SYSTEMS MOVEMENT: AUTOBIOGRAPHICAL REMINISCENCES

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Introduction. I have been invited by George Klir to contribute to this journal "information regarding...thought processes and individual motivations" that animated my career in the systems movement. This invitation was right on target with my wishes for writing this article. At the outset I will tell the reader that **my motivation was to develop a systems science; a science that extended all the way from its foundations through a sufficient number of applications to provide empirical evidence that the science was properly constructed and was very functional; a science that could withstand the most aggressive challenge.** Part of my strategy was to discover any prior scientific work that had relevance and to show its place in the science. The motivation for developing such a science was founded in the belief that many problematic situations could readily be observed in the world, and that a properly constructed and documented systems science would provide the basis for resolving these situations.

I now believe that I have succeeded in accomplishing what I set out to do. I also think that the reader will find this to be a very dubious statement, and that my challenge in this paper is to convince the reader that what I have said is true. The reader may keep this in mind in going through the paper, and may write challenges as the reading progresses. It is my hope that whatever challenges arise in the reading will be met with responses as the reading proceeds. Also, in order to lend credibility to my assertions, I have included numerous pictures of individuals whose work in some way has contributed to the satisfaction of my purpose.

To Write History. The skills needed to write a high-quality article of this type include that of writing history (about myself). **Is there somewhere I could turn to learn more about writing history to try to make this article more accurate and more interesting?** The twentieth-century French author, Michel Foucault, was head of the department of the history of thought at the Collège de France for more than a decade, and wrote widely-acclaimed publications such as *The Archaeology of Knowledge*, *The Order of Things*, and *The Discourse on Language*. Why would I want to look to a historian for help in writing autobiographical reminiscences? Several philosophers, including the great American logician Charles Sanders Peirce, have taken pains to emphasize the fallibility of the human being. Also there is an obvious hazard in writing about an area where there is every reason to be biased, to paint a favorable impression of ones' self. **Mix fallibility with personal bias and you have a formula for a bad story.**

Foucault's View. Shortly before his death, Foucault was interviewed by Paul Rabinow who wrote *The Foucault Reader* that would include the interview records. In an interview reproduced in that book, Foucault stated his opinion of how history should be written. In effect, he partitioned writing of history into parts: the **recordables** consisting of the identification of key actors, events, and dates (those items familiar to the beginning history student); and **two synthesized parts** consisting of (a) the historian's reasoned concept of what **problems** those actors were trying to solve through the events that they brought about and (b) the historian's reasoned concept of what their **motivations** were in choosing those problems to attack. To do these syntheses, Foucault decided that **"problemization"** by the historian was necessary. Problemization requires that the historian construct the problems and motivations. The "thought processes" and "motivations" that George Klir proposed as the centerpiece of this series of articles would best be divined, according to Foucault, from the narrative of the recordables: the actions, the events, the chronology.

Mises' View. A similar perspective has been set forth by the great Austrian economist, Ludwig von Mises. While the style of today in economics is largely positivistic, along the lines set forth by the sociologist Auguste Comte in the eighteenth century in France, and reinforced by the work of the influential English economist William Stanley Jevons in the nineteenth century (both of these actors having enormous influence on what is going on today in academic economics), both Mises and Hayek were confirmed enemies of the narrowly-founded positivistic movement. Hayek amply illustrated his disposition in his book titled: *The Counter-Revolution of Science: Studies on the Abuse of Reason*.

What did Mises bring to the writing of history? Mises chose the term **"thymology"** to refer to a mode of behavior in writing economic history that is severely at odds with the positivistic philosophy that dominates much current thought. (Some writers see thymology as a synonym for praxeology.) As described by Mises,

> "Thymology is on the one hand an offshoot of introspection and on the other a precipitate of historical experience...[and] with historians, of a foreign milieu about which he has learned by studying special sources."

Further, thymology encompasses "knowledge of human valuations and volitions". When writing the history of ones self, **credibility (even to ones self) demands that an effort be made to derive the assessment of valuations and volitions from a demonstrated sequence of environments and events within those environments.**

History in Four Time Phases. It seems likely that the reader will find my article most useful if the thought processes and motivations are made transparent by a report of those events that took place within certain environments, the nature of which inevitably influenced the thought processes and motivations. By reading in this way, the reader cannot help but discern changes in the thought processes and motivations. For ease of reading, the chronology requires some markers in the form of categories. I think it is fair to say that I proceeded in **four career phases (the last, thankfully, incomplete!).** These were marked by changes in thought processes and motivations. While each phase was distinctly different from its predecessor, each of the last three phases owed much to its predecessor and evolved with some continuity from the interface with the predecessor.

History from a Variety of Environments. Before beginning to discuss Phase 1, it seems well to describe the variety of professional environments involved in my career. The beliefs and motivations that arose in my career were strongly dependent on the variety of environments. I was employed in 9 universities for an aggregate period of about 41 years. I was also employed in 7 industrial research settings for an aggregate period of about 11 years. The latter employments ranged from a small electronics firm to a large computer company in the for-profit sector and from two government-sponsored research laboratories to one not-for-profit contract research organization. In retrospect, the question clearly arises as to whether I could not hold a job, or whether there was some force telling me to stay on the move. **My thought processes and motivation were strongly affected by this mix of experiences, which saw me bouncing around from university to university and from university to industrial research. Among the strong interests produced by these movements was an interest in organizations and management of organizations, which became a key part of my work in Phase 3.**

- ***Phase 1. The Seventeen-Year Electrical Engineering Faculty Phase (1948-1965).*** I began my university student experience with little insight into what I might do with my life. In my first two years at the University of Missouri, I developed an interest in mathematics and chemistry within the College of Arts and Sciences. My initial college exposure was interrupted by World War II. After being drafted into the infantry, I was eventually placed in the Army Specialized Training Program where I was able to experience five quarters of education in electrical engineering. Upon returning to campus after the war, I completed both undergraduate degree programs, and continued with a masters' degree in electrical engineering.

The Penn State Connection. Then I took an appointment as a faculty member in electrical engineering at what was called Penn State College. It appeared that my career would be as an electrical engineering faculty member. Given that assumption, the motivation was to excel in the faculty environment. This clearly required a doctorate. I completed the doctorate at Purdue in 1952, working on the synthesis of slowly variable control systems under Dr. John G. Truxal. He was only one year older than me, and had just completed his doctorate under the famed Professor Ernst Guillemin at MIT.

Building a Digital Computer. Returning to Penn State in 1952, I accepted an assignment to manage the development of a digital computer. To help prepare me for this, I was sent to visit the Institute for Advanced Study (Princeton) to speak with John von Neumann. Most of our conversation was about long-range weather forecasting. I was surprised to see, on the computer tour of the Johnniac, that the storage tubes had been positioned almost on the floor where every passerby posed the threat of destruction of information due to vibration. (Later, when the Johnniac became the model for the Illiac, the storage tubes were placed at the top of the computer cabinet where they would be more immune to vibration.) Then I was sent to the Harvard Computation Center to speak with Howard Aiken. There I had a tour of the Mark III (I believe, but I may have the Mark number wrong.)

The Ordnance Research Laboratory Connection. While at Penn State, I held a part time appointment in the Theory Section at the Ordnance Research Laboratory, reporting to Mr. Arthur T. Thompson, who would later be the founding dean of the School of Engineering at Boston University, and one of my lifelong friends. There I wrote one of the first papers ever written on systems engineering, which was distributed through government channels, and gained an understanding of automatic control and of operations in the undersea environment.

The University of Illinois Connection. When the Penn State computer assignment was near completion, I left to take a position at the University of Illinois where I spent two years in the vicinity of (among others) Ross Ashby, Heinz von Foerster, and John Bardeen; names that surely must be known to many readers of this article. Then I was called to Purdue to direct the analog computer laboratory and to serve as associate director of the digital computer laboratory.

Unfortunately both of the people who had hired me for Purdue had gone elsewhere when I arrived, and what had been viewed as commitments had vanished like the wind. Accordingly I left Purdue after one year and moved to the University of Kansas.

The Ramo-Wooldridge Connection. Before moving to Kansas, I was able to spend two summers working in Inglewood, California, in the vicinity of Simon Ramo, a name well-known in systems engineering. The Ramo-Wooldridge company had undertaken to manage the development of intercontinental ballistic missiles. I spent part of one summer there in a systems engineering study and the other summer in studying precision electronic oscillators for space applications. Some of the people that I worked with included Dr. James Burnett, whom I had known at Purdue, and who later became head of Aerospace Corporation, and Dr. Lewis Terman, a son of the creative, one-time President and intellectual builder of Stanford University. These experiences opened new worlds to me that impelled me to consider whether to return to academia or to build a career in working with large systems. At this time, I was not ready to leave academia. I decided that California was not as interesting to me as Kansas at the time (I was a native of Missouri, and my wife Rosamond grew up in Lexington, Missouri, not far from Kansas. We see here the interaction of family origins and career aspirations).

The Sylvania Connection. During the Kansas appointment, I spent two summers in the Boston, Massachusetts vicinity, working at Sylvania Data Systems on the famous Route 128 that circles Boston. Through this experience, I gained a second exposure to the world of industry, and worked with people engaged in experimental electronics.

The Nanosecond Research Environment. At Kansas I taught electrical engineering for about eight years and had the experience of working closely with Norris Nahman and his graduate students. Norris invented, built, and tested (in a Quonset hut) the first superconducting coaxial cable for carrying nanosecond pulses. I had the honor of serving as his dissertation adviser. One of his graduate student associates whom I knew quite well, George Frye, later became a key player in the Tektronix company, and eventually started his own firm which became a big player in the hearing aid market. These experiences deepened my appreciation for design and for experimental studies based in hypotheses that drew on scientific foundations. I was being moved away from the theoretical end of engineering that had been exemplified by Claude Shannon and Norbert Wiener in his studies of non-linear systems and time series analysis.

The Wilcox Electric Connection. In my last year in Lawrence, Kansas, I commuted to Kansas City to work for one year as research director for a small electronics firm engaged in airborne electronics and electronic ground navigation. We designed, manufactured, and sold the first electronically-tuned VHF transceiver for the large jet marketplace (both military and civilian). This transceiver eventually became a part of a remote sensing operation, allowing remote tuning to traverse the electronic VHF spectrum in a programmed way, and transmit scanning information to a distant receiver. We patented the first VHF Omnirange transmitter with no moving parts, using a combination of electronic tuning and function generators to replace rotating mechanical equipment. This equipment served the navigational needs of small airports, where low-power omniranges were functional.

A Change in Motivation. My work up to this point in Phase 1 had finally changed my motivation. Originally I had planned to be an engineering faculty member (and had served that way for about 17 years), as I felt that electrical engineers performed a valuable public service, and that it would be possible in this role to have a family and a satisfying life. **But the excitement of discovery had become more than I could control.** I decided to take a full-time research position in a "systems section" at the Battelle Memorial Institute in Columbus, Ohio. While I would return to academia in an electrical engineering department eight years later as head of the department at the University of Virginia, I would continue there to carry out the research program started at Battelle, which began Phase 2 of my career.

While at Kansas I had been contacted by Harold Chestnut, a pioneer in the study of control systems, to serve with a small group whose goal was to establish a component of IEEE that would specialize in systems and cybernetics. Harold explained that while there were already components in IEEE that dealt with control systems and with systems and circuits, these were far too specialized to serve that audience who wished to help advance the systems movement into larger domains. Along with Harold came Hans Ostreicher from Wright Pattern Air Force Base who represented cybernetics: a study that was just coming into view with writings from Norbert Wiener and Ross Ashby, but which was beginning to focus on the human side of systems. I will not go into detail on my service in this activity, but it clearly played a role in my thinking at the time.

Phase 2. Fourteen Years in Starting a Research Career Path (1966-1980). The Battelle Memorial Institute in Columbus, Ohio, owned almost 300 patents on xerography. The story of how they developed the process that founded the copier revolution is exciting, but cannot be told here (it has been told well in a variety of publications). Its relevance to my story comes about because Battelle owned 22% of the stock in the Xerox Corporation. In 1970 that stock was worth about a quarter of a billion dollars. As I learned after coming there, Battelle intended to use income from this property to start a four-part, in-house-funded research program. The specific goal of one of the four parts was to develop a science to manage large, diverse, difficult programs. Battelle's slogan was "to apply science for the benefit of mankind". This inspirational slogan resonated with my interests. So I was extremely happy when I was tapped by Battelle to head what was called the "Science Base" project in a larger program called "Science and Human Affairs".

The Battelle Connection: The Science and Human Affairs Program. Battelle had both a scientific aim and a business aim. The former was to develop a science to underpin large programs. The latter was to equip Battelle to contract for and lead a variety of large programs with a variety of sponsors. To understand this aim, it will help to know that, at the time, Battelle had four research laboratories, two in the USA and two in Europe. In the Columbus, Ohio, headquarters, about 1500 technical people were employed in its Columbus Laboratories, which had almost the same number of research contracts per year. Clearly this meant that a lot of time had to be spent doing project development and writing proposals. The advantage of being able to take on some larger programs was clear. While I was surprised by these numbers, the surprise was mitigated when I saw the statistics of research projects at the General Motors Research Laboratory. The average number of researchers per project at the time was about 0.8. In other words, research programs carried out in professional research establishments could hardly be thought of as "systems" programs, when fractional people were involved in a very high percentage of projects.

My interest in developing the Science Base was also spurred by the turmoil going on in U. S. cities in the late 1960s and early 1970s. It was amazing how little was known about what to do to help cities constructively to cope with civil rights issues, Vietnam War issues, and other social dissonance all taking place at the same time! **A science base to support systems programs that was not limited to automatic control interests was an aspiration worthy of a researcher.**

Early Thoughts. Worthy, perhaps. But what to do? At the beginning, I reviewed what was going on in the systems movement. Who were the main actors? What had they contributed? How relevant was their work to the contemplated science base? Some of the principal actors are illustrated in **Figure 1.**

Insert Figure 1 about here.

Evidently my task could be limited to building material complementary to what they had already done. This simplified a task that was clearly in need of simplification, but still extraordinarily challenging. Also I reviewed a paper that I had written in the early 1950s on Systems Engineering. It emphasized thinking about the whole situation and what might be observed as a way to proceed. Some of the thoughts that came to mind from that perspective are as follows (and I express them as axioms, to indicate both their fundamental impact on my thought processes and the implicit use of mathematical thought as a basis for my self-discipline):

Axiom 1 (The Collaboration Axiom). Large programs require people working collaboratively. Naturally, if a lot were known about groups working together on large programs, one could merely aggregate that literature and incorporate it into the science base. Can you believe that little was known at the particular time of the beginning of my Phase 2? When I say little, please realize that I am interested in what can be called "actionable research results". It is little help to hear such words as "Of course much more research remains to be done before we can apply these results...". Mayors of burning cities are not very friendly to ambiguous propositions or incomplete constructions. Following the line of thought that urged aggregation of what was known that could be applied, matters related to human behavior seemed to be of considerable importance. (Fortunately the last half of the twentieth century produced much significant research on the behavior of people that relates strongly to their ability to work together effectively on large programs. I have discussed this research in many publications.)

Axiom 2 (The Behavior Axiom). Knowledge about human behavior in working effectively in teams will be critically important to the success of large programs. At Battelle I sat up and funded about 20 people part time known as the "Large City Design Team", comprised of a mix of Battelle people and outside people with reputations in urban environments (e.g., the sociologist who helped plan Columbia, Maryland, and the investor who represented John D. Rockefeller III in financing high-rises). This group met three days a month for a year and a half. **It was both a great success and a great failure.** It was a great failure in that it could not produce anything actionable that treated the city as a system. **It was a great success to learn that research was badly needed to find a process that would enable people who have something to contribute toward resolution of complexity through system design to pool their knowledge and arrive at actionable conclusions. This was a key finding that would animate all of the succeeding research activity.**

Axiom 3 (The Design Axiom). Teams working on difficult issues require collaborative support through strongly-disciplined science-based processes which enable a group of expert individuals to aggregate their individual (and often conflicting) beliefs into actionable designs. A further thought ensued. The scholars who had thought most about thinking were philosophers. Which philosophers could contribute to the ongoing research? It was by sheer accident that the work of the great American philosopher, Charles Sanders Peirce, came into view. This was enabled by a book by the late Professor Goudge of the University of Toronto that summarized and interpreted much of Peirce's work. Then a second book came into view: the amazing and dedicated work by Father I. M. " Joseph" Bochenski on a history of formal logic. And a third book arrived at just the right time: Harary's book on structural models, which tied together Boolean algebra, Boolean matrices, and digraphs. These works collectively fortified belief in a fourth Axiom.

Axiom 4 (The Graphics Axiom). Organized beliefs reflect structure. When numerous ideas are linked together, communication implies structural graphics, as engineers have long recognized intuitively. Why not just transfer engineering graphics into the larger arena and declare victory? Yes, engineers have learned to apply structural graphics in problematic situations. Unfortunately they have not learned how to construct such graphics systematically and how to explain them to others in a clear way. Their "immune systems" exclude cognitive aspects of the structural graphics. In applications, they typically reflect the perceptions of one or two people who arrive at the graphics without any clearly-defined procedures that have scientific bases. They have also traditionally accepted the constraint of small spaces, not being able to reflect the intricacy of communication in the size of displays. Moreover the balance between explicit and implicit in such displays is strongly tilted in favor of the implicit by the constraints of limited space. Future research would establish just how poor communication that mixed structural graphics and prose had become, and how recognition of this had eluded people whose careers required that they produce and propagate such mixtures.

Axiom 5 (The Structure Axiom). It is necessary to develop a systematic means of synthesizing structure of systems in order to gain understanding and shared comprehension. The work of a variety of other authors also contributed to the research in its early stages. In retrospect, it is somewhat remarkable that the annotated research bibliography compiled later (titled the "IASIS File") occupied 88 pages.[1] It became clear from the work of Peirce and others mentioned, and from consulting assistance from Frank Harary and George Klir that the theory of relations would be a critical asset in developing system structure. Following this, another Axiom could be stated.

Axiom 6 (The History Axiom). There exists a historical chain of "thought leaders about thought" extending all the way back to Aristotle, whose work remains to be exploited in developing relational patterns in problematic situations. The development of such patterns could now be seen as a key objective which, if it could be satisfied at all, would demand a mathematical basis.

Mathematics as a Model of Reasoning. Having been a mathematics major as an undergraduate, it is inevitable that I would reflect on what mathematics had to offer to systems thinking, beyond the well-established use of ordinary differential equations to model control systems and partial differential equations to model atmospheric diffusion. The Peirces, Benjamin and Charles, father and son, agreed on the definition of mathematics. It is **"the science of necessary conclusions."** This definition, given credibility in the light of Euclid's formulation of geometry, and of subsequent developments in mathematics, reflects these concepts:

- There is a **core body of axioms**, from which all else flows

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[1] When I retired in the year 2,000, George Mason University requested that my research materials be deposited in their Fenwick Library in what came to be known as the "Warfield Special Collection". As libraries measure such contributions, the collection occupied 93 linear feet, and is still growing. A report called the "IASIS File" can be found there which provides the bibliography mentioned. It was compiled with the help of my wife, Rosamond. This collection is cataloged at this URL: http://www.gmu.edu/library/specialcollections/warfield.htm

- There is a **foundational relationship (implication)** that animates the flow
- The flow is **deductive** in nature, based in **Aristotle's syllogism**
- The flow evolves as theorems and proofs developed in **a pattern of antecedents and succedents**. (This language was set forth by Peter Abélard in the twelfth century, as Bochenski had discovered. It enabled a one-sentence statement to replace the three-sentence statement of the syllogism, as I would later describe in my 2002 book *Understanding Complexity.)*
- The pattern is almost never drawn out for inspection as a whole. It does not fit the standard formulations in the form of books and journals, because it is too large. So it is constrained to be learned sequentially, **even though the underlying pattern almost never takes the form of a linear flow**, necessitating a perpetual backing-and-filling by a reader, posing a significant **cognitive burden** to the learner

Second-Order Thought. What was being studied here could be described as "second-order thought", i.,e., thought about thought itself. Figures 2 and 3 illustrate some of those who were leaders in this area, extending from Aristotle in the early days to George Friedman (who produced " constraint theory" and who studied and reported the principal reasons for failure of large systems) today.

Insert Figures 2 and 3 about here

It seems clear that little could be done to develop a branch of mathematics without a beginning that incorporated a set of axioms. Of course iteration could be done in developing such a set. (As the various forms of set theory and Kenneth Arrow's work illustrate, it is not always easy to find a consistent set or the most suitable set to serve a given purpose). Inability to find a suitable set of axioms from which to develop what Foucault calls "les sciences humaine" might be construed as one of two major de-motivating influences affecting workers in the behavioral sciences; the other being the presumed inability to replicate findings. Both of these influences, I have concluded are specious.

Replicability in Science. Even in the physical sciences, **replicability at the micro level** is clearly impossible, since the same set of molecules will never be present in any two physical or chemical experiments. **Replicability occurs only at the macro level**, hence we are disposed to identify a few key categories that will subsume the lesser concepts. Typically one would require that the methodology be replicated and that the form of analysis of results be replicated. These are macro attributes, and their replication in social systems is demonstrably feasible.

What seems not to have been recognized are the true obstacles to macro replication. The combined impact of a set of six obstacles makes the work difficult: (1) the difficulty in bringing together in the same space at the same time the quality human resources that collectively own the set of beliefs needed to construct structures of the problematic situation; (2) unawareness of the methods that will be replicated, (3) non-recognition of the experienced staff to carry out the methods, (4) absence of the physical infrastructure to support the group work; (5) shortage of talent experienced in analyzing and interpreting the structural results of group work and, finally, (6) absent incentives and passion from managers in organizations to implement what has been learned from a quality, science-based, collaborative research effort. [As I will indicate in this article, I now believe that I have already described fully the resources that are required, the methods to be replicated, the demands on the staff to carry out the processes, the kind of physical infrastructure that is required, and the forms of analysis that apply.]

Digraphs and the Theory of Relations. Once it became clear that Harary had linked Boole's algebra, De Morgan's theory of relations, and an unknown author's invention of the digraph [2] (directed graph), it was clear to me that I could implement the requirement stated in Axiom 5 (foregoing) if I could find a process that would use the aggregated mathematics of Harary and would enable groups to work together to organize their collective thoughts. Harary had done the analysis. I would do the process synthesis.

As Harary showed, there is a one-to-one correspondence between a certain type of binary matrix and a digraph. It was clear from his work that it would be much easier to find a way for a group to fill a binary matrix through voting than to construct a digraph directly. Accordingly I set out to discover such a process. Harary had already clarified the concept that "reachability" on a digraph could be isomorphic to any member of the entire class of transitive relationships. This clarification was sufficient to tie Harary's work directly to De Morgan's. **(De Morgan had noted that the validity of the syllogism depended on whether the relationship involved was transitive. This seems to have escaped scholars of the syllogism for more than 2,000 years. After De Morgan, C. S. Peirce noted the importance of this**

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[2] I have searched intermittently to try to find the origin of the digraph. The earliest one that I have seen appeared in Bertrand Russell's 1919 *Introduction to Mathematical Philosophy.* One can hardly accuse Russell of setting a trend, for he used only one drawing in the entire book, apparently out of necessity to clarify an explanation.

insight.) Hence a second condition that I had to satisfy was to determine relationships that groups could use to construct structural models. Harary had used the term “structural model” to correspond to a digraph. I appended the word “Interpretive” and named the process that I would develop “Interpretive Structural Modeling”, to mean that the structure created would be subject to interpretation based upon the elements, the relationship among the elements, and the digraph-like structure. The latter would be structurally like a digraph, but would have words in boxes in place of the circles, and the directed paths on a structure would represent the transitive relationship used to do the construction of the interpretive structural model..

Interpretive Structural Modeling (ISM). One of the necessary conditions to improve the state of the science of modeling (such as it was) was to enable large numbers of elements to be related systematically. How many elements might be involved? George J. Friedman noted that the equations of physics seldom involve more than six elements. How about starting with an aspiration to involve at least 30 elements? With 30 elements, the binary relational matrix would have 900 entries. I could insert 1's along the main diagonal, but still had to allow 870 binary digits to complete the matrix. How much time would it take for a group to construct such a matrix? And how difficult would it be? Would management support the activity? Each entry in the matrix would correspond to a question that could be answered “ yes” for 1. What if the group didn’t know the answer? **I decided that 0 in the matrix could represent the belief that the relationship didn’t exist or that the voter in the group didn’t know the answer.** This would satisfy two conditions. First, individuals didn’t have to say that they didn’t know; they could just vote “no”, leaving the meaning of the 0 in the matrix ambiguous. Second, every path on the digraph would indicate group majority belief that the relationship was in effect. This meant that the digraph wouldn’t have to be cluttered with representations of “no”. But even then, if it took 870 binary digits, and if each one required one minute to emerge from group deliberation, the time involved to fill the matrix would be 14 hours. I made a judgment that this would not be initially acceptable to managers of potential participants in the matrix-filling group process; maybe later, after some experience were gained, but not initially..

A further difficulty involved in matrix filling was the requirement for consistency in the model. A model that is inconsistent is not likely to be very helpful. But Harary’s equation that showed the necessary conditions for transitivity also represented the necessary condition for consistency; a greatly underemphasized condition in modeling. Suppose each time an entry was made in the matrix, all possible inferences were drawn by a computer, and then entered. This would mean that any opportunity for the group to be intransitive in its voting process would have been eliminated by the program. And it also meant that perhaps **a significant shortening of the time to fill the matrix would be brought about by letting the computer share the work of filling the matrix.**

Still another saving of group time could be gained if the computer were allowed to determine the sequence of questions involved in filling a binary matrix. At any point in the matrix-filling process, Harary's equation could be used to compute the possible inference available from all possible unfilled entries in the matrix. By carrying out this computation, the computer could be provided with a decision rule to determine which question to ask next. Some questions, if given one of the two allowed answers, might enable a lot of inference while, if given the other answer, might provide little or no inference. I decided it would be possible to go too far in saving time. It was not necessary to minimize the time. What was desired was **to maximize the learning that went on during the process**, so that the product would be high in quality. Accordingly I decided to use a maxi-min rule, whereby the computer chose the next question to maximize the minimum inference. This decision tended to offer frequent, but not excessive, inference, meaning that the computer "answered" for the group based on transitivity. With this scheme, the time required to fill a 30 x 30 matrix came down from, e.g., 14 hours to a typical value of 3 hours. Later experience showed that, while the time to fill such a matrix could not be predicted with any accuracy, the matrix-filling time for ISM hardly ever posed any difficulties for groups, mainly because the computer algorithms were sufficiently fast that it appeared to the casual observer that the computer required no time to do the computations. The enthusiasm generated by the learning experienced typically prompted groups to insist on more time if the time allowed was insufficient. This was very satisfying, and lent credence to the philosophy used in the process design.

Axiom 7 (The ISM Software Axiom). In order to carry out the operations needed to structure problematic situations, interactive computer software for developing structural models would be essential. At the present time, many versions and extensions of ISM software have been written. The first version was written at Battelle in 1973 in a Fortran language, and installed on a Control Data Cyber machine. It was expected that it would be used remotely through a telephone connection. The first such computer-assisted, long-distance application was held in Dayton, Ohio. It was led by Brother Raymond

Fitz, a member of the Society of Mary, who had a Ph. D. in electrical engineering from Brooklyn Polytechnic, and who was on leave from the University of Dayton at the Kettering Foundation. He arranged for a group of planners to meet and apply ISM to study the improvement of highway transportation in the Dayton area. Later he would lead other sessions, including one in Kent, Ohio, which was videotaped and stands as an example of the application of ISM to urban budget planning in a budget-deficit situation. Later Dr. Fitz's team would write a new version of the ISM software to be used on a Univac 1100-series mainframe under a subcontract with the University of Virginia, supported by a contract with the Office of Environmental Education through Mr. Walter J. Bogan. (Ultimately Mr. Bogan would be the principal intellectual leader in the new science high school being originated in Newark, New Jersey and Dr. Fitz would become President of the University of Dayton, a position which he held for many years, retiring only recently.)

The Cedar Falls Application. Even before the Dayton application, an application was carried out in Cedar Falls, Iowa, but without telephone connection to Columbus, Ohio. This application was led by Dr. Robert James Waller, then an associate professor in the Business School at the University of Northern Iowa. This session, which was very successful, established priorities for urban projects in the Cedar Falls area. Later Dr. Waller would lead the development of a version of the ISM software. (Still later, he would become a famous author with the publication of his best-selling book *The Bridges of Madison County* and its release as a movie starring Clint Eastwood and Meryl Streep*.)*

The Nominal Group Technique (NGT). Once it was clear from empirical study that the ISM process met the expectations and conditions contained in the Axioms, it became clear also that the ISM process alone was not sufficient to enable groups to work together effectively in problematic situations. Fortunately, the Nominal Group Technique (NGT) appeared in the literature from a group of authors at Kent State University at almost the perfect time from the standpoint of serving as a partner of ISM. This process, with a very sound behavioral design, has since proved to be everything that was desired to complement ISM. **In fact, the two processes taken together are the only methods that are needed to enable a group to move from a point of no integration of their ideas to the point where they have reached a preliminary structural design of a system for resolving the complexity inherent in the problematic situation.**

The Role of Theoretical and Positivist Methods. The theoretical and positivist methods that are often set forth as part of systems science or systems engineering are both a blessing and a curse. They are a blessing in that they often provide the opportunity to develop needed numerical results to complete a structural design developed with the help of ISM and NGT. They are a curse in that they are not well-suited to structuring problematic situations, and are only helpful when the constraints that these methods represent happen to be compatible with the constraints of a particular (local) problematic situation. Such methods may often be **appropriate to the domain of systems science[3], but not to systems science itself**. When needed, they are called into play by what has been discovered in prior work with neutral methods. The bulk of the contributions of the systems pioneers shown in **Figure 1** were typically theoretical or positivistic in nature or both, and were mostly not sufficiently fundamental to provide the kind of material needed for the science base. In retrospect, a principal shortcoming of those contributions lay in the failure to connect them to human behavior. My task was beginning to take recognizable form. Although I did not know it at the time, Phase 3 was about to begin, and would continue for two decades.

The India Connection. Through the work of Dr. Robert W. House, my supervisor at Battelle, acting in his role as an official in the Institute of Electrical and Electronics Engineers, a trip to India was arranged as a guest of the Tata Corporation, via Mr. Faqir Kohli, a graduate of MIT, and a member of the board of directors of Tata Corporation. At the time of this trip, Mr. Kohli was in charge of the Tata Consultancy Services (TCS), largely a computer-oriented company, with about 700 employees (at the present time, it is up to more than 20,000). I was invited to make presentations in Mumbai, Pune, New Delhi, and Bangalore, and was privileged to visit the Taj Mahal. One of the lectures was attended by a young Indian mathematician named G. S. Chandy. I will mention Mr. Chandy again later in this piece, as he is doing some excellent work now as director of a small software firm in India. This trip, along with other travels to other countries, enlarged my horizons, and contributed to my world-view.

The Japan Connection. While working on ISM at Battelle, two visitors came to Battelle and

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[3] The domain of systems science includes the systems science along with what I have called the "Arena", the site of applications. Systems science is viewed as the means of arriving at the overview plan for resolving complexity through the Work Program of Complexity. In carrying out this program, it will often be necessary to import specific methods in order to perform tasks that require computations. The distinction between the generic and the specific is one that I have used for more than decade and I note that Michel Foucault makes the same distinction. The fundamental idea is that the systems science must be a neutral science that is applicable across the board, but which will usually have to be supplemented by experts from the specific sciences or from other areas where relevant experience is found.

were accommodated in Dr. House's Theory Section. One was Professor William ("Bill") Linvill, Head and founder of the Department of Engineering-Economic Systems Ph. D. program at Stanford University. He was taking his sabbatical and was financed in part by the Battelle Science and Human Affairs Program. The other was Koichi Haruna, a young employee of Hitachi, Ltd. Koichi was on leave from Hitachi to spend 6 months with Bill Linvill. Both of them became acquainted with the ISM system. When Koichi returned to Hitachi, he began to work on it right away, and developed applications for Hitachi. I will say more about him later in this piece.

Phase 3. Twenty Years Accruing Empirical Evidence and Developing Components of Systems Science (1980-2000). At the close of Phase 2, seven Axioms had been identified, and some progress had been made in arriving at conclusions. All of the seven Axioms could be integrated as follows: ***we can see from history (both ancient and recent) that relationships among elements are the basis for arriving at necessary structure. We recognize that the resolution of problematic situations requires a group design capability, and demands combined prose-graphics structures, arrived at through collaborative processes which will benefit if behavioral aspects of humans (especially those that are responsible for fallibility) are taken into account. Two methods (NGT and ISM) had been found (one in the literature, one developed in the research program) that, properly facilitated, appeared to be sufficient to carry out the kind of work required. Complexity is a major enemy of progress. The mistaken belief that science which involves human beings as actors cannot be developed by replication, so that only theory should be discovered, was an obstacle that demanded verifiable evidence. Finally, the role of the computer in helping groups had come to the fore, and ISM software would provide a capability never before available to people working together collaboratively.***

Axiom 8 (The Verifiability Axiom). To gain acceptance for the work, it would be necessary to verify the kinds of progress that had been made in the area of methodology, and to establish that the work could apply around the world in many different situations; not being limited to any particular disciplinary domain or to any cultural domain. This Axiom undoubtedly was formulated in the light of the nineteenth-century work of Charles Sanders Peirce. He had been given the assignment to enhance ocean navigation by establishing the variation of gravity. For this purpose, he began to " swing pendulums" at many different locations, gathering data from the pendulum motion to determine how well pendulum motion could be used to verify position data for ships at sea. (Even today there is a well-equipped, ocean-going vessel, which bears his name as a testimonial to his early work.) But he was able to carry out this work as one individual. To gain empirical evidence of the work that had been done, additional requirements could be posed.

Axiom 9 (The Process Leadership Axiom). To do the necessary testing, a cadre of staff at different locations around the world would be required. They would need to know what to do to provide the process leadership needed to involve groups in local designs. This axiom defined a huge challenge. How could individuals be found that would undertake the necessary learning processes? And how could these processes be defined? It turned out that this situation would be resolved by a combination of authorship, laboratory experience, and chance–authorship that produced materials suitable for training, laboratory experience in several settings where group work could be carried out, and chance where individuals identified themselves as being interested in the work and began to apply the materials in various settings and on various topics of their own choosing.

Axiom 10 (The Undiscovered Actors Axiom). There exist, throughout the world, people who want to resolve complexity and are willing to spend considerable time learning how to do so. These may be called the "undiscovered actors" because there is no apparent way to identify them, other than to wait until they surface with requests for information and, in some cases for training. In almost three decades, numerous undiscovered actors have surfaced, and have become involved in process leadership roles, either directly by leading projects, or indirectly by offering relevant course work, or both. **Figures 4, 5, 6, and 7** illustrate some these leaders. They are called "action leaders in Interactive Management". The term action implies that they have become involved in work that yields empirical evidence of the type called for in Axiom 8. The term "Interactive Management" was chosen to represent the action component of the science base, as will be explained later in this paper.

Insert Figures 4, 5, 6, and 7 about here

Axiom 11 (The Behavioral Pathologies Axiom). Human beings are victims of certain behavioral

pathologies that limit or nullify their potential for working together to resolve complexity in situations where they have something to contribute. This Axiom became clear slowly as the second half of the twentieth century brought forth research findings concerning human limitations. It became clear that some limitations were individual, some were associated with small groups, and some were almost inevitable in large organizations. **Figure 8** shows thought leaders on **individual** behavioral pathologies. **Figure 9** shows thought leaders on **group** behavioral pathologies. And **Figure 10** shows thought leaders on **organizational** pathologies.

Insert Figures 8, 9, and 10 about here

Axiom 12 (The Necessary Organization Axiom). For most problematic situations that demand a systems science as a basis, at least one organization will necessarily be involved that provides the resources that are seldom available to the individual process leader. This Axiom caused attention to be directed to a study of organizations. In a typical application, a group would be formed from a larger organization. As the group work proceeded, using NGT followed by ISM, it became very apparent that organizational communication was a severe difficulty. While scholars have often recognized the difficulty in communication, specific evidence has seldom been provided except, perhaps, when some kind of crisis or catastrophe occurred that could be traced directly to communication failures. Because of the insistence in the NGT process that a specific component of time be dedicated to clarification only, any participant or observer who watches the performance of participants could see how severe the communication problem was. A participant would often say, in effect, "this idea that I have produced is obvious to everyone", only to find that it had been completely misinterpreted. Sometimes a participant would produce an idea which he could not explain when asked to do so. It is not far from the truth to say that " people don't know what they are talking about until their statements have been honed through human interactions." Hence, Axiom 13.

Axiom 13 (The Language Axiom.) Much attention must be given to the matter of gaining a common perspective in any local situation, hence the process that is being applied locally must allot significant time to the construction of local language (even though it will be a by-product of the learning process, and not advertised as a main goal). In effect, each small group is a "founder of

discursivity" (a term set forth by Michel Foucault) in a local situation. This means that the concept of language-duality is critical to a scientific base for working with complexity. The processes used must have language neutrality as a major attribute while greatly facilitating the construction of local language that is particular to the problematic situation undergoing description and design.

It is not reasonable to expect that sponsoring organizations will pay to construct local languages, in spite of the necessity to do so. Hence linguistic constructions must be a by-product of the work program aimed at resolving the complexity.

Thought Leaders on Language. The necessity and ability to speak precisely on matters of substantial difficulty have been illustrated or enhanced by thought leaders on language shown in **Figure 11.**

The ideas developed by these thought leaders reinforce my belief that NGT and ISM are sufficient processes to enable the necessary local work to be carried out preparatory to applying any positivist processes that may lie within the domain of a science of systems (but not be part of such a science).

Insert Figure 11 about here

The Center for Interactive Management–1 and 2. Early in the 1970s, Dr. Alexander Christakis worked with Professor Hasan Ozbekhan of a University of Pennsylvania Wharton School program in Social Systems Science to develop a prospectus for the forthcoming Club of Rome. Both of them believed that the Club of Rome would apply broad-brush treatments to world problems. They were greatly disappointed when the Club sponsored a project with Dr. Jay Forrester to strive to apply systems dynamics to world problems, so they resigned from the Club.

At the time a representative of the Battelle Geneva Laboratory was a member of the Club. Through association with Christakis, Battelle became aware of his activity, and he was ultimately hired by Battelle. Earlier Christakis had tried to develop a mathematical social science, under the sponsorship of the Greek architect, Dinos Doxiadis, who was in the business of urban planning and new city design and construction. He had decided it could not be done, but when he observed the ISM work, he became quite excited. His interest grew, and he began to take part in applications in conjunction with Dr. Kazuhiko Kawamura, a control systems engineer at Battelle (who is now a professor at Vanderbilt University)..

The University of Virginia Connection. When Christakis joined the faculty of the University of

Virginia, where I had migrated in 1974, Christakis and I planned a Center for Interactive Management. The Center would aim to provide evidence of efficacy of the research results that had been documented in my 1976 book titled *Societal Systems: Planning, Policy, and Complexity.* The late Dr. John E. Gibson, who was then Dean of the School of Engineering and Applied Science, allowed the design and construction of a special facility, just to carry out the work program as it stood at the time, for sponsors that might be attracted. I had already concluded that a dedicated facility would be required to carry out the work. This viewpoint came from the experience in running several ISM sessions in the late 1970s. These sessions typically involved several television sets wired to a telephone which was wired to a remote computer via the lines of some telephone company. Large cables ran around the room, and participants were frequently likely to trip over them. Moreover the long-distance connection to a host computer was often lost, interrupting the process and wasting group time–a very valuable asset.

The Saudi-Arabian Connection. A most amazing workshop was held in Riyadh, Saudi Arabia, in 1980 in a conference room at the Riyadh Palace Hotel. The sponsor was the Saudi Arabian National Center for Science and Technology, represented by Dr. Hashim Yamani (a Ph. D. in physics from Harvard.) The computer connection was arranged by Dr. Kazuhiko Kawamura who was still employed at Battelle. A telephone line from the Riyadh Palace went down the street to the local offices of the U. S. Treasury. From there, a loaned satellite channel (of many used for transmitting oil and dollar statistics) went to the big dishes in West Virginia. From there another telephone line carried the messages to Washington, D. C., from which still another made the connection to Battelle in Columbus, Ohio, to their Control Data Cyber mainframe computer. We had been assured that the equipment would all be " locked in" for us, so that our work would not be interrupted. While we expected to have quite a few participants, only three Arabs came. This matched the staff of three consisting of Dr. Robert James Waller of the University of Northern Iowa who served as facilitator, Dr. Kazuhiko Kawamura (who operated the computer terminal and took advantage of the shopping experience to buy an Arab costume for himself), and me who helped with various duties, part of which was providing a short course.

The Designed Situation Room. The Center for Interactive Management facility, 1st version, was completed in the School of Engineering and Applied Science at the University of Virginia in 1981 using a room design that I had constructed, with very few departures from my design. The name was chosen to reflect the belief that high-quality management of information would be critical to resolve complexity. Its early clients came from U. S. state and federal forestry resource groups. Another group that became active was the U. S. marine fishery resource groups, which were part of the U. S. National Oceanic and Atmospheric Administration. (Some of the work carried out with these groups was videotaped, and can be seen today at the George Mason University Fenwick Library in Fairfax, Virginia.) This work helped to provide verification along the lines of Axiom 8. One of the things we found in using this facility was that air-conditioning equipment made too much noise, just from blowing air, and made it difficult for participants to hear one another. This was corrected to some extent in the design of later situation rooms. The Center fell victim to high-level institutional disagreements in 1983, and moved to George Mason University (GMU) in 1984, where it resumed its work.

The Burroughs Corporation Connection. Meanwhile, because of the institutional problems at the University of Virginia, I had taken a position with Burroughs Corporation in Detroit, where my assignment was to make "computers for education software" contracts between Burroughs and universities who would develop educational software for Burroughs microprocessors. I was able to get this program approved and to encumber the first year's budget at a portfolio of several universities, but was fortunate to learn of a position opening at George Mason University for a director of an Institute for Information Technology. I was offered and accepted this position in 1984, terminating my one-year stay at the corporate headquarters of the Burroughs Corporation, which had added to my understanding of organizations and what they needed to work with complexity. A new Center for Interactive Management would be part of the new Institute for Information Technology.

The George Mason University (GMU) Connection. Before inaugurating the new Center, a new facility was built to reflect my original design with a few improvements, and it was inaugurated in October of 1995 with Dr. Alexander Christakis as Director, and his associate Dr. David Keever, a Ph. D. in Systems Engineering from the University of Virginia, as Associate Director. This new facility enabled Interactive Management work to be carried out for five years, which furnished many valuable insights into how to make groups successful. IASIS, the parent Institute, also provided opportunities for visiting scholars from several countries, including China, Greece, India, and Mexico. But most importantly, it gave us extensive confidence that the various Axioms described earlier were correct.

The Center for Interactive Management, 2nd version, was terminated in 1990, once again involving matters not related to its work, but rather to high-level institutional actions. The Institute for Information

Technology, which had been the parent organization, had never materialized, due to institutional matters at the state level in Virginia. Hence, while I continued to direct an Institute, the name was changed to the Institute for Advanced Study in the Integrative Sciences (IASIS). This name was jointly chosen by the late Provost, Dr. David King, and myself. Regrettably he fell victim to cancer, and new institutional arrangements followed. As part of these arrangements, IASIS became a part of TIPP, The Institute for Public Policy, which later would evolve into the School of Public Policy. Directed by Dr. Kingsley Haynes, a compatible home was found for IASIS that would persist for the decade (1990-2000).

One of the visitors to the Center, Surinder K. Batra, who stayed six weeks as a representative of the Tata organization in India, returned to India, eventually obtained a doctorate, and started his own company known as the Centre for Interactive Management India.

Design Science and Interactive Management. Late in the 1980s I wrote a book titled *A Science of Generic Design: Managing Complexity Through System Design.* This was a heavy book with about 600 pages, sized 8 ½ by 11 in order to make possible the inclusion of readable graphics. The first edition of this book was published by Axel Duwe's Intersystems publishing operation in California. Regrettably it became necessary to choose another publisher later, which enabled some modest additions, including study questions, to be added. The second edition was published by the Iowa State University Press, thanks to the efforts of Dr. Robert James Waller, who had his earlier essays published there. In the same year, another book was published. I had been working on this book for several years. It was titled *A Handbook of Interactive Management*, and went into great detail concerning the processes, roles, and other factors required to carry out the group processes. Early informal editions of this work were published at George Mason University. However the version that finally reached the Iowa State University Press was a joint effort with my long-time colleague, Prof. Dr. Alda Roxana Cárdenas of the Instituto Tecnologico y de Estudios Superiores de Monterrey (ITESM) who came to George Mason University to spend her sabbatical period studying Interactive Management and Generic Design Science. Her stay was a key component of a Mexico connection which began in Budapest, and which continues to this day and has been very professionally rewarding.

Roxana had first encountered the ideas involved when she met Dr. Alexander Christakis at a systems society meeting in Budapest. My wife Rose and I were riding on a tour bus and notice the two of them sitting on an outdoor bench having a conversation, and wondered what it was about. We would learn later that this was the beginning event in what would become a long-standing and very valuable interaction with quite a few Mexicans. Roxana was accomplished in both English and French, since she had studied economic systems at Aix-en-Provence some years earlier as part of a faculty development program of ITESM. She revised the manuscript of the *Handbook of Interactive Management* completely and made it much more readable. Accordingly, she was named as co-author—a title that she had not asked for, but richly deserved. She had already been teaching generic design science in Mexico by satellite to students and faculty in many of the 28 branch campuses of ITESM.

Windows™-Based Interactive Management (IM) Software. Professor Benjamin Broome of the Communications Department at George Mason University had become aware of the Center for Interactive Management. He established a connection with the GMU Center early in its existence, and began to learn how to facilitate using the two main processes, NGT and ISM. Because he was quite knowledgeable of computers and software, he would eventually help oversee the writing (mainly by Mr Dangsheng ("Daniel") Ma) of a Windows™ version of software for ISM that incorporated the primary elements of NGT. While the original software had been written for a mainframe, due to the need for speedy computations, improved versions of the original mainframe software had been written since the first version appeared at Battelle. The improved versions used the "Bordering Algorithm" in which the binary matrix evolved from 2 x 2 to 3 x 3 to 4 x 4 , etc. This plan fit in comfortably with Harary's Algorithm. One of the locations where new software was written was the University of Dayton, where Dr. Raymond Fitz led the development. This project was sponsored as part of a multiple university team, with funds from the United States Department of Environmental Education, supplied with great insight by Mr. Walter J. Bogan, Jr., who was its Director. By the time 1984 rolled around, the time had come to do a PC version. Dr. David Keever developed this version as an extension of the mainframe version. For the first time, the Interactive Management work, using NGT and ISM, was divorced from a mainframe and telephone connections. This greatly increased the flexibility of the work.

The English (City University) Connection. With the Center just beginning at the University of Virginia, F. Ross Janes took a Fulbright sabbatical from his position with the Systems Science Department at City University in London, and spent a year at the new Center in order to learn the processes, which he intended to apply upon his return to City. Ross was very helpful in getting the Center started, and carried out some projects there. Later, following his receipt of his doctorate on the subject of ISM and application, he served as dissertation adviser to both Henry Alberts and Roxana Cárdenas. Regrettably that department was discontinued upon the retirement of its former head and the ascendency of a new Dean. The faculty were dispersed to other parts of City. This phenomenon of the " disappearing systems department" came to be very common, a similar fate befalling the very successful Social Systems Science Department at the University of Pennsylvania Wharton School, coinciding with the retirement of Dr. Russell Ackoff.

The Ford Motor Company Connection. At about the time the Center at GMU was terminated, the Ford Motor Company Research Laboratory was beginning to look for a scientific base for system design. Dr. Scott Staley, who was a group leader there, contacted me at GMU to find out what was happening. By that time, the book on Generic Design was in the hands of the first publisher.

Quality Function Deployment (QFD). As it turned out, Ford had been using something that had become known as "Quality Function Deployment)". This process was described in the *Harvard Business Review* (HBR) in 1987 in a paper jointly authored by faculty of Harvard and MIT. I was surprised when people pointed out to me that what was described there was essentially the same system that had been published by J. D. Hill of Battelle and myself in an IEEE journal 15 years earlier, under the heading "Unified Program Planning" (UPP). This system consisted of a set of matrices that were linked to each other. Some of these matrices were triangular, and others were rectangular. Collectively they formed a feedback system with many interrelationships. The IEEE paper that Hill and I had published had even showed an insert drawing of an extended set of such matrices linked together in a feedback loop and applied to U.S. Air Force planning for a VSTOL aircraft. The HBR paper attributed the QFD process to Mitsubishi Shipyards. Under the influence of the HBR paper, Ford had committed significant resources to program the QFD process, which required construction of large matrices by hand, using inputs from various engineering staff. I saw immediately and was able easily to convince Dr. Staley that the Interactive Management system was far superior to the QFD system. In fact, the early work on ISM as opposed to UPP was initiated in recognition that there was no obvious way to get consistency in filling large matrices without using Harary's key theorem that became the basis for ISM, and which I have credited in many papers written since that time.

As the use of Interactive Management spread within Ford, members of Ford's international community began to take part in some of the sessions. Included in some of these were Owen Berkeley-Hill and Roy Smith from Ford's English operations. Both became supporters of Interactive Management. Roy took advantage of Ford's liberal policy of allowing employees to engage in community activities as part of their Ford service. Roy ran several sessions, including one to explore declining attendance in the Catholic Church in England. He also lectured on Interactive Management at City University. Now retired, he continues to apply Interactive Management from time to time.

The Defense Systems Management College (DSMC) Connection. A gradual buildup of sponsored work for IASIS with Ford came about. This occurred with some overlap with another significant effort involving Interactive Management that went on at the Defense Systems Management College (DSMC). Located at Fort Belvoir, Virginia, this College had been established around 1960 at the request of Dr. David Packard, a co-founder of the Hewlett-Packard Company, who had accepted temporarily a high position in the US Department of Defense, where major shortcomings in understanding of systems had become apparent. One of the faculty at DSMC (among several) who became interested in Interactive Management and Generic Design Science was Professor Henry C. Alberts. At the beginning of this interest we had run one session at George Mason University involving Henry and his colleagues. About a year later, an agreement was signed where I would offer some short courses at DSMC to familiarize people there with my work. At about this time there was a major upheaval going on because of what had become to be symbolized as the "800-dollar toilet seats" bought by the Defense Department; namely very high-priced acquisitions. Henry took it upon himself to start working to change the acquisition system. In this activity, which ultimately required five years of work using Interactive Management with more than 300 program managers, Henry was supported by several of the leaders at DSMC, mostly at the level of Provost or the Commandant of the College. As a result of that, Congress passed, in 1994, something called " The Federal Acquisition Streamlining Act of 1994". There was no clear trail appearing between IASIS, Henry Alberts, DSMC, and the Congress. But nonetheless, this is the trail that actually produced that Act.

As part of ongoing contracts, DSMC and Ford provided some support to construct the Windows™

version of the Interactive Management software. The detailed work in software development was shared by several graduate students supported by the IASIS Contracts, but the outstanding User Guide was written by Professor Benjamin Broome, also supported by IASIS contracts.

The Mexico Connection. In the meantime, developments were proceeding apace in Mexico. I had offered a short course in Monterrey on "The Mathematics of Modeling"; a topic I would later change to "The Mathematics of Structure". Attending this course were two from the USA, Dr. Scott M. Staley of the Ford Research Laboratories and Walter J. Bogan, Jr., who had been so instrumental in sponsoring my developmental work on ISM software and applications at the University of Virginia. Also attending were ITESM faculty from several campuses. From the Monterrey Campus there was Roxana Cárdenas, who had initiated my short course, and her friends and colleagues Carmen Moreno and Mary Carmen Temblador. From the Léon Campus (in the state of Guanajuato) there were Carlos Flores and Reynaldo Treviño. Shortly after this one-week course was finished, Roxana and Carmen gave a second course in Léon at the request of Carlos and Reynaldo and their associates.

Carlos, Reynaldo, and other colleagues at ITESM Campus Léon then began to run sessions with various sectors in the Mexican State of Guanajuato. Along with the Governor's office, they had initiated a comprehensive planning activity for the State in particular, and Mexico in general. This extensive planning continued with what was dubbed the "first Interloquium" in which several hundred people gathered to listen to speakers talk about the future of the world, and to observe Interactive Management sessions on the stage of the State Auditorium of Guanajuato. This remarkable event was followed some years later with a still more remarkable event. The former Governor of Guanajuato was elected President of Mexico. Earlier Carlos and Reynaldo had left ITESM to become part of the State government. At this writing, they are both prominent members of the federal government of Mexico. From their location in Mexico City, they continue to carry out Interactive Management work involving democratization of Mexico through participative processes.

The African Connections. Interactive Management sessions were held in three African nations: Ghana, Liberia, and South Africa. Two were held in Liberia, both being conducted by Carol Jeffrey. Carol learned the subject at the Defense Systems Management College. When her husband accepted a position as president of a bank in Monrovia, Carol chose to become active in local situations. She conducted an Interactive Management session with Liberian women, in preparation for the international meeting of women in Beijing, on the topic of the status of women in Liberia. Later she conducted a session on disarmament and demobilization with warlords and warriors, which culminated in a respite from the hostilities that had plagued the country for years. Later one was held in Ghana which I planned in collaboration with Dr. Moses N. B. Ayiku, and which was facilitated by Carol Jeffrey, Roxana Cárdenas, and Ghanains who were being trained to carry out this work. This session had to do with the reorganization of the Centre for Scientific and Industrial Research (CSIR) in Ghana. It seems that researchers in the universities in Ghana had been supported 100% (though not lavishly) by the Government, which had grown tired of the failure to see any return for the nation. Accordingly the Government had warned that within three years only 70% subsidy would be available, and researchers would have to find clients to serve to earn the remaining 30% of their income.

The application in South Africa was held at the university in Cape Town on the subject of how to organize the publishing industry in South Africa. This particular application suffered from poor facilities planning, being held in a hotel room with abundant vehicular traffic just outside the window, and with a very long table so participants couldn't hear each other. The insensitivity of planners to the requirements for effective communication was never more clear than in this application which, fortunately, was able to move to a better room for the second and final day.

The Verifiability Axiom (Axiom 8) Revisited. The reader may have noted the importance attached to Axiom 8, and to the documentation above of a variety of connections that ultimately provided excellent evidence of what Axiom 8 described as essential. With all of the activity that had grown up (much of which has not been mentioned here, but is documented elsewhere), I confidently assumed that the work would be recognized and become a part of curricula in higher education. It did so in Mexico, and to some extent in other countries and has footholds in both India and China. But in the United States it is best described as neglected. Universities in the USA have their own tracks and are generally determined to follow them. These tracks have been found to be functional in terms of tenure and promotion, where they rely almost entirely on individual activity as opposed to group activity. Even in a few US universities where group activity got a foothold in educational programs, the extensive documentation hopefully foreseen in Axiom 8 drew little or no attention. Cognitive burden is almost uniformly neglected and the importance of software for structuring goes largely unrecognized.

A great deal of emphasis had been placed on the resolution of complexity in the numerous publications that my colleagues and I produced. But complexity, as a subject of research and writing, starting to appear in quantity in the late 1980s. My computerized search on " complexity" in 1972 (at the Ohio State University) had generated a grand total of two articles, both by John Kemeny. But now, people who were well-connected at high levels or who were independently wealthy and looking for something to do were able to gain support and following. A recent search on " complexity" via the Google system produced 279,334 articles on complexity and 4,900,001 on systems. My work was generally not even acknowledged by those engaged in these activities. This was very disappointing, and it showed that my Axiom set was hardly complete, and severely overlooked some important considerations.

I felt that I had failed significantly to gain acceptance of ideas and methods that were very sorely needed in society. There could be no doubt that there were many local situations that would benefit greatly from the use of Interactive Management. Even in Ford Motor Company, where a whole series of successes had been achieved (and which continue to this day), acceptance has been very strong among middle managers, but formally absent in top management. This does not mean that top management did not become aware of what was happening with Interactive Management. On the contrary, a certain pattern began to manifest itself. Whenever an issue arose at corporate level that could not be dealt with, a request came down the bureaucracy to carry out Interactive Management work. While Ford had provided funds to support a situation room along the lines designed first for the Center at the University of Virginia, it would not provide corporate funding on anything but a piecemeal basis for Interactive Management. This was all the more troublesome, since Ford established a vice presidency for process leadership, while ignoring the process that was demonstrably providing great benefits to the company.

Even the US Congress, which had passed legislation in 1994 to correct defects in acquisition management was now backtracking in ways that showed that it did not understand the problems of acquisition management, and was ready to respond to lobbyists whose firms did not enjoy the changes that had been made in acquisition practices. Attendees at congressional hearings reported to me that Senators Roth and Thurmond had made particularly unresponsive comments in relation to the changes that had been worked out over a five-year period involving more than 300 program managers in redesigning the system.

A New Challenge. A new challenge faced me. Where had I overlooked important matters, and what could I do to correct this situation? At this point in my career, I felt that the axiomatic base represented by the twelve Axioms set forth here could now be taken not merely as assumptions upon which further actions would be based, but rather as proved ideas, founded in excellent science, that warranted broad acceptance. But clearly that set was not adequate. It was now time to reconsider.

The China Connection. In the late 1990s, China became very interested in the subject of complexity. As part of my thinking on organizational issues, I developed what I called "Killer Assumptions". These Assumptions had two key properties: (1) they were often valid in normal circumstances and (2) they were usually invalid when complexity was involved. When I was invited to China to give a plenary address, I chose the subject of the Killer Assumptions for the talk. Also speaking at the talk was Cheng Siwei, director of the management department of the Chinese National Science Foundation. This remarkable man apparently liked the talk, because he and Dean M. C. Jackson of the University of Hull jointly invited me to present again in Shanghai at the ISSS Conference in 2002. But before that took place, I had a guest at IASIS in George Mason University. He was Dr. Xuefeng Song, who at the time was the Dean of the Graduate School at the Chinese University of Mining and Technology (CUMT). His six-month stay was sponsored through Cheng Siwei. Xuefeng had already become familiar with the positivist trend in the study of complexity, and must have found the behavioral side somewhat disconcerting at first. Anyway this very bright and personable young man returned to China after his 6-month sabbatical and became Dean of the School of Business at CUMT. Some of his students are now translating my book on Generic Design Science. By working with him I was able to gain additional insight into the organizational nature of complexity. And soon another opportunity arose.

The India Connection Revisited. For some years, Mr. G. S. Chandy (mentioned earlier) had been considering the possible use of ISM in managing large companies. In the late 1990s, he started a small company to create what he called the "One-Page Management System" and "an operating system for the human mind". This concept used an extensive set of "options fields" for different parts of an organization, all reachable from a central point, the "one page". It is now in the final stages of its testing, and may soon be evaluated in a large-scale test in India.

The Japan Connection Revisited. I was pleased to learn that my friend from HItachi, Koichi Haruna, of several decades had risen through the ranks to become the head of Hitachi's three systems laboratories. Now in virtual retirement, he continues to do research to expand the horizons and management methods of Hitachi.

The Mexico Connection Revisited. Many of the Mexicans who learned and applied Interactive Management are either in high-level positions in the government of Mexico, or have been promoted into the higher echelons of their universities, or have started their own consulting businesses.

The Study of Management Beliefs in the Killer Assumptions. Until the late 1990s, the idea that managers believed the Killer Assumptions lacked empirical evidence. Now an opportunity arose to gather empirical evidence. Professor George H. "Tony" Perino at DSMC became interested in collecting data on two aspects of my work. We had already come to suspect that people lacked the skills to read the graphics that they developed.

***Testing Structural Reading Skills*.** Tony tested this idea with more than a hundred managers or aspiring managers to see how well they could gain insights by examining a structure related to the manufacturing of pumps. He made the startling discovery that the managers' performance was poorer than what would be expected from a random respondent. In retrospect, this was explained by the fact that many of the managers were educated as mechanical engineers, and responded to the questions based on their experience instead of on what the structure revealed. (Tony chose part of a structure that appeared in *The Science of Generic Design*. In the situation represented there, a group of engineers used the results of ISM structuring to study a five-factor influence group on pump manufacturing. This work with a set of five, instead of an individual cause, produced excellent results. But alas, the managers who were tested behaved more like the engineers working fruitlessly with the pump manufacture until they applied the ISM process with the help of Dr. Robert James Waller!!!). After those results were obtained, Tony tested the beliefs of a sizeable group of individual managers and also of faculty colleagues at DSMC concerning the Killer Assumptions versus what I had identified as the "Demands of Complexity". The results were reported in his doctoral dissertation in the School of Public Policy at GMU and, later, in part, in the *Acquisition Review* (from the Defense Acquisition University).

Manager and Faculty Beliefs in the Killer Assumptions. A great deal of data was reported by Professor Perino. It is not appropriate here to repeat all the data or even a significant amount. But suffice it to say that several of the key Killer Assumptions enjoyed considerable currency among both groups tested. On the other hand, it was encouraging to note that, for every one of the Killer Assumptions, the faculty group's support was less than that of the managers.

- ***Phase 4. Three Years of Aggregation and Reorganization: The Emergence of Systems Science.*** The applications work done in Phase 3 furnished significant insights into human behavior when faced with complexity. Data had been accumulated showing what I called "Spreadthink". This readily reproducible phenomenon could be seen, for example, in voting results obtained each time the Nominal Group Technique was used. As a published article explained, it is a mark of the presence of complexity that every individual who knows something about a problematic situation sees it quite differently. The correlation between what any two individuals believes to be most important is extremely small. This finding is of great importance to managers for two reasons. First, the manager is also a victim of this particular behavioral pathology, and should not anticipate ready acceptance of the manager's point of view in any problematic situation. Second, the manager should realize that a major challenge to be faced is that of bringing any measure of consensus to a group of partially-informed individuals who might be expected to form a work team. Spreadthink assumed a place in the panoply of other pathologies such as Janis' groupthink and Miller's "magical number seven".

The Greek Connection. The Spreadthink (group) pathology needs to be seen in the light of another discovery that was made. Through the connection with Dr. Alexander Christakis, one of the visitors to the GMU Center for Interactive Management was Iannis Kapelouzos, a social scientist. He spent a sabbatical leave there from his position on a supreme court in Greece. He undertook to study the correlation between points of view held by participant groups before and after the application of ISM. Much to my amazement, and I suppose the amazement of everyone who had been associated with Interactive Management, **he found zero correlation between beliefs before and after application of ISM**. Since ISM had been developed as a group learning tool, this finding was not only amazing but also very gratifying. Kapelouzos concluded that there could only be one explanation of this effect: the ISM process enabled the participants to learn from one another, and their views changed as a result of this learning.

Please recall that whenever a relationship appears graphically on a structure, it is a result of voting in the ISM process as to whether a relationship exists. This means that any structure developed by a group with ISM necessarily portrays the majority viewpoint in every detail. No relationship among elements is shown there unless it has received at least a majority vote from the participants. All of the elements being related have been subjected to an extensive period of clarification by group discussion.

Laws of Complexity. This situation and others that had been observed along the way led me to construct and publish **"20 Laws of Complexity"**. Seen in aggregate, these furnished a kind of composite that an individual could use to assess the merits of what had been discovered. I viewed them as critical components of a science of complexity, and trusted again that their statement would help gain acceptance of the work.

Metrics of Complexity. Those who write about systems tend, for the most part, to be mathematically and numerically inclined. I felt that this group would be more likely to study and accept the work if it were possible to define and obtain numerical values for metrics of complexity. I developed two types of metric. One type assigned numerical measures to each of the problems that had been defined by the group as among the most important during the NGT process. The other type assigned numerical measures to the structures developed during the use of ISM. My colleague, Dr. Scott Staley of Ford, computed numerical values for quite a few of the sessions run there, published the results, and later noted that by comparing alternative designs for a product information management system, Ford had decided to choose a system architecture with the minimum "Situational Complexity Index" for its corporate-wide use. As far as is known, other application groups have taken little note of these metrics, probably because they had already determined that the processes were successful and needed no numerical reinforcement. On the other hand, those who had not tried the processes apparently had either not become aware of the metrics, or had not valued them significantly.

Organizational Factors. Participative processes must be threatening to powerful executives. They are not accustomed to accepting painstakingly worked out plans for resolving complexity. This view is most sharply seen in the actions of a certain manager. He had already decided upon a course of action. When the participant group proposed a different course of action he asked to know how they had arrived at their conclusion. After some discussion, he made this classic remark: "I disagree with and will not accept your conclusion, but I admire the way in which you reached it." A year later he asked the group to review their work and come in with a new recommendation. They made the same recommendation again.

While I recognized early in the work, thanks to writings of scholars like Argyris and Downs, that organizational factors would be very important in getting acceptance of my work, I constantly underestimated and did not plan for the negative aspects of the great strength of the higher levels of power structures in organizations. When I see what appear to be very poor, self-serving, or illegal decisions being made, I often remember a remark by my one-time-research sponsor, Walter J. Bogan. Walter said "there is no law against stupidity". In many situations, it is virtually impossible to make a distinction between stupidity and an illegal intent as the motivating force in a particularly bad decision. I had always felt that if a well-documented, successful process could become the source of key decisions, it would become much easier to determine whether managers were not very bright, or whether they were operating on personal agendas which negated the results of quality group work.

Any high-level organizational decision often has impacts extending outward in the organization or even beyond it into other organizations. Once this situation is taken seriously, there is a virtual torrent of possible explanations for bad decisions, recognizing that no one has the insight required to determine all of the ramifications and consequences of a particular decision. Whether it would help to have an organizational model is debatable. But I have concluded that the tried-and-true organizational model known as the "organization chart" has been extended far beyond its useful range. For many purposes, a virtual chart will be much better than the actual chart and, if it is not adequate, one can always supplement it with the actual chart.

The Coherent (Virtual) Organization. The chosen virtual organization is a three-level organization. It becomes coherent when people in the three levels of the organization are, as the saying now goes, "on the same page".

In the Vertically-Coherent (VC) Organization, everyone in the organization shares a certain awareness of what is going on, to the extent that decisions are not made blindly, but with substantial insight into the likely effects of the decisions. Since the concept of VC Organization is fundamental to the resolution of complexity it will be helpful, when discussing it, to have a model of the organization in mind. For this purpose it will be assumed that the organization has three levels: the **strategic- or topmost level**, with the fewest membership of any of the levels; the **tactical- or middle level**, with a larger membership; and the **producing- or lowest level**, with much larger membership than either of the other two levels. For convenience in discussing this three-level organization, it will be assumed that everyone is a manager.

There are first-level (strategic) managers; second-level (tactical) managers, and third-level (operational) managers. These distinctions enable us to discuss responsibility for taking actions, and to suggest an allocation of authority, hence responsibility among the three levels. It will be assumed also that there is some oversight body to which the first-level managers report, called the "Board".

The Alberts Pattern (Alberts, 1995). In his work using the Interactive Management system to lead the redesign of the U. S. Defense Acquisition System, **Henry Alberts**' groups identified 678 problems (intermittently, over a five-year period). These were placed into 20 categories. The categories were placed into 6 areas. If we call the lowest level in the hierarchy the Producing Level, the middle level the Tactical Level, and the top level the Strategic Level, we see that the existence of this 3-level structure provides the basis for vertical coherence in the organization. This 3-level pattern is called the "**Alberts** Vertical Coherence Pattern". It is illustrated in **Figure 12.**

Insert Figures 12 and 13 about here

The **first-level managers**, with six areas to deal with, can see from the details contained in that pattern how the 20 categories of problems reside within the six areas, and which categories are part of more than one area. This enables effective communication among the first-level managers, and between the first-level and second-level managers. The **second-level managers**, in turn, can see which of the 678 problems lie within their unique oversight and which lie in more than one category. As problems are solved, the descriptive structures are modified, enabling a constant up-to-date portrait of what progress is being made in enhancing the ability of the organization to perform its duties effectively and efficiently. The **third-level managers**, who are intimately involved at any one time in resolving some of the 678 problems, are able to convey to the higher levels what they perceive as essential to resolving those problems; and the higher levels are able to perceive the potential impacts of high-level decisions that might be made, or the potential impacts of decisions that might be promoted or made by their Board[4]. An organization so equipped is in a position to be a VC-Organization.

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[4] One should not suppose that what has just been described was only an academic study. The work described here became the basis for legislation by the U. S. Congress, known as "The Federal Acquisition Streamlining Act of 1994".

The Cárdenas-Rivas Pattern (1995). Working independently, but using the same Interactive Management system, researchers found a similar pattern to represent redesign options for a systems engineering curriculum at ITESM (The "Monterrey Institute of Technology") in Monterrey, Mexico (**Cárdenas and Rivas, 1995**). That pattern is structurally very similar to the **Alberts** Pattern. The **Cárdenas-Rivas** Pattern is shown in **Figure 13.**

The lowest level in the 3-level hierarchy consists of 270 design options. These appear in 20 categories (the same number as in the **Alberts** Pattern!). The categories appear in four areas.

The work cited shows that these "coherence patterns" can be developed with the support of the IM system. The development of the **Alberts** pattern took place over an elapsed five year period. The development of the **Cárdenas-Rivas** pattern required less than an elapsed year. I speak of "elapsed" time, because all of this work was intermittent with a sequence of Interactive Management activity interspersed with regular work assignments of all the actors that were involved.

As in many instances, these empirical discoveries stimulated the development of a generic idea; in this instance, the Vertically-Coherent Organization. The principal benefit of this probably is that it provided a discursivity component that enabled the work of groups to be discussed in organizational settings without worrying about or incorporating specific organizational details unless and until they were needed as part of a broader action perspective. Unfortunately this addition did not necessarily resolve what I had come to view as a top-level negative in an organization: namely the agenda of an executive who was not prepared to be moved by careful work done at lower levels in the organization. This particular situation could benefit from a small number of observations that arose from the many instances of empirical evidence available to me. This observation was that whenever the individual who had the power to make a final decision on a major program had been involved in the detailed work using Interactive Management, that individual made the decision according to what was learned from that experience. To use a common expression, he "owned" the thinking that came from that experience. If, however, the person who held the final decision-making power had not been involved, the decision tended to emerge from the prior agenda, and everything else was window-dressing. (This generalization awaits further experience, where it may be reinforced, amended, or discarded.) It is likely that much of the behavior of top-level executives arises from their interaction with management gurus, consultants described in a book using the language "witch doctors" by two editors from *The Economist*.

External Factors for the Coherent Organization. Two independent practitioners worked with quite different groups on quite different issues, using Interactive Management. Both found that results of their work could be represented by ***three-level inclusion structures***. One practitioner found a problems field. The other discovered an options field., It is only a small step to imagine that the organizations can be viewed as virtual three-level entities. Still this does not mean that the power structure is fully represented by the three-level structures. Alberts' teams discovered that about 10% of the problems that they identified could not be resolved by their acquisition system design. Upon investigation, they discovered that the power to correct those problems lay outside the Department of Defense, in the administrative branch of government. A similar recognition was found in the work in Mexico, where a modest part of the problematic situation involved factors lying above the Systems Engineering Department, where the work took place. A similar observation was made at Ford Motor Company, as mentioned earlier. To add to that description, the first group that used Interactive Management at Ford discovered that about 75% of the problems that emerged from group work had a "cultural component". The participants were surprised to discover that about 70% of those problems lay within the prerogative of the sponsoring management, and only 30% were the responsibility of higher-level groups. But one must be aware of the fact that the top level exerts continuing long-term effects on lower levels, hence can actually undo advances made at the lower levels. These insights are all relevant to the way in which Interactive Management is applied in organizations. There is no proof to validate the next Axiom, but it should be taken as a working Axiom until reasons to do otherwise are found.

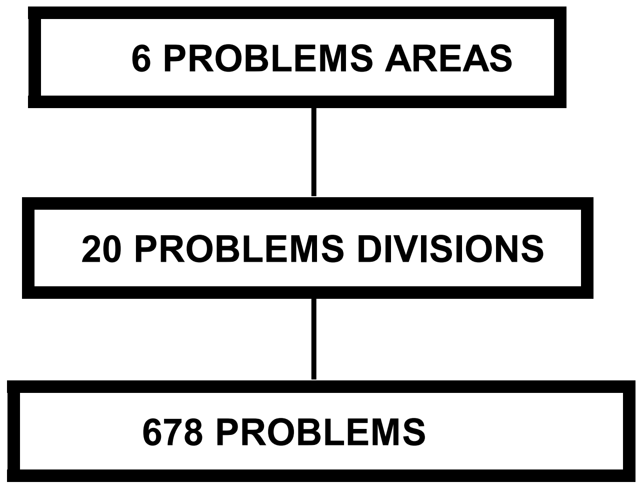

Figure 12 The Alberts Pattern (Of Problems)

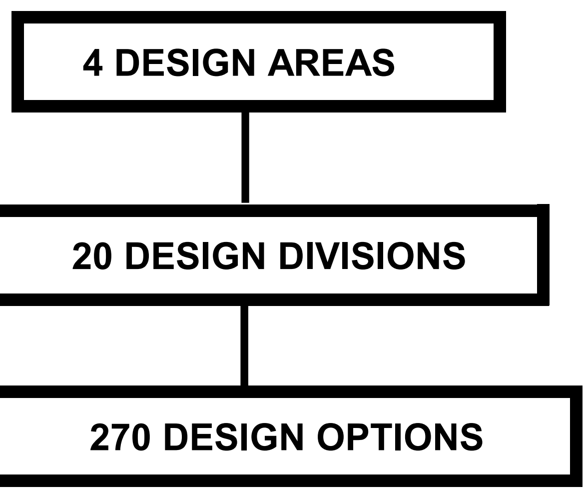

Figure 13. The Cárdenas-Rivas Pattern (of Options)

Axiom 14 (The Ownership and Commitment Axiom). Whenever a group expends great energy and applies scientifically tested processes to a problematic situation, with strong process leadership, the participants and observers (if any) can be expected to "own" the results, because they all reflect majority points of view. But since evidence exists to show that such results are readily bypassed by top-level executives, an important part of the plan of action to develop the results will include participation of a "champion" from the strategic level in the Virtual Organization. This means that the top-level management of the organization should be represented, and should make a commitment to deliver a strong endorsement of the results to those who have the power to make the final decision.

Even if that power lies outside the host organization (as Alberts understood to be true in part in his work) it will be desirable to have links to that power represented in the work. In Alberts' case, he kept the attorneys who served the relevant House and Senate committees appraised of the work throughout the five year period, so when the time came to press for a legislative act, the 1994 law was passed in near-record time. Axiom 14 is a special case of a broader Axiom that will provide general guidance until demonstrated to be faulty.

Axiom 15 (The Coherent Virtual Organization). The Three-Level Model of the Coherent Organization should be formally recognized in developing a plan for any program of work on a problematic situation. (If more than one organization is involved, the Axiom should be adapted to that circumstance.) Specific components of the plan of action should incorporate actors from all three levels, with articulated commitments for followup to the program of work.

Along with Axiom 15, it became clear that the early thoughts about working environments deserved more attention, which is now given in Axiom 16.

Axiom 16. (The Infrastructure Axiom). Because of the extreme challenge associated with complexity, all possible factors should be brought to bear on its resolution, and the establishment, budgeting, and operation of two physical facilities is vital to this purpose: the situation room and the observatorium. The former is equipped and staffed to provide the working space for group activity, including space for both participants and observers. The latter is a facility organized to

hold the products of group work for later observation, study, discussion, and amendment (if required).

The Organization of Systems Science. With the foregoing concepts as resources, it gradually became clear that it would be possible to organize systems science. The epiphany related to this occurred, through the idea that a systems science could, and necessarily should, have sub-sciences. This idea came about by recognizing that systems science should be an over-arching science that is competent to serve all kinds of problematic situations. Because of this, it had to be a neutral science, not constrained to work with a particular class of problematic situations. These thoughts posed two main challenges:

- How to reconcile the neutral nature of systems science with the existence of many methods that have been proposed for working with systems, some of which are probably presumed to be a part of systems science
- How to decide which sub-sciences would be a part of systems science, given that there are already in existence a variety of areas called “science” that might make claims to be part of systems science

Resolving the First Challenge. The groundwork for resolving the first challenge had already been laid by distinguishing between a science and the domain of a science. The **domain of a science** has been described to consist of the science itself and another component that involves the collection of problematic situations in which the science could be applied. All of the non-neutral methods could be thought of as available in this larger component which might be called the “Arena”. From this Arena they could be called into play whenever it is determined from the application of systems science that they are needed. The products of application of systems science include the determination of what specialized or biased methods are needed to complete work that has begun by applying systems science.

Resolving the Second Challenge: Sciences Within Systems Science. The experiences gained from several hundred applications of Interactive Management offered significant empirical evidence of which sub-sciences should be thought of as component sciences of systems science. The diversity of these applications, linked together through the common methodology of Interactive Management, provided ample opportunity to respond to the second challenge.

Science of Description. It is commonplace in practice to strive to move too soon and too directly to a resolution of complexity, before the problematic situation is well understood. Following Foucault's conclusion, a science of description supports the description of a problematic situation by problematization. Using NGT to generate problems, and using ISM to form a problematique and a problems field, the problematic situation is described in ways that are highly responsive to local concerns. Quality control is obtained by using Harary's Theorem of Assured Model Consistency, and the problem field makes the description suitable for later application of Ashby's Law of Requisite Variety at design time.

Science of Design. In applying the science of design, NGT is used to generate sets of options, one set for each category in the problems field. Then applying Ashby's Law, one or more options is chosen from each category in the problems field. The latter are referred to as dimensions of the problematic situation.

Science of Complexity. The science of complexity draws on what is known about behavior and the products of prior work to enable computation of metrics of complexity. These numbers along with the structures generated previously provide significant and interpretable inputs to the next science. From this science comes the **"Work Program of Complexity"** detailing what has to be done, in what sequence, to resolve the complexity in the problematic situation.

Science of Action. The established system of Interactive Management forms the science of action. It is quite sufficient to carry out most of the Work Program of Complexity, carrying it to the point where methods are drawn from the Arena, and where major decisions are made at the Strategic Level of the Coherent Organization.

Figure 14 summarizes the foregoing comments, with respect to the sub-science components of systems science.

Insert Figure 14 about here

The Complexicon. Because what has been described here constitutes a unique body of discovery and aggregation, a unique name is needed to describe it. A name chosen for this purpose is " Complexicon". **Figure 15** summarizes the Complexicon.

Insert Figure 15 and associated notes about here

Managing the Unmanageable. I chose the phrase "managing the unmanageable" as an overview term for a series of slide presentations. These presentations are numbered and are identified with the various boxes in Figure 15. Table 1 lists the titles of the slide presentations and the headings under which they have been placed. These titles are given here to illustrate the variety encountered in the research, and the central topics identified. I think that these support the general flow of information presented in the foregoing.

Insert Table 1 about here

Conclusions. In retrospect and hindsight, my central goal was to create and document a systems science complete with foundations, theory, methodology, and empirical evidence from the domain of the science. . I chose this goal in the belief that there are many problematic situations on earth that can benefit greatly from collaborative work—collaboration that is not likely to take place productively without the benefit of the sciences described here that make up systems science.

Whether this goal is the one that I started with is debatable; but it seems that I can make it believable to myself in light of the various environments, events, dates, and empirical evidence that have accumulated over the years.

List of Figures.

List of Tables.

List of Axioms