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**Citation:** Rahmandad, Hazhir. "Connecting Strategy and System Dynamics: An Example and Lessons Learned." *System Dynamics Review* 31, 3 (July 2015): 149–172 © 2015 System Dynamics Society

**As Published:** <http://dx.doi.org/10.1002/sdr.1541>

**Publisher:** Wiley Blackwell

**Persistent URL:** <http://hdl.handle.net/1721.1/111110>

**Version:** Author's final manuscript: final author's manuscript post peer review, without publisher's formatting or copy editing

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# Connecting Strategy and System Dynamics: An Example and Lessons Learned<sup>1</sup>

Hazhir Rahmandad, MIT Sloan School of Management

[Hazhir@mit.edu](mailto:Hazhir@mit.edu)

## Abstract

This article is based on my talk at the 2015 International System Dynamics Conference upon receiving the Jay W. Forrester Award for the article “Impact of growth opportunities and competition on firm-level capability development tradeoffs” (Rahmandad, 2012). It summarizes how that research connects strategy concepts with System Dynamics (SD) modeling to inform the pressures managers face to focus on the short-term as a result of endogenous growth opportunities and competition. Drawing on this example I discuss some potentially useful research tools and assumptions. I close by sharing personal reflections on the process of writing for non-SD academics and why I think making those connections is worthwhile.

**Keywords:** strategy, capability, tradeoff, short-termism, competition, game

Over the past decade and a half that I have been part of the system dynamics community, I have been inspired by the impact and quality of research by Jay W. Forrester award recipients. So I am truly humbled by, and deeply appreciative of, this honor. I thank the award committee for their generous consideration of my work and for their openness to extending this award to research that lies at the intersection of classical system dynamics and theoretical research in strategy. I am also very much indebted to Jay Forrester who founded system dynamics and whose work has been a constant source of inspiration; as well as Ali Mashayekhi, John Sterman, and Nelson Repenning who introduced me to this beautiful field and have supported and mentored me ever since. I feel very lucky to be part of the system dynamics community: from my first days in the PhD program I had the privilege of learning from a wonderful group of friends including Laura Black, Brad Morrison, Paulo Goncalvez, Mila Getmanski, Jeroen Struben, Gokhan Dogan, Dan McCarty, and Tim Quin, as well as thoughtful teachers such as Jim Hines, Bob Eberlein, and Jim Lyneis. George Richardson, David Anderson, and the Albany crew provided great feedback and a supportive environment in our biannual MIT-Albany PhD colloquiums; and Jerker Denrell, Rogelio Oliva, Mohammad Mojtahedzadeh, Scott Rockart, Navid Ghaffarzadegan, numerous seminar participants and two anonymous reviewers offered great feedback in the process of developing this research.

I am also thankful for the opportunity to reflect on this research and use this opportunity to pursue two objectives. First, I provide an overview of the research and findings in my article “Impact of growth opportunities and competition on firm-level capability development tradeoffs” (Rahmandad, 2012) that has been recognized by this award. Second, I reflect on the process of conducting system dynamics research that targets academic audiences outside of our community by highlight some of the

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<sup>1</sup> This is a pre-publication version of the article which appeared in the System Dynamics Review, 2015.

assumptions and methods used in this research, my learnings from this process, and why I think it is important to engage other academics.

## **Modeling Capability Investment Tradeoffs**

The research that has been recognized by the Jay W. Forrester award committee lies at the intersection of system dynamics and strategy. It addresses the tradeoffs managers face in allocating organizational effort and resources. On a daily basis managers have to balance investments of organizational members' time and various resources into distinct organizational activities and functions, such as production, sales, product development, and process improvement. Finding the right balance is the key to success, and neglecting an area for long may hurt the organization irreversibly.

The theoretical analysis of this tradeoff builds on the notion of organizations as bundles of capabilities and resources. This conceptualization underlies the resource based view of the firm in the field of strategy (Barney, 1991; Peteraf, 1993). Resources include various tangible (such as machinery, money, human resources) and intangible (such as reputation and intellectual property) assets relevant to the functioning of an organization. Capabilities represent the organizational routines through which organizations undertake various tasks and produce products and services (Nelson and Winter, 1982; Winter, 2000). In this view understanding the relative performance of an organization compared to others in the field entails identifying the distinct configuration of resources and capabilities that shape the focal firm. Moreover, under competitive pressures, firms are constantly in search of better resource and capability configurations through which they can excel. Therefore, whether by imitating successful firms or discovering best practices through learning by doing, these capabilities and resources are being updated. Strategy scholars hypothesize that those resources and capabilities that are hard to build and have few substitutes are the likely determinants of variations in organizational performance.

Further elaborations on the resource based view have distinguished between different types of capabilities: some, called operational capabilities, allow a firm to "make a living in the short-term" (Winter, 2003). Examples of operational capabilities include production, customer service, and sales. The higher order, "dynamic", capabilities are routines firms use for changing other capabilities. For example an electronics company's product design capability allows it to build new products which form the basis for future manufacturing capability investments. Other examples of dynamic capabilities include process improvement, knowledge creation, and routines for accomplishing mergers and acquisitions. Since updating and renewing operational capabilities is so critical in dynamic markets, in recent years many scholars have turned to dynamic capabilities as the engine of competitive advantage in modern industries (Teece, 2007).

System dynamicists naturally gravitate to the conceptual framework of the resource based view and often use it in modeling organizational processes, albeit with different labels. "Resources" are represented as the key stock variables for modeling an organization's performance. Moreover, the routines that constitute various capabilities inform the production functions used in system dynamics formulations and the inertia of those routines justify the use of stable decision rules for modeling flows. In fact the synergy between SD and the resource based view has been emphasized by several SD experts (Gary *et al.*, 2008; Warren, 2002).

Moreover, tradeoffs in allocating organizational resources among various activities has been the theme emerging from many impactful system dynamics studies. The delays between investing in organizational capabilities and observing the performance outcomes of those investments vary across different types of capabilities, thus shifting investments could lead to worse-before-better reference mode observed in maintenance (Zuashkiani, Rahmandad, and Jardine, 2011), process improvement (Repenning and Sterman, 2002), and other settings. Prior research has identified these delays and the resulting dynamic complexity, as one important source of poor learning (Rahmandad, 2008; Rahmandad, Repenning, and Sterman, 2009), attribution bias, and capability traps (Repenning and Sterman, 2001, 2002). Faced with a choice between investments with quick return (e.g. working harder to produce more widgets) and those that take time to pay off (e.g. investing in process improvement to reduce errors and increase efficiency), many managers are tempted to prioritize the faster and more salient route. That choice may, with a delay, reduce organizational performance as quality and productivity erode in the absence of longer term investments, leading, in a vicious cycle, to even more re-allocation of resources towards short-term initiatives. The resulting pressures also activate defensive routines, disrupt trust among stakeholders, and close the communication channels inside the organization needed for reversing harmful dynamics. Thus the system dynamics literature predicts a systematic bias to exist in many organizations against investments in capabilities with long lead time, including the dynamic capabilities.

The genesis of the current research goes back to a conversation I had with a software entrepreneur. Cognizant of the capability trap dynamics discussed, I emphasized to this friend the importance of building his web-based platform on a sound architecture and investing in some testing tools to enhance quality. He acknowledged the importance of those investments, but found my argument unconvincing in light of the market pressures he faced. His argument, however, was more subtle than a myopic sacrifice of long-run for short-term gains. He noted that his company's access to funding, and thus his chances of any future investments in both short and long term, depends on the short-term performance perceived by investors. Moreover, he was reluctant to make any decisions that could delay the launch of the product and allow the competition to get the first mover advantage in this fast-paced market. In essence, he seemed to have a reason for underinvesting in long-term capabilities that reflected a concern about opportunities for growth and the role of competition not captured in prior system dynamics research. This conversation motivated me to take a closer look at reasons beyond dynamic complexity and myopia that induce managers to focus on short-term investments, and how those mechanisms depend on the market and firm characteristics.

### **The Model**

The two types of capabilities, operational and dynamic, provide a convenient entry into building a very simple model of capability development tradeoffs. Specifically, one could conceptualize an organization to consist of two stocks of capabilities, one operational, which produces the performance of the firm, and the other dynamic, which regulates the efficiency of investments in operational capability. For example the production facilities of a manufacturing firm may be represented as operational capability, and its product development process captured as dynamic capability. The latter enables the rejuvenation of current product portfolio, making future investments in production facilities more profitable. In this set up, the manager's role is to allocate organizational effort<sup>2</sup> between investments in

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<sup>2</sup> I use the term effort to represent the time of the organizational members, financial resources, and other resources the management can allocate to building and sustaining various capabilities.

dynamic and operational capabilities. Dynamic capability investments directly accumulate into this stock variable, whereas investments in operational capability are multiplied by the efficiency of those investments, a function of current dynamic capability level. Finally, both stocks of capabilities are subject to erosion (Helfat and Peteraf, 2003; Rahmandad and Repenning, Forthcoming) as current technologies become obsolete, new market demands emerge, physical assets depreciate and organizational forgetting erodes routines. Figure 1 summarizes this simple model of a firm<sup>3</sup>. A linear relationship between operational capability and performance, along with a decreasing return function for impact of dynamic capabilities on efficiency of operational investments (e.g. a logarithmic function), would provide a simple and general structure for the model.

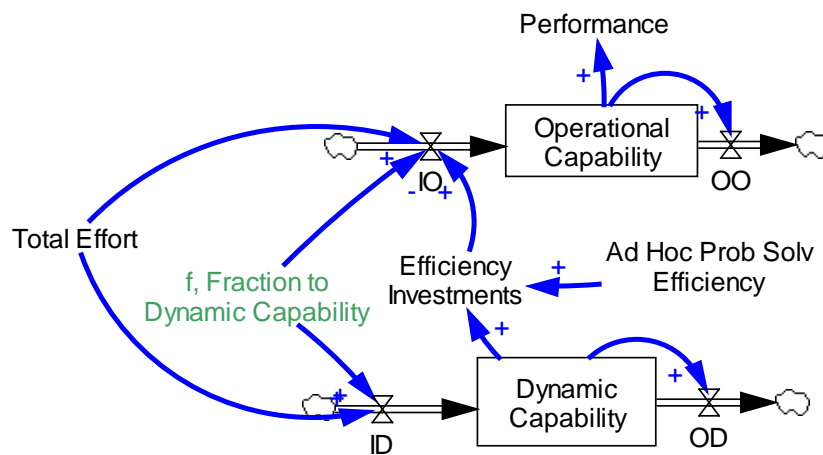


Figure 1- A simple model of firm consisting of operational and dynamic capabilities

A theoretical elaboration on the role of dynamic capabilities is needed for increasing the realism of the model. Specifically, operational capabilities could be developed both through routinized dynamic capabilities, and through ad hoc problem solving on a case by case basis (Winter, 2003). The latter pathway to building operational capabilities does not rely on dynamic ones, and as such puts a lower bound on the efficiency of investments into operational capability.

This model abstracts away many complexities of actual organizations to focus on the tradeoffs managers face in investing among various capabilities. A manager's allocation decision is summarized into a single parameter,  $f$ , which represents the fraction of effort allocated to dynamic capabilities. It can be shown that in the steady state and when total effort is constant, the performance of the firm is maximized at a unique value of  $f$ , which we call  $f^*$  (See Figure 2-a). Therefore biases in management's decisions could be seen in deviations from this optimum allocation policy. Interestingly, at very low levels of  $f$ , steady-state performance declines with increased emphasis on dynamic capabilities, because the resulting levels of dynamic capability are less effective than ad hoc problem solving and the resources allocated to building small levels of those capabilities are wasted. Further investments are however justified once efficiency of dynamic capabilities exceeds that of the ad hoc problem solving, and up to the allocation level of  $f^*$ .

<sup>3</sup> All analysis reported here use the models fully documented in (Rahmandad 2012) therefore I do not report the detailed equations here.

Beyond  $f^*$  more emphasis on dynamic capability is again wasteful, as that effort could be better invested in building operational capabilities.

The dynamics of performance over time in this simple model replicate some of the well-established temporal tradeoffs discussed in the SD literature. Specifically, investments in operational capability pay off faster than investments in dynamic capability: the former directly contributes to performance, but the latter only regulates the future benefits of investment in operational capabilities. Therefore, starting from a near-optimum allocation ( $f=0.24$  in our setting) a manager who (myopically) shifts effort towards operational capabilities (e.g. to  $f=0.14$ ) gets credit for a temporary boost to the firm's performance, before the operational capability declines due to reduced dynamic capability (See Figure 2-b). This is the better-before-worse scenario discussed in the SD literature: managers often get a temporary boost when they push employees to work harder and focus on producing output at the expense of longer term investments. That boost may indeed reinforce the incorrect policy and shift the organizational routines towards more myopic allocations. On the other hand, if a manager recognized her organization to be under-investing in dynamic capabilities and mobilizes effort towards these longer-term investments, the performance of the firm undergoes a decline, before it recovers and the actual benefits of this reallocation are realized (See Figure 2-b for a shift from  $f=0.14$  to  $0.24$ ). This worse-before-better behavior is another hall-mark of temporal tradeoffs that motivate capability traps. Modeling managerial learning in similar contexts shows how myopic decision rules may evolve and dominate (Rahmandad, 2007; Rahmandad *et al.*, 2009).

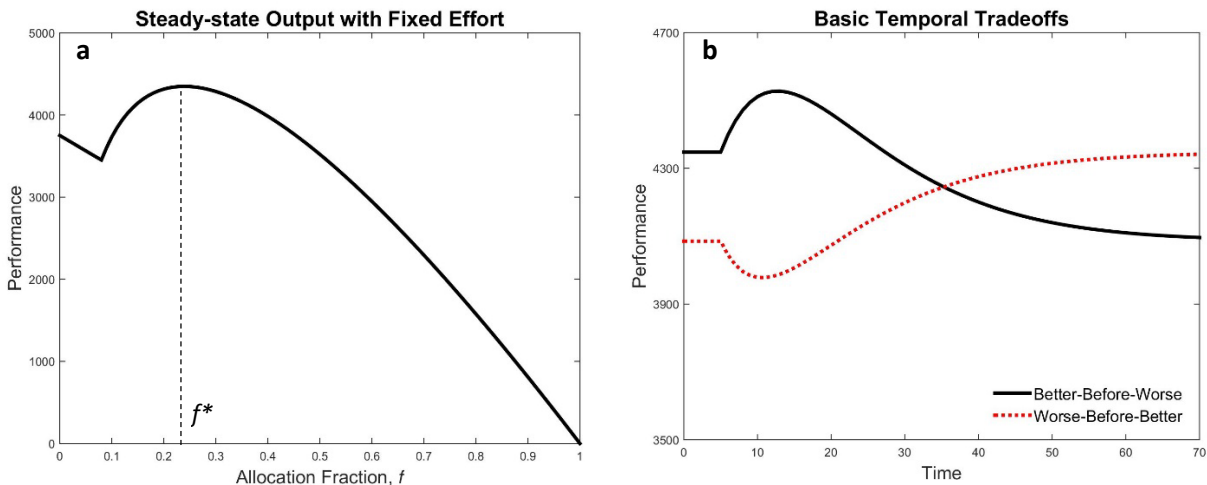


Figure 2- Behavior of model with fixed effort a) Steady state output as a function of allocation fraction b) Temporal tradeoffs in allocating effort between two capabilities

The simple model so far replicates the basic dynamics identified in various prior case studies. Yet, those studies are often concerned with the dynamics inside a single firm. By assessing a firm's allocation policy in isolation, they implicitly assume the effort available to the organization is exogenous. Yet, this was the assumption that my entrepreneur friend had challenged. So in the next section we turn our attention to the implications of relaxing this assumption. The simple structure of the model provides a clear benchmark,  $f^*$ , which can inform comparisons for assessing how the optimum managerial allocation policy changes under various scenarios.

## Endogenous Effort

I first expand the model's boundary to capture the potential endogeneity of organizational effort. The effort available for investment is partially a function of prior performance. Firms with better performance earn more resources to spend on expanding their capabilities. More indirectly, the high-performers are in a better position to attract investors and to get better terms in financial markets. Through both these channels the effort available for investment will increase (decrease) when firm's performance increases (decreases). For simplicity we first assume a linear and instantaneous relationship between performance and available effort, and later explore the impact of delays between the two.

Endogenizing effort closes two reinforcing loops including performance, effort, and investments in each type of capability. As a result, the manager's allocation decision not only directly influences performance, but also changes the gains of these two reinforcing loops, which we may call long-term and short-term growth (Figure 3). Shifting  $f$  towards dynamic capabilities increases the strength of the long-term loop at the expense of the short-term one, and vice versa. The basic mode of behavior that emerges from these loops is exponential growth in performance (and capabilities). Simulating firms with various allocation policies ( $f$  values between 0 and 1), we could identify the firm with the highest performance at any point in time. Figure 4 shows which allocation policy provides the best performance outcome up to any point in time when the endogenous growth loops are active. When the numerical simulation limits the numbers resulting from exponential growth, we mark the policy that allows a firm to first reach that limit.

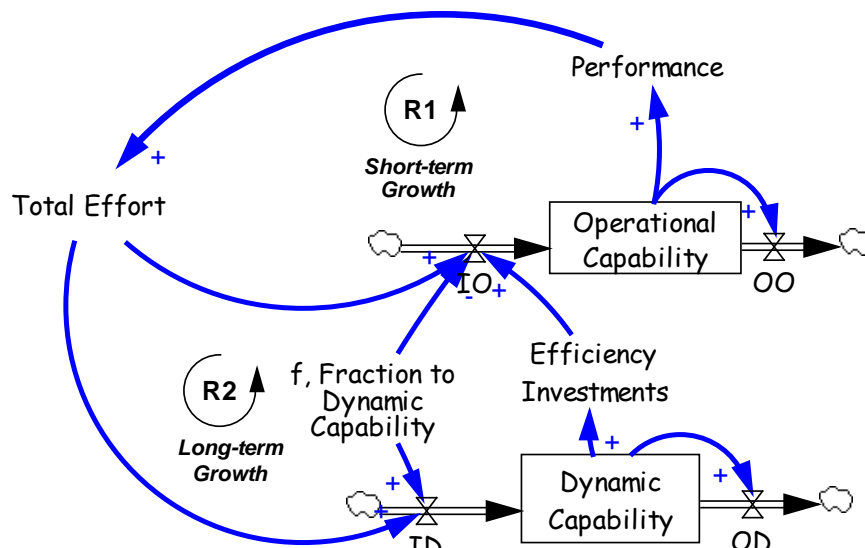


Figure 3- Firm model with endogenous effort

A marked shift in best-performing allocation policy emerges. Whereas with the fixed effort the best allocation policy required about a quarter of effort to go to dynamic capabilities, with strong reinforcing loops active, the preferred policy goes down below 10%. Two mechanisms explain this shift. The first,

and more important one, relates to the trade-off between the short-term and long-term growth loops. Short-term growth loop has shorter delays and thus is more potent for enabling exponential growth. In a fixed time horizon, the firm can benefit from more cycles around this loop, and thus faster growth. Therefore, when competing with the long-term growth loop, allocating effort to the operational capability, which activates the short-term loop, becomes more efficient, compared to the case when neither loop was active. The benefits of enabling short-term growth to grow the effort-base for future investments could be substantial, in this case cutting the preferred resource allocation to dynamic capabilities by two thirds. The temporal tradeoffs still exist, in this case in the form of a gradual shift in the best performing policy towards higher levels of long-term investments. Early on very small or negligible investment in dynamic capability dominates other policies, but gradually benefits of building some dynamic capability surface, leading to dominance of firms with slightly higher allocation fractions in the later stages.

A second mechanism also contributes to the shift in the preferred allocation in the presence of endogenous growth. As the size of a firm's capability stocks grow the return on dynamic capabilities shrink (since we assumed a decreasing return on these capabilities). Therefore very large firms would prefer to allocate a smaller fraction of their net revenue to dynamic capabilities because they do not need to grow their dynamic capabilities proportional to the operational capability. This second mechanism also explains the small drop in the best allocation policy around time 28 in Figure 4: as the firms grow and returns on dynamic capabilities go down, there is a slight shift in best performing allocation policy towards focusing on operational capabilities.

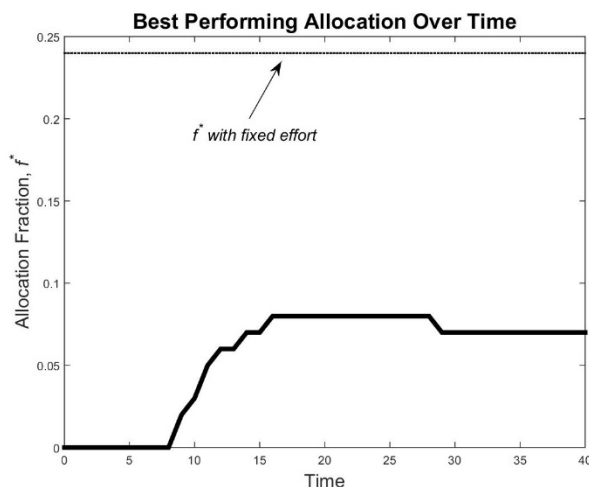


Figure 4- The best performing allocation policy over time for a single firm under endogenous growth setting

Depending on the strength of the endogenous growth loops, the preferred allocation policy varies. Figure 5-a reports the best performing policy over a hundred period time horizon (or until the first firm grows too big to simulate) with different levels of endogeneity in growth. When endogeneity is put to zero, i.e. only a fixed effort is used, the best policy invests 24% of resources in dynamic capabilities. This fraction goes down as more weight is given to the endogenous effort (and less to the fixed effort), so that for largely endogenous growth scenarios as little as 7% allocation in dynamic capabilities is preferred.



The strength of the reinforcing loops could also be changed without considering any fixed effort. Rather, one could vary the fraction of earnings that is reinvested in the capabilities (rather than being paid to the shareholders). In the previous simulations this fraction was kept at a level that puts a firm with  $f=0.5$  in equilibrium (leading to a 79% reinvestment rate). Figure 5-b shows the best performing allocation policy in the long term when this fraction is varied between 0 and 1 and no fixed effort is assumed. For fractions lower than 0.45 none of the firms are profitable, and thus no reinvestment in the capabilities happen, leading to the demise of the simulated firms. Above this threshold, it is first the most short-term focused firms who can survive by activating the short-term growth loop vigorously. Once enough reinvestment is made to avoid early bankruptcy, slightly more long-term focus ( $f \sim 7\%$ ) becomes dominant. For very high levels of reinvestment the fast speed of growth allows firms with  $f=6\%$  to dominate in light of decreasing returns from dynamic capabilities.

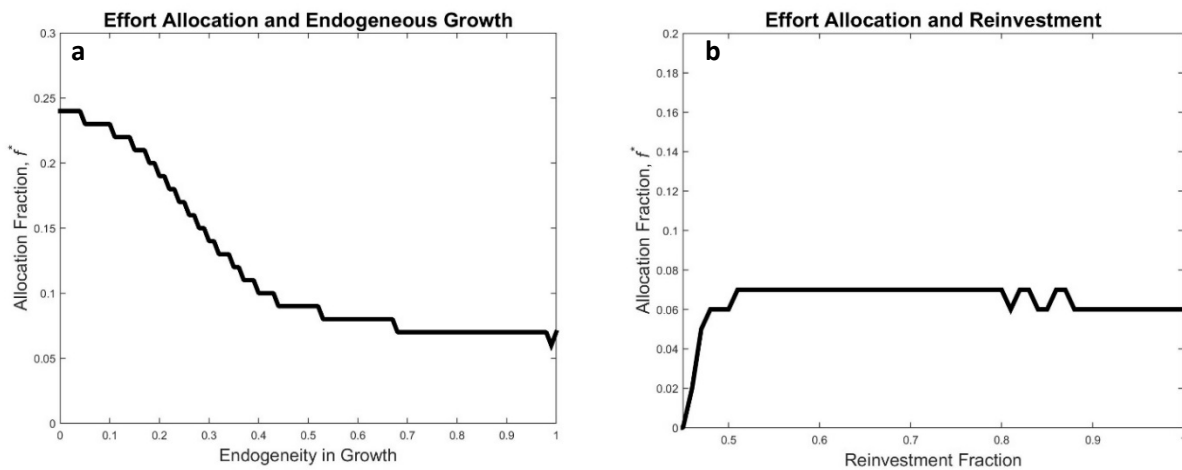


Figure 5- Variations in efficient allocation policy for a single firm under a) A continuum between fixed effort and fully endogenous effort b) various fractions of to net profit invested in capabilities

## Competition

A second mechanism that may change the calculus facing a manager concerns competition. Most firms do not enjoy monopoly power in an unlimited market (which we assumed in the previous section) and should compete for market share against other players. We can extend the boundary of our simple model to include multiple firms and market's growth and maturity. Specifically, the endogeneity of growth in effort is kept and two adjustments are made. First, I replicate the model of the firm to represent multiple players. Second, I incorporate a limited market and a process for calculating the market share of each player when their potential outputs exceed the total demand in the market. A simple market clearing mechanism calculates each firm's potential output based on its operational capability, and allocates the existing demand to various firms proportional to their potential output, but not to exceed that potential. Thus, with a fixed total market size, early in the competition all firms face little constraint from the demand side and would grow. Once sum of potential outputs exceeds demand, firms' enter a zero sum competition for market share and stronger firms get a proportionally larger share of the market. Figure 6 represents the basic causal structure of this model.

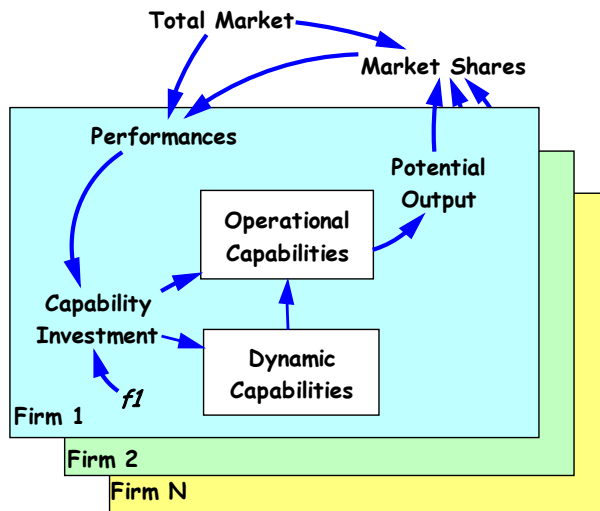


Figure 6-Overview of model structure with competition

Various formulations can be used to represent how managers allocate organizational effort among the two capabilities. As before, one can imagine firms that adopt a fixed value of  $f$ , irrespective of what other firms do. This is in line with our prior formulations and highlight the inertia in managerial heuristics and the limited adjustments made to those heuristics in light of bounded rationality and uncertainties involved in the marketplace (Morecroft, 1983). A more rational perspective would expect managers to consider what their competitors would do, and knowing that their competitors are also rational, decide on an allocation fraction that maximizes their performance. In fact a very rational manager would also change the allocation fraction over time depending on the state of the market and competition. I consider the first two of these alternatives in this research.

First, when firms are assumed to pick their allocation policy irrespective of competitors, then the distribution of those policies among market participants and the size of the market are critical in determining the winner. Figure 7-a presents a scenario where four firms with different allocation policies ( $f=[0, 0.05, 0.1, 0.15]$ ) start in a relatively large market from identical capability endowments. In this scenario the large market size allows all firms to initially grow based on their potential endogenous growth rates. The more short-term focused firms ( $f=0$  and  $f=0.05$ ) grow faster and gain market share at the beginning. Next the firm with  $f=0.1$  catches up as the longer-term benefits of dynamic capabilities show up. At this point the market saturates and the remainder of competition shows the zero sum competition of these firms in which the firm with  $f=0.1$  is able to slowly grab market share from others. Note that the size of the market allowed some of the benefits of dynamic capabilities to surface. If the same competition was run with a small market in which the zero-sum game started from the beginning, the most myopic firm would have won the competition (Figure 7-b). On the other hand, if the competition included a much larger market, then decreasing returns on dynamic capability would have kicked in, bringing up the firm with  $f=0.05$  to the top (not shown). In general, the tighter the competition (e.g. due to larger number of players and more limited market size), the more short-term policies are preferred, because they allow firms to grow their market share at the expense of other players. If competition is very weak, then the policy preferred under endogenous growth scenario is also preferred in competition.

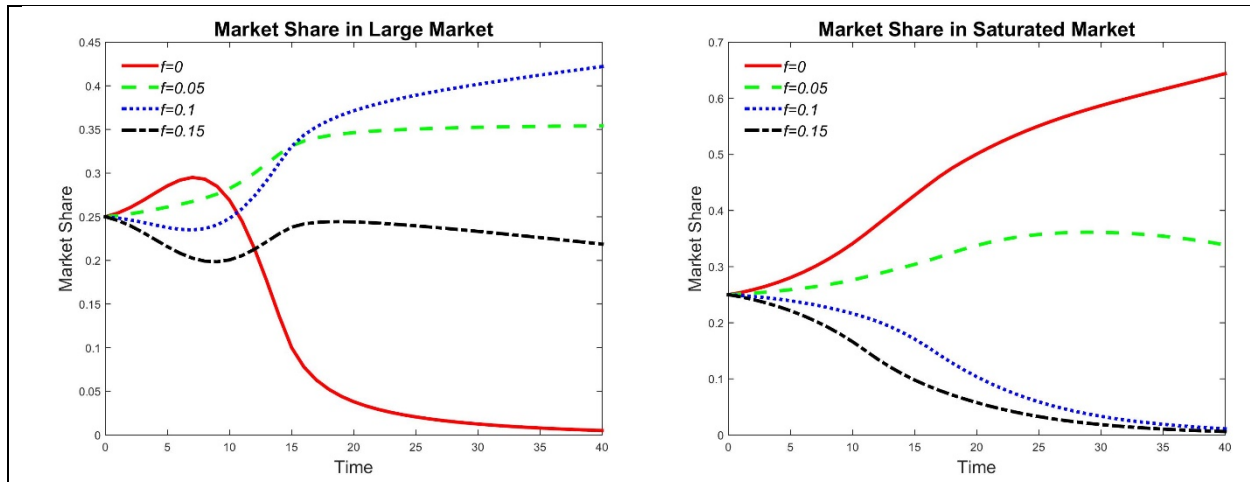


Figure 7- Competition among four firms under a) A large market that enables short-term growth for all the firms b) A saturated market where competition is a zero-sum game

A rational view of the firm assumes that managers foresee these dynamics and select their allocation policy after accounting for the competitive reactions of other players. This view leads to a game theoretic understanding of competition in which the Nash equilibrium, the set of firm policies from which no firm can profitably deviate, determines the normative outcome of the competition. This equilibrium could be numerically estimated using an iterative approach and building on the identical structure of the firms, which leads to equilibrium solutions symmetric for all firms. In this method, selecting from initial arbitrary policies and identical capability positions, firms are simulated in a competition and one firm's allocation policy is optimized keeping the others constant. In the next round all firms adopt the policy followed by the optimized firm in the previous round and again one firm optimizes its policy, and the iterations are continued until there is no change in the winning policy, which is the Nash equilibrium for the competition. Using this method and exploring various competitive settings, I found that in many numerical examples the most myopic policy ( $f=0$ ) dominates the other strategies. Specifically, firms in a zero-sum competition, even if they know that dynamic capabilities will boost productivity and output, find it risky to invest in those capabilities. In the very short-term those investments distract from boosting output, and thus lead to a loss of market share to competition. That, in turn, reduces the resources available to the firm for future investment, and leads to a vicious cycle of deteriorating performance and declining resources for investment in both types of capabilities. This pressure is harshest when market is saturated and there are many similarly sized competitors. In such settings any long-term investment by one firm is taken advantage of by other firms, triggering the demise of the focal firm.

To summarize, the discussion so far identifies two interlinked mechanisms that moderate the tradeoffs between short-term and long-term investments. Comparing with a base case in which organizational effort available for investment into capabilities is fixed, these mechanisms shift the preferred allocation towards short-term focus. First, when effort is endogenous, i.e. generated based on past performance of the firm, allocation of resources between operational and dynamic capabilities regulates the relative strength of short-term and long-term growth loops. A shift towards operational capability investments is supported by endogenous effort because that shift strengthens the fast-acting short-term growth loop

and allows the firm to see faster growth and thus more effort available for future investment. The second mechanism builds on the endogeneity of effort and brings in the impact of competition. When competition is relentless, firms that try investing in longer-term capabilities are more likely to see short-term decline in their output. This decline in turn erodes their effort available for later investment and thus can reduce the chances of future investments in both types of capabilities. Therefore market pressures promote firms with a focus on short-term performance. In fact, if firms foresee these dynamics, they are likely to significantly reduce their investments in dynamic capabilities, and in extreme completely abandon those capabilities in favor of ad hoc problem solving. Figure 8 summarizes the shifts in optimum capability investment allocation that ensues the consideration of these two dynamics. Starting on the right with the allocation fraction maximizing firm's performance with fixed effort, consideration of endogenous growth leads to a lower preferred investment in dynamic capabilities (S1 Shift). Adding competition further reduces the best performing allocation fraction, and the extent of this reduction depends on the competitiveness of the market and the level of rationality of the firms involved, with those in the most competitive markets and the most rational players allocating the least effort to dynamic capabilities.

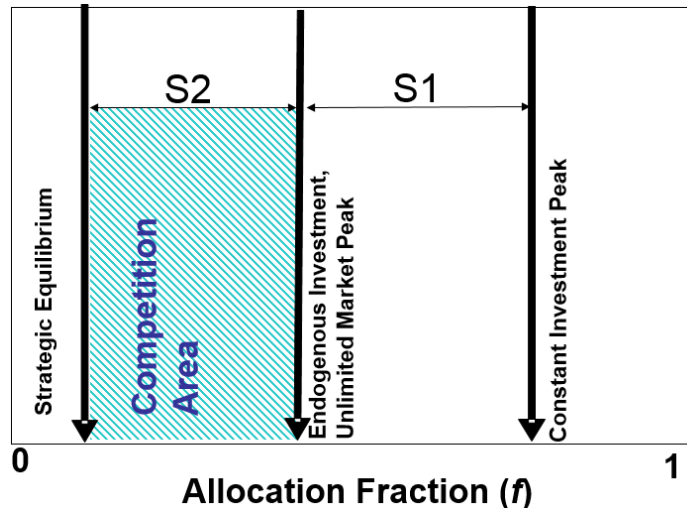


Figure 8- Overview of shifts in efficient allocation policy. Figure adopted from (Rahmandad 2012)

### Robustness and boundary conditions

Extensive sensitivity analysis, reported in the original paper, builds confidence in the key findings of the study. Specifically, besides testing parametric sensitivity, various functional forms and assumptions for the performance function, for the relationship between capabilities and performance, and for delays in re-investing profits are also explored. These analyses inform the strength of the mechanisms we discussed under different market settings and scenarios and allow for a more nuanced appreciation of the tradeoffs involved.

One factor that has some influence on the results is the delays in re-investing profits into capability building. Adding those delays slows down both reinforcing loops and thus reduces the preference for short-term growth loop over the long-term one. With delays the core mechanisms are active, but may be less powerful depending on the length of the delay.

Two other structural assumptions also prove relevant. First, the base model assumes increasing returns, as doubling the effort available more than doubles the performance. Reducing the returns on operational capabilities so that overall the firm faces decreasing returns to scale will reduce the potential growth rates. Slower growth slows down the relative benefits of the short-term growth loop and weakens the core dynamics. A second mechanism relates to time compression diseconomies, i.e. the increasing inefficiency of attempting to build capabilities very fast. Capturing this factor will also slow down overall growth as the firm becomes bigger and thus finds it more costly to invest in either capability. It also makes unbalanced investments in a single capability more wasteful than before, weakening the preference for a short-term focus and reducing the shifts we observe between fixed and endogenous effort. Both these effects change the magnitude of the shift in efficient allocation, but do not change the qualitative results.

Finally, initial capability endowments for firms in competition may matter. Various firms typically enter competition with different initial capability levels, and those initial points may set them up for success or failure. In fact firms that by luck have higher initial capabilities can afford to mis-allocate effort, i.e. use allocation policies different from the most efficient, and still dominate the market. However this result is contingent on existence of increasing returns in that market, so that the benefits of initial size and the resulting period of growth propels these firms ahead of their otherwise more well-managed competitors. At some point the additional benefits that come with size will allow these firms to outperform the competition despite allocating efforts inefficiently. However, similar dynamics are not observed when competition is in markets with decreasing returns, where ultimately the winners are the ones with the most efficient allocation policy.

## Implications

A few implications follow the basic dynamics discussed. First, while there is strong theoretical and empirical support for the idea that many managers under-invest in long-term capabilities due to dynamic complexity and learning challenges, other reasons for perceived underinvestment could also exist. The current study shows a significant gap between the allocation policy that is efficient under fixed effort and what is optimum when growth is endogenous and firms are in a competition. Because many case studies of capability investment tradeoffs are at the single firm level and do not consider the growth and competitive implications, the two mechanisms may be conflated. Some firms may be seen as under-investing in dynamic capabilities, while their allocation decisions may be forced by competitive pressures. In fact the standards for judging the quality of management should be contingent on the growth stage and market setting of the firm: when major growth opportunities exist, or competition is severe, then an increased focus on the short-term may be justified. On the other hand in more stable or less competitive markets firms will have the opportunity to grow their dynamic capabilities.

The results inform the contingent value of dynamic capabilities. An interesting hypothesis, which goes counter to many arguments in dynamic capabilities literature, is that more dynamic markets may require *lower* investments in dynamic capabilities. If market dynamism is seen as the speed by which various capabilities erode, then more dynamic markets lead to faster erosion of both operational and dynamic capabilities. Faster erosion of operational capabilities requires more investment in them, while faster erosion of dynamic capabilities favors ad hoc problem solving for building the operational

capabilities. Thus the net effect is a reduction in returns from dynamic capabilities, and a shift towards ad-hoc problem solving.

Given the challenges of sustained investments in dynamic capabilities, one may wonder what conditions allow for building those capabilities. First, note that investments in dynamic capabilities are generally expected to be much smaller than those in operational ones. The latter provides the core operations of the firm and engages the largest number of employees. So investing a small fraction of effort into dynamic capabilities is not surprising by itself. Moreover, a few factors can encourage investments in dynamic capabilities. First, firms that maintain a buffer between performance and investments (e.g. a significant cash reserve) can benefit from the resulting stability as well as opportunities to build dynamic capabilities even in competition. Moreover, dynamic capabilities are also more valued in firm structures and sizes that include decreasing returns to scale and time compression diseconomies, limiting the growth opportunities for the firms and promoting balanced investment portfolios. Given that during their life cycle firms pass through various stages, market sizes, buffer levels, and competitive settings, they face periods during which investments in dynamic capabilities are vital, and periods in which a focus on short-term is more viable. The ability to identify those investment opportunities and the leadership to implement the required changes may distinguish highly successful managers from the rest.

## **Engaging Other Academics**

This research was largely conceived and performed with the strategy audience in mind. A few of my other research projects also target academics outside of the System Dynamics community (e.g., Fallah-Fini *et al.*, 2014; Rahmandad, 2008, 2014; Rahmandad *et al.*, 2011; Rahmandad and Repenning, Forthcoming). I have found building SD models that target non-SD academics to be rewarding and challenging, and in the next section I share some of the experiences that may be useful for others who contemplate this path. I start with discussing specific modeling tools and assumptions that relate to the research discussed in this paper, then offer my suggestions for how to better connect with other academic audiences, and close with a discussion on why I think it is fruitful to make the effort and create those connections.

## **Modeling Tools and Assumptions**

The assumptions and tools used in this research departed from those in classical system dynamics projects on a few fronts, requiring trial and error learning on my part to find viable alternatives to the well-established SD practices. First, typical SD projects start with concrete empirical observations, often from one or a few case studies, and generalizations are followed based on the learnings from those cases. This approach allows modelers to stay close to the empirical phenomenon and motivate their formulations based on tangible examples. In contrast, the current project, while loosely motivated by a case, targeted a theoretically-focused audience and sought to build a generic model from the beginning. While being a student of system dynamics I have a preference for starting with concrete and practical problems, many academic audiences mainly appreciate the generalizable theoretical findings that can shed light on a larger category of problems. This preference is in line with Forrester's emphasis on modeling a category of problems, the case at hand only being one example of that category (Forrester,

1969), but some modeling communities may tackle that goal starting primarily from theoretical constructs and mechanisms.

In the absence of guidance from case examples or project clients, I had to use other criteria to settle on the level of aggregation, selection of stocks, various formulations, and the content of analysis. Three such guidelines proved helpful. First, it was critical to focus the modeling on only capturing the key mechanisms of interest: tradeoffs between short-term and long-term, and how they are influenced by endogenous growth opportunities and competition. Any model construct that did not directly contribute to those mechanisms had to be left out. While the clarity of mechanisms increased with the progress of modeling, it took some discipline to avoid adding pieces to the model that were tangentially relevant but not core to those mechanisms. The second guiding idea was building on existing theory in strategy. The basic building blocks of the model in this paper have been discussed in the resource based view. For example prior work has conceptualized firms as bundles of capabilities and resources, and has introduced the various levels of capabilities that include operational and dynamic ones. Using these ideas was essential both to connect with that audience and to benefit from robust concepts they had developed. A side benefit of grounding the modeling in theoretical constructs is that the model can also reveal the potential inconsistencies and shortcomings of existing theory (e.g., See Sastry, 1997). For example the modeling work here showed that more dynamic environments do not necessarily call for more of dynamic capabilities. If anything, higher erosion rates of dynamic capabilities may actually favor ad hoc problem solving. Finally, in the absence of direct empirical guidance for formulation selection, extensive sensitivity analysis was needed to increase confidence in the robustness of the results. Therefore I had to test the results under various alternative assumptions about capability and performance formulations. To summarize, in the absence of a guiding case study I had to focus on only capturing the mechanisms of interest (and not more), ground the work in theoretically well-known concepts, and enhance confidence in the results by extensive sensitivity analysis.

A second departure from classical SD came in selecting the basic assumptions about managerial decision making. Typically the decision rules in a SD model are formulated based on empirical cases at hand. These decision rules are at the heart of any model-based study and in the absence of such a case it would be hard to justify any single decision rule in abstract. As a result I summarized the main decision rule into a single parameter,  $f$ , and studied the implications of various levels of that parameter. This conceptualization brought forward the value of  $f$  that maximized a firm's performance as a clear benchmark which could be tracked across different scenarios. This approach does not assume that managers are rational and follow the efficient decision rule. Instead, it side-steps the problem by analyzing a range of assumptions on the level of rationality. This alternative also allows a larger audience to appreciate the implications of the paper and does not provoke objections for or against the rationality assumption. On the other hand, by exploring various assumptions on the level of rationality of firms in competition, the paper allows for assessing the sensitivity of findings to this core assumption. Such approach to modeling managerial decision making is only possible when models are very simple and decision rules could be simplified into a couple of parameters, yet another reason to keep such theoretical models very simple.

A third, methodological, departure from traditional SD came in the form of applying game theoretic concepts in this research. Such application followed from the openness to considering the rationality assumption (the point above) and required some tools not commonly utilized in the SD literature. To be most rigorous when applied to typical SD models, which includes continuous time dynamics,

competitive solutions should be implemented as differential games. Full analytical solutions to this category of games is often not available, and when available, the analytical complexity of exposition may detract from the basic insights and intimidate a large portion of potential audience. Therefore I opted for using a numerical method for solving a simpler, static, version of games and relegated much of the implementation details to online appendices of the original paper. This method is only approximate and does not provide proofs for uniqueness of the solutions found, nevertheless, when it works, it can provide an attractive balance between rigor and accessibility of the results. Many SD studies include competition among various players, and thus can utilize this basic idea, potentially in assessing the robustness of findings to alternative rationality assumptions.

### **Writing for Other Academics**

This research was conducted and written up with a strategy audience in mind. For a SD practitioner writing for non-system dynamics audiences comes with additional costs and many pitfalls, yet it is necessary for many academics in the SD community, and for the health of the field in the long-run. In his Forrester award acceptance paper Nelson Repenning has nicely articulated some key guidelines for succeeding in this task, which I find worth repeating here (Repenning, 2003):

- Ground your work in the language and literature of your audience
- Develop simpler models
- Build intuition about the link between structure and behavior
- Target scholarly communities who are interested in the phenomenon you are studying (and not only in modeling methods)

I have found these guidelines to be right on target, and also have benefited from advice by another awardee, Brad Morrison, on how to conduct collaborative interdisciplinary research (Morrison, Rudolph, and Carroll, 2013). In practice, I have found it helpful to further operationalize these guidelines using concrete rules of thumb to help assess my progress and allocate time more efficiently. Here I summarize some of these heuristics.

**Name the top ten researchers in the target community-** Getting into a new field of inquiry is no simple task. Not only one needs to understand the phenomenon of interest (be it product development, robotic surgery, or climate change), but also we need to get conversant in the language and concepts used in the target academic community. In fact, it is not clear from the outset who this community is. For example there may be a dozen such communities who write about product development, but use different basic assumptions, methods, and publication outlets. Each such community typically consists of a few dozen researchers spread across different universities who regularly meet each other in small conferences, review each others' papers and promotion cases, and have similar research interests. It is extremely helpful to find a community with a good fit before we get too far into our research project, and definitely before we start writing the results of our modeling work. This will require exploration and talking with experienced researchers in the related field(s), and looking at review papers can help us identify relevant communities. We should select a community whose interests in some way overlaps with our model. That overlap may be either in the empirical phenomenon of interest (e.g. supply chain management, epidemics, cyber-security, project management) or the theoretical framework they use (e.g. resource based view, market design, institutionalism). Note that we also need to ensure the basic



assumptions of the target community are not directly opposed to those we use in our modeling work. By the end of this phase we should be able to name the top ten researchers who are at the center of our target community.

**Read until we know 50% of the citations we see-** Once the target community is identified (typically in the form of a few central researchers), we need to read that literature to learn the basic concepts and assumptions used by this community. It is in this step that the 50% rule of thumb is a helpful stopping criteria. Only once we have read the majority of citations in the core papers of a literature, we will be able to frame our research using that vocabulary and drawing on the theoretical framework of the target community.

**Frame the research for our audience-** Having identified our target community and learned about their concerns and interests, we need to identify the “gaps” in their literature, and motivate our contribution within their conceptual framework. Depending on the type of community (e.g. phenomenon focused or theory focused) different types of “gaps” may be more amenable to our purpose. Some communities are focused on solving policy problems in a specific domain, and they are usually the easiest to connect to for system dynamics contributions, given that typical SD models are built around addressing a policy question. Even for those communities, one should be able to demonstrate how the new model compares with what has been done in the literature, what different mechanisms and/or empirical set-ups are explored, and what new insights have emerged. For more theoretically leaning audiences the gap often takes one of a few forms; an empirical puzzle is an attractive angle when an empirical observation contradicts the predictions of existing theory, and thus requires new theoretical elaborations. We can then put forward our contribution as solving this empirical puzzle through modeling. Another avenue is to identify under-appreciated mechanisms, i.e. showing how relevant mechanisms absent from prior research are important for outcomes of interest in a field. My research on capability dynamics under resource endogeneity and competition falls into this category. Much empirical research in social sciences uses a third angle for framing: testing existing theory. In this approach the quantitative strength of a few causal mechanisms are empirically estimated using a large number of data points. If SD models are estimated based on large samples of cases, this framing could be applied (Hosseinihimeh *et al.*, 2015; Parvan, Rahmandad, and Haghani, Forthcoming) but this approach is not very common in the SD literature.

**Do not exceed the complexity of existing models-** We want to keep our models simple, but simple is in the eye of beholder. Some communities do not build formal models, others are used to relatively simple analytical models, and yet others may build very complex dynamic models. For example the organization studies and strategy audiences are used to relatively simple, insight-focused models, with a handful of key variables. Others, such as those conducting policy analysis to control epidemics, are used to very detailed individual based models with millions of variables (Eubank *et al.*, 2004). When reviewing our work for publication, members of these communities draw on their past experience to evaluate our models. If it is much more complex than what they have seen before, they are likely to give up on understanding the model and see it as a black box which cannot be trusted. Therefore we need to adjust our model’s complexity based on the models we find in the target literature.

**Emphasize what is in common with prior models-** While we are ready to list the distinct features of our model, it is easy to forget that the audience’s confidence is best built by seeing familiar components and formulations. Both in formulating our model and in presenting it we want to highlight the parallels with

existing published models in the literature to build confidence and reduce the cognitive load of readers who are trying to understand our work. In fact we do not want to introduce a novel feature unless that feature is central to our new contribution. For example many audiences are unfamiliar with table functions, so it is better to use alternative analytical functions in many cases.

**Test our understanding of mechanisms before writing-** Clear explanations that connect model behaviors to the underlying structures are at the heart of any model-based contribution. Modelers may use a variety of formal and informal tools for gaining that understanding (Ford, 1999; Kampmann and Oliva, 2006; Mojtahedzadeh, Andersen, and Richardson, 2004). Yet more often than not our understanding of those linkages are less than complete. A good test for assessing our understanding is to predict the changes in our key outcomes (e.g. strength and periodicity of oscillation in a supply chain model) if we change a series of model parameters up/downward and test those predictions by conducting the indicated sensitivity analyses. It is critical to write down our predictions before running the test, and then go through comparisons of our predictions vs. observed results one by one. Only when all our predictions are correct, we can feel confident about our understanding of the model's core mechanisms.

**Conduct counterfactuals-** Convincing and interesting structure-behavior relationships are key to successfully communicating the insights of our modeling projects with non-modelers. To this end, model analysis is a great venue to benefit from the power of scientific method by formulating hypotheses and testing them until we know why the model behaves the way it does, and have the evidence to back it up (Homer, 1996). Most explanations of model behavior are in the form structure "X" leads to behavior "Y". X could be in the form of a feedback loop, a set of exogenous drivers and intermediate causal links, or more complex combinations of model structures, and behavior Y is the model output we are interested in, such as sales trend. In search for explaining Y in our model it is often easy to come up with an X that sounds plausible, and declare victory. Yet our explanation is only a hypothesis that needs to be tested through the scientific method. We need to find a way in our model to remove X, and ONLY X, and then see if behavior Y changes significantly. If it does, we would have more confidence in the validity of our hypothesis, if it does not, we should rethink our explanation. The key factor in designing the test is to only change X, or otherwise we may inadvertently also remove other explanatory mechanisms and confound the impact of X on Y with the impact of those other mechanisms on Y. For example in testing the impact of a feedback loop we need to isolate a causal link unique only to that loop and freeze it mid-simulation (i.e. fix the output variable of that causal link) at the time we want to explain the behavior Y (Ford, 1999). Often we think we fully understand our models and we have plausible explanations for a behavior that strengthen our confidence; those explanations may even convince others. Yet doing the counter-factual tests are indispensable for building our understanding, convincing a larger audience, and being honest about the results. We may not have the space to report all those counter-factual tests in a paper's body, and could relegate some to online appendices, yet they should not be skimmed. Compared to those conducting empirical studies, we modelers have the luxury of cheap and quick experiments, so it is even more critical that we follow the scientific method!

### **Why Engage Other Academics?**

So far I focused on practicalities of communicating with academic audiences outside system dynamics, leaving the critical "why?" question to this last section. Writing for other academics calls for a major

commitment of time and energy and cannot be adopted lightly. I therefore close with a few personal thoughts on why we may want to seriously engage other academic communities in the process of doing SD research.

First, I think this engagement can enhance the potential of SD for tackling complex real-world challenges. Unlike some engineering problems, real world challenges SD targets are not only dynamically complex, but also include diverse human actors. Solving such problems entails both a dynamically sound solution, and an implementation process that engages stakeholders, communicates with them in a language they connect with, and leads to changes in mental models of decision-makers (Andersen, Richardson, and Vennix, 1997; Forrester, 1985; Sterman, 2000; Vennix, 1996). Many academic communities may not be aiming at solving practical problems nevertheless they impact the solutions and implementations. They define the concepts and assumptions many policy-makers draw upon (e.g. adoption of economics vocabulary and assumptions in the development policy debates), are gatekeepers to policy discussions in some other settings (e.g. public health), and are consulted as experts and symbols of legitimacy when competing solutions are contemplated. The net effect is that in many policy problems, even if ones understanding of the problem is objectively better than the alternatives discussed by others, it is not effective to go against academic experts as an outsider, and less so, to be ignored by them. By understanding and appreciating the conceptual framework and assumptions of impactful academic communities and communicating with them effectively we increase the legitimacy and impact of our work. In the longer run we also influence those assumptions and concepts and help shape those academic communities we are working with.

Second, our understanding of the problem can rarely become better than the alternatives in other fields, unless we seriously study those alternatives and build on those. There is a community of researchers interested in virtually any problem a system dynamicist may tackle. Web of Science lists more than 3.4 million academic papers published only in 2014, and the body of research is growing ever faster. Thus in today's world it is unreasonable to expect any single researcher to come up with new insights without building on the large body of work that has come before them. On the other hand, by building on what has been done in other academic communities, we enrich and expand the knowledge and tools of the system dynamics community and can train better modelers.

Third, professional growth and success of SD modelers and SD as a field partially depends on the strength of our relationship with other academic communities. Individual academics who practice SD need the support and recognition of their academic colleagues, often including many from outside system dynamics, to thrive in their professional career. Access to data, friendly reviewers for papers and promotion cases, new faculty positions, and opportunities for building new programs all depend on the respect and support of other academics. Such mutual respect is only built through serious engagement of those other academics, building on their work, and showing them the value of SD in tackling problems they care about. Promotion and recognition of individual SD modelers is also critical for the growth and health of the SD practice outside academia, where academics, through student training, are the bottleneck to the supply of capable modelers. Finally, in the process of writing for other academic audiences we build professional relationships with others who care about the phenomena we are interested in. Those relationships could grow to be among the most rewarding aspects of one's professional career and are one more reason to write for other academics. Overall, while writing for other academic communities requires a serious commitment, I think it is both necessary and rewarding for many academic system dynamicists, and for the field in general.

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