Selling system dynamics to (other) social scientists

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Abstract

In the last decade I have tried to use system dynamics to do research that is acceptable to scholars from other social science communities. In this paper I reflect upon this experience and outline several errors that reduced the accessibility of my work to those outside the system dynamics community. While some of these mistakes are likely unique to me, others are more common to research that uses system dynamics. Acknowledging these errors has several implications for the future organization of the field. Copyright © 2003 John Wiley & Sons, Ltd.


My sincere thanks to both the awards committee and the society for this year’s Jay Wright Forrester award. To be recognized by one’s peers is truly a high honor, one for which I am deeply appreciative. The award is especially meaningful for me because it represents an affirmative answer to a question that has dominated my professional life for almost a decade: Can one do research that both meets the standards of good system dynamics practice and is acceptable to other social scientists? I am grateful to the Forrester award committee for both their stamp of approval and for the opportunity to (publicly) reflect upon what I have learned in my efforts to do research using system dynamics in ways that are appealing to other social scientists.

Prior to doing so, however, thanks are in order for the efforts of many people without whom the work leading to the award would not have been possible. Drew Jones, Laura Black and Paulo Goncalves all made important contributions to the project. Brad Morrison, Scott Rockart, Anjali Sastry, Ed Anderson, Tom Fiddaman, Liz Krahmer and Rogelio Oliva all read countless drafts and sat through innumerable presentations of the work as it progressed. I also owe an immense debt to my mentors and teachers. Mark Paich introduced me to the field and, since then, John Sterman has been my near constant tutor in both its theory and practice. I count both of them among my most valued friends and trusted colleagues. Finally, special thanks goes to the intellectual forefather of us all. In the decade that has passed since I started my first system dynamics project, there has not been a single day that I wanted for something interesting on which to work. I feel very fortunate to have happened upon a field so vibrant and full of potential. Thanks for this good fortune goes, of course, to Jay Wright Forrester. I have been particularly blessed in this regard, not only...
Firefighting in new product development

The research recognized by the award committee originated in a problem faced by practicing managers. Scholars studying new product development (NPD) have identified several activities that, when executed early in the development process, contribute to project success (Cooper and Kleinschmidt 1986, 1987; Gupta and Wilemon 1990). These activities often go under the heading of concept development (see Ulrich and Eppinger 1995). While the efficacy of these activities has been well-documented, research by myself and others (e.g., Griffin 1997) suggested that using these tools and processes is easier said than done. In a series of interviews with managers concerning the state of their product development processes we found that, despite the widespread consensus concerning the utility of these early phase activities, project managers and engineers found it difficult to make a commitment to them on a day-to-day basis (see Repenning and Sterman 2002). The people necessary for those activities would rarely be available during the early phases of a development project (because they had responsibilities on projects closer to their completion dates), and when those engineers did arrive, the project manager often felt that, because the project was already behind schedule, the early phase activities should be skipped in favor of beginning the detailed design of the product. While such a decision speeded the apparent progress of the project, it also often resulted in the discovery of major problems late in the development project. Such errors or “late changes” required additional resources, rarely included in the initial work plan, to fix the problems before the project reached its completion date. Many organizations called the flurry of activity that accompanied such problem solving efforts “fire fighting.”

That firefighting occasionally occurred when developing new products was not surprising. Developing new products is a fundamentally uncertain task, often including the use of new and unproven technologies, process equipment and suppliers. More perplexing, however, was the observation that, for many companies, firefighting was not an occasional and isolated occurrence; firefighting was the development process. The organizations we studied rarely executed the upfront portion of their development processes and, consequently, most projects ran into trouble later in their development cycles. Moreover, all of the organizations we studied had developed “official” NPD processes that placed heavy emphasis on the early-phase activities that were so often skipped in practice. The following quotes, taken from the initial series of interviews, nicely outline both the reference mode and the beginnings of a dynamic hypothesis:
If you look at our resource allocation on traditional projects, we always start late and don’t put people on the projects soon enough . . . then we load as many people on as it takes . . . the resource allocation peaks when we launch the project.

Manager at Alpha Automotive

... the completion date [the date at which the project is ready to launch] is getting later and later each year. We are starving the ensuing model years to make the one we are on. We never have time to do a model year right, so we have lots of rework and so on.

PD Engineer at Mighty Motors

To explain how firefighting could be so persistent despite widespread awareness of its costs, several graduate students and I developed a series of system dynamics models (see Black and Repenning 2001; Repenning 2001, 2002; Repenning, Goncalves and Black 2001). Analyzing these models suggested that persistent firefighting in NPD results from a positive feedback process whereby lack of attention to the early phases of the development process results in serious problems when projects reach their downstream phases. Those problems, in turn, consume scarce engineering resources that would otherwise be dedicated to the early stages of the next generation of products. Firefighting persists, the analysis suggested, because managers often fail to recognize how their efforts to save troubled projects create the problems that they try so hard to solve. NPD practitioners resonated with this line of work almost immediately. Academics, however, were not similarly receptive.

Mistakes and lessons learned

Early presentations of this work received a critical reception from the scholars to whom I presented it and the first two versions of the paper I wrote were rejected from a prominent journal. Initially, I interpreted these reactions as evidence of irreconcilable differences—similar to those outlined by Meadows (1980)—between system dynamics and other social science disciplines. With the benefit of retrospect, however, it is likely that I made at least four mistakes in how the work was executed, presented and positioned, errors that significantly reduced the accessibility of my work for scholars outside of system dynamics.

Do your homework

Since its earliest articulations, good system dynamics practice has emphasized operational representation and the value of choosing variable names that closely correspond to the labels used by the client of the modeling effort (Forrester 1961, Sterman 2000). While I put this principle to good use when working with practitioners, I largely ignored it in my early efforts to sell the firefighting
research to other social scientists. For example, figure 1 shows an overview diagram of the first version of the firefighting model.

The model contained two phases, labeled *Advanced* and *Current*, and depicted a product development system in which there were always two products in progress, one in the advanced phase and one in the current phase. Following good SD practice, the initial choice of labels came from the company I was working with at the time. But, while my industry collaborators resonated with the basic assumptions of the model, academics did not. Several colleagues who saw early versions of the model or read early versions of the paper simply did not understand what I was trying to model. For example, one referee wrote:

> My problem with this model is that it is not depicting important elements of the NPD process . . . In the model advanced tasks accomplish nothing other than making later tasks more likely to be completed. So the implicit objective of such tasks is not design for manufacturability or design for marketability, but “design for designability”. I’m not sure why we should care . . .

Several portions of the referee’s comment are interesting. The second sentence suggests that he or she did understand the core assumption of the model; completing upstream tasks does increase the probability of successfully completing a downstream task. But, as evidenced by the first sentence, the reviewer did not believe that this feature was a particularly important element of new product development—a surprising claim in light of studies showing the importance of early-phase activities. The third sentence, however, gives some hint to the confusion. The referee refers to design for manufacturability, a popular research topic at the time, suggesting that my conceptualization did not capture something similarly important. Ironically, my conceptualization of advanced tasks was expressly meant to encompass innovations like design for manufacturability, in which early investments yielded downstream gains. Clearly, I had not connected with this referee.
Comments from several of my colleagues in the NPD community helped me trace the misunderstanding to my choice of labels. While the advanced task labeling was common usage at the industry site, it was not in the NPD literature. Rather than distinguish between advanced and current tasks, scholars studying NPD tended to focus on the different phases of the process, calling those things that happened in the early phases concept development and those activities that occurred in the later phases detailed design and testing (see Ulrich and Eppinger 1995). Changing the labels to match the existing literature was a critical step in gaining acceptance for my model. In subsequent presentations and submissions, I received far fewer questions about the details of the model (there were several other concerns to which I will turn in a moment).

Failing to develop a formulation that corresponds to the mental models of the “client” for the modeling effort is a classic error in SD practice. The only wrinkle is that in my case the client was not a manager or set of managers, but rather an academic community. This, however, makes the problem easier rather than harder. Whereas managers’ mental models can be fuzzy, ill-specified and difficult to access, academics spend much of their professional lives explicating and formalizing their mental models in the form of theories. The existing literature provided ample data concerning how scholars went about making sense of new product development. In the first version of my model, however, I failed to use this information to inform my model.

I have made this mistake in other work. In Repenning (2002) I present a simple model that captures how commitment to using a new innovation might diffuse through an organization. The model’s structure was drawn from an earlier study (Sterman, Repenning and Kofman 1997), and in the first version of the paper I justified that structure with field observations from the original study. This was, however, an error. Had I bothered to search the relevant literature, I would have discovered a significant body of work on each of the three main constructs in my model, commitment, goal adjustment, and diffusion. The referees were, quite correctly, critical of a paper that proposed to link these concepts but failed to cite any of the previous attempts to define and understand them. Moreover, when I finally did delve into these literatures, not only did the findings they presented substantially improve my paper, but they also made motivating the analysis far easier. Several scholars who had studied commitment realized that there were complex dynamics associated with its development that had yet to be analyzed. For example, one review suggested the existence of reinforcing feedback:

... what is important to recognize is that the development of commitment may involve the subtle interplay of attitudes and behavior over time ... the process through which commitment is developed may involve self-reinforcing cycles (Mowday et al. 1982:47)

With this review in hand, my focus on understanding the dynamics of commitment was far easier to justify.
My experience is that this phenomenon is not limited to studies of commitment and diffusion. Organizations are dynamic places and scholars who have studied them often recognize this fact. Unfortunately, they rarely have tools adequate to this dynamism and are left with few options beyond calling for more attention to these features in future work; such calls are open invitations to use system dynamics in academia.

**Appropriate model size**

Figure 2 shows the next version of the model, to which I made two major changes. First, following the distinction used in the literature, I re-labeled the two phases, *Concept Development* and *Product Design and Testing*. I also changed the model so that the tasks in the two phases were different (in the old conceptualization the tasks were the same, but were done differently in the two phases). The two formulations turn out to be virtually identical (the original version can be found in Repenning 2000), but the second version conformed better to existing conceptualizations.

The basic feedback structure of the model is shown in Figure 3. The balancing Rework loop works to close the gap between the desired and actual quality of the product in the design and testing phase; as defects are discovered, resources are allocated to correct them. If resources are scarce, however, then fixing defects in the design and testing phase comes at the expense of progress on the product in the concept development phase. The reinforcing Tipping loop captures these dynamics.

As resources dedicated to the product in the design and testing phase increase, those dedicated to the project in the concept development phase decline. The declining attention to concept development results in even...
more problems when that project reaches its design and testing phase. Figure 4 shows the model’s base case, along with additional runs detailing how the system responds to transient increases (pulses) in workload.

The model is initialized such that, in the base run, all of the planned concept development work is completed each year. In the first test, in model year one the workload is increased by 20 percent and then returned to its base case level. In the second test, the workload is increased by 25 percent, again for a single model year. The two simulations demonstrate the key property of the model. The system is able to accommodate the 20 percent increase, eventually returning to its pre-shock behavior, but the 25 percent shock pushes the system onto a new trajectory that ends in a different steady state. Following the larger pulse, the system settles into a new “firefighting” equilibrium in which fixing problems in the downstream project occupies all of the organization’s resources and attention is never turned to concept development. When the firefighting mode prevails, the system’s capability is compromised. The model thus suggests that firefighting can become a steady-state phenomenon generated by well-intentioned managers who fail to understand the long run consequences of their actions. Practitioner audiences often resonated with this argument, suggesting that it helped them understand why they were never able to escape the firefighting mode in their organizations.

Despite the changes in the model conceptualization, many academics still did not find the model similarly illuminating. Several criticisms were offered. Some reviewers felt that the model offered nothing new, nothing
interesting, or both. One reviewer wrote, “this paper is a mere repetition of earlier work in system dynamics.” Others felt the model was too complicated. Another reviewer wrote, “. . . the model is unnecessarily complex.”

These comments were perplexing (and more than a little demoralizing). I could find no study, in the system dynamics literature or otherwise, that captured the dynamics I sought to explain; what I was merely repeating was not clear. Moreover, the existence and costs of firefighting were well documented, so the problem at least seemed important. Why did I (and the practitioner audiences that had seen this work) think the analysis was interesting
while the academics saw little value? While this set of reviews provided another opportunity to chalk up my difficulties to the hostility of mainstream academics to system dynamics, I eventually arrived at another diagnosis.

After receiving the referee reports I had a conversation with the editor handling my paper. He summarized the reports by saying that everybody already knew that organizations could get into “death spirals,” so my paper, by adding to the list of potential death spirals, didn’t contribute much. This comment was, unfortunately, a familiar one. When presenting system dynamics models to academics, I have often received comments like “… so you are saying there are positive loops, what’s new?” System dynamics (SD) has more to offer than simply demonstrating the existence of one or more feedback loops, but SD work is often interpreted this way. Readers failed to see that by understanding the structures that created self-reinforcing processes one could develop insight both into which situations might be most prone to such pathologies and into what could be done about them. My analysis certainly suggested that a product development process could get stuck in a “death spiral,” but it also offered more. These contributions were not, however, apparent to the reviewers.

My initial reaction to the charge that I had offered little insight was to extend the model in ways that demonstrated the utility of my findings. For example, the existence of firefighting has important implications for how organizations should introduce new tools and processes in the development process. In one version of the paper I show how introducing new tools in the concept development phase can actually push the system over its tipping point, making performance worse rather than better (see Repenning 2000). This insight, I believed, legitimated my analysis by showing how the existence of positive feedback had important implications for understanding pathological behavior in new product development. Later, however, I arrived at a different conclusion. Based on my experience with another piece of research, I now believe that the model was not too simple. If anything the referees were right, it was too complicated.

In Rudolph and Repenning (2002), Jenny Rudolph and I use a system dynamics model to study how organizations react to an ongoing stream of interruptions to normal activities. The focus on interruptions was motivated by case studies that detail how major disasters are often preceded by a stream of seemingly innocuous interruptions, any of which, on its own, poses little threat. Most notably, we drew on Weick’s (1993) analysis of the Tenerife air disaster, the worst accident in aviation history, which details the numerous minor events and errors that led to a KLM pilot clearing himself for take-off and flying directly into another plane that had just landed. One could imagine that a “classical” system dynamics analysis of this event might yield a reasonably complex model, operationally representing several features of the situation at hand. The model we eventually published, however, was far simpler (see Figure 5).
The model is effectively first-order (the desired resolution rate does update with a short delay). As the stock of Interruptions Pending rises, the Desired Resolution Rate increases, which, in turn, creates Stress. Stress, modeled as the ratio of the Desired Resolution Rate to the Normal Resolution rate, determines the rate at which interruptions are resolved. The key assumption in the model is that stress is linked to the resolution rate through a relationship known as the Yerkes–Dodson curve (Yerkes and Dodson 1908). The Yerkes–Dodson curve—originally derived from experiments testing how rats running through a maze responded to electric shocks of varying size—depicts a curvilinear relationship between stress and performance. Initially, stress improves performance, but, as it continues to increase, performance eventually declines. The justification for this relationship lies in the physiology of autonomic arousal. When people are under stress, autonomic arousal, which manifests as faster breathing and a higher heart rate, heightens awareness and focuses attention on the sources of stress. For modest levels of stress, this reaction improves task performance in most contexts. However, as stress grows, autonomic arousal also rises and consumes scarce cognitive resources. That is, people become distracted by the physical sensations and, consequently, performance declines.
The model is simple. It contains only one stock and two important feedback loops. Moreover, its dynamics can be summarized in a single figure (see Figure 6). The system has two equilibria, one stable, residing in the upwardly sloping portion of the curve, and one unstable, residing in the downwardly sloping portion. If the level of interruptions pending, and therefore the amount of stress, stays below the unstable equilibrium, the system will tend to return to the stable equilibrium. If, however, the level of stress pushes the system beyond the unstable equilibrium, then performance quickly collapses in a vicious cycle of accumulating unresolved interruptions, mounting stress, and declining performance.

In the paper we used our characterization of the system’s dynamics to outline a new crisis archetype, the quantity-induced crisis. While the existing literature focused on how novel events could precipitate a crisis, our simple model suggested that disaster could also result from a stream of more routine interruptions. Distinguishing between two types of organizational crises, novelty-induced and quantity-induced, and their associated dynamics has several implications for both practice and research. Most notably, our analysis suggests that the strategies often proposed for mitigating novelty-induced crises—stepping back and reframing the situation—can be counterproductive...
when confronting a potential quantity-induced crisis. Any action that temporarily slows the interruption resolution rate can push the system closer to the unstable equilibrium, making collapse more likely.

As Jenny and I prepared an initial draft of the paper, we worried that the model it presented would be too simple for publication. Could a one stock model offer any useful insight into a phenomenon as complex as a major disaster? In contrast, to my (then ongoing) experience with the firefighting work, the initial reactions to this paper were a pleasant surprise. Both the referees and the editor liked it and believed that it offered a contribution to the literature on disasters. While previous work had largely ignored the role of time pressure in precipitating crisis, our analysis suggested that it was a critical variable worthy of further investigation.

Writing and eventually publishing the disaster paper caused me to rethink my approach to model complexity. Whereas in traditional applications model size is often dictated by the complexity of the problem under study, for academic applications I now believe that two other concerns merit consideration. First, there is the state of existing theory. Theory development proceeds largely incrementally and new contributions usually build on past work. For areas of inquiry where there have been few attempts to understand dynamics in a systematic fashion, simple models are needed, not because the phenomenon is simple, but because there is little on which to build. In the case of understanding disasters, our simple model was appropriate because it was one of the first attempts to develop formal dynamic theory in this particular domain. It would have been difficult to both develop a more sophisticated model and pursue all of its implications in a one article.

Second, there is the modeler’s ability to develop her audience’s intuition for how the model’s structure is linked to its behavior. Just as clients are unlikely to take a model’s results on faith, academics are unlikely to value the output of a modeling exercise unless they understand how the resulting insights are generated. It is on this point that the disaster paper succeeded but the initial versions of the firefighting paper did not. Note also that the two determinants of model size are not independent. When there is little existing dynamic theory, it is harder to build the audience’s intuition into the behavior of a particular model. The model of disasters described above is trivial by SD standards. Because it tackled an area where there was little previous work, however, its simplicity was central to its impact on the field to which it was targeted. A more complicated formulation would likely have been too much for a non-SD audience to absorb in a single article.

**Building intuition**

The experience with the paper on disasters led me to a new hypothesis concerning my difficulties with the firefighting work. Whereas I had initially believed that the problems somehow lay within the model itself—it was too
simple and produced too few implications—I now believe my difficulties were rooted in my failure to build the reader’s intuition. Although not immediately obvious, the reviews mentioned above, which claimed triviality, also support this conclusion. A claim that a model is trivial is as much a statement about the reader’s understanding as it is about the underlying model. Recall that several people thought the point of the paper was to argue for the existence of “death spirals.” Had this been the only insight in my paper, then the charge of triviality would have been well-founded. Similarly, recall the referee who proposed an alternative formulation. The alternative was a classical production model with two stages, the gist of which was simply that if stage one was the bottleneck, then resources should be rebalanced between the two stages until their throughputs were equivalent. In one sense, my model was not different; when operating in the firefighting mode, performance can certainly be improved (in the long run) by reallocating resources from design and testing to concept development. But, this is not the question I was trying to answer; past scholars had already established that many organizations under-invest in upstream activities.

The question I was trying to answer was “why?” The referees, however, did not understand the answer I was providing and concluded that I was saying something simpler. I had failed to communicate the essence of my model to the reviewers. They thought I was either arguing for the existence of death spirals (which had certainly been done) or trying to show that upfront tasks deserved more attention (which had also been demonstrated). Neither of these were my main objective. What I hoped to do was establish the conditions under which the well-intentioned efforts of manager to deliver high-quality products could inadvertently create a self-reinforcing cycle of firefighting. Because the referees had not developed deep intuition for why the model produced the behavior that it did, they did not see these deeper and potentially more interesting contributions.

As an aside, note that identifying cases in which a paper fails to build a reader’s intuition is not easy. People often do not recognize gaps in their intuition and, even when they do, are unlikely to say they don’t understand a model. I doubt it ever occurred to the referees mentioned above that they missed elements of the story I was telling. Instead, they quite naturally (and implicitly) assumed that they gotten from the model all there was to be had. Any discomfort that readers do experience from not fully understanding a model is likely to manifest in indirect ways. Suggestions that a model is “too complicated,” that the “insights are trivial,” and that more analysis is needed are all likely responses.

Why, then, did the referees not understand my model? In presenting it I had followed the standard SD protocol, outlining key assumptions, presenting simulations that showed the key points, and then supporting those points with sensitivity analysis. The answer is, I now believe, straightforward, if sobering: standard modes of presenting SD models, while potentially effective for SD
audiences, are ineffective when presenting to non-SD audiences, even those with technical backgrounds.

In early versions of the paper describing the firefighting model, I used a small set of simulations to summarize the central finding from the analysis. For those within the SD community this was largely sufficient because they could develop intuition for how the model’s structure linked to its behavior by drawing on past experience with similar models. Those outside the field, however, had no such mental library from which to draw. Consequently, they were unlikely to develop deep intuition for the dynamics of firefighting by seeing a causal loop diagram and a few simulations; I now find it hard to believe that I ever expected otherwise. After more than a decade in the field, I still find it difficult to understand a model by watching a presentation or reading a journal article. It now seems quite fantastic to believe that standard modes of model presentation would ever significantly impact academics from outside the SD community. Such an approach presumes a knowledge of dynamics that the existing experimental evidence suggests does not exist (e.g., Booth Sweeney and Sterman 2000).

The field has made significant investments in the development of tools and techniques for helping managers building insight from system dynamics models. Group modeling building and management flight simulators are both responses to the challenge of helping practitioners gain insight into the structural theories embodied in system dynamics models. While there have been few similar investments in aiding academics, there is little reason to expect that they are more able to absorb system dynamics insights than practitioners. To the contrary, the popularity of modeling and estimation methods that build on the assumptions of equilibrium and mono-causality suggests that social scientists will find it more, not less, difficult to develop intuition from system dynamics models.

It was a suggestion by my colleague, John Sterman, that helped me confront this issue. By eliminating the assumed delays in discovering errors, I was able to reduce the model to a one-dimensional system. With the simplified model it was possible to summarize the dynamics in a form very different from that I had used previously. Figure 7 shows the one-dimensional model in the form of a phase plot. In the plot, \( f(s) \) represents the fraction of concept development work completed in model year \( s \). The solid black line describes the dynamics of the simplified system by showing \( f(s) \) as a function of how much concept development work was completed in the previous model year, \( f(s - 1) \).

The simplified system has three equilibria (represented by the solid circles). The stable equilibria reside at the two corner points while the unstable one appears in the interior. In the region where the phase plot lies above the 45° line (where \( f(s) > f(s - 1) \)), the trajectory of the system is such that it will converge to the upper equilibrium at \( f(s) = 1 \). Conversely, when the phase plot lies below the 45° line (where \( f(s) < f(s - 1) \)), the system converges to the lower equilibrium at \( f(s) = 0 \).
The phase plot analysis can also be used to derive the key boundary conditions on my results (something I did not really discuss in early versions of the paper). Figure 8 shows the four possible forms that the plot can take based on two conditions:

1. are resources adequate when operating in the desired mode;
2. are resources adequate when operating in the firefighting mode.

When resources are ample, regardless of the execution mode, then the phase plot looks like the one in the upper right quadrant. The system has a single equilibrium at the desired execution mode and a single trajectory that heads towards it. Conversely, when resources are insufficient, regardless of the execution mode, then, as shown in the lower left quadrant, the system again has a single, “firefighting,” equilibrium. When resources are adequate when operating in the firefighting mode but inadequate when operating in the desired mode, then the system has a single equilibrium in the interior. Note, however, that if this condition holds, concept development activities consume more resources than they save. In this case, the organization would be better off skipping concept development in favor of more design and testing. The efficacy of upfront tasks thus emerges as a key boundary condition for the results; if concept development tasks are not worth it, then the analysis is not pertinent.

Audience participants and many reviewers found this presentation far more satisfying. The phase plot analysis proved far more effective in building peoples’ intuition for the operation of the positive loop at the heart of the
Choose your audience wisely

While I now believe that many of the difficulties I experienced in selling my work were, to a large degree, self-inflicted, I did learn one lesson in doing this...
work that is not quite so internally focused. Initially, I assumed that the audience most interested in my work (beyond the SD community) would be the management science/operations research community. This collection of scholars was certainly interested in mathematical modeling and NPD provided the context for several recently published modeling efforts. This assumption, however, proved incorrect in many cases. While some members of this community were interested, the reactions of others ranged from indifferent to hostile. For example, one referee (reviewing my paper for a journal devoted to management science and operations research) wrote:

The author assumes that when resources are scarce, priority is given to the project in the downstream phase. While this assumption might correspond to some behavior observed in industry, it is not an acceptable assumption in a mathematical model. . . . I would advise the author to develop more intelligent resource allocation policies rather than rely . . . on field observation.

The importance of representing an organization’s decision policies in realistic form has been a foundational assumption in system dynamics since the field’s inception (see Forrester 1961). This particular referee, however, disagrees, suggesting that my focus on understanding how organizations actually function is simply a distraction from the more important work of determining what organizations should do.

On one level I would argue that this particular concern is not my fault. The referee clearly maintains a set of assumptions about the role of mathematical models in social and management science that are simply at odds with those that lie at the foundation of system dynamics. The only likely way to satisfy this particular referee would have been to reformulate my model as a dynamic program and then provide an optimal solution. Doing so, however, would mean that the analysis was no longer consistent with good system dynamics practice.

On another level, however, there is a lesson to be learned here as well. I now believe that my initial assumption that SD models would be easier to sell to other modeling communities may not always be correct. Practitioners of other modeling methods are certainly more equipped to understand the details of system dynamics models than those without technical training. However, membership in a modeling community (e.g., economics, operations research) also entails acceptance of the assumptions underlying that discipline. Consequently, optimization looms large in the minds of many people in these fields and certainly can, as in the case above, color their view of system dynamics models.

Other communities in the social sciences maintain different worldviews, often rejecting the logic of optimization and structural functionalism in favor of a view more closely aligned with the operation of actual firms and other organizations (see Pfeffer 1997 for a good review). Although there is certainly a
tradition of modeling (e.g., Levinthal and March 1981), the work in this vein tends to be more qualitative than in disciplines such as economics and operations research. System dynamicists thus find themselves in the position of sharing elements of two otherwise competing disciplines. We share the language of mathematics and the use of formal models as inquiry tools with our colleagues in economics and operations research, but our worldview is more consistent with those in psychology, sociology and anthropology. We should not abandon our efforts to become better connected with our colleagues in economics or operations research, but there are other points of connection. In my own work, I have found it easier to sell my work to those scholars whose primary interest lies in understanding real world phenomena.

In the case of the firefighting paper, I eventually abandoned my efforts to sell it to the MS/OR community and mailed the paper to a journal targeted at the product development community. The paper was accepted contingent on some minor revisions, most of which were focused on making the paper more accessible to the non-technical audience. The final version of the paper appeared as Repenning (2001) and eventually received an award for being the best paper to appear in that particular journal in the year it was published. Why did the product development community receive the paper with open arms while the management science and operations research community did not? There are probably several reasons, including the fact that the NPD community is much newer and may not be as competitive as the well-established management science and operations research community. Most importantly, for the present purpose, however, is that the NPD community has yet to standardize on a particular modeling approach and is more connected to the phenomenon it is trying to understand.

**Discussion**

I now believe that my early efforts to sell my work to others were characterized by at least four errors:

1. failure to ground my work in the language and literature of the field I was trying to enter;
2. developing models that were too large and too complex for the non-system dynamicist to absorb;
3. using inadequate methods to build intuition concerning the link between a model’s structure and its behavior;
4. targeting scholarly communities interested in modeling rather than those interested in understanding complex social phenomena.

Some of the mistakes can certainly be attributed to me. More worrisome is the possibility that some responsibility lies with the field and how its members are
socialized. Although I have not conducted a systematic survey, my sense is that many of the errors are not unique to my work. Worse, such errors may be a natural outcome of the way we organize ourselves—structure drives behavior—and will not change without significant intervention.

While a full diagnosis of these structural causes is beyond the scope of this paper, let me offer a hypothesis. From its earliest days, the SD community often equated economics and operations research with the whole of the social and managerial sciences (see for example the introductory chapter and Chapter 4 in *Industrial Dynamics*; Forrester 1961). There is a certain measure of irony here because it was during this period that scholars such as Herbert Simon and James March began codifying an alternative paradigm based on the earlier work of sociologists who were clearly systems thinkers (see, in particular, March and Simon 1993, Chapter 3). The SD community did not seem to have seriously acknowledged this or other alternatives until much later (see Morecroft 1985). Instead, it continued to define itself in reference to the most doctrinaire work in economics; work that was most at odds with system dynamics.

The consequence of using mainstream economics as a referent is that members of the field found little common ground with the rest of the social science world (as they defined it). The early focus on economics appears to have fueled a vicious cycle of increasing isolation from the rest of social science that persists to this day. Consider the positive feedback loop shown in Figure 9.

As SD was critically received by economists and their optimization-oriented brethren, prominent members of the SD community made a range of attributions concerning those scholars and the fields from which they came, ranging from the relatively benign—we just maintain different assumptions or we have different paradigms (e.g., Meadows 1980)—to the more personal and vicious.
These attributions are not of particular concern *per se* and are probably unavoidable. But what happens next? In many cases the answer appears to have been retreat. Having concluded that other scholars were not open to SD, practitioners of system dynamics retreated to the friendlier confines of their own community and reduced their efforts to communicate with others. The result is a positive feedback driving the field into increasing isolation; as SD was critically received outside the field, efforts to make it accessible to others were reduced, thereby insuring an even more critical reception in the future. As is now well documented in a variety of settings, such processes can be powerful forces for turning initial perceptions, even those that are incorrect, into a very concrete reality. As a dedicated student of system dynamics, to me the conclusion that we are at least partially responsible for the situation we now face seems inescapable. Moreover, evidence suggests that other communities within social science were amenable to formal models and computer simulation. During the late 1960s and early 1970s James March and colleagues produced a number of computer models departing from standard economic logic that were widely influential in both political science and organization studies (e.g., Cyert and March 1992; Cohen, March and Olsen 1972).

The result is a community that, today, is largely isolated from mainstream social science. While psychologists, sociologists, anthropologists and others struggle to make sense of a complex and changing world, many SD scholars remain focused on upending economics, a discipline that is quite content with its existing assumptions and methods. The use of new modeling methods is on the rise in other parts of social science, but this growth has not included system dynamics. For example, in organization theory, the field with which I am most familiar, papers applying so-called “complex adaptive systems” models and agent-based representations far outnumber system dynamics models, although a strong case can be made that the latter is more appropriate to the task at hand.

But, should we care? The field certainly shows many signs of health. The society continues to grow and demand for SD consulting services appears strong. Moreover, developing stronger linkages with other academic communities is undeniably time-consuming. My efforts to publish the firefighting research spanned several years and likely consumed more than a year’s worth of my time. Had this effort been dedicated purely to system dynamics it would have certainly produced more than a single model with one important equation. Despite these costs, however, developing a thriving academy sub-community that is tightly integrated with other communities in the social science mainstream is an important goal, the achievement of which is likely central to the long run success of the field.

Authors writing for the popular business press are often fond of criticizing business schools for being hopelessly behind the frontier of management thinking. Academia, the line of argument goes, moves too slowly to keep up with the pace of modern business—by the time business schools start teaching a
particular innovation, its day is long gone. The critiques launched in these lines of argument are likely well founded, but in my view they distract attention from a far more important point. While business and management schools may be slow to adopt or discredit the latest managerial fad, they play a central role in maintaining a far deeper set of assumptions and beliefs about the role of organizations in society, assumptions that are so deeply embedded that they are rarely acknowledged as such.

For example, in the introductory system dynamics course at MIT, my colleague John Sterman leads a discussion about the goals of a for-profit organization. Is it enough, he asks, to maximize shareholder value, or do companies have other responsibilities to their employees, their communities, and, ultimately, the planet? Many of the students, despite often being in their final semester of graduate school, report that this is the first time that such questions have been seriously considered. Moreover, few recognize how strongly the current approach to management education leads them to a particular answer. While management schools do increasingly give lip service to such concerns, they rarely appear in more practical manifestations. In economics problems set costs are minimized; environmental impacts are not. In accounting you learn to calculate the net present value of potential investments, but not the cumulative impact on the local community. Worse, the implicit assumptions underlying the widespread use of inter-temporal discounting are rarely acknowledged.

Management education defines and sustains many, if not most, of the rules of the game in modern business. Consequently, there are few institutions that offer a better platform for changing the way managers think and act. Without a strong hold in this process, system dynamics will continue to face an uphill battle. Consider one simple example. Many firms continue to invest countless dollars in the development of sophisticated statistical models for forecasting, despite the fact that these techniques are often outperformed by far simpler techniques available in modern spreadsheet programs (see Sterman 2000, Chapter 16 for a summary). Why? One reason is that such techniques continue to be a major part of the business school curriculum, implicitly receiving the sanction of the academic establishment.

Currently, the practicing system dynamicist faces a far different challenge in selling her wares. Whereas the utility, perhaps even the necessity, of forecasting is a taken-for-granted assumption in business culture, the value of simulation in developing more robust social systems (thus reducing the reliance on forecasting) is not. The system dynamicist must sell her client on the fundamental premise of her enterprise; the forecaster faces no such barrier. Few changes in the institutional structure of business and management would do more to facilitate the growth of the field than receiving the legitimacy and implicit acceptance that comes with being a standard part of professional education.

Gaining such sanction, however, requires that scholars who use system dynamics get and keep jobs at management and other professional schools.
The price of entry to such positions is a vibrant and productive research program that is widely accepted as legitimate. Developing research using SD that is of interest to the larger social science community is more than an effort to broaden our influence; it is central to achieving the mission that Forrester laid out at the inception of the field, having impact on the important problems facing firms, communities, and society at large.

Achieving this objective requires change in the SD community at several levels. For individual scholars, I’ve already outlined much of my thinking. I would add only one suggestion, collaborate with academics from the fields you are trying to enter. I have had the opportunity to work with several scholars outside of system dynamics (e.g., Rudolph and Repenning 2002). Each collaboration has given me the opportunity to learn something new and to bring new ideas and people into the system dynamics community. I highly recommend this approach to doing research.

The System Dynamics Society can also play an important role in more tightly integrating SD with the social science mainstream. Collaboration with scholars from other disciplines should be rewarded in both System Dynamics Review and the conference. Moreover, the community should actively seek the comment and critique of academics from other disciplines. One unfortunate consequence of the trend towards isolation is that work in SD often does not meet even the most basic requirements of good social science research. Careful and regular evaluations by scholars from other fields (in the form of both refereeing and being a discussant at conferences) would provide valuable feedback to us all.

Pursuing such an agenda would be costly and does entail some risk. In pursuing alliances with other fields SD could lose the features that make it unique. Few disciplines in the social sciences emphasize operationally grounded modeling, the use of soft variables, and the fundamentally practical orientation that lies at the heart of system dynamics. Pursuing publication in journals and conferences that do not share these aims could result in their loss. Similarly, pursuing alliances with others could potentially weaken both the conference and System Dynamics Review. Resources are finite so academics focused on other communities may choose to send their papers elsewhere and to attend other conferences.

While these risks are real, there are several ways in which they might not only be mitigated, but turned to the field’s advantage. Most importantly, the vitality of both the conference and the journal will be improved if they are positioned, not as competitors to other outlets, but rather as complements. That is, a study using SD might spawn several models of different sizes and several papers. Papers focused on building intuition using a small model might be targeted at journals in the discipline being addressed (e.g., management theory). A larger more detailed model might then appear in System Dynamics Review and the paper containing it could pursue methodological implications of the work not discussed in the other paper. In such a vision,
System Dynamics Review becomes the outlet of record for system dynamics models and grows increasingly connected to other fields via cross-citations.

A more serious risk is the loss connection to SD practitioners. While I have argued for the importance of developing more scholarship using SD, having a thriving and active practitioner community is equally if not more important to the health of the field. My best work has certainly arisen from a tight linkage between my research objectives and practical problems faced by managers. System dynamics should not become like other academic societies that have no representation from the communities that they purport to study. To the contrary, one good barometer of the field’s health is probably the balance between academic and practitioner representation. Maintaining this balance will likely go a long way towards mitigating the risks discussed so far.

Conclusion

The world certainly needs system dynamics now more than ever. While a cliché, it is certainly true that our social systems are more complicated, more interconnected and likely more fragile than at any previous point in the history of humankind. Worse, while we are ever more in need of a fundamentally holistic, systems-oriented perspective, there is good reason to believe that the theories and ideologies dominating social discourse are becoming more shortsighted and individualistic. As argued in a forthcoming paper by Ferraro, Pfeffer and Sutton (2003), the move towards self-interested, local modes of thinking is rooted, in part, in the growing dominance of economics and economic logic in the social sciences. While the conventional wisdom suggests that reality is causally prior to theories that attempt to explain it, it is clear that the causality runs in both directions. Theories and beliefs, once widely accepted, shape behavior in ways that make reality consistent with the theory, even when it was not initially the case. The social sciences are in desperate need of an alternative to the growing swell of theories and notions that focus on individual self-interest with little regard for the larger system in which those actions are embedded. Scholars in a variety of fields have supplied the components from which such a conception could be built. Growing literatures on decision making, group dynamics, technology implementation, and organizational pathologies all highlight the non-rational elements of social life. Yet such analyses often suffer in the marketplace of ideas for lack of integration, frequently being perceived as isolated analyses lacking theoretical cohesion and mathematical rigor. The system dynamics method is, in my admittedly biased view, uniquely suited to the task of tying these pieces together into an account of the social world that is far more generative and empowering than alternatives based on economic logic.

If the field of system dynamics is to realize its potential for changing the world, not only does it need maintain the focus on solving important
problems, but it also must change the fundamental premises from which such problems are approached. As evidenced by his focus on primary school education, Jay Forrester figured this out long before the rest of us. There is, however, more to be done. Developing an active community working within the mainstream of the social, managerial and policy sciences is also likely to be a necessary step towards achieving this goal.

References


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