars, (6) models, and (7) values.

How far has work toward a general theory of systems progressed in each of these seven characteristics of a body of knowledge? The concepts involved in general systems theory manifest themselves as shared elements of language, derogatorily termed "jargon" by people outside the field. Jargon in systems theory would consist of the names of identified isomorphies and/or processes typical of most systems. With no consensus it is hard to estimate how many systems concepts have been identified but somewhere between 30 and 150 systems terms are recognized. The high level of abstraction required for recognizing isomorphies and the lack of immediacy of experience of them in the daily life of humans renders general systems terms especially susceptible to the acucsation of being merely jargon. Ironically, the very disciplines whose own scientific terms are so much jargon to the general public are the loudest in the criticisms of the systems field for creating jargon. We just must stand up to such criticism and remind them of the abstruse nature of their own words. Several attempts are being made by sociologists and general systems theorists to collect glossaries of systems terms and to pin down their usage and their meaning. Sessions on this particular topic have occurred in the last two meetings and will continue (see work of Oliva, Rogers, Robbins, Jain, or Troncale). However, the attendance at these sessions has been low and the impact of their attempts has thus been small. Do all theorists use these terms? No! Most of the papers observed in the Proceedings and the Journal of the Society focus on one or another isomorphy or process and few show a concern for using increased numbers of isomorphies and processes simultaneously in explaining natural phenomena. Certainly, the focus on a specific isomorphy or process is necessary in the beginning to define it and isolate its effect (if this is possible given the holistic framework we support). We are

immediately caught by our own declarations of reductionist approaches in these cases. Can we truly understand any one isomorphy or process without considering the others? Further, it appears that many workers only know of a small number of the total available isomorphies or processes and prefer to work on those alone rather than expanding their horizons to the total set. This inhibits the general systems approaches that are necessary and provides weak, overly simplistic models that are easy to criticize.

The second characteristic of a mature paradigm, that of "symbolic generalizations" presents an even more discouraging picture. In our field, I would define symbolic generalizations as the linkages between or the interactions among the various isomorphies that give rise to the processes which are observed in different systems. Aside from the mathematical strains of general systems theory, I would have to conclude that the linkages between systems concepts do not exist in much of the literature. Unfortunately, the conceptual strain of systems theory and the mathematical strain are sufficiently separated that even when symbolic generalizations occur in the latter they do not filter into the work of the former. Beyond this, there is always the danger of what may be called "empty formalisms" in mathematics. These would be symbolic generalizations of relationships that although highly specified in mathematics do not generate new understandings, or adequately model deep characteristics of the phenomena under investigation. At the present time, it is very hard to distinguish between empty formalisms in mathematical systems theory and truly innovative results of the type characterized by Wilson in the Guest Editorial of this issue. We have much to accomplish in the area of symbolic generalizations (which should be our forte) but little will be accomplished if we do not clearly state the deficit and every paper published attempts to satisfy the need.

The third Kuhnian characteristic of an orthodox field is the identification of precise methodologies/techniques and tools for arriving at knowledge in the field. General systems theory, by borrowing from associated fields such as systems engineering, logical systems analysis, mathematical systems analysis, operations research, artificial intelligence, and numerous fields, can boast of too many methodologies rather than too few, with none tailored specifically to advancing the field of *general* systems theory. It is difficult to determine whether this prolificacy of methodologies is healthy or whether it is merely a symptom of neglect at founding or discovering an appropriate methodology for this particular use. In actuality, few of the journals associated with the general type of systems theory normally includes precise specifications for a methodology section for submitted papers. And yet this is the main criteria for peer review of papers in virtually all other rigorous fields. The absence of such criteria for papers submitted to general systems journals and proceedings is a glaring omission and opens us to widespread criticisms. One could argue that specification of methodologies and approaches sanctioned by the field would constrain creativity. But not requiring specification of methodologies in detail leaves the field open to sloppy thinking. In such cases, the noise produced by so many papers of low quality greatly exceeds the signal necessary for colleagues to build upon each other's works. But the most telling point of all is the absence of a transdisciplinary methodology inherent to GST; one that exists only in work on GST models and nowhere else; one that emerges from the very nature of isomorphies.

Examination of the Kuhnian characteristic of a paradigm entitled "guiding questions" reveals a similar dilemma. J. R. Platt in his excellent article, "Strong Inference," indicates that the most fertile fields are those which suggest very detailed but *multiple alternatives*, each a mechanical explanation of a phenomenon, and then proceeds to eliminate most of the alternatives through carefully controlled experimentation. The multiple alternatives posed may be seen as the "guiding questions" which direct future work in the field. He suggests that fields such as molecular biology and subatomic particle physics (and since then astronomy and geology) proceed fastest when their guiding questions are the most specific and as much attention is paid to formulating essential and intriguing guiding questions as to their resolution. It would be interesting to sample a large variety of papers in the proceedings, journals, and yearbooks of the Society and see whether or not they begin with a statement of a clear question to be resolved through the investigations reported in the paper. My current opinion is that most papers do not even state a question to be resolved much less a set of multiple alternative questions, and so they leave the field to wander rather aimlessly rather than build upon itself successively each year.

The next two Kuhnian characteristics are exemplars and models. Exemplars consist of successful solutions of problems using the concepts and tools of the body of knowledge. We must be very cautious in claiming the general systems theory has any exemplars at the present time. Certainly, the systems approach has been applied to help understand a number of technological and societal problems, but one cannot disassociate in most cases a general systems approach from its more rigorous ancestors in the fields of applied systems analysis. There is a tendency in our enthusiasm for general systems theory to blur the distinction between systems analysis, systems theory, and general systems integration. I discussed this helpful distinction in the preface to the Proceedings for the 1982 Annual Meeting. Certainly, every attempt at systems anlysis or disciplinary systems modeling improves our chances of later achieving the true, detailed integration across the disciplines characteristic of a general systems theory. But that is not to say one is the other. This is confusing disciplinary ontogeny with transdisciplinary phylogeny.

The last Kuhnian characteristic of a viable field was a surprise to scientists who usually maintain science is value-neutral. Kuhn and other sociologists have pointed out that all sciences possess unstated value assumptions. A number of people have pointed out that once achieved, a general systems theory would be of great significance to the formulation of values. The Fuschl Conversations are exploring this point. Here a group of systems scientists from across the world are studying the

applications of systems to improving human understanding, human living conditions, and the quality of life. Their emphasis is on values. I am currently writing The Tao of Systems Science to illustrate detailed cases wherein there exists significant commonality between ancient Eastern value philosophies and modern systems theory. Our discussions at annual Meetings on the philosophy of systems theory are very lively and widely attended. The premiere systems philosopher, Mario Bunge, has explored the significant differences in philosophy and values which result from systems assumptions. All of these are examples of the potential significance the general systems approach has for recalculation of human values. However, we must again express some skepticism and caution if we are not to sound pretentious. We have to take a hardnosed look at what general systems theory has already uniquely contributed to value systems as opposed to the ever-talked about potential for delivery. Synthesis-oriented people tend to blur distinctions, and although the hope for significant developments seems well warranted from our systems intuition, the reality of our current contributions and communication of those potential contributions lags far behind.

Finally, the thorny issue of verifiability/falsifiability confronts all those engaged in holistic approaches. The tendency is to attack those who attack us. Since the disciplines are so much oriented toward ultra-reductionistic experimentation and testing, general systems theorists tend to overact by condemning specialist colleagues as much as those colleagues condemn those who attempt systems integration. Perhaps holists like Ghandi and Jesus Christ would be appropriate models at that point. It is my opinion that systems theorists should always praise the dedication and hard-work of disciplinary specialists in investigating the minutiae of natural and manmade phenomena. We rely on them for the data and the ideas that we seek to compare. Without their efforts, comparison would be relegated to such an extremely vague and high level of absraction that our attempts would be indistinguishable from those of mystic philosophers. In any case, it is counterproductive to attack verifiability/falsifiability. Without it, there is little progress in a knowledge base. Without it, the Kuhnian paradigmatic cycle does not exist. And if anyone should be concerned about developmental progression, phylogenetic improvement, evolution, and cycles, it should be workers in the systems field. But given that some of us would praise the disciplines while ourselves taking another methodological course, there still remains the thorny issue of verifiability/falsifiability because we don't know how to do it in systems theory. In mathematical systems analysis, a certain amount of proof is possible using the standard procedures of the field, but when one is dealing with networks of cause and effect, it is very difficult to isolate and control one item in a field when our working hypothesis is that all working items in the field are coordinately changed as a unit. Ignoring this issue and the dilemma or paradox which we face does not improve the situation. So, I raise the issue in this essay as a direct challenge to be joined with the issue of the lack of methodology/technique/tool descriptions in most of our papers as an issue that once faced and dealt with will greatly improve the respect for our discipline and its productivity.

In summary, every candidate GST should (i) have a built-in, *non-anthropomorphic typology/taxonomy* showing clearly both the *extent and* the *limits* of its application; (ii) include a precise indication of its position in a *set* of *polymorphic* GST's; (iii) consist of a *highly detailed* set of isomorphies, propositions or axioms; (iv) exhibit an *appropriately high level of abstraction* from the real systems it models to prove that it is transdisciplinary or truly isomorphic, (v) prove use of the *minimal full set of isomorphies* and not just a favored few; (vi) *prescribe specifically* symbolic generalizations of the *interactions between isomorphies* and indicate how these automatically generate the characteristics of systemness; (vii) explain its *methodology for* demonstrating and elucidating the above elements, and for *verifying or falsifying* the full set; (viii) indicate how the above leads to still more specific, penetrating, and challenging alternatives (guiding questions) to explore, in ever expanding recursive cycles; (ix) have *demonstrated* successful *deabstraction* of the GST model to specific disciplines or real systems leading to a much better understanding of the target area than the discipline could provide, or solving some of its heretofore unsolved problems, (x) reveal its *implicit values* and showed them to be generalizable values of widespread significance to our future.

I wonder if any of us could convince a critically-minded audience that they have such a theory? If not, let's say much less about what we have, how everyone should beat our doors down to apply an essentially non-existent GST model to their fields and let's say more about what we are trying to accomplish by the *end of the century* and how we humbly intend to get to work doing it.

as I read this with my students in 2005, this is still not well begun, much less accomplished

note added in Proof:

The productivity which Dr. Wilson calls for in GST arises first and foremost from the formulation and investigation of isomorphies. Methodologies for dealing with complexity, or applications of isomorphies do not lead to *general* systems models as directly as focused work on elucidation of isomorphies. These methods and applications reside more in the area of *systems analysis* than *general* systems *synthesis*. Of papers submitted to-date for our next annual meeting only or % are clearly on studies of an isomorphy. A single Institute is responsible for the scheduling of or % of these. Not a single paper studies the interconnections between isomorphies, although this and the investigation of isomorphies were specifically included in the Call for Papers, and explicit Paper Session titles were given. On a broad scale, it would appear that we are not accomplishing the foremost objective of our Society.