THE FUTURE OF GENERAL SYSTEMS RESEARCH: OBSTACLES, POTENTIALS, CASE STUDIES

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FORWORD

One of the tasks of IIASA is to keep track of new developments in the sub-specialities within the larger field of systems science for their potential relevance to its mission. Although focused on systems analysis and its tools, the Institute can profit from discoveries and improvements in allied fields like systems methodology and general systems theory. This paper provides a broad-based overview of the field of general systems science and clearly distinguishes it from attempts at systems analysis and theory, while describing mutual impacts. The paper includes some very practical information as well as useful indicators of progress in the field, but, equally important, gives detailed evaluations of a large number of obstacles preventing its further progress. The author argues convincingly that improvement of the knowledge-base of this field depends upon clear recognition of these obstacles and formulation of mechanisms to overcome each one. To this end, he provides a detailed cross-impact matrix of the interactions and dependencies among the thirty-three obstacles described. Many of these obstacles are also true of the tools of systems analysis and the modeling attempts at IIASA, and so this paper contributes to the broadest perspective of our mission.

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The Future of General Systems Research: Obstacles, Potentials, Case Studies

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Key Words—Systems methodology; hierarchy; isomorphy; linkage proposition; futures; general systems theory; systems applications; cross-impact analysis; systems glossary; GST performance criteria; discinyms; scale translation protocals.

Abstract—This paper attempts to provide an evaluative and prescriptive overview of the young field of systems science as exemplified by one of its 'specialties' general systems theory (GST). Subjective observation and some data on seven vital signs are presented to measure the progress of the field over the last two decades. Thirty-three specific obstacles inhibiting current research in systems science are presented. Suggestions for overcoming these obstacles are cited as a prescription for improved progress in the field. A sampling of some of the potential near-term developments that may be expected in the three rather distinct areas of research on systems isomorphies, improvement of systems methodologies, and the utility of systems applications are illustrated with mini-case studies. Throughout, there is an attempt to identify 'key' questions and practical mechanisms that might serve as a stimulus for research. Finally, a set of criteria defining a general theory of systems is suggested and illustrated with a case study. The paper concludes with a projection of the long-term contributions that systems science may make toward a resolution of the growing chasm between high-tech solutions and high-value needs in human systems.

1. INTRODUCTION: USEFUL LIMITS AND DISTINCTIONS

It is probably foolish for anyone to attempt to predict the future accomplishments of a reductionist speciality much less a transdisciplinary field such as systems science whose practitioners have not yet reached even an initial consensus. However, the need for self-reference and internal critical debate is also very great in such immature fields. So while the limitations of this paper must be severe, it is nevertheless a sincere attempt to open to conscious discussion specific obstacles inhibiting timely development of a general theory of systems.

It is much safer and more informative to concentrate on the important needs of a field than to try to project its near-term developments so more obstacles will be cited than potential breakthroughs. Wherever possible each obstacle cited will be matched with a discussion of its consequences, current activities addressed to it, and a set of detailed prescriptions for overcoming the obstacle. This analysis is not presented in the tradition of a research article; it is, instead, a detailed, but opinion-oriented editorial statement examining the organizational and methodological process of a field-in-formation.

There are several reasons why so many obstacles have been included. Each needs to be stated explicitly so that it can become the center of a widespread debate. Change in the social structure of the field will not occur unless increased resolution of its obstacles occurs first. Increased resolution depends upon intensive study of detail. Change in a field also depends on leverage to cause movement in its ideas and customs. Leverage requires the existence of firm foundations to serve as a fulcrum for the levers. The debate surrounding each obstacle should serve as a fulcrum for leverage. Change in a field depends on the formulation of 'key' questions that stimulate future research (*dimidium scientiae quaestio prudens*). Nothing exposes fundamental questions more than reflection on obstacles inhibiting research in the field. Further, detailed citation of needs is an interesting way to organize a guide to the literature which goes beyond the conventional categories of the field. Both such approaches are represented here, because the outline is conventional, but the literature is linked to the obstacles. Finally, change in a field also depends on the emergence of leadership. New researchers in the field could profitably center an entire career on answering the problems posed by any one of the obstacles listed here. And it is to them that this detailing of problems of the field is dedicated.

The few areas selected to represent potential rapid development are presented as 'mini' case studies to keep the paper reasonably concise. Rather than detailed explanation of a single case that serves to represent a class of problems or solutions, these 'mini' case studies are introduced briefly and literature references cited to provide the usual level of

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detail. Throughout, two representative professional societies, the Society for General Systems Research (SGSR), and the International Federation for Systems Research (IFSR), are used to illustrate organization-based obstacles, thus providing two detailed case studies of this important dimension. Selection of these organizations and case studies is a matter of personal bias and experience. Doubtless my colleagues would favor other selections. Each of those selected, however, serves as a vehicle which indicates how some of the obstacles cited may be squarely dealt with and vanquished. In this way, the purpose of the paper is fulfilled; it is intended to help 'enable' future breakthroughs by pinning down and expressing in detail obstacles impeding them rather than attempting to predict them.

1.1. *Cross-impacts among obstacles*

Special attention has been paid to citing the numerous cross-impacts among the obstacles because just as they feed upon each other in creating chaos, so also their solutions would synergistically interact to rapidly improve the future of the field. A compact listing of the 33 obstacles and their associated cross-impacts can be found in the Appendix. No order of importance is implied. They appear in the order they relate to the conventional outline headings used in the paper on vital organizational signs, isomorphies, methodologies and applications. The cross-impacts were detected by asking the following simple question of each pair-wise combination of obstacles. 'Is there a component of "*x*" that influences a component of "*y*" (in some specifiable way), and vice versa?' The cross-impacts, therefore, have three meanings: (i) solution of the obstacle under consideration would have a positive effect on solution of the other obstacles clustered with it, (ii) the list of cross-impacted obstacles associated with any obstacle could be restated as a set of specific prescriptions for solution of that obstacle, and (iii) the list of associated cross-impacted obstacles describes in detail the corollary needs and criteria for overcoming the obstacle to which they are attached. Although the resulting matrix is based on subjective judgements, it would be interesting to see follow-up studies on this complex set of cross-impact using such techniques as: (i) high to low ordering of obstacles by the number of times each is cited, or by the number of obstacles associated with it, (ii) visualization and analysis by set theory, (iii) analysis by graph theory (since obstacles may be considered as nodes in a connected graph), and (iv) critical path analysis. This last technique might expose the seven most critical obstacles whose solution would have the greatest positive effect, of obvious importance to a field with very limited resources and manpower. The result of this paper goes beyond a detailed listing and discussion of 33 obstacles. It also includes the information for three important lists: one contains from six to 20 criteria describing the context for solving each obstacle; another contains from six to 20 specific suggestions for overcoming each obstacle; and still another contains from six to 20 barriers inhibiting the eventual solution of each obstacle. All of these are useful permutations that can be made from the Appendix and used for different purposes, and different clients.

1.2. *Boundaries of the field and past assessments*

The boundaries of the field of systems science are nebulous. For the most part this paper will concentrate on the future of the sub-field popularly called 'General Systems Theory', which is more accurately termed research toward a general theory of systems, a subtle but significant difference intended by the coiner of the phrase in the original German [16]. This paper will not cover obstacles or projections for the areas of systems analysis or disciplinary-based system theory.

There have been several attempts to assess developments in the field before. The *General Systems Yearbook* has been published by the Society for General Systems Research (hereafter SGSR) since 1958, now having 26 annual volumes [86]. The articles selected for inclusion were to be the best attempts at synthesis in systems science during the preceding year. Only the first volumes included articles that engaged in the needed self-referential and self-critical view of the field attempted here. Some Proceedings of general systems conferences bear titles such as *Applied General Systems Research; Recent Developments and Trends* [49] or *A General Survey of Systems Methodology* [111], or the six-volume *Applied Systems and Cybernetics* [54]. As collections of contributions of many independent authors with the very minor editorial control typical of major meetings, these compendia are inadequate for the purpose of concise and self-conscious assessment of the field. The introductory textbooks of Iberall [45], Churchman [30], Ashby [6, 7], Waddington [130], Klir [48] and Dillon [33] are useful each for unique audiences, and have historical relevance. However, for the purpose of this paper, which is a direct assessment of the organizational and theoretical mechanics of the field, they are either quite dated, or introduce GST from the standpoint of a particular application area, or cite relatively few isomorphies.

Some articles and even books have been addressed to advances or trends in the field notably Klir [49], and Gaines [38]. Another direct attempt at measuring the state-of-the-art of a general theory of systems was the Cavallo report [27]. Although a number of volunteers active in the field participated, this report needed considerable follow-up to reach its potential. It did stimulate a series of conscious internal criticisms of the field which have lately taken the form of Guest Editorials by Miller [68], Wilson [136] and Troncale [118] in the SGSR's quarterly *General Systems Bulletin.* At present, an ambitious attempt is underway to write an extensive critique and cumulative state-of-the-art report to be produced and published annually, with each annual version built upon and extending the last. The responsibility for this has been accepted by the newly formed SGSR Council, a body composed of the 'grassroots' leadership of the most international professional society with the largest membership in the area of general systems theory. The seven-page proposed outline for the initial version of this white paper can be found in the *Bulletin* [119]. The present paper, with its detailed listing of criticisms, obstacles and therefore needs of the field is a contribution to Sections III, C and IV, D of that project.

Unfortunately, any attempt to assess the future of systems science has as its first obstacle the lack of concensus in the field on usage of a plethora of basic terms. So young is the field that even the terms describing the different sectors of research carried on under the banner of systems science are often used inconsistently. This identifies the first obstacle to progress in the field.

Obstacle 1 : *There is a Need for a Consensus Glossary of Precise Definitions for the Principal Concepts Used in Systems Science.* Several attempts are currently underway to answer this need. They will be cited in Section 3.1. In this paper the terms describing the various sectors of systems science, and so the overlapping boundaries of the field, will be used with the following meanings (not to be taken as consensual meanings of the field, nor as hard and fast distinctions, but rather as useful clusterings along an actually indivisible spectrum).

Systems analysis. The most reductionist of systems approaches; the collection, treatment and validation of concrete data on the multiple components of a specific real system; often leads to simulation of the system for the purposes of quantitative prediction; results are context-dependent; does not focus on isomorphies; relies heavily on the use of mathematical formalisms and use of the computer; usually restricted to the detailed study of one particular system so it is less comparative across different cases, even within a conventional discipline, than systems theory; when it does involve multi-disciplinary comparisons, it focuses on one problem, or design goal, using one tool, as in the study of acid rain, or global climate.

Systems theory. Generalization of explanations from several analytical studies in order to understand a complex phenomenon or process within a conventional discipline; often leads to a model of the process with the purpose of achieving a subjective, qualitative understanding of the phenomenon that goes beyond the quantitative knowledge obtained from the specific cases studied by systems analytical techniques; the resulting understanding is usually context-dependent; emphasizes broader comparisons than systems analysis across different specific systems within a discipline or phenomenon; uses some isomorphies, but many fewer than the full set; overall, systems theory is one step more abstracted than systems analysis in its use of mathematics.

General theory of systems. The most abstract of the trio requiring very broad comparisons across many different scales of systems and across many different conventional disciplines; leads to very abstract and qualitative descriptions (not properly called models) of generalized systems functions such as systems stability, structure, function, origins, development, evolution, emergence and decay; by emphasizing systems-level functions it de-emphasizes component-specific differences of the multitude of disciplinary systems being compared; results are fully context-independent; uses the full set of isomorphies, however the lack of quantification leads to much reduced predictive power as regards specifics in favor of broadscale form; explains the mechanisms that give rise to the aforementioned systems functions.

Systems science. A collective, non-specific term that refers to any work of the above three aforementioned types since they all focus on the 'systems' level of reality. It is a questionable use of the term 'science' similar to that found in 'social science' no matter what region of the above spectrum is cited. Even the most quantitative work of the reductionist–holist hybrid type found in systems analysis would be challenged (and indeed is) by hard scientists as inherently and demonstrably unscientific. Science may be broadly defined as the extension of an organized body of consensually-shared knowledge among experts by some attempt to limit and guide changes and additions to that knowledge by empirical testing or other means. If we recognize that these other 'means' may include the logical constraints typical of theoretical mathematics, and theoretical physics/cosmology, then the convenience of the term to define similar attempts to study systems may be allowed and is certainly useful. Given the ambition of proponents, and the natural inflation of terms, it is likely that this will become the most convenient, popular, and encompassing term for the entire assemblage of specialties.

Systems thinking. The most all-inclusive and vague term of this list, and perhaps the most honest, systems thinking refers to the tendency of some workers to emphasize the many connections between phenomena and their abstract similarities rather than emphasize differences and limit interactions to simplify research. Often the term 'holism' is used in this context, although holism has been used in cases where the thinking is so general and vague that few, if any, aspects or functions of 'systemness' are described. In this sense systems thinking is more specific and definable a term than holism and they should not be used interchangeably. While more rigorous than holism, systems thinking is a term less rigorous in usage and intent than systems science since 'thinking' often is limited to an Aristotelean, logical approach toward knowledge which does not utilize the empirically-based falsifiability procedures or, at the very least, the formal constraints of mathematics typical of systems-science-based approaches.

The systems approach renders all of the above part of the same spectrum whether they favor the analytical or the synthetic end of the spectrum, whereas in other human pursuits the analytical function and the synthetic function are often so widely separated as to be described as entirely different and opposed pursuits. This is simultaneously the strength and the weakness of systems science. Inherently, it is a paradox to itself. This creates another obstacle.

Obstacle 2: There is a Need to Transcend Internal Conflicts Within the Field. Systems science requires its workers, and its critics both internal and external, to perceive both the extreme reductionist and holistic approaches as equally and simultaneously useful, even necessary to explain 'systemness'. The inability to maintain this paradoxical footing lies at the basis for many books critical of the systems approach in general [15, 17, 44, 58]. Most of us are trained as physical or natural scientists, or in the social science and humanist traditions. Even if interested in the systems phenomena, our training at the extremes renders the two approaches mutually exclusive. This internal conflict occurs even in those working full time in the systems area. For example, one of the popular systems approaches derives from the work of Checkland [29]. With some apologies for the necessity of summary, this approach may be characterized as holistic, using an heuristic methodology that claims no special knowledge because it is an interactive learning process based on sensitivity to systemsness. As such it is a non-specific process that adapts to its use, is non-prescriptive, non-deterministic, subjective, human and applications-oriented. Lately, advocates of this approach debate fiercely with those advocating the isomorphy-based approach to a general theory of systems. The isomorphy-based approach is more natural systems-, and basic research-oriented, claims special knowledge, is prescriptive, and has deterministic potential, and offers a relatively modest future for empirical and objective approaches (see references for Section 3). To the latter school, the former appear vague without much of a knowledge base, while to the former the latter appear too reductionist—to the point of abandoning the systems approach—and too interventionist. Upon close examination, however, the point could be made that both approaches are quite complementary, each necessary for different problems demanding attention, and both eventually destined for fusion into some future, more powerful systems science than we can presently imagine. The differences seem to emerge from the original disciplinary tendencies of the workers. It will be necessary for each to encourage, monitor and use the other if the field is to proceed. Note that the above obstacle states that 'transcendence' is necessary, not capitulation by either approach. The preferred future would be eventual mergence of the two, both remaining strong proponents of their portion of the spectrum.

Taken together, the several types of systems persons described above, their activities and their organizations have been called the 'systems movement'. As in many now historical cases of currently well-established sciences, this early phase of the 'movement' is best characterized as disorganized and fragmented. It is aggravated in this field by this last described paradox which is required for the study of systems. This inherent paradox inhibits quick resolution of internal conflicts, the description of hard boundaries for the field, and also slows the appearance of adequate assessments of the state-of-the-art. There is still another way to view the boundaries of the field and its sub-fields. These boundaries also have the aforementioned inhibiting effects on development of the field.

1.3. *A utilitarian distinction: isomorphies, methodology, applications*

The future examined in this paper will necessarily have to include aspects of the future of systems analysis and systems theory even though it intends to focus on general systems theory for two reasons. First, all work on a general theory of systems is based firmly on results from the more detailed studies of systems analysis and systems theory, which are in turn based firmly on the results of the conventional disciplines. For this reason members of the systems movement are advised not to push holism as anti-reductionism or anti-disciplinarian as was popular decades ago [52, 61, 100, see 61, 62 for reaction], but rather should regard disciplinary reductionists as allies, even in the face of their criticism of a field they cannot be expected to completely accept. Second, the distinctions between the three areas of systems approach are so fuzzy that vital signs for one are partially shared by the others. Consequently, organization of the future needs and potentials of the field will be along the lines of the levels of product of the systems approach, which are somewhat more easily distinguishable than the continuous spectrum of analytically- to synthetically-oriented approaches themselves. Again these must be defined in terms of their meaning for this paper.

Isomorphies. A formula, pattern, structure, process or interaction demonstrated to be precisely the same, but in general terms, across many disciplines and many scales of magnitude of real systems despite the obvious difference of the parts of the diverse systems. Isomorphies are completely context-independent and content-rich (have meaning in themselves and alone). They are manifest only in context, and observable only by comparison of many contexts. In mathematics a formula is isomorphic to another formula if it has the same form. The use of the term in a general theory of systems, however, has a more general usage with implications unique from its use in mathematics. The existence of the same interaction across many separate levels implies that the isomorphy is actually as fundamental and real, perhaps more fundamental and real than the parts at different scales of magnitude that exhibit the relationship. In this formulation the abstract isomorphy-across-systems and the physical manifestations-of-systems are equally 'real'. Thus physical systems are more than merely isomorphic to each other (which emphasizes that only the physical systems themselves are real and important). They are actually only different permutations of the primary reality which are the isomorphies. In this view, the isomorphies are proper objects of study even though they can only be seen 'through the veil' of their myriad physicalizations in objects which heretofore science thought were the only proper things to study. This is a turnabout perception that has revolutionary potential. Since proving it may take a century, it is better called an 'evolutionary' potential.

Systems methodology. An algorithm or sequence of steps in a procedure useful for elucidating significant features of a system. At the present time, systems methodologies are most noted for their ability to render a hopelessly complex and untractable number of variables and observations somewhat more manageable in human or computing terms. Since these steps are functions to be performed by humans in order to observe systems, it should be clear that the methodological tools are not themselves isomorphies. Similarly, a microscope is not what it enables us to observe, nor is an experiment the object studied. Systems methodologies are also context-independent like isomorphies, but they are content poor. They have little internal, phenomenological meaning except that deriving from the isomorphies which validate and empower them. Some workers spend their entire professional lives elaborating better tools for studying systems without direct or explicit work performed on isomorphies, although many systems methodologies are based on one or more isomorphies. The distinctions between isomorphies and methodological tools are often overlooked because of the obvious interconnections between them with the result of confusion, miscommunication, and fragmentation in the field.

Systems applications. Systems applications occur when either a single isomorphy, or a set, or a verified systems-methodological tool is used to elucidate or solve a problem of function in a real target system. Trivial cases of analysis of real systems that do not explicitly use established isomorphies or tools should not be called systems applications as they amount only to vague holism. Such initial attempts give systems applications a bad name because the improvement of resolution of the problem, or its understanding over conventional or intuitional approaches is insufficiently dramatic to impress the critically minded. Since we have so far to go to improve our understanding and verification of systems isomorphies and methodologies some feel it is dangerous to overplay the role of systems applications in this young field. However, others cite the pressing need of the problems themselves and what they describe as tangible benefits of even very holistic approaches.

Distinguishing isomorphies, systems tools and systems applications is not intended as an academic enterprise. It is intended to improve communication, rationalize appropriate expectations, guide research methodology, sharpen meaningful critiques and enable meaningful transfer across basic to applied portions of the spectrum. It perhaps is as useful to carry out this discrimination on the mental level as it is useful for us to distinguish colors in the light spectrum, or types of electromagnetic radiation on the perception level.

Some of the obstacles facing the field are of an organizational or institutional nature. These will be clustered around the following analysis of the vital signs of the field to be followed by obstacles relating to the needed developments in basic research

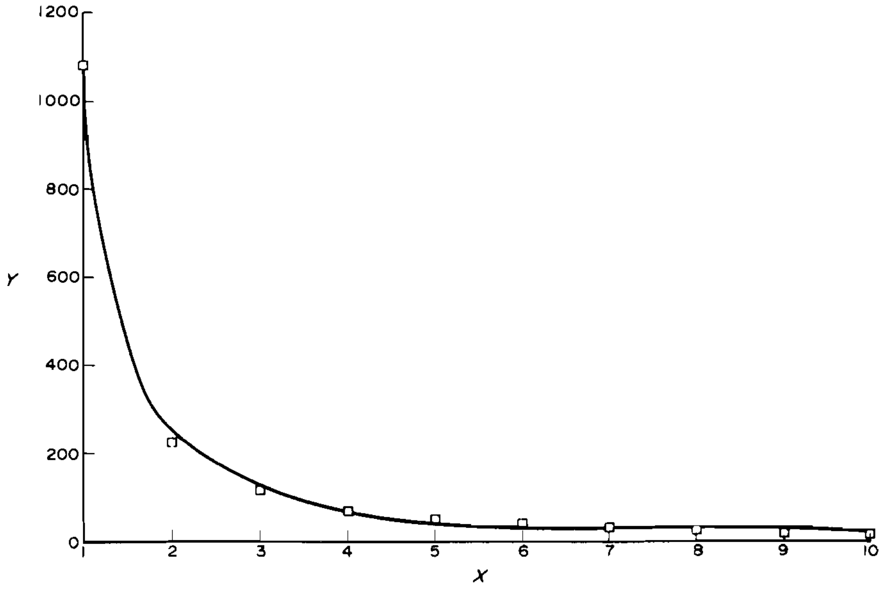

Fig. 1. Number Y of authors with X or more publications in the bibliography. Figures 1–5 are reprinted from *Basic and Applied General Systems Research: A Bibliography.* (SUNY, Binghamton, 1977) with permission.

(isomorphies), systems methods, and systems applications.

2. SYSTEMS SCIENCE: A SUMMARY AND PROJECTION OF VITAL SIGNS

2.1. *State of the literature: future of the literature*

There are several independent ways to measure the activity of the literature of a field, namely, trends in the number of articles, books and proceedings published on the subject, trends in the number of articles published on specific concepts useful to the field, and the appearance of new journals serving the field.

The largest bibliography of systems-related articles and books published to date is that of Klir, Rogers and Gesyps [39]. It contains references to 1409 books and articles from a bibliographic search of 22 systems-related periodicals up to 1977. Though large, it is not comprehensive due to the exclusion of many relevant proceedings and active investigators. Still, it is the most extensive bibliography to date, possessing such useful indexing features as an authors listing, key word listing, key-term-in-context permuted index, and listing of complete bibliographic information. Some simple statistical data is provided which indicates trends in the general systems literature.

Figures 2, 3 and 4, adapted from Klir, Rogers and

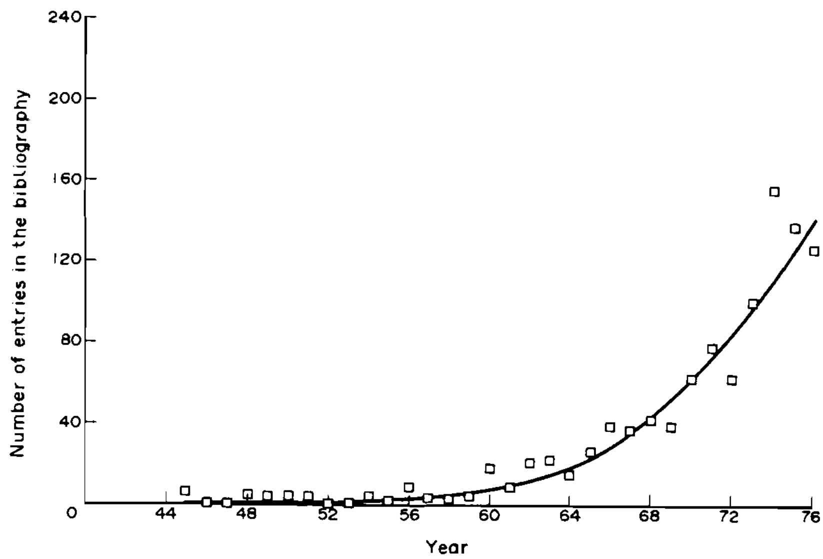

Fig. 2. Growth of general systems literature in the period 1945–76.

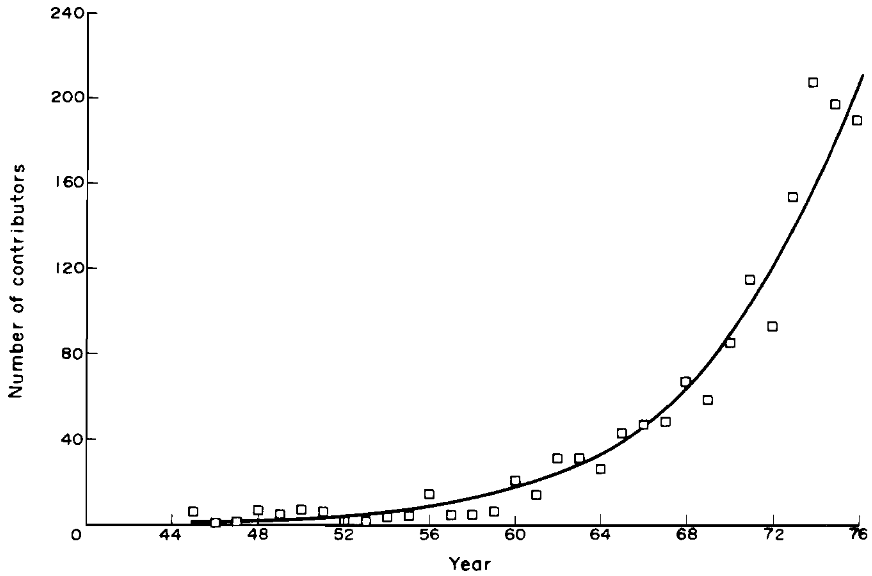

Fig. 3. Increase of general systems contributors in the period 1945–76.

Gesyps show the growth in the literature and contributors from 1945 to 1976. In all cases the growth is exponential, indicating a healthy increase in numbers as well as in rates of growth. Comparing the growth in articles and new contributors with the growth in numbers of departing contributors (Fig. 5), however, highlights a problem. Contributors leave the field as quickly as they join it. This may explain Fig. 1 which shows that 79% of all authors ($n = 1084$) have only one paper cited. This can also be observed to be generally the case up to 1984 if one examines either the Yearbooks of the SGSR or its Annual Proceedings. It is true that a few investigators have been inspiring in their tenacity in developing one theme: Rosen in systems theory applied to biology [92, 93]; Klir *et al.* in systems methodology [42, 48–51]; Bunge in systems philosophy [21, 22]; Miller in living systems theory [67]; Warfield and Ackoff in systems management [1, 131]; Varela *et al.* in autopoiesis [60, 127]; and von Foerster in control theory [128], to name a few. The point here is that these are the few exceptions with many of them near or past retirement. The majority of investigators in general systems theory are rather unstable contributors who work for a short while on some aspect of the theory or its application, then either jump to a seemingly unrelated area or leave the field for extended periods, leading to recognition of another obstacle.

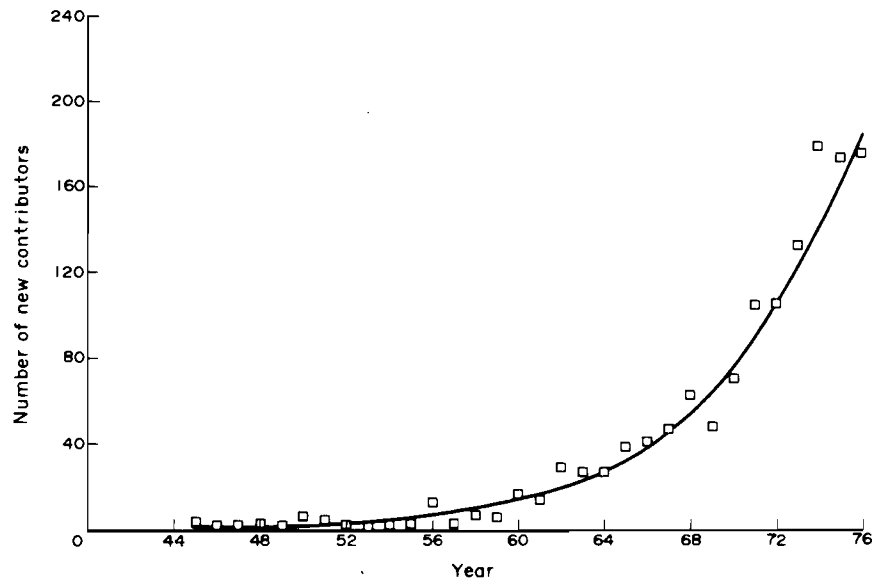

Fig. 4. The increase of new contributors to general systems research in the period 1945–76.

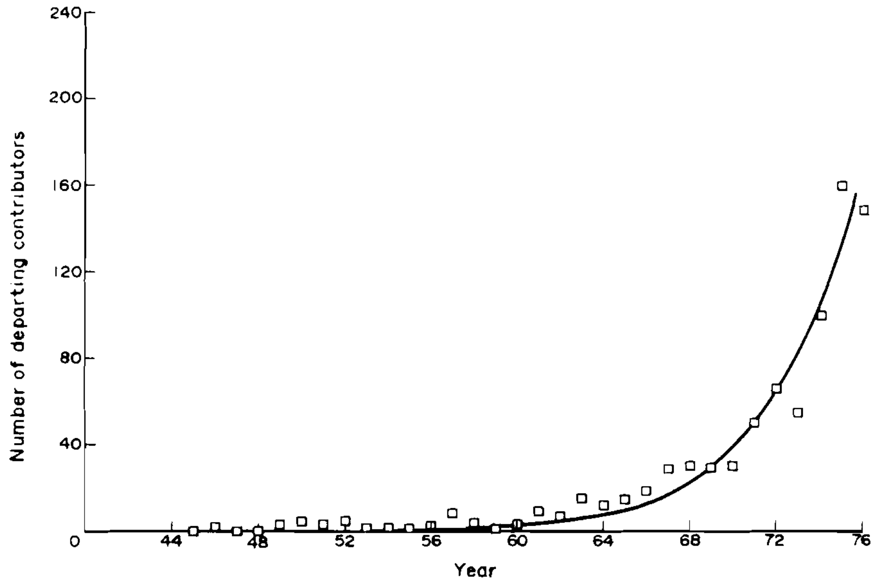

Fig. 5. The increase of departing contributors to general systems research in the period 1945–76.

Obstacle 3: *There is a Need for Long-Term Lineages of Papers and Investigators.* In most sciences the typical productive worker will devote virtually his entire professional life to a single project. Such single-minded devotion is required in order to achieve significant advances in a specialty area. One would expect that even greater devotion would be required to master and advance a transdisciplinary subject area, but the above data indicate the opposite is true at present. The relative sluggishness of development of a theory of general systems is partly the result of this tendency to stop-in and step-out of the field. Monolithic themes of annual meetings which change drastically from year to year with little continuity with the topic of the previous year exacerbate this obstacle. Platts' advice on how to achieve 'strong inference' [81], demonstrating why some fields advance more rapidly than others, is clearly not followed in GST work. A worker can hardly construct exhaustive multiple alternative hypotheses on some relationship, then carefully eliminate all but one, if he produces but one paper. Equally, not much is accomplished by rewrites of one paper every year to match some highly generalized, global, application theme selected as that year's conference theme.

Overcoming this obstacle would help overcome other obstacles. A list of eight specific suggestions or criteria for overcoming or understanding this obstacle can be obtained by changing its associated cross-impact obstacles listed in the Appendix to positive statements. For example, workers on GST have a penchant for broad, conceptual schemes and very generalized thinking. Testing of such nets of hypotheses is impractical compared to reductionist formulations, and is simply not achievable by a single mind in a single lifetime (Obs. 7). The reward system for extended transdisciplinary work does not exist leaving the GST worker without a professional environment or support system to accomplish breakthroughs (Obs. 13). The number of systems science educational programs are few with the number at the doctoral and post-doctoral level, where most fertile lineages of work occur, still fewer (Obs. 12). Annual meetings of the SGSR have been radically altered to incorporate the traditional, unifying President's Theme (which changes each year) as a sub-conference of the main conference. Part of the radical alteration is a series of sessions devoted to integrative discussion only without any papers presented, as well as reducing the number of papers in each session in order to provide time for synthetic interaction within each session. The main conference will sponsor a consistent set of session topics repeated year after year to encourage continued progress in those topical areas. New topics and a monitoring of progress on old topics will be carried out by the SGSR Council while the topical sessions each year will be run by the respective Special Integration Groups (SIGs) of the Society. This prescription and those that will be suggested as practical ways to overcome the other obstacles cited above may increase the number of lineages of workers and investigators needed for steady progress on a GST and consequent improvement of the literature of the field.

The latest edition of the Klir/Rogers/Gesyps bibliography should provide additional data on the

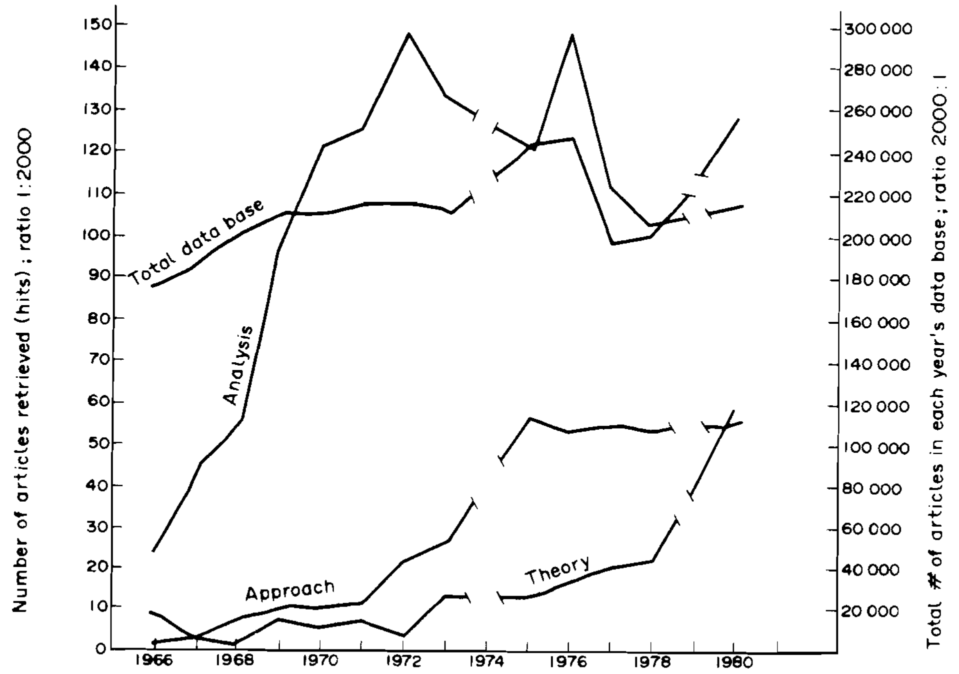

Fig. 6. MEDLINE, year-by-year numbers of articles using keywords on systems methods, 1966–81; 3.08 million articles searched.

state of the literature. It is being prepared by the International Federation for Systems Research (IFSR) under the direction of Dr. Robert Trappl and funded by the Austrian Federal Ministry for Science and Research [104]. It contains listings for the literature from 1977 to the present, and numbers 1569 articles and books at present.

Growth trends in the literature on use of particular systems concepts in the applied fields provides different information and is easier to trace than total number of articles in the entire field. Figure 6 shows the rise and fall of citation frequencies for the general terms 'systems analysis', 'systems approach', and 'systems theory' in the literature of medicine over the 15-year period from 1966 to 1981. Over 3 million articles were surveyed in the medical computerized database MEDLINE. When BIOSIS, the biological science data base, and MEDLINE are both searched for the same period, the total number of articles retrieved is 3648 for the above terms from a total of 6.3 million titles. This demonstrates that a healthy systems-related literature is building up, even in such non-technologically oriented data bases as the biomedical sciences. The trend of the sample shown in Fig. 6 shows rapid increases in use of the term 'systems analysis' in the 1960s, leveling off to a respectably high equilibrium in the 1970s, with 'systems theory' showing rapid increases after a 10-year lag period and not yet reaching a plateau phase. Note that the overall extent of increase in usage for each term exceeds the baseline extent of increase for the total data base for certain periods.

These data are only within a single field and do not catch the full scope of the transdisciplinary phenomenon of systems science. However, the greater resolution provided by looking at the literature concept by concept and tracing it through 'user' fields leads to significant observations. For example, Fig. 7 shows the trends in citation of systems concepts such as 'entropy' and 'hierarchy' in medicine and biology. Both experience a rapid rise during the 1970s (again suggesting about a 10-year lag time from a concepts first use in GST and its uptake by user fields) followed by a fall in citations. One interpretation of this cycle, which has been observed for several systems concepts [123], could be that a term becomes popular for a while then fades from view presumably due to lack of continued robust discoveries in the area (Obs. 3). Whatever the attractiveness of the original idea, the disciplines require substantive new developments and insights to drive its continued application to their field. The same data also gives some indication of the magnitude of the literature base available to build from even in one, single discipline.

When the scope of inquiry is widened as regards disciplines searched but the focus on one systems concept is maintained, other insights emerge. A recent literature survey [122] of four data bases, MEDLINE, BIOSIS, INSPEC and SCISEARCH for the usage of the systems term 'hierarchy' retrieved 2658 research articles published in refereed journals in just a five-year period. Investigators conducting this research were from 32 disciplines and

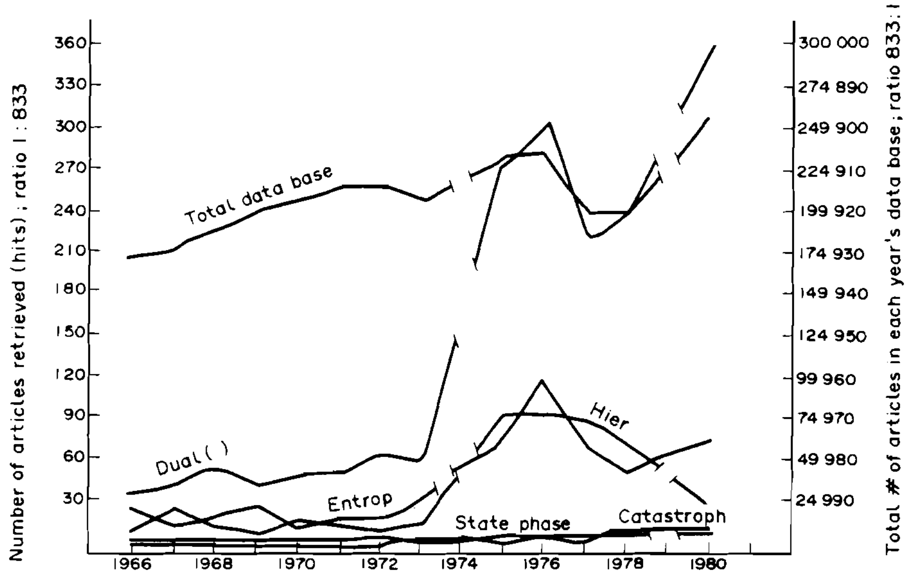

Fig. 7. Year-by-year search, MEDLINE. Numbers of articles found using keywords denoting inter-level dynamics or transitional phenomena of systems, 1966–81; 3.08 million articles searched.

represented 27 countries. Besides indicating how widely concepts such as hierarchical form have penetrated the disciplines, these data raise the spectre of increasing fragmentation and wastage of potentially important results. Workers in one area of the literature do not encounter the work reported in the literature of other specialties, which is usually not a problem when the research focuses on a phenomenon unique to a discipline. But when the phenomena are transdisciplinary much is lost.

Obstacle 4: There is a Need for a Mechanism and Motivation for Synthesis of Literature Findings Across Disciplines. The problem is larger than the data suggest since only the activity of the relatively 'hard' sciences on hierarchical form and function are captured and most of the work reported in the mammoth social sciences areas is missed. At present no effective methodology exists for sharing the insights and conclusions so carefully obtained by these isolated investigators. In fact, the methodologies, attitudes, expectations, reward systems, measurements, traditions, even the manner of thinking and valuing differs so markedly across even neighboring disciplines (Obs. 5) that attempts at formulating modest syntheses across literatures are easily destroyed. A recent example is the uproar resulting from the suggestion that the methods of biological genetics (mathematical, molecular, and population) might be of utility in understanding certain aspects of human behavior [23, 24, 56, 61, 137, 138, and 55 for negative evidence]. Yet, it is hard to believe that sociobiology will be any less successful in 100 years than have other integrations across the interface of two previously isolated disciplines such as biochemistry, biophysics, molecular evolution, geology and evolution, or the union of population genetics and evolution. In fact, much of the best of current systems synthesis is occurring across the biological and sociological interface, for example, Boulding [18], Miller [67], and Wilson [137, 138].

One prescription for overcoming Obs. 4 is to gather a critical mass of investigators interested in the systems concept under consideration and create an *ad hoc* organizational unit just for them that will provide a 'nest' or supportive environment that reverses the many problems cited above (see Obs. 7, 11 and 13). The SGSR is presently carrying on an experiment to accomplish this synthesis across literature by organizing a three-year conference on Hierarchy Theory characterized by face-to-face meetings at each annual meeting (the conventional aspect) joined to the unconventional aspect of continuing to work vigorously on actual integration of the disciplinary findings via an Integration-Directed, Iterative Dialogue (IDID) throughout the year. The IDID consists of a carefully worded and targeted questionnaire designed to lead each specialist in presenting his results in a form digestible by other disciplines, at a level of generality which encourages comparisons across disciplines, and with a great deal of attention paid to identification of 'key' questions of mutual impact. This is clearly not a Delphi questionnaire on several grounds discussed elsewhere [122], but most essentially because it does not lead to predictions of any kind. It stresses data comparisons, methodological fusions and immediate juxtapositions of results and conclusions. About 40 specialists have joined the experiment from an original invitation list of 200. This initial group

Table 1. A sampling of periodicals that regularly publish general-systems-level articles

- Annals of Systems Research
- Behavioral Science: J. of the Soc. for Gen. Sys. Res.
- Cybernetica
- Cybernetics and Systems: An International Journal
- General Systems Bulletin
- General Systems Yearbook of the SGSR
- IEEE Transactions on Systems, Man, and Cybernetics
- International Cybernetics Newsletter
- International Journal of General Systems
- International Journal of Systems Science
- Journal of Cybernetics
- Kybernetes: An Internat'l J. of Cybernetics & Sys. Sci.
- Kybernetika
- Mathematical Systems Theory
- Soviet Cybernetic Review
- Systemique Informations
- Systems Research: Official J. of the IFSR

exemplifies the diversity of specialties interested in a cross-disciplinary concept like hierarchy, with mathematicians, physicists, astronomers, chemists, geneticists, molecular biologists, cytologists, zoologists, ecologists, psychologists, medical specialists, sociologists, political scientists, linguists and philosophers attempting to communicate beyond their specialties for the common cause. If successful, the IDID method can be applied to any of the systems concepts now under study helping to overcome Obs. 4, 5 and 20.

Table 1 cites some of the periodicals serving the general systems movement. The list would be much longer if journals were included that specialize in a specific zone of applied systems analysis, for example, computer systems analysis, or engineering systems analysis, or modeling and simulation. Editors of the journals shown generally state that there is a need for an increased flow of competent submissions, although noticeable improvement has occurred recently both in the quality and quantity. Even though the demand for publication space is not exceptional, the area is characterized by frequent initiation of new journals. Each new periodical is favored with much attention when it appears, but readership remains small and stabilizes quickly. Many new journals duplicate the coverage and editorial policies of previous journals. The literature submissions for these journals may best be characterized as in an early entrepreneurial stage with each special interest area endeavoring to capture the market. A superior strategy might be less journals with each enjoying a more competitive submission rate, and subscription audience, but the organizational diversity of the field (Obs. 8) is so great that it could not support such a development at present.

Similarly, the literature is characterized by appearance of a number of new book series on general systems theory, for example, *The Series on General Systems Research* published by North-Holland, Amsterdam, and edited by Klir [51], *Progress in Cybernetics and Systems Research* published by Hemisphere, and edited by Trappl *et al.* [117], *The Systems Inquiry Series* published by Intersystems, Inc. and edited by Banathy and Klir [11], *Frontiers in Systems Research: Implications for the Social Sciences* published by Kluwer/Nyhoff, and edited by Klir, Braten and Casti, and the IFSR Book Series published by the International Federation for Systems Research. The rush of new journals, book series, proceedings and collections indicates a healthy growth trend in the field apart from questions of rigor and quality.

In summary, the state-of-the-literature in GST is one of rapid, but fragmented and faulty growth. Methods of integrating diverse studies are underway, but more are needed. More robust research is needed for each systems concept. Perhaps this is achievable only through the continuous efforts of a lineage of investigators willing to devote their life work to developing a single concept. The quantity of the literature is moderate; quality is lacking, but developing.

2.2. *Impact on the disciplines: zones of acceptance*

In general, the relationship between GST and most of the disciplines is still one of restrained antipathy. Because GST emphasizes transcendence of reductionist approaches, it alienates most physical and natural scientists who have successfully followed the Cartesian strategy for 300 years. It is difficult to argue with such success. For their part, workers in GST forget that their best and most developed examples of systems concepts derive from comparisons across the results of the hard work of the specialists, so they descend into an anti-reductionist stance. Statements against reductionism are still commonly found in GST literature. These holists may be missing two important points. First, they miss the point that GST transcends, not replaces, the specialty results. Its *raison d'être* is the synthesis and integration of the results won from the sweat of the specialists; it does not create these results *de novo.* Without the specialties it would not have anything to integrate. This may be the modern counterpart to great debates during the Greek era on what was more fundamental... nominalism or realism, the abstract name of a thing or the thing itself. Systems theory itself argues that this is a false dichotomy, both being equally fundamental and necessary (Sections 1.3 and 3.1). Isomorphies can be experienced only through their many physical manifestations and physical reality only appears through iterative emergence of the same isomorphies on ever greater scales or levels of things. Reality is a metaphor of itself.

The second error of holists may lie in their overextension of Heisenberg's Principle of Uncertainty and/or Goedel's Theorem which some of them use to imply that the physical and natural sciences can make no predictions about nature, and never could. Thus, the relativity and subjectivity of systems science is needed to model reality. It would seem that all experimentation is invalid to them because experimenters now have defined the true limits to their experimental findings. Actually the above theorems only point out that reductionism alone, by itself, can never capture reality totally. But clearly neither can holism alone. A macro-uncertainty principle is also in operation. So these results cannot be used to vanquish reductionism; they only put a foot in the door that eventually may allow systems approaches to enter the room if they earn their way. Measurements and testing are still useful even if they cannot settle questions completely. For a mutual truce to occur, reductionists must also give way and admit that 'reality' flows between the artificial separations they call their disciplines, and not only within the confines of each discipline.

Based on these observations, it is not surprising that the less reductionist social sciences embraced systems science upon its appearance and some actually heralded it as the method for which they had searched; it seemed 'tailored' to their needs. It is common to find references to systems approaches in many social science texts on the one hand [20, 40, 41, 101], and at the other end of the scale, references frequently appear in technological-based engineering and computer science texts. During the same time period, systems science was roundly criticized by hard scientists and often by philosophers steeped in logical positivism.

At the present time a subtle reversal has appeared. Some social scientists of the new generation have reacted against the earlier, and necessarily qualitative treatments of systems-oriented social scientists like Parsons [77, 78], Deutsch [32], Rapoport [87, 88], Boulding [18], Easton [34], Singer [99] in favor of analytical-reductionist approaches more in tune with logical positivism. Many of the aforementioned workers have continued refining their original insights with data-oriented studies since that time. While use of systems analytical tools has grown in the social sciences, so has a backlash against the utility of systems methods to interpret complex behavioral events. They are seen as too deterministic and reductionist by humanities-oriented members of the social science community. Meanwhile, as more and more systems concepts appear, and as the theory and models of the hard disciplines mature, physical and natural scientists are beginning to find use for these ideas in their hypotheses and explanations of natural phenomena. The growing number of systems theoretical concepts applied to various bio-sub-specialties is an example [2, 64, 92, 123]. Or consider the utility and frequency of citation in the physical and biological sciences of new systems concepts like 'fractals', or 'solitons', or 'non-equilibrium dynamics'. A tentative prediction for the future might be a surprising one. Social scientists will look more critically at GST, demanding more robust results and tools from it than before, while physical and natural scientists who once ignored or vilified the systems movement will begin to actually work with it to improve its utility for them. If this is the case, two obstacles will impede the desired rapid development.

Obstacle 5: There is a Need to Transcend Disciplinary Training. In the debates within the field of GST it is evident that despite their participation in the attempt to forge a systems model, many systems theorists themselves are highly constrained and biased in their conception of what a GST model should be by the original disciplinary training that they received. For example, the definition of 'system' is one of the most fundamental concepts in the field, yet you will still hear heated debates on whether or not the concept of 'purpose' is essential to defining a system. Natural-science-trained systems workers disallow 'purpose' according to the standard results of their parent disciplines; natural systems have functions, not purposes. Purposes imply a conscious controller. Purposes are teleological, an -ism that continues to persist despite many past disproofs. Meanwhile, social-science-trained systems workers insist that all systems have purpose, as certainly their best-known examples do. Even the intermediate position—that of restricting oneself to usage of the word 'function' because it subsumes purpose—is apparently unacceptable. How can an integrated systems model emerge if its proponents require that it favor the particular scale of reality which they once studied? Perhaps progress on this obstacle must await progress on Obs. 4, 6 and 21.

Table 2. A sampling of professional societies which serve the general systems community (abbrevations used in this paper)

- American Society for Cybernetics (ASC)
- Association Internationale de Cybernetique
- Austrian Society for Cybernetics
- Deutsche Gesellschaft fur Kybernetic
- Greek Systems Society
- IEEE - Section on Systems, Man, and Cybernetics
- International Federation for Systems Research (IFSR)
- London Cybernetics Society
- Mexican Association of Systems and Cybernetics
- Polish Cybernetic Society
- Sociedad Espanola de Sistemas Generales
- Society for General Systems Research (SGSR)
- Society for Management Science and Applied Cybernetics, India
- Study Group of Integrated Systems, Argentina
- Systeemgroep Nederland
- United Kingdom Systems Society
- World Organization of General Systems and Cybernetics (WOGSC)

Despite subliminal attachments to the overall conception of reality that remain in each systems worker, most agree with a generally anti-disciplinary stance in keeping with the general systems hypothesis. This leads to a paradox because acceptance of the general systems hypothesis ultimately depends on the disciplines.

Obstacle 6: There is a Need to Demonstrate Any One Isomorphy in All Disciplines Possible and Across All Scales of Real Systems. For a theory of systems to be general, it must by definition be able to prove that its isomorphies are present at every scale of reality, in every mature system, that is, in every discipline. Successful fulfillment of this task will require general systems theorists to carefully survey, evaluate, and integrate the reductionist output of the major disciplines, hard and soft. For example, positive feedback and its consequences, or hierarchical structure or autopoietic processes must be observed across the range of disciplines in terms of the standard falsification procedures accepted by the host discipline before any one of these putative isomorphies could be accepted as part of a GST thereby answering Obs. 9 and 18. Even if one is working on a subset of the hypothetical GST, this cross-disciplinary verification of the existence of isomorphies is necessary. This task requires a healthy respect for, a deep understanding, and even an intimate knowledge of the entire spectrum of disciplines in terms of the isomorphy under study. Clearly this is a philosophical position diametrically opposed to antipathy to the disciplines, or to a restricted *Weltanschauung* that results from disciplinary training (Obs. 5).

2.3. *Growth in professional societies*

The last decade has witnessed a significant growth in professional societies with 'systems' as their focus. Table 2 is a partial listing of only those that focus on general theory; the list would be much longer if societies interested in systems analysis were included. Membership of most of these societies is small relative to membership in societies of well-established disciplines or technologies (n = 100–1500 in most cases). In Table 2 both cybernetics and systems societies are listed together. On the Eurasian continent 'cybernetics' means approximately what 'general systems theory' means on the North American continent, while in America 'cybernetics' usually refers to the several isomorphies dealing with regulation and control processes which are just a portion of the full set. Ironically, it was in Europe that the phrase 'general theory of systems' was born [16], while the term 'cybernetics' was initiated in the United States [134]; the term that became popularly recognized in each case was the term originating on the other continent [Mayon-White, personal communication]. Sometimes the usage of different terms like this interferes with formation of a consensus or divides the very limited resources of new organizations.

Subjective reports from at least some of the societies (e.g. SGSR) indicate that membership numbers are increasing rapidly after a period of retrenchment. In addition, independent national societies, although small, are appearing in increasing numbers and gathering into critical masses by joining federations (e.g. IFSR). Geographic regions formerly without representation, such as developing countries like Spain and portions of South America have initiated national societies that favor general systems research. It would be reasonable to predict that after many small societies form there will be a period of competition and a 'shaking out' resulting in fewer, but stronger organizations offering more services.

Are these societies achieving their objectives? For example, is the SGSR demonstrably aiding the conduct of research on a general theory of systems? Obstacles 5 and 6 suggest that disciplinary input is essential to the proper conduct of general systems research.

Obstacle 7: There is a Need for Adequately Transdisciplinary Research Teams. The SGSR has about 1000 members from 40 different countries. If one examines the specialties of the members, it is clear that virtually every recognized field of study is represented. Potentially the required transdisciplinarity is present. But are the specialists integrating their results across the disciplinary barriers; are the activities equal to the potential? An analysis of the programs of study and proceedings of the SGSR indicates that increasing 'fragmentation' into special interest groups occurs proportional to the increase in membership. SGSR members interested in mathematical systems theory, or simulation and modeling, or applications to business and industry, or systems philosophy tend to interact at high frequencies only within their own groups. Too little interaction occurs between these focused approaches. This is quite natural since each of these special interest groups tend to share the same vocabulary, values, goals, and methodological preferences, but encounter obstacles in all of these areas when entering other groups. This presents general-systems-oriented societies with a special problem not encountered in disciplinary societies.

Obstacle 8: There is a Need to Counterbalance the Natural Trend Toward Fragmentation. Research in duality theory indicates that in addition to the existence of complementary, opposing forces or processes existing on many levels of natural and social systems, there are master complementary forces that span all levels and scales [5, 107, 113, 117, 129]. One of the most potent and least studied of these is the opposing forces of 'fragmentation' and 'integration' which appear in sub-atomic particle systems, astronomical systems, geological systems, biological systems, sociological and symbolic systems and seem to alternate in cycles of dominance with each other. At the present period modern intellectual movements are in a phase dominated by fragmentation (specialization). Nobel laureate I. I. Rabi once stated that modern reductionist scientists could be likened to an incredibly active and productive mining community that with great effort brings precious ores to the surface. But he complained that they tend to leave these precious ores at the mouth of the mine in huge piles, relatively unused, since they feel their task at that point is finished. He emphasized the great need for integration of these fact-piles into useful systems. Thus, two decades ago he was pointing out the need for a greatly increased effort at synthesis to counterbalance the current dominance of fragmentation. To date this counterbalancing movement is still anemic.

The whole purpose of the SGSR and like organizations is helping this needed integration movement, yet even it exhibits the universal trend for fragmentation. To counteract this force, the SGSR has initiated SIGs (Special Integration Groups) which focus on a specialized area. In this way SIGs fulfill the practical necessity for constraining the universe of inquiry so that detailed, rigorous results are produced. But their primary purpose is evaluating those results for use in integration across the disciplines. These Special Integration Groups are the diametric opposites of special interest divisions in reductionist societies and in our social systems. The Hierarchy Theory SIG is discussed throughout this paper as a case study. It has representatives from many disciplines. It has specifically demarcated one domain of inquiry (hierarchies), but does so primarily to compare them and elucidate their integrative, or transdisciplinary aspects.

The many isomorphies studied in GST (Obs. 14), are each in themselves examples of integrations and fragmentation; they are anasynthetic [110, 125]. Each one represents a reducible part of what it takes to define 'systems' or 'wholeness', and so in this reductionist role each is analytical. But simultaneously, each represents a process or structure which is true of all mature systems, across all scales of reality, thus rendering as similar on their level of abstraction the immense number of different particular systems; this is a synthetic and integrative role. As they are 'anasynthetic', isomorphies are at one and the same time contradictory, and supportive of themselves. They are a microcosm of the paradox inherent in the field itself.

2.4. *Growth in activities, meetings and conferences*

Concomitant with the growth in professional societies, there has been a growth in the main services provided by such societies. Table 3 lists some of the periodic national and international conferences and congresses now a regular feature of the general systems landscape. Again this list could be multiplied many times by the inclusion of the systems analysis-based societies, or, more specifically, the portions of disciplinary societies using systems analysis as the tool to study the discipline. Attendance at GST meetings averages from 100 to 250, with representatives attending from virtually all disciplines and as many as 25 countries. Proceedings of the conferences are often issued at the meeting (e.g. SGSR, WOGSC) which has the advantage of currency compared to many disciplinary proceedings, but suffers the trade-

Table 3. A sampling of periodic conferences which regularly sponsor sessions on general systems research (frequency, last)

- American Society for Cybernetics (annual, interrupted series)
- Applied Systems and Cybernetics (biennial, title changes)
- European Meeting on Cybernetics and Systems Research (biennial, 7th)
- Fuschl Conversations (twice per year on average, 5th)
- International Congress on Cybernetics (annual, 11th)
- International Congress of Cybernetics and Systems (biennial, 6th)
- N.A.T.O. Conference Series No. II. on Systems Science (occasional, 7th)
- Nederland Systems Conference (biennial, 4th)
- Society for General Systems Research - International Conference Series (annual, 29th)
- Symposium for Industrial and Systems Engineering (annual, 3rd)
- Systems Science in Health Care (every 4th year, 3rd)

off of poor editorial control and weak peer review due to rushed deadlines. Further, insufficient time is provided between annual conferences for real progress to be made on any specific lineage of research (Obs. 3) which calls into question the wisdom of an investigator attending much more than one meeting a year unless it is to contact a unique and different audience (Obs. 11). Recently evaluation questionnaires issued at a typical annual meeting (SGSR) have revealed a wave of dissatisfaction with the quality of papers presented, leading to recognition of another obstacle.

Obstacle 9 : *There is a Need to Dramatically Increase Rigor.* This is a thorny challenge. Since GST spans the disciplines from sub-atomic particle physics to cosmology, including representation of every discipline in between, it is clearly impossible to establish a common meaning of the term 'rigor' according to conventional disciplinary protocols; they disagree. Yet, outsiders tend to judge GST attempts according to the standard measures of rigor in their home disciplines. The failure of GST to provide its own criteria for a GST (Obs. 30), or its own methodology (Obs. 20, 21) results in an impasse concerning this obstacle. Its status as a truly anasynthetic enterprise, makes it impossible to use the definitions of rigor supplied by analysts/reductionists or holists. Others note that such an early stage in a knowledge field is not the time to be excluding anyone's work for lack of rigor. Without a consensus who is to judge, they ask? At the same time, there are clearly major differences between papers and presentations in terms of internal consistency, use of detail, extent of literature cited, attempts at constraining theory by some mode of choice or judgement, or even appropriate understanding of what is the main product of general systems research. The establishment of Special Integration Groups may help solve this problem since it will provide a pool of experts on a sufficiently defined aspect of GST to allow review procedures to begin and will provide opportunities for partial consensus to guide judgements. Establishment of an internal tradition of self and collegial criticism, although it will make meetings more uncomfortable than at present, would substantially add rigor. This is beginning to appear, even if it is weak compared to the harsh challenges typical of the hard sciences. Here the societies have the biggest contribution to make by organizing and enfranchising review procedures and required formats for papers for meetings.

Once achieved, these improvements will contribute to a solution of another obstacle currently inhibiting progress in the field.

Obstacle 10 : *There is a Need for Consensus Producing Processes or Mechanisms.* Constraints on the multiplication of ideas leads to the survival of the best ideas, eventually. This leads to consensus. However, the difficulty in achieving consensus in the case of GST must also overcome disciplinary blinders (Obs. 5), and inadequate specification of methodology (Obs. 20), integration mechanisms (Obs. 4 and 21), and testing (Obs. 18 and 19). Some prescriptions to overcome Obs. 10 include: (i) the planned annual state-of-the-art report by the SGSR Council, (ii) recent and dramatic alterations of standard meeting formats in the American Society for Cybernetics and the SGSR which set aside significant portions of meeting time for comparing, arguing about, and synthesizing papers rather than just delivering them, (iii) the IDID technique described above to carry on synthesis-directed interchange among meeting participants between annual meetings, and (iv) current debate over which terms and definitions should be included in a systems glossary (see Section 3.1). They also have the potential for adoption by disciplinary societies for the improvement of synthesis across their numerous specialties.

Whatever progress is made toward consensus in GST conferences, the field will still be faced with the paradoxical need to remain ever open to new models and theories. It will have a greater obligation for preserving this feature than reductionist fields due to its special role in exploring what always appears at first to be unorthodox—the elucidation of cross-disciplinary comparisons. Somehow an appropriate balance must be found between rigor and judgement (which reduces variety in theories), and open-systems acceptance (which increases variety). The current imbalance in favor of 'everything goes' simply cannot be justified by the argument that anything else would be a closed system. Disciplinary fields that are quite rigorous eventually accept what at first are very unpopular ideas (e.g. continental drift or nonsense regions within DNA gene sequences) once enough evidence accumulates. New ideas in GST must soon face a similar uphill, cleansing battle. Open systems in nature still find boundaries and limits necessary.

Obstacle 11: *There is a Need for Improved Institutional and Investigator Networking.* Only a small number of disciplinary specialists are interested in synthesis and integration across disciplines. Only a few are sensitive to the advantage they gain by being able to transfer results from one discipline to their own despite the many examples of such successes in the past. Only a few recognize in accounts of the history of science that often it is the very best, the elite of a discipline that are the most open and sufficiently widely read to fertilize their own thinking with results which at first appear to be quite distant—until their creative genius makes the breakthrough obvious. So few are these types that the dispersion of them across disciplines, sub-disciplines, occupations, and continents is very great. This results in a greater need for networking. Typical conferences and meetings are simply not sufficient mechanisms for them to find each other. Some of the remedies for this obstacle are now being tested by the SGSR. The initiation of Special Integration Groups, especially those focused on putative isomorphies, will gather together specialists that would not have any other framework for interaction (e.g. SIGs on Hierarchy Theory, Duality Theory, Self-Organization/Autopoiesis). The SGSR is also constructing computerized, relational data bases designed for remote, real-time inquiry by its members of the following subjects: (i) membership interests in GST by keyword, (ii) general-systems-based organizations, institutes, and research programs, (iii) systems models and putative systems theories and (iv) systems education programs. Since members will be able to design their own pathways according to their special interests when using the data base, this new tool will significantly improve networking [121]. The usage of computer based conferencing throughout the year, and throughout the world, currently in the planning stages through the SGSR, will be an immediate boost to interchange among widely dispersed workers. The trend toward unselfish sharing of resources and memberships by federations of systems societies will further improve networking (IFSR).

In summary, the rapid growth in conferences and meetings is more the result of the rapid growth in professional societies and their natural desire to sponsor their own meeting series than it is a result of rapid and significant advances demanding greater frequency of meetings. The paucity of lineages of work (Obs. 3), the conventional, non-integrative methods used in conferences (Obs. 10) and the need for increased rigor (Obs. 9) suggests that practitioners in the field should emphasize quality more vigorously and deemphasize quantity of meetings.

2.5. *Signs of recognition*

Considering that general systems theory is only about 35 years old and systems analysis about 40, it is perhaps surprising that the traditional signs of recognition are already appearing. A sampling of the growing number of academic institutes, research centers and government bureau's with 'systems' as a part of their title and their work is indicated in Table 4. The increasing number of systems education programs is shown in Table 5, and discussed in the next section. In 1981, the National Center for Educational Statistics established a HEGIS code recognizing for the first time the new field called 'systems science'.

There are several awards for outstanding work in the field now offered by the professional societies. Each year the SGSR presents the von Bertalanffy Award for 'outstanding leadership in the field of general systems theory', as well as an award for the best student paper submitted to the annual competition. Each year the IFSR presents the Ashby Award and Lecture at the Annual SGSR meeting, as well as awards at its biennial meetings. The World Organization for General Systems and Cybernetics (WOGSC) presents the Norbert Weiner medal for outstanding systems research every three years.

The substantial numbers of research and teaching organizations, awards, and government involvements in the field suggest a future of increased acceptance and service. However, certain desired responses have been slow to appear. For example, the U.S. National Science Foundation has no program of funding for this field, although some of its more established sections cover areas that overlap. One of the Engineering Sections has systems analysis in its title but accepts only mathematical systems analysis proposals. Another accepts GST proposals that relate to decision and management theory [69].

Table 4. A sampling of research institutes and government organizations that, in part, sponsor general systems programs

- Bureau for Systems Analysis (Hungary)
- Commission on Cybernetics (Rumania)
- Council of Industry and Cybernetic Service (India)
- Cybernetics Academy
- Decision and Management Sciences Program (National Science Foundation, USA)
- Institute for Advanced Systems Studies (Calif. State Polytechnic University)
- Institute for Systems Studies (USSR)
- International Systems Institute (Far West Labs for Educational Research & Development)
- NATO Special Program Panel on Systems Science
- Office of Education (USA)
- Royal Institute of Technology (Sweden)
- Systems Science Institute (Chinese Academy of Science)
- UNESCO Task Force on Systems Research

plus all Departments and Institutes listed in Table Five

The Fulbright program has no specific category for systems analysis or theory, although such programs as the N.A.T.O. Conference Series II on Systems Science, and the AAAS-organized Gordon Research conference series have occasionally supported the field. As the obstacles mentioned in this paper are overcome, additional funding programs can be expected to appear.

2.6. *Systems education programs*

Both the strategy for initiation of systems education programs and their fate differs when you contrast the experience of the fields of systems analysis and systems theory. Typically, systems analysis tools do not qualify as fields of study in themselves and so are incorporated into previously established, often disciplinary-based education programs. Or tools that have multiple applications such as input–output analysis become parts of graduate programs in the field of application, as in ecological modeling or economic theory, and so do not spawn stand-alone curricula. Systems theory as defined early in the paper also tends to remain disciplinary bound. Only general systems theory appears to be assembling a special and very detailed knowledge of its own. This unique and clearly transdisciplinary knowledge base is primarily composed of the isomorphies and their connections (see Section 3) which can be taught, and indeed demand the formulation of new and rather revolutionary curricula and pedagogies.

Universities, however, are slow to change despite their supposed role as the major initiators of change for society. Independent curricula for general systems theory frequently have not survived the scrutiny and decision making of faculty senates and administrators or the scramble for financial support. Typically, general systems based courses are inserted into more recognizable, traditional departments. In the Nordic countries and Greece, GST is often associated with Informatics and Systems Science Departments (similar to Information Science Departments in the United States), while in the United States such courses may be taught as parts of

Table 5. A sampling of general-systems-oriented education programs (portion of courses on GST)

- College de Systemique de L'AFCET, France
- Cybernetics Systems Program, San Jose State University, USA
- Dept. of Medical Cybernetics, University of Vienna, Austria
- Dept. of Systems Science, State University of New York, USA
- Dept. of Systems Science, The City University, Great Britain
- Informatics and Systems Science, Stockholm University, Sweden
- Social Systems Sciences, University of Pennsylvania, USA
- Systems Science Dept., Open University, Great Britain
- Systems Science Institute, University of Louisville, USA
- Systems Science Ph.D. Program, Portland State University, USA

Departments of Computer Science, Management Science, or in specialized institutes and centers. Table 5 lists some of the types of systems education programs available. There are programs on virtually all levels including adult, undergraduate, graduate, post-graduate and certificate. Systems science, particularly the general systems aspects, is very useful as a minor program for more conventional majors. A reasonable prediction is the gradual evolution of these 'piggy-back' presentations into full-scale independent curricula.

Few students encounter the ideas of systems science in earlier grades and so are unaware of advanced curricula. This may be ameliorated in the future by a million dollar project sponsored by the now-defunct Environmental Education Program of the U.S. Office of Education which guided the design of curricula using systems ideas in the teaching of environmental problems from grades K – to college, including even adult education [9]. Because of its deeply philosophical and integrative nature many young students who are exposed to general systems theory are attracted. However, it is impossible to measure potential demand at such an early stage in the development of a subject matter. In a recent study by the National Science Foundation [71], the fastest growing occupation with greatest demand for trainees was the field of computer systems analysis. Annual growth rates of 6% per year creating a net growth of 70,000–85,000 jobs in the next five years were cited. By 1987, 55% of science jobs will be in the area of computer system specialties. It was already noted that systems science provides a good, basic theoretical background for the tools of this science. As the field solves the obstacles cited here it will provide a richer and deeper training. It is unclear how long it will take before training in general systems theory would be able to stand on its own. Many systems scientists believe it will always have to be taught as a useful adjunct to a traditional field of study.

In the last decades there have been several attempts to assess the state of systems education programs including Troncale and Banathy [124], and Cavallo [27]. Building on this work, the SGSR is sponsoring a study by the International Systems Institute and the Institute for Advanced Systems Studies aimed at assembling a comprehensive, interactive data base survey (and report) on worldwide offerings in systems science, especially general systems theory [121]. Already 25 programs are described [10]. The theme for the SGSR Conference and Proceedings in 1985 is 'systems competence'. Its results are intended to be a guideline for design of better systems education programs. Each of the obstacles and prescriptions in this paper could also be used in the design of better systems education programs. The best of the programs must solve these obstacles if they are to train their students as leaders guiding the future of the field.

Obstacle 12: *There is a Need for Educational Programs that Answer All Needs Cited* (*see Appendix*). A review of the history of several of the programs listed in Table 5 leads to the conclusion that their experiences parallel that of the interdisciplinary programs popular during the 1960s and 1970s, for which a significant literature already exists [3, 28, 57, 97, 141]. A related list of obstacles just as lengthy as those cited here could be produced describing why such programs fair badly. This leads to recognition of another obstacle.

Obstacle 13 : *There is a Need for Adequately Revolutionary Institutional Arrangements.* The list of obstacles thwarting such programs includes the following: interdisciplinary subject matter is poorly defined; it does not fit into the disciplinary divisions of the universities; it violates the territoriality of the disciplines threatening both their funding and prestige rankings; it does not fit into current power structures for decision making on resources; peer review for retention, tenure and promotion are decidedly disciplinary-based; current student and town bias emphasize practical job-training which to them always means traditionally recognized departments; and our Western culture favors reductionism over holistic approaches. Thus dominated by existing stable institutions, acceptance of transdisciplinary programs depends on either reforming current structures or devising entirely new or alternative structures. Fundamental to both strategies is the existence of a critical mass of students and professionals who have experienced a shift in *Weltanschauung*. Not only does the subject matter of GST rest on bootstrapping, its very survival is based on social bootstrapping. Unfortunately such experiments as establishing non-departmental institutes and centers, innovative schools or colleges within traditional structures, even state-funding of an entire university (UC, Santa Cruz) based on interdisciplinary studies have not fared well. And that is why the problem is described as bootstrapping. Until a population demanding systems studies appears, until a subject matter brimming over with special and useful knowledge appears, until a large professional society appears, and most importantly, until job opportunities specifically designated as systems science appear, we cannot expect sufficient context to exist to support a purely systems educational program. But several such programs are necessary to obtain the demand, subject matter, professional interest groups and requisite job skills. Thus, the field is required to lift itself up by its own bootstraps. Time, and continued work on all of these obstacles is the answer.

2.7. *Exemplary systems research today*

Which are the sites of greatest, current activity in general systems research? Where does the highest quality general systems research occur? Are the general systems professional societies and general systems education programs sponsoring the best systems research? Answers to these questions are another subjective measure of the efficacy of the organizational and methodological mechanisms of groups purporting to represent the general systems movement. A reasonable answer would segregate these questions according to the three major categories used as subdivisions for the remainder of this article, that is, progress on systems isomorphies, systems methodologies, and systems applications.

The most impressive progress on discovery, elucidation and refinement of systems isomorphies is not restricted to, and may be even characterized as occurring outside of general systems professional societies and education programs. Even by their own citation activity, GST workers are recognizing that a list of the exemplary project lineages would include the following: (i) Prigogine, on irreversible thermodynamics and order out of chaos [84, 85], (ii) Thom, on discontinuous change (catastrophe theory) and cobordic surgery [103], (iii) Mandelbrot, on fractal processes in natural systems [59], (iv) Eigen, on hypercycles and systems origins [35, 36], (v) Haken, on physical systems mechanics as a unified theory of synergetics [43], (vi) Jantsch, on systems evolution [47], (vii) Simon, on systems dynamics of organizations and artificial intelligence systems [98], and (vii) distributed work on various topics (e.g. solitons, work that is carried out across the physical sciences by a number of investigators [37, 143]). Although, some of these individuals have a passing acquaintance with GST and general systems professional societies, they are not active members, do not attend conferences, and do not publish in general systems journals. Most of the original founders who were active in the professional societies, journals and publications are no longer as active in the pursuit of isomorphies, perhaps with the notable exception of Miller's work on cross-disciplinary hypotheses, and Boulding's continued efforts at syntheses between evolution and economic/social theory. Modest attempts at bringing more of the above exemplary work into the SGSR have been initiated by establishing SIGs on the topics of hierarchy theory and duality theory. Still, much of this work is carried on in various disciplines, surprisingly mostly in the hard sciences.

The professional societies and education programs are much more active in the area of systems methodology and tool-building. Some exemplary project lineages include the work of Klir [48–50], Warfield [131], Pask [79], Beer [13, 14], Checkland [29], Ackoff [1], and Samuelson *et al.* [94]. All of these individuals are active in the general systems professional societies and many are 'key' figures in a systems education program. It is interesting to note that virtually all of these methodological research lineages are human systems or computer systems oriented, and all are based on design of a process for attacking problems of interest to human organizations.

In the area of general systems applications to societal problems this reviewer would have to conclude that exemplary work simply does not exist. Yet, this is the single most active area in most general systems conferences and publications. But the work showing the highest quality is actually work on the level of systems analysis, not general systems. Examples would include the various projects at the International Institute for Applied Systems Analysis, and their counterparts in various national universities, such as models of climate impacts, minerals markets, changes in the biosphere, energy resources and population dynamics (see IIASA Annual Reports). These examples of detailed attempts at systems analysis of complex societal and natural systems problems cannot be used to improve the reputation or acceptance of GST because they belong to the other end of the spectrum. As a symptom of this condition, general systems theorists have not earned the respect of systems analysts any more than systems analysts have earned the unconditional acceptance of disciplinary specialists.

Ironically, level of activity of work in the general systems research professional societies and systems education programs is inversely proportional to the fundamental nature of the work. Research on isomorphies, which is the most basic general systems research product, shows the least activity in the organization; applied research which ultimately depends on isomorphies for its robustness, shows the highest level of activity. Quality of work is higher in the area of isomorphies, but does not originate from general systems research organization; while quality of work is lower in applications, but is popular in the organizations even though it is questionable whether or not it is work of a general systems nature. Work on systems methodology is intermediate in all of these categories. This dilemma is clearly the result of the concerted effects of the 33 obstacles.

3. FUTURE RESEARCH ON ISOMORPHIES: THE FUNDAMENTAL PRODUCT OF GST

The word isomorphies was not invented by systems specialists. Mathematicians have used it to describe formalisms and equations which maintain similar (iso) form (morph) across many levels of nature and many disciplines. In a general theory of systems (GST) it is used to describe a wider range of items; it is used for processes, algorithms, structures (that is, real forms not abstract ones), and even verbal descriptions in addition to mathematical equations. Although some would like to change the word to 'universals' or 'laws' the original founders and many current workers prefer the less presumptuous term of isomorphies. Since they are the most fundamental level of recognition of 'systemness' that cuts across all traditional disciplinary boundaries, and since they are also the most fundamental level of explanation of systems function, isomorphies may be described as the primary product of GST work. The collection of isomorphies would be the primary components of the special knowledge whose organization would constitute the substance of the field, and the basis for the curriculum of any systems education program.

3.1. *Identification and use of greater numbers of isomorphies*

Ironically, this is not the case in practice. Both ongoing GST research programs and the few recognized educational programs use only a small portion of available, putative isomorphies. Sometimes their programs are based primarily on a select set of systems analytical methods with isomorphies occupying a much less central position. This situation partly derives from the necessity for most GST programs to earn their way in this discipline-oriented world by emphasizing applications to a specific system rather than GST for itself (e.g. the rapidly growing computer and information sciences). It further rests on the need for applications that demonstrate power which any new field of knowledge must attain before basic research is taken seriously. It is clear that the search for and elucidation of isomorphies is the basic research arm of GST. And as was the case many times in the history of science, the ultimate health of the field, as well as the utility of its applications rests on the foundation of adequate basic research.

Obstacle 14: *There is a Need for Use of the Full, Minimal Set of Isomorphies.* No one knows what the full set of isomorphies would be, much less the minimal set. But the concept of the existence of a large set of isomorphies may help workers resist the temptation to consider only the restricted set with which they are currently familiar. The GST researcher more than any other must be committed to lifelong learning and study. New isomorphies are appearing with startling rapidity. On the other hand, the concept of a minimal set might encourage workers to restrict their use of the term isomorphy for only those processes and patterns which define functions of systems and not for all of the plethora of jargon terms associated with systems analysis, systems theory and general systems theory.

Even experienced systems workers are often amazed at the extent of the list of isomorphies shown in Table 6. This is just one reviewer's suggestion of proposed isomorphies culled from the systems literature. Neither the number of isomorphies listed (75) nor the hierarchical ordering of them is representative of a consensus in the field. The number and identity of isomorphies included in the list was constrained by the application of a dozen criteria defining what would constitute a true isomorphy [106]. The ordering of them accomplished two purposes. First, it makes the rather long list more comprehensible and more easily assimilated. Second, it relates the existence of each isomorphic process or structure to one of the major systems functions, themselves not isomorphies. These systems functions are hypothetically the result of the concerted action of the isomorphies listed as subheadings under each function. Without the cooperative action of these isomorphies in causing this function any particular manifestation of a system would cease to exist.

Study of Table 6 might lead one to the insight that the potential literature on isomorphies and so the foundation of GST is much richer than generally recognized. Rather than being vague and ethereal, the field is actually potentially quite specific, precise, fertile and rigorous. However, this potential is not realized due to repetitive use of only the most common isomorphies, a restricted understanding of the actual meaning of isomorphies, and by the many other obstacles that cross-impact with and exaggerate this obstacle.

The restricted understanding of isomorphies which works against their improvement derives from the original meaning of the term in mathematics and its original use by the founders of the systems movement. In this use isomorphies are clearly placed in a subservient role to the specific, real systems that exhibit them. Suppose we compare two real systems at different scales of magnitude (different disciplines) in terms of at least one structure or process, for example comparison of galactic spiral flows and climatic flows in a planet's atmosphere. They are found to be isomorphic. As far as most humans perceive, the only real items involved in the comparison are the physical systems. The isomorphy is often regarded as a vague, abstract, human-based, and relatively unreal state or process, not deserving of status as a stable entity.

The future of isomorphies, suggested here, is a less subordinate and dependent position. This view considers the reoccurrence of the same process or

Table 6. Towards a comprehensive glossary of phenomenological isomorphies. A table of 75 principal systems concepts

1.0 TYPES AND TAXONOMIES
- 1.1 Definition of Systems
- 1.2 Parts/Components/Entities/Elements
- 1.3 Purpose/Function/Equifinality
- 1.4 Subsystem/Supersystem
- 1.5 Open Systems
- 1.6 Closed Systems
- 1.7 Types of Systems
  - 1.7.1 Decomposability (Fully, Nearly, Non)
  - 1.7.2 Linearity, etc.

2.0 SYSTEMS ORIGINS
- 2.1 Boundary Conditions/Closure
- 2.2 Autopoiesis
- 2.3 Allopoiesis
- 3.4 Self-Referential Mechanisms

3.0 SYSTEMS FORM/STRUCTURE
- 3.1 Structurprocess
- 3.2 Duality (Origins of)
- 3.3 Hierarchical/Heterarchical Form
- 3.4 Structure of Voids
- 3.5 Fractal Structure
- 3.6 Principle of Plenitude
- 3.7 Symmetry/Asymmetry

4.0 SYSTEMS MAINTENANCE
- 4.1 Static States
- 4.2 Stability
- 4.3 Metastability
- 4.4 Steady State/Dynamic Equilibirum
- 4.5 Transtemporal Stability
- 4.6 Control/Regulatory Mechanisms
  - 4.6.1 Negative Feedback
  - 4.6.2 Positive Feedback
  - 4.6.3 Coupled Feedback
  - 4.6.4 Feedforward
  - 4.6.5 1st, 2nd, 3rd Order (Cybernetics)
  - 4.6.6 Single-Loop/Multiple Loop Feedback
  - 4.6.7 Hierarchical/Cross-Level Feedback

5.0 SYSTEMS FLOW PROCESSES
- 5.1 Flow Turbulence (Power Spectrum)
- 5.2 Restructuring/Throughput/Temp. Capture
- 5.3 Orthogenetic vs. Dispersive
- 5.4 Energy-Based
  - 5.4.1 Entropic
  - 5.4.2 Negentropic
  - 5.4.3 Synergistic
- 5.5 Information-Based
  - 5.5.1 Law of Requisite Variety
  - 5.5.2 Permutation/Recombination Mech.
- 5.6 Optimality Principles
  - 5.6.1 Principle of Least Action/Energy
  - 5.6.2 Principle of Least Time/Space
  - 5.6.3 Principle of Least Matter/Energy

6.0 SYSTEMS GROWTH AND DEVELOPMENT
- 6.1 von Baer's Laws
- 6.2 Zipf's Law
- 6.3 Morphometric Laws
- 6.4 Allometric Growth (Proportionality)

7.0 SYSTEMS TRANSFORMATIONS
- 7.1 State Determined Systems
- 7.2 Phases/States/Modes
- 7.3 Catastrophe's
- 7.4 Bifurcations
- 7.5 Cobordism Surgery
- 7.6 Cyclical Behavior
  - 7.6.1 Life Cycles
  - 7.6.2 Limit Cycles
  - 7.6.3 Periodic/Oscillatory Behavior

8.0 SYSTEMS LINKAGES
- 8.1 System Context or Environment
- 8.2 Input/Output
- 8.3 Entitation
- 8.4 Complexity Measures
- 8.5 Coupling Types
  - 8.5.1 Insulated/Non-Insulated
  - 8.5.2 Strong/Weak
  - 8.5.3 Synergistic/Antagonistic
  - 8.5.4 Linear/Non-Linear, etc.
- 8.6 Coupling Magnitudes/Distances
- 8.7 Macro-Uncertainty Principle

9.0 SYSTEMS FIELD CHARACTERISTICS
- 9.1 Resonance Phenomena
  - 9.2.1 Consonance
  - 9.2.2 Dissonance
  - 9.2.3 Transgressive Recursion
- 9.3 Soliton's
- 9.4 Anticipatory/Precocious Vectors

10.0 SYSTEMS EVOLUTION
- 10.1 Randomness/Chaos Mechanisms
- 10.2 Concrescence Ratio
- 10.3 Neutrality Principle
- 10.4 Logarithmic Spiral of Variants
- 10.5 Transgressive Variation
- 10.6 Ontogenetic/Phylogenetic Mechanism

11.0 SYSTEMS EMERGENCE
- 11.1 Stability Limits-Isomorph Network
- 11.2 Parameter Trends
- 11.3 Process of Emergence
- 11.4 Complementarity/Counterparity
- 11.5 Transgressive Equilibrium
- 11.6 Exclusion Principle
- 11.7 Deutsch's Law

12.0 SYSTEMS DECAY PROCESSES

structure over and over again in the progression of origins of real systems—from the big bang to modern times, repeated on every scale of reality—simply too improbable to be explained by coincidence. Thus the isomorphies so observed must be very real, more fundamentally real than the real systems which exhibit them since the physical systems which appear much later in time follow the same form as those systems which appear very early in time. How could such a repeating form occur across demonstrably separated systems? A less presumptuous and probably more correct position would be that both the real systems and the isomorphies are equally real, each meaningless or unactualized without the other. A similar, but much less encompassing argument is accepted in the hard sciences. Consider how intrigued theoretical physicists are by the fact that the observed concentration of matter in the universe is only one order of magnitude removed from that required to slow expansion of, and recycle the universe. The similarity of both numbers sends them on a search for underlying principles to explain the improbable likenesses. Likewise, the similarities we call isomorphies, which are hypothetically observable across all of the major scales of magnitude of reality, suggests the existence of underlying principles of such breadth and fundamentality that our very conceptions of what is real and what is abstract are challenged.

Surely someone will argue that the similarities cited in physics are empirical while those of putative isomorphies are not. A textbook-in-preparation tries to find evidence for each of the isomorphies listed in Table 6 in the literature of each of the following disciplines [110]: cosmology; astronomy/planetary science; sub-atomic particle physics; chemistry; geology; molecular biology/biochemistry/molecular genetics; cell biology; general biology; physiology; ecology; psychology; sociology; and the information sciences. A series of obstacles cited in this paper indicates the need for or the absence of methods of empirical refinement and rigor in GST (Obs. 9 and 18). The above approach attempts to provide some hard evidence for similarity across the many disciplines and levels of reality expected of isomorphies, as an interim method of empirical refinement in GST.

A series of articles has appeared that attempts to provide a glossary of systems terms. These may be used as a shopping list for recognition of isomorphies as well as raw materials for a glossary and consensus. Young was the first to attempt such a list [142], and although his list is dated, it is historically and methodologically of interest. Ackoff carefully defined a list of 32 such concepts [1]. Von Foerster defines and explains 238 terms in an introductory text [128]. Jain [46] added the dimension of classification of the concepts so listed. Most recently, Robbins and Oliva have conducted a number of empirical studies [89–91] on the sociological dimensions of use of 51 of the concepts. Troncale used a list of criteria to further reduce the list of possible isomorphies to only those (currently 75) having a phenomenological base whether or not they were used commonly in the field or were useful as part of the fields jargon [106, 112, 116]. The last two investigators clearly specify the purposes of their listings and critique each other's formulations. A much longer and unrestrained list of holistic concepts ($n = 421$) is included in a collection edited by the Union of International Associations [126]. Overcoming Obstacle 13, however, does not stop at assembling and elucidating a long list of isomorphies. It requires specification of their actions such that both the 'full' set, and the 'minimal' set can be unambiguously defined. In other words, the set of isomorphies must be proven necessary and sufficient. Despite the increasing attention paid to this obstacle cited above, it is clear that the majority of work is in the future of research on isomorphies.

Obstacle 15: *There is a Need for an Operational Taxonomy of Isomorphies, Systems, Types and Tools.* At the present time, GST, and much of systems theory and analysis, is in a pre-Linnaean state. Recall the extensive awareness of organisms, but the lack of a clear recognition, presentation and formalization of their similarities and dissimilarities before Linnaean classification was widely accepted. Eventually the static, creationist classification of Linnaeus was improved, and made dynamic by the discovery of the process of evolution. A similar two-stage advance is needed in GST. Work has only begun on the first stage. Klir has described some initial taxonomies of systems types [48]. His classification scheme, however, is heavily influenced by his viewpoint of systems from the perspective of systems engineering and the tools utilized. Miller classifies living systems according to types and levels [67]. This ignores most of the matter of the universe which is non-living. Oren has produced an admirably detailed classification of computer simulation tools [73–75]. Something of this nature is needed for general systems models and tools. Tabor has recently reviewed the usage of nonlinear equations in modeling systems dynamics and presented an interesting taxonomy [102]. The important aspect of this modest taxonomy is that it accomplishes synthesis and integration in spite of its use of analysis as the approach and manages to link this new synthesis to the classical literature. Table 6 is a temporary classification of proposed isomorphies according to their functions in systems and has been presented in greater detail elsewhere [106, 112, 116]. It also attempts to accomplish synthesis of ideas and past literature while using detailed analysis as its mode of operation. The important interrelations among these early versions of a GST taxonomy should be mapped and discussed in detail. The above-cited criticisms and goals could be rewritten as performance criteria for the taxonomic approaches needed in GST. Work on the second step in this mimicking of progress in the biological sciences by GST is discussed further in Section 6.

Most workers would agree that no single taxonomy will be entirely satisfactory to all users because of the diversity of systems. The concept of a hierarchy of general theories of systems, with some more inclusive than others, has been suggested in debates at conferences, but not systematically and formally, except in the somewhat specialized work of Klir *et al.* Until many of these issues are debated, progress toward consensus will be very slow. Meanwhile, progress on such important breakthroughs as a theory describing 'emergence' phenomena and mechanisms must await such mundane accomplishments as taxonomies. Just as the process of evolution was more difficult to perceive without a consistent organization of the plethora of species, so also the process of emergence is difficult to perceive without a consistent taxonomy (or taxonomies) of systems.

3.2. *A case study: linkage propositions between principal systems concepts*

In 1971, Ackoff called for design of a 'system of systems concepts' [1]. Although he wrote an article on the topic, it falls short of its goal of systematizing interactions and appears to be more of a short glossary. Many of the founders of the general systems movement, such as Margaret Mead, have criticized the development of the field for its lack of application of systems methodology to itself. Part of this criticism focuses on the apparent stand-alone nature of isomorphies once discovered. Connections between them are not systematically or formally studied and explicated. Miller, in his *magnum opus* on living systems [67], made a special effort to suggest numerous cross-level (and so cross-disciplinary) hypotheses. These, if restated, could become a list of connections between isomorphies. The work of Robbins and Oliva, and Jains cited above also try to trace sociological linkages among systems terms, however these may have no basis in the phenomenology of systems since they are mainly based on human usage. A review of the systems literature would reveal an almost complete lack of systematic study of interactions among the isomorphies.

Obstacle 16: *There is a Need for Systematic Specification of the Linkages Between Isomorphies.* A lineage of papers which attempts to overcome this obstacle invents and defines 'linkage propositions' as a way to connect any two or more isomorphies by describing their specific cross-impact on each other in a semantic sentence [106, 112]. Using a modest general morphological method [146, 147], the isomorphies listed in Table 6 (and their literature) are explored for proven and hypothetical mutual influences. One hundred and forty-two linkage propositions have been formulated to date. A graphic presentation is utilized to ease assimilation of the complex set of interactions and appears as a net with isomorphies as nodes and the linkage propositions as lines. At the present time the 'net' so formed is being examined with the techniques of graph theory in an attempt to derive more information than just the statements of interconnections themselves [4]. It is thought that the linkage propositions are unique and distinct from precursors such as the correspondence principles of the hard sciences [95, 96], the cross-level statements of Miller [67], the entailment networks of Pask [79], ahd the correlates of usage described by Jains and Oliva (above), although they bear partial developmental relationships discussed in [112] and [116].

The full set of linkages is expected to be much larger than the current set. Fortunately, the many linkage statements fall into a much more constrained set of 'association classes'. The 'operators' which describe the influence of one isomorphy on another tend to reappear over and over again. This not only simplifies the presentation of the Linkage Proposition Template Model (LPTM), but suggests that the linkage propostions could be formalized by assigning symbols to the operators. This development, in turn, enables the manipulation of the mathematical symbols expressing currently known linkage propositions to derive as yet unknown linkages. This amounts to the possible construction of a special new formalism derived from and unique to the field of a general theory of systems. Seven performance criteria for this new formalism, and six techniques of representation and usage (some quite innovative) are presented and discussed in [116].

There are a number of ways that even microcomputers could be used to make the Linkage Proposition Template Model extraordinarily utilitarian. Some initial attempts are underway to make the net of linkages accessible by computer graphics and stepwise refinement techniques using a new program called the *Lifework Integrator* (©) [66]. A related approach is the design of an expert system (GENSYS) which would guide any specialist in recognizing the isomorphies in his target system, and then guide him through the linkage propositions between those isomorphies [66]. This would enable a specialist in a target field, such as transportation systems, to more quickly achieve answers to problems, or achieve a more complete systems analysis of the target field, even without previous systems analytical experience. Another project underway is studying the feasibility of an extension of GENSYS, named METAGENSYS, which would be devoted to further evolution of the Linkage Proposition Template Model, and empirical refinement of a general theory of systems.

As many as a dozen specific uses have been cited for the computerized Linkage Proposition Template Model [106]. It is a 'template' model for a general theory of systems because both the isomorphies and the linkage propositions should be true for mature systems on virtually all disciplinary levels. Thus, the LPTM is a putative general theory of systems under construction that is immensely detailed, both analytic and synthetic, and which is amenable to empirical refinement. Whatever the outcome, the series of papers involved is a case study of the efficacy of a lineage of papers with contributors from several disciplines and tools which is also attempting to make GST more rigorous and user-friendly (Obs. 3, 7, 11, 24).

Obstacle 17: *There is a Need for a Self-Generating Set of Isomorphies.* It is insufficient for a general theory of systems to merely describe detailed interactions among its many isomorphies. The interactions must possess, in addition, the special quality of self-organization demanded by the performance criteria for the theory (see Section 6.1). This quality is demanded by the very definition of the theory. The position stated above, which suggests that isomorphies are as 'real' as systems of 'things', has a corollary which states that the isomorphic processes and structures exist because they require the least time/space and matter/energy resources. They are optimal, minimal arrangements of all possible interactions. This is why they recursively reappear in the same form at each new scale of magnitude of real system. Apart from the real systems that manifest the isomorphies, the set of isomorphies have life of their own. In a sense they generate the real systems. But what generates them? In a sense they generate themselves, even though the previous level of real system is necessary for their generation or manifestation to man's senses. Due to this very fundamental nature of the general theory, its isomorphies must possess an internal autopoietic nature; they must be able to generate themselves as a self-consistent, self-organizing set of axioms.

With the record of research on interactions between isomorphies being as poor as just outlined, it is not surprising that this autopoietic nature of the set of isomorphies remains unelucidated, even though most general systems theorists include autopoiesis high on their personal list of isomorphies. The self-generating feature of isomorphies and the Linkage Proposition Template Model is a meta-level of autopoiesis.

The Linkage Proposition Template Model has some interesting features which might lead to realization of this level of autopoiesis. For example, the array of linkage propositions attached to any one isomorphy greatly enriches dynamic understanding of that isomorphy in such a way that it is easy to understand how that isomorphy arises from the others. But the other isomorphies share the same fate. They, each in turn, arise from the interactions with the others. No first cause can be identified; they all require the whole set; no linear cause and effect can be invoked without referring to the full set. This is an excellent example of the 'bootstrapping' mentioned earlier in the article. It has been found to be true of sub-atomic particle systems using empirical and theoretical studies. That is where the first use of the term 'bootstrapping' occurred in the sciences. It is not surprising that the general theory would have the same feature as fundamental quantum thermodynamics. And we cannot rest until our GST research demonstrates this relationship more fully. Otherwise holism will always sound empty in its claims. Note how the deep analytic nature of the many linkage propositions, even though they seem to diametrically oppose the whole-oriented, synthetic approach, actually result in a manifestation of the indivisible wholeness of the set of isomorphies and their autopoietic linkages. 'Anasynthesis', one word, one entity [110, 125].

3.3. *Case study: future of a representative isomorphy—hierarchy theory*

Hierarchy theory is a good example of the obstacles facing development of any of the isomorphies listed in Table 6, as well as their potential should the obstacles be overcome. The history of usage of the systems aspects of hierarchical structure in biomedicine can be seen in Fig. 7. It is fairly representative of other disciplines. Although the 'social' meaning of hierarchy had been studied for many years, the application of that concept to explaining a much wider range of reality such as astronomical, chemical, and biological systems did not begin until the late 1960s and early 1970s. These were the first comparative, transdisciplinary studies of hierarchical structures and processes. Immediately, the established meaning of 'social' hierarchies and their features began to interfere with communication across the specialties and recognition of those aspects of the '*structurprocess*' [125] that were similar across levels. This interference continues today. The early literature on hierarchy theory beings with Whyte, Wilson and Wilson's conference in 1969 [133], and continued with monographs by Pattee [80], Mesarovic *et al.* [65], Weiss [132], and key articles by Simon [98], and Platt [82]. But then after the ferment of the early 1970s there is a 10-year lag period. Why? Presumably the early insights were insufficiently tied to the disciplinary data bases (Obs. 19), with the consequence that no lineage of research and papers (Obs. 3) were derived from the first useful insights. Empirical research is necessary to provide the detailed type of results that sustains a field between its theoretical leaps.

Obstacle 18: *There is a Need for Empirical Refinement of Isomorphies and Linkage Propositions.* There is much opposition to the mere proposal of any version of falsifiability or verifiability in some holistic-oriented systems circles. Before showing how empirical refinement can help in hierarchy theory, this position should be examined. It arises from the misconception that any reduction-oriented approach is opposed to the very basis of holistic general systems theory. As Medawar points out in an otherwise faulty and subjective review [61], it has become popular in the social sciences to recite anti-reductionist doctrines using such logic as *argumentum ad hominem et extensum ad absurdum.* Overextension of Heisenberg's Principle of Uncertainty [25], and Goedel's Theorem, as well as Popper's observations on the process of science are used in defense of holism [83]. This work is cited as proof that no proof is possible, that no ultimate measurement or axiomatic argument finalizes fact, and, therefore that reductionism is dead. While it is true that this work has changed forever our old concepts of what is a fact by limiting the claims of reductionist science for ultimate authority,* it

* How many revolutions against 'ultimate authority' must we humans endure? First, our inner voice was dead; then, God was dead; now, science is dead. When will 'ultimate authority' as a power concept be dead? As axiomatic as hypothesized, let me make clear that no one is claiming that isomorphies are 'ultimate'.

cannot be used to invalidate the value of detailed study and empirical measurement. The practical value of empirical disciplinary approaches are proven every day. People rarely die of pneumonia any more at the age of 35, and huge buildings keep us warm, and do not often fall on our heads. The above results merely indicate that neither reductionist nor holist work contains the whole answer to a question and thus proves that neither can survive alone. It still remains for the holists and general systems theorists to prove that their work is practical; this has never been done in a robust way. And it will not become practical in this reviewer's opinion until it engages in empirical refinement in addition to synthesis. Notice the subtle shift in terminology. Empirical 'refinement', used throughout this paper is not the same as claiming verfiability or falsifiability. Most of the tough obstacles listed here cannot be overcome except through the more detailed work required of at least semi-empirical approaches. In order to attempt some modified type of empirical refinement, data is needed.

Obstacle 19: *There is a Need for Data Bases Coupled to Isomorphies and Models: Correspondences.* Because of its nature, GST requires a much different scale of data base than most sciences. In order to refine transdisciplinary isomorphies and linkage propositions, GST must organize highly systematic data bases of hard data from virtually all of the disciplines possessing such data. At first it may seem that there could be no relation between the postulates of a general theory and such a wide range of data, but the experience of the hierarchy theorists will show how useful a close coupling of data and holistic propositions might be for enriching the initial general systems propositions. And this in turn would be an example of transcending disciplinary training while using it (Obs. 5), increasing rigor (Obs. 9), synthesis of literature across disciplines (Obs. 4), and the two just mentioned (Obs. 17 and 18).

How can hierarchy theory exemplify the above obstacles and their solution? Clearly hierarchy theory is a transdisciplinary problem. In a five-year period, 1978–83, there were 2658 research articles published on hierarchy theory according to a search conducted of the computerized data bases MEDLINE, BIOSIS, SCISEARCH and INSPEC [122]. All were disciplinary based articles reporting on primarily empirical research. A relational data base analysis of a sample of 225 of the authors of these papers revealed that the group spanned 32 disciplines and represented 27 countries. This research community is a good example of the potential research communities of general systems theory, even though few of this particular group would identify themselves as such. It represents an impressively diverse and interesting mixture of disciplines, scalar levels of inquiry into natural and man-made systems, institutions and countries. The fact that all of these researchers felt it justifiable to use the term hierarchy to describe a portion of their studies indicates that they are recognizing the most abstract features of hierarchy in their respective specialties and this supports the isomorphic nature of this pervasive *structurprocess* [110, 125]. However, the term has so many specific disciplinary-based meanings that recognizing the commonalities between these meanings is inhibited. The simplest example is the dominance of the social meaning of hierarchy, clearly the first recognized, but also a special meaning which in the context of social systems is burdened with such characteristics as teleological purpose, authority/control, and top-down dominance and determinism. All of these meanings are foreign to natural systems scientists, yet their systems also exhibit hierachical form and process. Possibly the attributes of social hierarchies are not the most fundamental or transdisciplinary characteristics of hierarchy. A context-independent meaning for hierarchy is needed in the tradition explained by Klir [48]. Thus, establishing a communication and consensus is the first task.

Considering the immense amount of data available in the literature just cited, the opposite of communication is occurring in practice. It is customary for researchers to examine only those papers directly related to their immediate specialty. Results of other specialties are often unintelligible due to extreme differences in jargon, methodologies, and even implicit values. Thus much of the potential hierarchical literature goes unexamined by potentially interested readers. In addition to the above mentioned obstacles, solution of this problem would help overcome such obstacles as: the need to demonstrate any one isomorphy across all disciplines (Obs. 6); the need for adequately transdisciplinary research teams (Obs. 7); the need for consensus producing processes (Obs. 10); and the need for improved institutional and investigator networking (Obs. 11).

There is ample precedent for cross-speciality communication within the local scales of magnitude (disciplines) which could be used to rationalize the argument for the benefits of transdisciplinary communication. As fields mature comparison becomes very beneficial. Many new discoveries resulted from initiation of such specialties as comparative anatomy, comparative physiology and comparative paleontology. Interfaces between fields yield exciting results as exemplified by biochemistry, biophysics and potentially sociobiology [137, 138 and 55 for negative evidence]. Cross-fertilization yields new hypotheses and suggests new avenues of empirical inquiry. At the Macy conference of the Interdisciplinary Communications Program, 'feedback' was first recognized after comparing such diverse fields as medicine, electrical engineering, computer sciences and mathematics. It is anticipated that comparing hierarchical form, process, measurement and representations across the 32 fields represented will also lead to cross-fertilization, creativity and better empirical inquiry even within the disciplines represented. Thus, hierarchy theory is a model of the potential of isomorphies in the future, if the obstacles to its progress can be overcome. What is being done about these obstacles in GST?

The SGSR has initiated several methodologies to overcome the obstacles. It has established a Special Integration Group (SIG) on hierarchy theory to improve networking of interested researchers spread across a wide range of countries, institutions, and specialties. Under the leadership of Troncale, Salthe and Allen it has begun a conference of three years' duration with face-to-face meetings once a year. Integration work is continued throughout the year guided by a mailed questionnaire, the Integration Directed Iterative Dialogue (IDID), wherein the questions are designed by the group itself to improve communications and detailed comparisons across the fields. Synthesis of findings is pushed by directed questions and by the successive rounds of refinement of ideas. Such determined interaction schemes are needed to speed up integration of phenomena as disparate as hierarchical form in subatomic particle physics, chemistry, mathematics, cosmology, bioscience, sociology, linguistics and philosophy. A computer conference is underway [122] on the COM system in Europe [76], to be extended to North America soon, to optimize integration work beyond the IDID technique. A computerized data base containing detailed information on a thousand systems-oriented members of systems professional societies is under construction which will allow customized printouts of researchers working on this (or any other isomorphy) [121]. This transdisciplinary attempt at improving hierarchy theory is in close communication and synergy with an independent group on the same topic led by Stan Salthe (City University of New York) for the discipline of biology. This group includes molecular biologists, ecologists and specialists in the theory of evolution. The hierarchy theory SIG will sponsor paper sessions and panels on this topic on a continuing basis in conferences of various professional societies. These activities taken together should improve the chances of lineages of papers and networking of institutions on hierarchy theory.

The most significant feature of these processes is that each is a method that could be used for *any* of the isomorphies listed in Table 6. Systematic extension of these processes, provided they prove effective in this initial experiment, would help overcome many of the obstacles that face GST.

Another example in this case study of hierarchy theory illustrates the utility of empirical refinement of isomorphies and their coupling to data bases. The Institute for Advanced Systems Studies (IAS) has a project and lineage of papers devoted to assemblage of a massive data base on hierarchical levels across all known disciplines [105, 108, 113, 114, 122]. Data from each discipline is entered into a computer system which links each item to its original source in the refereed literature. Data on 12 different Newtonian parameters (time/space, matter/energy) as well as five different information parameters are included [114]. Clustering theory algorithms are applied in an attempt to non-anthropomorphically determine which are the levels in hierarchies and what are the quantitative characteristics of the 'gaps' between the levels. Here lies a context-independent, which is to say a disciplinary independent (trans-disciplinary) feature common to all hierarchies [108, 113, 114].

Rather than apologizing to the disciplines, or rejecting them, this example of data base coupled to isomorphy for empirical refinement actually cooperates with the disciplines, while it transcends them for integrative and holistic purposes. Most exciting is the prospect that this type of hierarchy theory might suggest important new ideas and tests to the disciplines that they had not yet identified. The biologists working in the area may be the first to realize this goal [2]. This is a spin-off which would validate the value of transdisciplinary research to the disciplines perhaps for the first time. Further, this kind of testing may add rigor to disciplinary hypotheses ignored for decades. Most disciplinarians cite hierarchies (e.g. many introductory textbooks in biology and sociology), but do so only on the basis of assumption and logic. Most of the proposed disciplinary hierarchies (outside of astronomy, sociology and particle physics) have not empirically tested their hierarchies. Systems groups may accomplish this for them.

An interesting feature of modern science has been the blending of the hierarchy of one discipline into the hierarchy of the next (e.g. biochemistry into cell biology via the origins of life experiments). The outcome of this largely empirical work has been the recognition of a concatenation of hierarchies into one sequence with times of origins and possible mechanisms of origin empirically understood and demonstrated. The above data base will be able to study this (meta) hierarchy by testing hypotheses that could not even be identified in the separated disciplines. What may emerge is an understanding of the 'broad scale structure of the universe' [109, 113, 117] to add to our current work on its large-scale structure. There are several difficulties still to be overcome in this project concerning the limitations of current tools, such as computerized clustering methods. But cooperation among members of the SGSR Hierarchy SIG that possess different skills already has helped. For example, Zupan [145] has solved certain problems handling large amounts of data in clustering theory by using an insight from Mandelbrots fractal theory [59].

This will contribute substantially to the meta-hierarchy project. Although significantly different from disciplinary based empirical studies, with significantly different expectations and predictions [115], this transdisciplinary empirical refinement project could be used as a model for many other isomorphies, and for overcoming Obs. 17 and 18.

4. THE FUTURE OF SYSTEMS METHODOLOGY

There are two classes of methodology of concern to systems workers. The first is a hurdle for all fields. Each field must formalize the traditions, values, processes and standards internal to the field. For most scientific fields this becomes a simple, context-dependent extension of the regular scientific method. There is often a close coupling between this methodology internal to the field, and the second which is the methodology by which the phenomena of the field are studied. One may characterize this as the 'external' methodology applied by the community of scholars to the world around the community. The methods shared by the social fabric of the research group have many correspondences to the 'tools' used to study nature. For example, molecular biologists have an internal methodology [81], and this is different from one of their tools, e.g. density gradient ultracentrifugation, but both are linked to the theories or explanations of the field by long established correspondence principles [95, 96].

The case is somewhat different in systems methodology. Table 7 is a sample of both the 'soft' and 'applied' tools used in systems approaches. Most are found in the domain of systems analysis, few in systems theory, and very few in the domain of general systems theory. It has been argued that these are specialties along a spectrum, quite distinct from each other [see 111, Introduction], and the entire spectrum distinct from what may be called normal science [53, 115]. So at the very outset there is confusion in the field because it spans such an immense array of phenomena types studied at different levels of abstraction. This results in greater distance between the internal methodology the field applies to itself and the external methodology it applies to what it studies. The distance widens as one moves from systems analysis to systems synthesis. This may explain why the founders criticized the movement for not applying its tools to itself. Even worse, one of the tenets of holism is a rather fundamentalist belief against the dichotomies of subject: object and observer: observed which even denies the ability to couple phenomena with methodology. This makes correspondence principles untenable towards the end of the spectrum dominated by general systems approaches. You would expect systems analysis to have more consistent correspondence principles and therefore a higher reputation with the disciplines because it is closer to the data and methods of the disciplines. In actuality, systems analysis has been the recipient of a great deal of criticism from the disciplines in the past [15, 17, 44, 58] (and witness reactions to the International Institute for Applied Systems Analysis, IIASA). The following assessment of where systems methodology might need to go in the future is influenced by this perspective. It will be a look at both the internal and external methodology and the need for correspondence of some unique sort between them.

More comprehensive studies of general systems methodologies may be found in the reviews of Klir [48–50], the proceedings of certain conferences [54, 111, the biannual conferences of the Austrian Society for Cybernetics, and the triannual W.O.G.S.C. series], as well as recent reference works devoted to this subject [70]. Predominantly, the tools and methods of the systems approach are confined to the domain of systems analysis. General systems theorists use them indiscriminantly without recognizing that they are several orders of magnitude away from what is needed for general systems approaches.

4.1. *Common problems in general systems methodology*

There are several ways to 'measure' the internal methodology of GST. Normally, one reviews the editorial policies of the journals serving the field, the review panels for approving grants, the conduct of criticism at the annual meetings, the review procedures for acceptance of presentations at the meetings, and the review of candidates in the educational systems both at the student, professorial and working levels. In this new field, review panels for grants and review of candidates for the educational programs are too few in number to judge. Members of the natural sciences that attend GS conferences usually describe them as exhibiting a few very creative ideas and intriguing people, but as generally unrigorous and very loose. For the first time, the last evaluation of an annual conference

Table 7. A non-comprehensive listing of 60 techniques for systems analysis

ARTIFICIAL INTELLIGENCE AND PATTERN RECOGNITION
CATASTROPHE AND BIFURCATION THEORY TECHNIQUES
CLUSTERING THEORY AND ANALYSIS
COMPARATIVE SYSTEMS ANALYSIS
COMPUTER MODELING AND SIMULATION (MANY PROGRAMS)
COMPUTER-AIDED DESIGN
CONFLICT ANALYSIS
CONTROL THEORY
COST-BENEFIT ANALYSIS: COST-EFFECTIVENESS ANALYSIS
CPM (CRITICAL PATH METHODS)
CROSS-IMPACT ANALYSIS
DECISION ANALYSIS
DELPHI CONFERENCING TECHNIQUES
DIVERGENCE MAPPING
ENVIRONMENTAL IMPACT ASSESSMENT
EXPERT SYSTEMS
FIELD THEORY
FLOWCHARTING
FRACTAL ANALYSIS
FUZZY SET THEORY
GAME THEORY
GENERAL MORPHOLOGY
GPSS - GENERAL PURPOSE SYSTEMS SIMULATOR
GSPS - GENERAL SYSTEMS PROBLEM SOLVER
GRAPH THEORY
HEURISTICS
INFORMATION THEORY ALGORITHMS
INPUT:OUTPUT ANALYSIS
INTUITIVE EXPLORATION/BRAINSTORMING/METAPHOR AND ANALOGY BUILDING
LATERAL THINKING
LIFECYCLE ANALYSIS
LIFEWORK INTEGRATOR PROGRAMS
LINEAR PROGRAMMING TECHNIQUES
LIVING SYSTEMS ANALYSIS
LPTM (LINKAGE PROPOSITION TEMPLATE MODEL)
LINKAGE PROPOSITION EXPERT SYSTEM
MATRIX ANALYSIS
MEANS:ENDS ANALYSIS
META-METHODOLOGICAL STUDIES
NETWORK THEORY
NON-LINEAR MODELING
OPTIMIZATION THEORY
PERT (PROGRAM EVALUATION AND REVIEW TECHNIQUES)
PROBABILITY TREES
QUEUEING THEORY
RELATIONAL DATA BASE ANALYSIS
RELEVANCE TREES
RECONSTRUCTABILITY THEORY
SCENARIO BUILDING
SENSITIVITY ANALYSIS
STATISTICAL TECHNIQUES RELEVANT TO SYSTEMS DYNAMICS
STRATEGIC PLANNING ALGORITHMS
STRUCTURED PROGRAMMING
SYNECTICS
TOPOLOGICAL ANALYSIS (INCL. COBORDISM SURGERY)
TRADE-OFF AND VALUE-ADDED ANALYSIS
TECHNOLOGICAL ASSESSMENT
VENN DIAGRAMMING

(SGSR) indicated a widespread displeasure with the review procedures for selection and rejection of presentations. This is an encouraging sign of the growth of critical-mindedness within the GS regulars. In fact, personal communications indicate that few, if any, papers are rejected so that the quality of the deliberations and the proceedings suffer. Each journal differs, but editors are faced with proposed articles that span natural and man-made phenomena across all disciplines. This presents them with a unique review problem. This new journal is an example. Although it is sponsored by an organization that primarily is interested in general systems theory, its editorial policy reads like that of a journal solely dedicated to human-based systems research. This omits most systems-level work on the physical and biological levels which this reviewer feels will be the breakthrough areas in the near future. Even in the area of human systems there is a wide range of specialties represented. This publication, in response to this challenge now has a very large list of reviewers that also span the disciplines; it remains to be seen if this increases quality and rigor of the review process. Even with this innovation, papers seemingly quite profound to reviewers of one discipline may seem vapid to reviewers of another discipline.

Obstacle 20: *There is a Need for a More Highly Specified Research Methodology Internal to the Field.* Recommendations for solving this obstacle include: (i) statistics on acceptances and rejections of papers for both conferences and journals should be published, (ii) journals and conference organizers should specify required sections for papers which describe minimal procedures necessary before a research or application project is acceptable (such sections must go beyond introduction, and conclusions including at least the following items), (iii) papers should state at the outset into which domain they fall, systems analysis, systems theory, and/or general theory so that appropriate standards can be applied, and confusion in the field reduced, (iv) some form of empirical refinement must be demonstrated in each paper (note empirical refinement is not falsification/verification), (v) international federations might set up international unions of scholars to standardize basic terminology, (vi) a tradition of self and collegial criticism should be encouraged, (vii) explicit lists of criteria should be described for each crucial step in attempted improvements of understanding of isomorphies, methodologies and applications, (viii) assumptions should be relentlessly searched out and exposed, (ix) multiple alternative explanations (mechanisms) should be suggested before empirical refinement techniques are designed or applied. Lists of 'shoulds' such as this are odious and meaningless unless they emerge wholeheartedly from the research community itself. However, if they are not debated and frequently mentioned they will not emerge.

Obstacle 21: *There is a Need for Improved Methods of Integration and Synthesis.* The deficit in effort exerted on synthesis in the modern age of reductionism has been a consistent theme throughout this analysis. The enfranchisement of professional societies devoted to the effort (Table 2), the appearance of educational programs training young professionals in the effort (Table 5), and the improved networking of disciplinary specialists and institutions resulting from computer conferencing and initiation of such 'invisible colleges' as the Special Integration Groups of the SGSR, may help overcome this obstacle. The special software programs described in the next section have a similarly targeted purpose. Little known, but useful synthesis techniques such as general morphology [146, 147] should be evaluated, improved and taught. But the event most needed is a shift in *Weltanschauung* on the part of the most talented minds extant internationally which would endorse both the significance and feasibility of transdisciplinary comparisons. The authenticity of the fundamental value of isomorphies as described here needs exposure and debate. An expanded view of the 'guarantors of truth' is required, but not without demonstration that the expanded view is necessary. Finally, the different levels of synthesis typical of systems analysis, systems theory, and general systems theory require explication so that inappropriate expectations and measurements of success are not mistakenly applied by those from inside or those from outside the field.

In this context, it would be inappropriate to ask any of the tools listed in Table 7 to bear the responsibility for demonstrating a general theory of systems. They are useful, but only within limited application domains. The techniques of systems analysis simply do not constitute attempts at a general theory although they may expose singular, isomorphic processes. This observation suggests two additional obstacles.

Obstacle 22: There is a Need to Recognize that Most Techniques of Systems Analysis are Based on One Isomorphy.

Obstacle 23: There is a Need to Resist Over-reliance on Available, but Limited Tools. Examination of Table 7 indicates that many of the most popular tools of systems analysis are actually based on one or another of the longest recognized isomorphies in Table 6. Input–output or means–ends analysis with its many applications in management systems, operations research and now in ecology is a clear case in point. Modeling techniques using linear equations and matrices could be explained in terms of interaction and coupling types between system parts as well as to aspects of oscillations. Even exotic techniques for simulation or explanation like catastrophe theory are based on the emergent leap across gaps between the levels of stability that are characteristic of levels in hierarchies.

Certainly it would be easy to attack such a position with exceptions, but the purpose of the observation is not to uphold an absolute position. Rather its modest intent is to enable a simple utilitarian insight. If some of our most useful tools are based on isomorphies then: (i) there are many more tools possible if only we would expend the basic research effort needed on isomorphies, (ii) because of the many linkages between isomorphies (note the LPTM [106]) we may conclude that no single technique of systems analysis will ever describe a system anywhere near adequately. This is sobering. Models and simulations used on real world problems that are too reliant on any one or even a small set of the tools in Table 7 must be very incomplete. This may be the reason why a founder of a movement like operations research such as Ackoff might declare it dead. By focusing on its specific tools alone, albeit their utility in certain situations, the field has forgotten the work it has yet to do. Too much relative success, and too established an educational tradition may have halted its needed evolution. While systems analysts may condemn general theorists correctly for their lack of rigor, the general theorists may correctly scold the tool-users for wearing as thick a set of blinders as their disciplinary counterparts. Clearly a new field is needed, perhaps called Comparative Systems Analysis, that rigorously describes the limitations as well as the potential of each available tool, shows in what specific domains its application is efficacious, and juxtaposes the strengths, weaknesses, and trade-offs of each technique. An important spin-off of this development would be demonstration of the value of a general theory of systems and its inherent taxonomies since these would map the domains of application of various tools, serve as a valuable 'toolbox' for invention of new tools, and as a theoretical foundation and eventually scientific rationale for what is now a haphazard and fragmented conglomeration of techniques. The disciplines would benefit from a Comparative Systems Analysis because it would map where tools were being used in their specialty, and where they were absent, but could be used.

Do we really need new tools? Many seem alienated and confused at the profusion of tools already available. To the uninitiated the 'tools' for 'simplifying complexity' are already too complex and inhumane themselves.

Obstacle 24: *There is a Need to Make GST More User-Friendly.* This is even a more difficult proposition for GST methodology than it is for tools of systems analysis. A list of isomorphies such as Table 6 may excite the minds of some devotees—the interconnected net of isomorphies of linkage propositions (LPTM) may increase that interest—but to most persons it amounts to too much detail. Yet that detail is exactly what is needed if tangible fulcra are to be found that can move the specialties and help us control the obviously complex phenomena that plague society. Recommendations to overcome this obstacle would include: (i) use of graphic techniques, (ii) use of topological mathematics and modeling, and (iii) use of computers for behind-the-scenes detail, but use of standard techniques to represent the detail in forms compatible with the averaged educated user. It is critically important not to sacrifice the 'span' or 'range of inclusion' (which is also required of GST) in the pursuit of simplification for its own sake, because one is pursuing the detail needed for the high resolution required to deal with complex problems. Nature somehow has succeeded in endless recursions of cycling between the extreme of atomistic detail (particularity), and the opposite extreme of integration of that profusion of particulars into the next level of wholes. GST should do nothing less. Some of these features are exemplified in the case study of work toward a system of systems concepts, the Linkage Proposition Template Model. Another development of promise is the use of expert systems to represent the complexity of various systems analysis applications and various general systems models. By their inclusion of 'inference machines', a 'knowledge base', and 'rules' an expert system could guide a user step-by-step, in a very human way, by a series of easily digested menus, through a maze that would otherwise be intimidating if viewed *in toto*.

4.2. *Case studies: computer based augmentation methods*

Each and every field of applied systems analysis (and there are dozens [106, 125]), deserves its own lengthy paper describing obstacles and potentials in the future. The general systems community, however, is more interested in an overview of the available techniques, such as catalogues and glossaries (see Section 3.1), and the trends that are developing in the entire class. Clearly, one of the trends is toward use of the computer to augment the capabilities of the human mind to encompass and deal with complexity.

One of the areas of potential utility in the future is that of metamethodological studies as described by Klir [50, 42]. The tool that he and colleagues (Cavallo and Higashi) have empirically tested in a lineage of papers makes feasible comparisons of various systems-problem-solving methodologies. The comparison using computer testing algorithms allowed them to develop guidelines for studying the trade-offs encountered in using different alternative tools including discrimination in performance, confidence intervals, applicability, what is to be gained and lost. Although currently tested for delimited classes of problems the long-term purpose of the enterprise is to establish both a theory and praxis of comparative systems analysis at a level above the tools themselves (thus 'meta-'). An important new development would be the joining of independently derived meta-methodological studies such as the Reconstructability Theory of Klir *et al.* [42, 50], and the Linkage Proposition Template Model of Troncale *et al.* as suggested by Orchard [personal communication]. The former approach has a strong computer-theory, probability and 'possibilistic' basis and reduces variety by selection algorithms, while the latter has tighter coupling to recognizable, context-dependent processes in real systems, increases variety and is coupled to a large data base.

Another area of potential for computer-based augmentation methods is the use of expert systems in GST. For example, the Linkage Proposition Template Model is being investigated for use as the rule base for an expert system that 'knows' (can manipulate according to the linkages) the inter-relationships between the isomorphies of Table 6. The feasibility of this approach is under study at the Institute for Advanced Systems Study and at the Department of Medical Cybernetics and Artificial

Intelligence. Interestingly, Klir's group has also predicted the potential of expert systems for advancement of methodology in the field [50].

Other computer-based tools are appearing which might give significant aid to systems-level integration and synthesis. The many relational data base programs now on the market can be used to interrelate data and information. Beyond these commercial programs there are general-systems-oriented tools in various stages of development from proposal initiation to user-oriented testing. These tools specialize in information connection where the connection rationale can be explained, extended to multiple meta-levels and exploited. This feature is required for general systems integration, but is not as fully developed in current relational data bases as needed for systems-level integration. The *Lifework Integrator* (©) [66] has built in provisions for 'flagging' interconnections across many hierarchical levels and among the many subsystems which most professional knowledge-workers study during their lifelong careers. Thus, the *Lifework Integrator* (©) may be used throughout that career to enrich the insights, correlations and cross-fertilizations that sporadically occur to an investigator. The L.I. maintains this growing pool of interconnections over time which gives them the ordering and stability necessary to serve as a foundation for still greater leaps of insight and connection. It also has the potential for integrating group work. By agreeing on a common, detailed, 'integration-outline', a professor, his international network of collaborators, and their students can focus individual efforts on very specific, delegated portions of the outline, yet still maintain and even increase the ability of the group to enrich internal connections within the group... all this while simultaneously maintaining the desirable, but opposing qualities of span and resolution mentioned earlier. This is accomplished using the same system that supports the ever-present conventional tasks of relating new bibliography and results to the growing data base and the preparation of new manuscripts.

Another proposal involves the design of a Matrix Builder (M.M.B.) suggested and discussed during the informal debates of two successive Fuschl meetings [12], and under development by Oren and Troncale. This tool would aid a user in identifying key parameters of a problem using general morphology, then use these to build several axes which describe a multidimensional space. Each axis would represent a distinct and separate, but critically important approach to the problem under study. Comparison between the axes would create many multidimensional 'intersect' spaces just as a two-dimensional matrix creates square intersects and a three-dimensional matrix creates cube intersects. Each multidimensional intersect would contain information on how the various categories of the various taxonomies of parameters (represented by all axes) interact with each other. In this way, each particular of each taxonomy would be systematically compared with each particular of the other categories and taxonomies. Each 'connection' specified in an intersect would be explained in semantic terms, linked to literature or data, and would have the capacity to be 'turned inside out'—that is, each connection would serve as a point of departure for 'tracing' through to other connections. This tool would combine features of expert systems, relational data bases, and some unique, new capabilities into a user-oriented tool.

One use of the M.M.B. might be to detail the immense number of useful interrelationships between the following: a taxonomy of isomorphies on one axis (Table 6), with a detailed taxonomy of tools of systems analysis on a second (Table 7), with a taxonomy of limitations and trade-offs on a third, with a taxonomy of complex societal problems on a fourth [126], with a taxonomy of resource limitations on a fifth. Provided the computational complexity can be overcome, such a tool transforms an otherwise hopelessly complex jumble of information into an overview usable by decision makers. They would only deal with their stated needs and the M.M.B. would allow them to trace across linkages to satisfy those needs without overwhelming them with the unnecessary details. Changing one of the above axes to a 'classification of the problems of developing nations' (Fuschl Two [12]) would alter both the purpose and usage of the M.M.B. but capitalize on the effort expended in constructing all the other axes. Thus, the M.M.B. uses the same strategy that nature apparently used in making the evolution of the cell more speedy and efficient. Functional pieces of molecular genes are interchangeable (like axes here). Once designed they can be recombined with other pre-existing functional pieces to create complexes for new uses (in this example, to create multidimensional matrices for new uses). This same tool could also be used in the manner of Mendeleyeev for detecting where research was needed on isomorphies or tools by searching for gaps in the available information that are exposed when new axes are exchanged for old ones.

The future of general systems methodology appears to possess vigorous potential, although only somewhat fragmented efforts can be detected at present. It is important to recognize that here we are speaking only of general systems approaches as the other end of the spectrum, systems analytical tools are enjoying much more intensive development. For both areas, there are more ideas and work available than there are workers to perform tasks that hold promise and significance for a humanity beset by many complex societal problems that need solutions now, not later.

5. THE FUTURE OF SYSTEMS APPLICATIONS

This area enjoys the most attention by systems researchers of all the areas mentioned to date. This despite the observation that a general theory of systems, in fact, does not exist, nor a consistent, proven methodology, or set of methodologies adequate to the task of application. Why? There are many answers: (i) problems will not wait, (ii) society pays for even attempted solutions to problems, (iii) applications are usually discipline-based so attempts enjoy to some degree the positive version of the seven negative consequences of working in the systems field listed in Section 2.2, (iv) the disciplines usually have not recognized many of the available systems isomorphies or tools so there is room for a cadre of specialized systems analysts to form in each specialty, (v) criticism at general systems conferences is so weak that survival of even poor applications is insured.

On the other hand, those who work in systems applications on the more rigorous systems analysis level, as well as those who do achieve deep insights in their general systems applications, are actors of considerable courage. They face directly in every encounter the coolness of the isolated specialties toward their attempts. It is sad that theory is not yet strong enough to give them the support they need. Still there are some obstacles that, if overcome, would enrich the applications almost everyone agrees the international human situation demands if survival is to be won.

5.1. *Common problems facing applications of systems ideas*

This section emphasizes general systems applications. The application of systems analytical tools is a much larger topic and aspects of it relate more to the last section than this one. Unfortunately, for many investigators there is no distinction between application of a single tool of systems analysis to a problem (which, if it does yield significant results is something for which GST cannot take credit) and a truly general systems application to a problem. The breadth of possible applications of systems ideas is immense by definition. It is also immense in practice as evidenced by the papers at conferences which survey such attempts year after year [54, 111]. Several obstacles appear at the interface between general systems and applied systems applications.

Obstacle 25: There is a Need to Scale Down Promises and Rhetoric in Favor of Demonstrations. Many of the most fierce critics of systems approaches use the device of comparing promises or predictions made on behalf of the fledgling field with its actual accomplishments [15, 17, 44, 58]. In fact, these critics primarily attack the areas of applied systems analysis, which are, if anything, more substantive than attempts at a general theory of systems. If they were more careful with the distinctions along the spectrum of systems approaches they presumably would be even more critical of GST. Rhetoric also has a counterproductive effect on membership growth and retention in the field. Many drawn to it by an initial intellectual excitement are disappointed by the subsequent activities they witness. Why this disparity between potential and realization, and what can be done about it?

The simplest solution would be to establish a tradition in the field that excludes all promises. This is difficult to achieve in practice, however, because the field's proponents are attempting to accomplish the 'bootstrapping' necessary to establish the field for the first time. They feel it is necessary to 'sell' the field in order to overcome the obstacles of institutional and professional roadblocks and the absence of well-established educational programs and employment opportunities. It is important for such well-intentioned individuals to recognize that well-documented demonstrations of the utility of GST are far more effective 'sales' arguments than any promise. There are four additional reasons for this tendency of the field for issuing inflated promises. The subject matter itself is so broad that just discussing it sounds inflated to disciplinarians with much more limited foci and ambitions. Also the workers attracted to the field generally come from the more creative, less boundary-conscious personalities in the population, and inflated promises are natural for them. That is exactly why the field needs to have a strict tradition forbidding promises relative to performance—to counteract this inherent tendency. Another cause of inflated rhetoric comes from the 'aha' effect. Some workers attracted to the field are so delighted to discover in their own minds the potential for broad synthesis across fields at meaningful depth that they become proselytes. They are intellectually 'born again' in modern parlance. Finally, some of the workers are so concerned and impassioned over the complex problems facing humanity that they leap from the potential of the field to hoped-for, but as yet non-existent solutions. They are driven by the tangible feeling that time is running out for solution of the many, interlocked systems-based crises. Despite all of these understandable human motivations, it should be clear that the effort expended in developing real, exemplary demonstrations of utility far outweighs efforts at salesmanship. Perhaps widespread focus on and discussion of these counterproductive tendencies of making promises will suffice to discourage their inflation.

Obstacle 26: *There is a Need to Balance and Couple Basic Research and Applications.* The most significant reason for inflated promises is the shallow nature of many of the applications attempted. A study which uses a small number of isomorphies (often not even isomorphies, merely jargon) to 'solve' or 'interpret' a difficult real-world problem only infuriates reductionist researchers that have labored over the problem in great detail for many decades. Systems-types will immediately counter this observation with the usual argument that the 'problem' itself derives from the systems-level, the 'connections' between elements of the problem, from its large-scale features, and that the reductionist (even the systems analyst) will forever miss the heart of the problem in their studies. That, they say, is why the problem persists. This is a valid response. The difficulty comes in the next step. The general systems worker then views the problem at such an abstract level, with so few new ideas, with so few 'correspondence principles' between those ideas and the real system, with so few new insights and practical prescriptions emerging that he invites scorn.

One recommendation is that more GST effort be allocated to detailed and vigorous basic research to counterbalance the tendency of workers to immediately try applications. How can effective applications of GST exist if there is no consensus in the field on a model general theory? If most workers use only a small portion of the isomorphies in Table 6, and only a small number of the tools in Table 7, how can they expect to be respected for solving complex societal problems? With use of more of these ideas and tools, more detailed prescriptions may be forthcoming.

The work of systems management experts using process-oriented, heuristic tools such as Klir's General Systems Problem Solver or Checkland's systems approach do not fall into this category of criticism [49, 29]. Since neither depends on the special knowledge of isomorphies or systems analytical tools beyond those immediately incorporated into their process, they escape shallowness. By using the inherent knowledge of the problem-experts participating in the learning process (which is part of their methodology), they can sometimes achieve improvement of awareness of the systems dimension, with resulting improvement of the problem. These approaches, however, would also gain a great deal if basic research provided them with more specific guidelines from an enriched special knowledge of isomorphies and their many linkages.

Not only must basic research be intensified, but it must be more closely coupled with both application attempts and with empirical refinement studies. As pointed out by Platt [81], the strongest fields have a tradition and a very systematic practice of close coupling between their theoretical and experimental approaches. Although, systems science resides on a part of the spectrum of 'ways of knowing' [115] that like ecology and sociology may never expect to use experiments in the sense of the hard sciences, it still has much to gain from empirical refinement. Empirical refinement constrains and inspires theory; it is the selective force which thrusts theories into competition, cooperation and evolution. Theory suggests new and fruitful avenues of empirical inquiry; it fuses a hodgepodge of data into understanding of a phenomenon. Experimental refinement and theory require each other for dynamic progress as much as any other coupled set of dualities across natural systems. Robust applications are built on the firm foundation provided by a healthy coupling and frequent interaction between both theory and empirical refinement.

Applications, if carried out in sufficient detail, with sufficient attention to some aspect of measurement or comparison, actually become an important source of empirical refinement in the case of a general theory of systems. Isomorphies must be demonstrated in many disciplines (Obs. 6), exemplars are needed (Obs. 31), operational taxonomies are needed (Obs. 15), and cases that illustrate correspondence between the isomorphies and the data are needed (Obs. 18 and 19). Attempts at truly general-systems-level applications can help fulfil all of these tasks. If a tradition emerges to carry out non-trivial applications in this manner, applications may themselves contribute to basic research and to their own improvement. In order for this to occur, a mechanism must be devised to correlate isomorphies with recognized terms (jargon) of the target disciplines to which the general theory is applied.

Obstacle 27: *There is a Need to Overcome Discipline-Based Focus on Discinyms.* Some isomorphies were given their name by the field that first recognized them. For example, 'feedback' was recognized in engineering systems analysis during the war years, and then in physiology and medicine. Now it is used in many fields. The word is used consistently, with even the many elaborations such as positive feedback, negative feedback, coupled feedback, feedforward, second and third-order feedback used rather consistently. Weiner's term 'cybernetics' is also used consistently to describe the whole set of control isomorphies [135], although it is preempted on the continent, and certain professional groups (e.g. ASC) to represent the entire field of isomorphies typical of general systems research. This class of isomorphic terms with consistent usage do not create obstacles to communication across disciplines as much as the following class.

This second class of isomorphies were independently recognized in different fields at different times without consistent usage across fields. Thus many different disciplinary-based terms exist that name the same isomorphy by focusing on its appearance in a restricted scale of magnitude of natural system. An example would be the use of 'homeostasis' in anatomy for the more general concept of 'dynamic equilibrium'. These diverse *disci*-plinary names for the same process or structure (isomorph) are syno-*nyms* for the general systems term. Combining the syllables we obtain the useful word 'discinym'. A discinym is a word used to describe the specific case of an isomorphic process realized on one scale of reality, but which maps with many other cases, on other scales. Use of the term discinym is not meant to favor the usage of the isomorphic term over the discinym. These terms clearly describe important and unique aspects of process in the phenomena at their level. But open thinking is needed. The disciplines train their students very carefully in the use of these disciplinary-based terms (that is what a discipline is for, at least in part). Consequently, professionals resist recognition of the general, abstract and context-independent aspects of their jargon term often arguing that the newer isomorphic term is just a case of '*x*' in their discipline. This saves them the effort required to transcend their disciplinary focus, world-view, and knowledge to recognize the isomorph. Lists of isomorphies and their corresponding disciplinary-based discinyms are needed to increase awareness of this block to communication and subvert it. These are in preparation [110].

Even with this recognition on the part of specialists, another obstacle would still inhibit work on GST. The components of each isomorphic structure, or the steps in each isomorphic process are so abstracted from the particular manifestation of that process on a particular scale of reality that most minds fail to perceive the connection. Further, the prescriptive value inherent in each isomorph is lost because few correspondence principles [96] exist between the isomorphs, their linkage, and the real parts of the system in the world. If the isomorphic interactions suggest an ideal relationship, few guidelines exist that help workers apply that to the parts of the real system. If a real system is obviously malfunctioning, the diagnosis is poorly aided by a complex set of ideal interactions on the abstract level that have not been translated to the real system. There is almost a complete absence of work on these 'correspondences' or 'rules for deabstraction' in GST applications research. Even basic research attempts have not described adequately the rules for abstraction in the midst of doing it. But, as argued above, well worked examples of 'deabstraction' of many systems isomorphies to a real system have not even been done, much less protocols designed for widespread dissemination of such application attempts.

Obstacle 28: *There is a Need for Rules for Deabstraction, or Protocols for Correspondence.* This obstacle is faced even by reductionist fields. Consider the difficulties faced by the pharmaceutical industry as it tries to 'scale-up' from laboratory to industrial quantities of production of very valuable biologicals like interferon or HTLV-III virus (both newly discovered and to be used in vast quantities for the public good in viral therapy and blood screening). Unless, massive quantities can be produced, there is no commercial benefit. But, to date, the only production has occurred on the basic research level in scientific labs. Another illustration of this same dilemma comes from basic research into proposed 'nuclear winter' scenarios. The White House Office of Science and Technological Policy is studying the feasibility of a five-year, 50 million dollar set of research projects, which, in part, will try to translate lab-sized experiments on parameters of smoke production in fires to mesoscale, and eventually to megascale climactics. On the theoretical level, scaling-up problems appear in the current attempts to fundamentally redesign individual computer systems so that they can act as a unified, parallel-processing, multigroup supersystem.

All of these are vital areas of reductionist, analytically-oriented research involving a great deal of money and human resources. All are characterized as having great potential impacts on the future of society. And all are problems because of the absence of 'scale translation protocols'. General systems research on the applications level could help itself gain acceptance, and would help these areas, if it manages to discover some generalized principles of, or algorithms for scale translations. By its very nature, its work demands establishment of rigorous 'rules for deabstraction' from the highly generalized isomorphies to the real systems targeted for the application. But most GS-applications in the literature do not explicitly identify these or any version of 'correspondence principles' that would be the beginnings of 'scale translation protocols'.

Recommendations for overcoming this obstacle might include: (i) requirements for inclusion of specific sections on this feature in papers submitted to conferences and journals, (ii) studies by philosophically-oriented systems workers on the similarities and dissimilarities between correspondence in the natural sciences versus the systems sciences, (iii) studies by the same group on relationships between deabstraction from proposed general theories of systems to real systems and the distinctions, if any, between this logical process and such standard processes as induction, deduction and abduction, (iv) identification of a single case study of deabstraction and correspondence for an exemplar application, and (v) encouragement of a Special Integration Group or other formal professional stable organization to pursue solution of this critical need.

Obstacle 29: There is Need for Tighter Coupling between Systems Modelers and Decision Makers. Finally, success in the process of systems applications on the theoretical level always faces the difficult hurdle of utilization. Many hundreds of computerized models of parts of natural systems exist in the literature. For example, at just one recent systems meeting [111], which was not on the topic of modeling and simulation at all, computer simulations of the following systems were presented: tumor growth; small group decision making; blood glucose dynamics; nuclear facility siting; renal artificial kidney function; schizophrenic dysfunction; transportation systems; lake ecosystems; hospital information systems; metabolic response to stress; international conflict; market behavior; natural selection; as well as many others. What happens to these models? The experience at IIASA (the International Institute for Applied Systems Analysis) whose task it is to make models of many complex crisis problems facing the international community, has been consistently the same. Even if a small percentage of the available models are sufficiently sophisticated to deserve influencing real world decision making, the decision makers ignore the models, or do not understand the models, or do not trust the results, or cannot find the appropriate models in the maze available, or are overcome by the complexity in the model and its presentation, or are frozen in indecision because of the political, social and economic constraints binding them (which are usually not included in the model). A recent series of papers and conferences is beginning to explore this specific problem. They provide a few recommendations pertinent to the above obstacle (contact Project Outreach, Dr. Jag Maini, International Institute for Applied Systems Analysis, A-2361, Laxenburg, Austria, about the workshop series entitled, 'Dialoguing with Decision Makers').

It is not surprising that general systems modelers have experienced this problem if the far more detailed groups in applied systems analysis (who work closer to the disciplines, real phenomena, and data) have not solved it themselves. The prognosis is not good. The crises, however, will not wait.

5.2. *Case study: one view of the future of modeling and simulation*

Reviews of this area of applied systems analysis are thorough and many [72–75, 144]. This section presents just one idea which may influence the future of this field and which would emerge from the presence of better general theories of systems.

Current simulations arise from the programming tools available (e.g. Simula, Dynamo), from mathematical algorithms available (e.g. linear vs non-linear programming, differential equations), directly from the data available about the real system, directly from understanding a specialty has accumulated about that data, or from pressing needs and malfunctions of the system studied. Notice that virtually all of these sources are bottom-up. The model that results is constrained to a remarkable degree by the limitations inherent in the program used, the mathematics used, the data available, the theory in the field, and the human purpose of the model. It is not a custom of this field to thoroughly expose these limitations or the many hidden assumptions which are promulgated thereby. Yet this is a first and foremost goal of most rigorous fields of study.

Further, the models that result from this rather blind use of available tools and data fall victim to an odd logical disorder which results from overreliance on what usually is an excellent scientific method... Ockham's Razor. William of Ockham formulated the principle that one should not multiply explanations unnecessarily, or include explanations of an untestable nature (e.g. spiritual). Today we interpret this rule as keeping the explanation (or mechanism) as simple as possible. But what many fail to recognize is that this judgement presumes we know enough of the relevant context of the system to know what is appropriate 'simplicity' in its case.

Let me try to illustrate this with a real world example. In molecular biology it was simplest to explain genes as integral (of one piece) and continuous. Recently, empirical research shows that they exist in as many as 52 pieces. Earlier suggestion of an hypothesis including this feature would have been vociferously rejected on the basis of its breaking Ockham's Rule; it would be deemed overly complex and unworkable. Vital pieces of genes could be lost; much energy would be required to select and sew the meaningful pieces together. So continuity of the gene was blithely assumed. But this assumption was wrong. The natural system here was trying to solve a problem man's limited focus was ignoring. The cell had a vested interest in speeding up production of variety and evolution. Genes in pieces may initially require more energy and risk serious mishaps, but they also greatly increase variety by allowing mixing and matching of the pieces (see Section 4.2). There are many similar examples in ecosystems modeling. Simplicity is sometimes invoked as authority only to mask poor recognition of assumptions. How do modelers avoid such errors, which they might as well presume are occurring often?

The answer to both hidden dilemmas cited above is the same. Bottom-up approaches might be used in equal balance with top-down approaches. General systems theories may some day provide an entire class of top-down approaches not yet used. A general theory of systems using 75 isomorphies and hundreds of specific linkage propositions between them (e.g. the LPTM) could be used as still another approach added to the seven sources mentioned above. With its detail it could suggest processes of cycling, feedback, autopoiesis, symmetry, duality, hierarchy, fractal structure, catastrophic discontinuities, phase shifts, field influences, etc. that otherwise would not be included in models because neither the tools, nor the subject field, had yet discovered these pervasive isomorphies in its domain. These 'template' models could be used for comparison and judgement of model 'completeness'. This depends upon the willingness of natural scientists to accept a general theory as our best, current understanding of the 'tried-and-true' patterns of those systems that have successfully survived up to 13 billion years of evolution. These general systems template models that describe such fundamental, and therefore optimal patterns, might also be used in a prescriptive sense to design better large-scale systems. As general theories of systems mature, their utility for top-down approaches may become increasingly important to modelers and decision makers.

5.3. *Case study: systems theory applied to biology*

The biological sciences are in a good position. Not as simple as physical systems, they approach the levels of complexity that social systems exhibit. Still, modern research has been successful in studying biosystems at the more simple, thus empirically-vulnerable levels of biochemistry and biophysics. They have features of both 'worlds'. Therefore, they are a good starting point for general systems research and education as has been pointed out before [64, 92, and Rosen's review in 49].

One could easily argue that the cellular level of bio-organization represents the most complex system organization known to man, even more complex than human systems. The cell has evolved for over 4.5 billion years. It has more components, in higher concentrations, at greater miniaturization, and with more intense interconnections than any social organization yet witnessed. However, biosystems are not as complex as social systems at other levels of bio-organization and in terms of degrees of freedom. Still, biosystems are good exemplars for systems research because they have been successfully studied on many empirical levels, have good correspondence principles with theories, and good tools of measurement. Due to their close apposition to human social systems and the environment, they are intimately involved in virtually all crisis problems facing the human race. For the systems theorist they provide the best of both worlds. Unfortunately, systems ideas are somewhat resisted by the rank and file reductionist biologists (cell and molecular levels) and are only recently being incorporated by the biologists working at larger scalar levels (ecologists and evolutionary biologists); recall Figs. 6 and 7. Still, there are many exciting and fruitful case studies which show the utility of certain isomorphies [2, 35, 36, 47, 60, 67, 102, 127], systems analytical tools [42, 50, 93, 134, 135], modeling and simulation tools [72–75, 111, 144], and tools of theoretical biology [64, 92] to the investigating and understanding of biological phenomena. The reviews of Rosen [in 49] and Troncale [123] survey this topic. It is reasonable to predict that a combination of systems theory and topological mathematics will figure highly in the future of applications of systems theory to biology. Isomorphies such as the 'feedback cluster', and hierarchy theory will also continue to figure prominently in systems applications to biology. Overall, it is a good case study of the potential for fruitful interactions between an established discipline and general systems theory.

6. CASE STUDY OF A TRULY GENERAL SYSTEMS BASED THEORY

Much of the debate concerning the efficacy of GST results from the absence of exemplars in the field. Kuhn included this concept in later developments of his widely cited 'paradigmatic' approach to description of the scientific method [53]. Exemplars are successful applications of the methods and theory of a discipline to one of its problems. They enjoy widespread consensus in the field and illustrate its power. GST needs some clearly demonstrated exemplars of the full-scale theory. Exemplars already exist of the fruitful application of knowledge from some of the first-recognized isomorphies such as feedback. But these exemplars exist only for isomorphies taken alone, and not taken together. Critics of the field do not regard this as evidence of the kind of general theory forseen by founders of the movement. Full-scale exemplars are yet to come. But they clearly will not come unless the confusion between systems analysis, systems theory, and general systems theory is reduced, because no series of attempts at the wrong level of generality can possibly lead to the next level in reasonable time. Vague distinctions between work on isomorphies, methodological tools, and applications also contribute to misdirected effort if a general theory is the goal. A guide to GST research is needed. This guidance could come from a well-defined, and widely-debated set of performance criteria which describe what a full-scale GST would look like [118], and which would make clear the distinctions cited above. Why has such a set of criteria not appeared, or if some have been suggested why are they not widely cited?

To those focused on the particular systems of their discipline (or scale of reality), it is hard to perceive important relationships which are claimed as part of their discipline, and yet part of every other discipline as well. To those focused on general theory, the *faith* that such a class of theories exists becomes the *raison d'être* for the field, obscuring better rationales and the drive to make explicit those criteria which adequately describe the class. There is no direction in a field that has not examined each and every criterium of the set that describes its product until that set is acceptable to a broad consensus of the field.

Obstacle 30: *There is a Need for Detailed Performance Criteria Describing a General Theory of Systems.* Once explicit and widely held criteria appear, the work on an appropriate exemplar can begin. But perhaps we should be Machiavellian in our description of what would be a practical and successful exemplar. Why not link pursuit of exemplars to the problems we are having convincing the disciplines that our theoretical research is utilitarian? Careful study of criteria for a GST indicate that its results should be very useful to disciplinary as well as interdisciplinary and transdisciplinary studies if the many obstacles cited here are overcome.

Obstacle 31: *There is a Need for Exemplars Wherein General Models Suggest Important New Hypotheses to the Disciplines.* The next two sections address both of these obstacles.

6.1. *What is required of a candidate general theory of systems?*

Klir suggests a number of features of a general theory of systems [48]. Troncale suggests a list of performance criteria for a general theory of systems [105, 112, 118]. Together they give the following:

(1) A GST would consist of precisely defined concepts
(2) A GST would be context-independent, invariant across all scales of magnitude, demonstrable in all disciplines
(3) A GST requires use of the full set of isomorphies, that is, the minimal, sufficient, and necessary set (probably large)
(4) A GST requires many, specific linkages between isomorphies
(5) A GST would be unobservable in one discipline
(6) A GST would be unverifiable, unfalsifiable, even unrefinable, in one or a few disciplines
(7) A GST would apply to both descriptional and operational views
(8) A GST would describe both continuous and discrete systems
(9) A GST would be limited in its range of application only by the current state of applied knowledge
(10) A GST would possess built-in rules for deabstraction, scale translation protocols, or correspondence principles
(11) A GST would possess a built-in operational taxonomy
(12) A GST would have isomorphies and linkages that were self-organizing.

It is true that no list of criteria can be complete in itself. The criteria for the criteria must be included until one recognizes an endless recursion, or spiral of ascending, ever more inclusive lists [Pruzan, personal communication]. But that does not diminish the real world utility and necessity of lists of criteria. They can make the vague, precise. They make the hidden, explicit. And the explicit can be openly debated in a group of humans struggling to agree enough that they might communicate efficiently and act together synergistically. Nothing less is acceptable in a research or applications community.

Do any candidate theories exist fulfilling these criteria? Repeatedly, this paper has characterized most GST research as skirting the edges of what it should be attacking directly. Perhaps it is too early to expect projects fulfilling the above criteria. Notable current attempts might include the work of Miller, Klir, and the LPTM cited above, yet all suffer in comparison to this list of criteria.

If a full theory does not yet exist, is there a project that illustrates the potential for a transdiciplinary theory which would have impact on all of the disciplines? Actually, there are a number of investigators trying to formulate a theory of emergence which by its very nature fulfills many of the above features, and holds also the promise of becoming an exemplar that would instruct the disciplines rather than the other way around.

6.2. *Case study: towards a theory of emergence*

In Section 3.3 the beginnings of an attempt at empirical refinement of the isomorphy 'hierarchy theory' was described. A 'meta' hierarchy which spanned all of the disciplines from subatomic particles to cosmic-sized strings of clusters of galaxies was suggested. The properties of this 'broad scale' structure of the universe and its natural systems is clearly transdisciplinary. In fact, here is a case where the hypothesis suggested *cannot even be perceived* within the confines of any known discipline. The form and process under study requires the observer to expand the field of vision to encompass the phenomena. And that phenomenon is context-independent. The tests suggested must also move in the opposite direction from the usual reductionistic approach. Although still empirically based, the tests must be replicated across many levels before they can address the phenomenon. Thus, the test strategy is clearly transdisciplinary. But the data bases enabling the tests are actually disciplinary-based data bases so that the transdisciplinary test strategy is coupled closely to real systems. This insures the eventual construction of correspondences and rules for deabstraction. Since the study examines hierarchical levels, it should also have inherent scale translation protocols—levels are scales.

The critical feature of this broad scale structure is the 'gap' between the levels of organization [113]. First, the levels must be determined non-anthropomorphically. But the real target of the inquiry is the 'gaps' not the levels. Never before have the gaps been quantified, and compared across the broad scale structure of the universe. Initial studies of the 'gaps' revealed the possibility of an unexpected regularity (see Wilson in 133). Although demonstrated only for a special set of systems in a special circumstance, it is possible that the phenomenon exists across all levels. The possibility is so exciting it deserves serious empirical follow-up [108, 114].

Such regularities as continuous hierarchical structure across all scales with each new level leaping a gap of some regularity just do not occur in nature by chance. The conjecture is that a regular 'process' describes the leap from one level to another [107, 117], that this process is the same process for each of the gaps and levels [113], and that this process is based in some way on the interactions between the isomorphies that describe systems at their most fundamental level [105]. The leap across levels bears relationships to such old concepts as 'emergence' (from the founder's work on GST), and such new concepts as discontinuities, autopoiesis, irreversible thermodynamics or order from chaos, and origins of topological form. It is associated with either the appearance of a new mathematical formalism or the fusion of several past formalisms. Finally, since it spans a much greater range of levels in nature than the biological process of evolution, and yet it is clearly distinct from even the suggested processes of macroevolution (punctuated equilibrium type) [109], it would become a truly major advance in science. Just as evolution has influenced many fields since its discovery, so might the theory of emergence.

Several investigators are now exploring this process, including, to my knowledge, Wilson [136], Alvarez [5], Voorhees [129], Winiwarter [139, 140], Auger [8], Czanyi [31], Cainiello [U. of Salerno, Italy], and Troncale. Jantsch [47] was also working on it before his untimely death. It will be a long time before the above is demonstrable to the disciplines, but the potential appears high in these studies for elucidation of a truly general systems exemplar.

7. CONCLUSIONS

Besides the list of 33 linked obstacles listed in the Appendix (and their permutations into lists of criteria for, contexts of, and suggestions for overcoming themselves), I would like to re-emphasize several high potentials of the field as a conclusion. One may witness the following in the next decade: an increasing interest by physical and biological scientists in GST; great contributions by them to new isomorphies; appearance of numerous competing glossaries and introductory texts; definitive progress in hierarchy theory and demonstration of its utility to the disciplines; appearance of a new specialty, comparative systems analysis; advances in meta-methodological studies until they become actually functional tools; sudden coalescence and increases in networking of the global, general systems community; and, slow, but steady increases in rigor in the products of the field.

There are two additional obstacles that should be mentioned. Considering all of the obstacles mentioned, it hardly seems necessary to address the critics of the field. The best critics should always come from within the field. If they do not, something is wrong with the field. But the critics of GST seem to focus only on the founders of the movement and not the most current, and best examples of the work in the field. Given the level of criticism included here, it is imperative that they focus more on what is inhibiting the field rather than the usual broadside and polemical attacks on the hypothesis which is the foundation of the field. That remains, and will for some time remain, an hypothesis. It probably cannot be disproven; it probably cannot be fully proven either. The important observation is this; does the work sponsored by the field lead to useful results? Often, the many unsuccessful attempts to prove a tough, old mathematical conjecture leads to useful mathematics. It should be deemed ridiculous to condemn the attempts at proving a conjecture, and likewise it should be deemed unconstructive to ridicule some intellectuals for having the temerity to suggest an unpopular hypothesis.

Obstacle 32: *There is a Need to Refocus Internal and External Criticism of the Field.* The long list of obstacles might be used by some to discourage others from entering the field. It is my experience that entry is much more inhibited by poor work in the field. The young live on challenges. And we all benefit from that. This field is *all* future. The societies are expanding, the literature is expanding, and the sense of what is needed in the field is becoming more precise. The problems it presents are inherently great theoretical problems and paradoxically, simultaneously, great application's problems. This is a field meant for the young, and it needs them.

Obstacle 33: *There is a Need for Young Leadership.* Few fields offer such an incredible opportunity for pioneering. So much remains to be done, or even correctly begun. GST is an unusual mixture of analysis and synthesis, basic and applied, theoretical and experimental, holism and reductionism, even science and philosophy. It needs the unbiased, fresh minds of the young who are are as yet unconvinced of, or uncommitted to these divisions. GST has even spawned some initial attempts to compare its tenets to Western and Eastern mysticism [26, 120], in a manner that suggests a possible bridge between these two seemingly opposed developments of humankind's culture. Although some will certainly scoff at such primitive beginnings at healing the division between science-technology and human-values, it is abundantly clear to others that something must be done soon about this fractionation of the 'self' of our species, if long-term survival is to be achieved. I began by dedicating this paper to the young. I end by entrusting the future of GST to those minds, young or old, who are still intellectually free enough to accept the challenge.

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APPENDIX

This listing of obstacles cited and explained in the text contains the cross-impacts for each obstacle. All pairwise combinations of obstacles were examined for mutual influences and cases of such influence recorded in each other's listing. Sometimes cross-impacts were one way only. Each set of cross-impacts may be interpreted as the context for or cause of the obstacle, as the prescription list for overcoming the obstacle (if restated in the positive), or as the list of criteria for defining the obstacle.

Obstacle	Essence (The Need.......)	Cross-Impacts
1.	Consensus Glossary	4, 5, 8, 10, 14, 24, 27.
2.	Transcend Internal Conflicts	5, 8, 10, 15, 21, 23, 26, 30.
3.	Long-Term Lineages	7, 8, 11, 12, 13, 20, 30, 33.
4.	Literature Synthesis Mechanisms	1, 5, 6, 7, 8, 9, 10, 11, 13, 14, 15, 16, 19, 20, 21, 22, 26, 27, 28.
5.	Transcend Disciplinary Training	1, 2, 4, 6, 7, 8, 10, 11, 13, 14, 15, 16, 20, 21, 22, 23, 26, 27, 30, 33.
6.	Demonstrate Isomorphy In All Disciplines & Scales	3, 4, 5, 7, 8, 9, 10, 11, 14, 15, 17, 18, 19, 20, 21, 22, 23, 26, 27.
7.	Adequately Transdisciplinary Teams	3, 5, 11, 13, 20, 26, 29.
8.	Counterbalance Fragmentation	1, 2, 3, 4, 5, 6, 7, 10, 11, 13, 21, 23, 26, 27, 29.
9.	Increase Rigor	3, 4, 6, 7, 14, 15, 16, 18, 19, 20, 25, 26, 28, 30.
10.	Consensus-Producing Mechanism	1, 2, 3, 4, 5, 6, 7, 8, 9, 11, 18, 19, 20, 21, 26, 27, 30, 31.
11.	Increased Networking	3, 4, 5, 7, 8, 20, 21, 26, 29, 33.
12.	Performance Criteria for Systems Education Programs	restatement of other 32.
13.	Revolutionary Institutional Arrangements	5, 8, 11, 12, 21, 29.
14.	Use Full, Minimal Set of Isomorphies	1, 4, 5, 6, 7, 8, 10, 15, 16, 17, 18, 19, 27, 30.
15.	An Operational Taxonomy	4, 5, 6, 9, 14, 16, 18, 19, 20, 21, 22, 27, 28, 30.
16.	Linkages Between Isomorphies	4, 5, 14, 15, 17, 18, 19, 20, 30, 31.
17.	Self-Generating Set of Isomorphies	6, 14, 15, 16, 18, 21, 30.
18.	Empirical Refinement	3, 4, 6, 7, 9, 11, 14, 15, 16, 19, 20, 26, 28, 30.
19.	Coupling To Data Bases	3, 4, 6, 7, 9, 14, 15, 16, 18, 20, 26, 27, 28, 30.
20.	Better Specification of Research Method	4, 6, 9, 10, 15, 16, 17, 18, 19, 21, 28, 30.
21.	Better Methods of Integration	2, 3, 4, 5, 6, 7, 8, 9, 10, 13, 14, 15, 16, 20, 26, 27, 28.
22.	Recognition- Tools of Systems Analysis Based on Isomorphs	4, 5, 14, 15, 20, 23, 26, 29, 31.
23.	Resist Overreliance on Tools	2, 4, 5, 7, 9, 14, 15, 16, 20, 22, 23.
24.	GST More User-Friendly	1, 15, 26, 27, 28, 29, 31.
25.	Less Promises and Rhetoric	3, 6, 9, 14, 15, 18, 20, 22, 23, 25, 29, 31.
26.	Couple Basic Research and Applications Research	2, 4, 5, 6, 7, 8, 15, 16, 18, 19, 21, 22, 24, 27, 28, 29, 31.
27.	Overcome Discinyms	1, 4, 5, 6, 7, 8, 10, 14, 15, 19, 21, 24, 26, 28, 31.
28.	Rules for Deabstraction	6, 7, 9, 14, 15, 16, 18, 19, 20, 21, 24, 26, 27, 29, 30, 31.
29.	Couple Decision-Makers and Models	6, 7, 8, 13, 15, 19, 22, 23, 24, 25, 26, 28.
30.	Explicit Performance Criteria for GST	2, 5, 6, 9, 14, 15, 16, 17, 18, 19, 20, 21, 28, 31.
31.	Exemplars	3, 4, 6, 7, 16, 18, 19, 23, 26, 28, 30.
32.	Refocus Internal and External Criticism	5, 6, 8, 9, 14, 15, 16, 18, 19, 20, 23, 24, 26, 28, 30, 31.
33.	Young Leadership	5, 7, 12, 13, 24, 30, 31.

This set of cross-impacts needs to be studied using cluster analysis, critical path method and graph theory when rendered as a connected net. Each cross-impact needs to be described in a sentence to achieve appropriate documentation, but that is beyond the scope of this paper.