ENLARGE THE PARADIGM? YES

But A Substantive, Not a Methodological, Extension is What We Need

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The question at issue before this session is whether the system dynamics paradigm should be expanded to include selected issues from the broader field of systems theory. Although some limited expansion in this direction is desirable and probably inevitable, I will argue that it is not the most fruitful direction for a broadening of our paradigm to take.

To the extent that we can control the evolution of system dynamics, we should seek to expand in directions that simultaneously build upon its distinctive strengths and compensate for its most critical weaknesses. Principal among these strengths, I number conceptual simplicity, analytical transparency and real-worldliness -- all attributes that are conspicuously absent in the bulk of what is called systems theory. Principal among the weaknesses of system dynamics is its lack of a substantive base -- a weakness shared by systems theory. Hence, to expand in the direction of incorporating more systems theory is to dilute our most important strengths and compound our most critical weaknesses. Not a very enticing proposition.

I will begin my argument by describing the predicament in which system dynamics finds itself today -- the predicament that has led to calls for extension of the paradigm. Next, I will seek to demonstrate why systems theory is not good for what ails us. Then, I will suggest a better option -- one which derives

from a hypothesis that seeks to account for what got system dynamics into its current predicament. Finally, I will conclude by summarizing the argument.

THE CURRENT PREDICAMENT

System dynamics was born when a brilliant young electrical engineer sat down to give some concerted thought to how several well-developed concepts, which had stood him in good stead in electronics, might be fruitfully transferred to the realm of management. The real power of the resulting progeny was its sweeping applicability, conceptual clarity and methodological simplicity. The feedback loop already was eons old at the time. But, casting this old codger in terms of a level and a rate; showing that virtually all important human decisions are generated via this generic form; and making the whole affair so simple to express and work with mathematically, was pure genius.

Before long, enhancements in DYNAMO and advances in computer technology, brought Forrester's original vision of a broadly accessible unifying conceptual framework to near full fruition. A skilled practitioner can address a problem through construction of a model which captures the richness and subtlety of the issues under investigation, yet which retains a transparency and simplicity that breeds understanding and insight. As a result, today, system dynamics models are used -- and, what is more important, can be understood -- by grade school children, college sophomores, business executives and PhD mathematicians.

But all is not roses. Although the use of system dynamics is indeed broad, it unfortunately is not very deep. Twenty-five years after its birth, system dynamics remains something of an "outlaw" paradigm. Few full-fledged system dynamics groups exist. Only a small handful of people possess PhD's in the field. Use of the system dynamics approach is not widespread in the management of corporations, cities or national economies. And, where it is used, the quality of application is uneven. Many unfounded criticisms of the methodology stubbornly persist.

This, then, is the predicament in which system dynamics finds itself today. On the one hand, most of us believe that we have a framework that is at once unparalleled in its conceptual integrity and ability to capture reality as it sits. On the other hand, we have been waiting twenty-five years for a take-off; a take-off that continues to show only faint signs of ever coming to pass.

SYSTEMS THEORY, AND THE NATURE OF AN APPROPRIATE EVOLUTIONARY RESPONSE

When the growth of a paradigm falters, there is inevitably increasing agitation for it to change. Such is the case today in System Dynamics. Selecting an appropriate course for change from among the crosscurrent of competing pressures is not easy. Yet it is critical to the continued viability of our field. How can we make an appropriate selection? In my view, an appropriate evolutionary response is one in which the paradigm retains and builds upon its distinctive strengths while infusing new ideas for addressing its existing weaknesses.

The strengths of System Dynamics lie in two closely-related attributes. The first, is the ability of good system dynamicists to represent reality without being compelled to emasculate the issues of concern by invoking assumptions merely to ensure analytical tractability or to satisfy *a priori* theoretical postulates. The second is the ability to achieve such representations in a simple and transparent manner -- one which fosters understanding and insight. Such attributes always will be in great demand in the market-place of paradigms.

Yet it is not these attributes that would be strengthened by a marriage of system dynamics with systems theory. Quite the contrary. Unlike system dynamics, the bulk of the work that has been done in systems theory is theoretical in nature and highly mathematical in expression. What such a union would appear to bring to system dynamics is a stronger theoretical basis for thinking about discontinuous events (catastrophies or bifurcations) and randomness (chaos), along with perhaps an enrichment of the methodological capacity for representing such phenomena. I say "would appear to bring" because there is some evidence to suggest that the existing system dynamics methodology already is adequate to capture such phenomenon [1][2].

One price of these potential methodological gains is a dilution of the simplicity, transparency and real-worldliness which constitute the principal methodological strengths of system dynamics. However, even more important, is the potential loss of formulation and conceptualization power that can come from the addition of "theoretical inertia." An important reason why system dynamics has proven so useful in attacking problems in a broad range of disciplines is that it possesses a relatively limited amount of a priori theoretical baggage. The concept of a positive and negative feedback loop, and a few basic structural elements (levels and rates), are sufficient to proceed. The more a priori's that a paradigm possesses, the more pre-structuring of problems that necessarily occurs. Such pre-structuring is antithetical to attacking a problem as it lies, a hallmark of good system dynamics practice. Assimilation of the plethora of systems theoretic postulates is likely to increase the "theoretical inertia" which system dynamicists will bring to the problem formulation process. The greater the inertia, the less imaginative the resulting conceptualization.

The real question, then, is: do the potential methodological gains from a systems theory/system dynamics marriage justify the loss of simplicity and transparency, and the likely increase in "theoretical inertia?" I think not. The reason that system dynamics is in a "no-takeoff" predicament really has little to do with its methodological limitations with respect to discontinuous events or chaos -- if, indeed, these exist. As such, augmenting the methodology by adding chaos, bifurcations, and strange attractors is unlikely to speed the take-off of our field; in fact, it could well have the reverse effect. What has throttled our growth is the way that we represent social phenomena. Our models are too often viewed, often correctly so, as substantively naive and simplistic. We are too often tabbed as dilletantes in the substantive arenas in which we ply our skills.

A successful evolutionary trial for system dynamics is, then, one that can rectify this substantive deficiency without compromising the distinguishing strengths of our methodology. A movement toward systems theory satisfies neither criteria.

WHY HASN'T SYSTEM DYNAMICS "TAKEN OFF," AND WHAT CAN WE DO TO ENSURE THAT IT DOES?

System Dynamics has not taken off because we have failed to sustain a coherent effort of sufficient magnitude in a narrow enough substantive arena for a long enough period of time. Instead, as Figure 1 suggests, our history is marked by a progression of bold, but shallow and short-lived, forays into a series of diverse substantive arenas. In each arena, we made a noisy splash, stayed for a short while, and then moved on -- leaving a wake of heated controversy and unresolved criticism, as well as a steadily mounting debt to future generations of system dynamicists. Now, with a scandalously high debt-to-contribution ratio, we have neither the substantive credibility nor the critical mass of satisfied clients needed to ignite and sustain a take-off. And, a lot of toes have been stepped on in the process.

So where do we go from here? To ignite a take-off, we must demonstrate the utility and power of the approach by concentrating, and becoming expert in, a particular substantive arena. To fan the resulting spark into a fire, the substantive arena should possess four key characteristics. First, it must fit well with the distinctive strengths of system dynamics. For example, we could choose ecology or theoretical physics. But, because deer can't talk and quarks move too fast to be interviewed, the fact

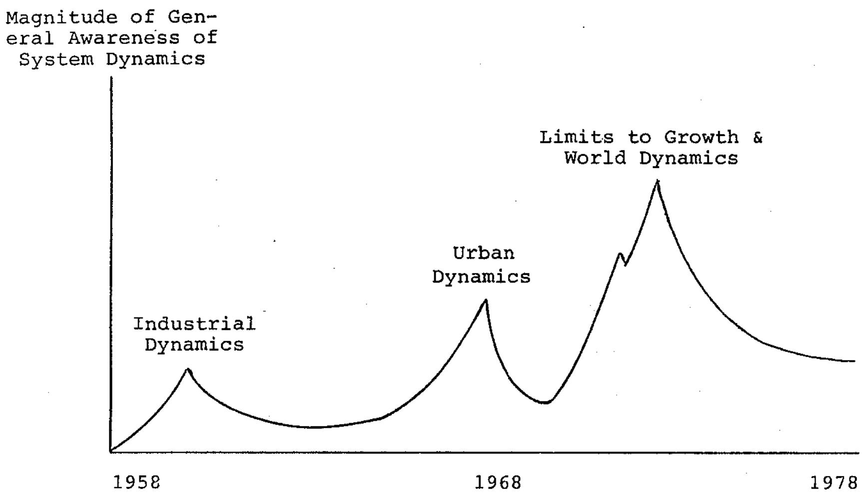

FIGURE 1. A HISTORY OF MAJOR SYSTEM DYNAMICS APPLICATIONS.

that a unique strength of system dynamics is its ability to generate macro-behavior from a representation of underlying micro decision processes would be of little consequence. Second, the arena must be large enough to accommodate both a significant number of applications and future practitioners. Third, the arena should be the source of problems that are of substantial concern to the general public. And fourth, the arena should not already be dominated by a paradigm whose members perceive them-

selves to be in direct competition with system dynamics; economics springs readily to mind.

An arena possessing all of these characteristics does exist . . . on earth. Indeed, it is right in front of us, and not far from where system dynamics got its start. The clients in this arena are organizations -- corporations, hospitals, government agencies, and the like. The problem of general concern faced by virtually all members of the class of organizations is how to realize the full productive potential of their human resources. The grossly overworked, but nevertheless inadequate, watch-word is "productivity." Probably a more accurate single-word descriptor is "coordination." This word spawns a more operational question. How does one get the functionally diverse cultures within an organization to play well together? A few quotations from an important sub-set of the corporate sector -- high-tech firms -- should help to illuminate the nature of the problem:

> "...both insiders and outsiders largely attribute Vidar's failure to software problems that other TRW experts might have solved and to a lack of cooperation between the division's engineering and manufacturing departments. To me, it was very frustrating...you had a bunch of engineers designing something who did not take advantage of bringing in the manufacturing people. They, so to speak, threw it over the wall and said, 'Make it!'" [3]

> "The biggest problem we're having now is that we're creating conflicts between our groups by not letting each other know what's going on every step of the way. Everyone is trying to do the right thing, but once the goals are out of 'sync,' you have a real problem on your hands." [4]

But isn't the application of system dynamics to problems of organizational coordination something that we have been doing for years? Isn't all of this just "Industrial Dynamics," revisited? The answers to these queries are "yes" and "no." Yes, we have been applying system dynamics to problems of coordination in organizations for years. And, no, what I have in mind is not just "Industrial Dynamics."

The great majority of corporate applications of system dynamics focus on a particular sub-system within the firm. By far the most popular sub-system to date has been that involving the production apparatus. You need only examine the disproportionately large number of teaching materials involving inventories, backlogs and order pipelines to convince yourself of the truth of this assertion.

Without doubt, the reason for the popularity of production-focused models among system dynamicists is the tangibility of the subject matter. This part of the corporation is most like a "machine." Hence, our characteristically bloodless, automaton-like decision-rules can pass muster here without strenuous objection. We are on "solid ground." Indeed, the vast majority of corporate-focused system dynamics models have scrupulously avoided the fluffier issues associated with human resources within the organization. Detailed treatment of things like decision-making under uncertainty, motivation, innovation, commitment, managerial direction, coordination and communication have been left to the OD people to tackle. And courageously tackle, they have. Indeed, Herbert Simon recently was awarded a nobel prize for his efforts in this arena.

It is to the substantive expertise of people like Simon, March, Cyert, Schein and Tushman that members of our paradigm should reach out. These people can teach us much about how to make our models more richly reflective of the actual stuff of human decision-making within organizations. And, when these more rich depictions are fleshed-out and operationalized via integration into system dynamics models, they will become even more powerful in their ability to spawn understanding and insight.

If behavioral science is to be our bed-fellow, it is important to determine how the organizational arena rates along each of the aforementioned dimensions which gauge the "marriage-potential" of a particular substantive arena. First, it fits very well with the distinctive competence of system dynamics, both in representing micro-level decision processes, and in working primarily with empirically- rather than statistically-derived data. Second, the multitude of corporations, hospitals and government agencies in existence provide an ample base for assimilating both a large number of applications and a sizeable number of future practitioners. Only, one or two of us can build and expand upon a world model. But thousands of us can construct individual corporate models. Third, the "productivity dilemma" is a problem of great national and international concern, one that is not likely to go away in the near future and one that poses a number of interesting intellectual challenges. And fourth, the community of behavioral scientists do not perceive us as a direct threat to their well-being. Quite the contrary. For the most part, work in behavioral science is qualitative in nature. Numerical precision has never been the preferred route to insight, nor do most behavioral scientists believe that it ever will be. The use of our type of simulation modeling, especially in bringing together the physical and human aspects of an organization, will compliment the efforts of behavioral scientists -- providing many opportunities for useful synergies. Because the two paradigms have complimentary needs, much can be gained in the interaction by both paradigms.

A PLAN FOR ACHIEVING A TAKE-OFF

If we are to succeed in both extending our paradigm to assimilate substantive contributions from the behavioral science arena, and in capitalizing upon these contributions to produce a take-off, we must be prepared to join together to launch a coherent effort. Such an effort will involve a redirection of both our teaching and research thrusts.

In teaching, two steps should be taken. First, introductory system dynamics courses such as POS1 at MIT or ES34 at Dartmouth -- i.e., those using Forrester's POS, Goodman's Study Notes, Richardson's Introduction, and the like -- should not be offered at the graduate level. These courses, with their emphasis on transferability of structure across a broad set of disciplines, should be taught to first and second-year undergraduates (and perhaps high school seniors). Growing computer sophistication -- thanks to video games, personal computers and the introduction of computing in elementary school -- should make such a shift increasingly implementable over time. The system dynamics framework, as a way of organizing and systematizing the learning process, should be introduced on the front-end of a formal education, not in the concluding years.

Second, at the graduate -- and upper-level undergraduate -- level, a curriculum focused on "improving the behavior of organizations" should be created. In 1964, in a paper which we should all re-read [5], Forrester laid the conceptual foundation for such a curriculum. He called it "Enterprise Engineering." I have revived the term and concept, and have begun to develop a curriculum around it at Dartmouth. In today's vernacular, "Enterprise Engineering" is billed as "CAD/CAM applied to the organization," or simply as "computer-aided design and monitoring of the organization."

As the curriculum has begun to take shape at Dartmouth, it now is slated to include the following core offerings:

- Enterprise Engineering I
- Enterprise Engineering II
- Applications of Enterprise Engineering
- The Management of Technical Innovation
- Strategic Management Information System Design

In addition, the curriculum will contain some "support courses" such as: Engineering Economy, Individual & Group Behavior and other electives from the Engineering and Business schools at Dartmouth.

The first two core courses introduce the system dynamics methodology in the context of organizations. Substantive emphasis is placed on understanding the dynamics of human resources in the context of the physical resources and information flows that give structure to an organization. Methodological emphasis is first placed on developing solid competence in model construction, and then on inculcating a testing process that examines the dynamics of each functional area of the organization, first in isolation, then pairwise, and finally as a whole system. The policy focus is: "what policies make the pieces play well together?" Jim Lyneis' book [6] probably can serve as a good text for at least the first course. The third course offers students an opportunity to try out what they have learned in the first two courses by applying it to a real problem.

The fourth course focuses on managing the process of technical innovation -- a process which unfolds against a backdrop of the product/process lifecycle and the longer-term product-generation transition. The short and longer-term interactions between various functional areas within the organization are examined with an eye toward designing internally-consistent strategies -- across functional areas and product-lines -- for managing innovation. Tushman's book [7] provides a good basis of readings for such a course.

The fifth course examines the principles and processes for designing and implementing real-time monitoring systems to feed back information across functions and product-lines within an organization. Much of the information picked up by traditional information systems offers little insight into the detailed workings of the underlying system. In this course, the system dynamics models employed in the third course will be used to isolate key control points within the organization from which

information should be collected. The course then focuses on how this information is to be used, how often it is to be collected, where to collect it, who should get it, and what the implications of all of these are for overall organizational behavior.

The program ultimately envisioned at Dartmouth is a one-year (twelve month) Master's degree designed primarily to attract engineers who have been working, as engineers, for several years and are now preparing to move into management. Such individuals usually cannot afford the two years that it takes to get an MBA, nor do they need a "general management" degree. Indeed, many high-tech companies make it a policy not to hire MBAs for technical management positions. The one year program is short and focused on what engineering managers need to know: how to manage in a functionally-interdependent, highly-competitive environment characterized by rapid technological and organizational change. Although the Dartmouth program will, at least initially, have a high-tech flavor, this need not be the case everywhere. Similar programs could be tailored for government officials, hospital administrators, and others. Or, a general curriculum could be designed. A hallmark of such programs would be their problem-oriented focus.

I also suggest that we redirect our research efforts in order to compliment the proposed teaching thrust. We must seek more extensive collaboration with those behavioral scientists who are trying to design ways to make the human resources within organizations more productive. Joint consulting and publishing ventures provide a very effective way to pursue such a collaboration. These kind of ventures also build substantive credibility, and pave the way for graduates to secure positions from which they can further the effort. Only when such a positive loop is firmly established can we hope for a take-off.

If we are willing to pull together to develop a coherent teaching and research thrust in Enterprise Engineering, hope for a take-off can be translated into reality. However, if we continue as a fragmented group of practitioners, each pursuing interesting methodological excursions like systems theory, or big-splash substantive "break-throughs," our field is likely to disappear as an independent paradigm. I, for one, do not wish to see this happen. The choice is yours.

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