Moving from Mental Models to Technological Rules for Evaluating Impact

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Last revised: August 3, 2018

* Presenting author. The 36th International Conference of the System Dynamics Society, Reykjavík, Iceland. August 6-10, 2018.

This paper takes up the question of we might position the utility of system dynamics scientifically rigorous by re-examining some of the core tenants of the field. The paper reviews the history of this dilemma between utility and scientific rigor, and situates the issue within a larger scientific and philosophical discourse in applied and design science about how to develop and use results to make a difference in the real world using mathematical models. The paper builds on prior work and argues that we need to shift from a focus on mental models to a more clearly defined set of technological rules developed with a progressive program of research. Implications are discussed for system dynamics practice, research, and more broadly, implementation and translational sciences.

Keywords: mental models, technological rules, implementation science, translational science, philosophy science, philosophy of technology

> *Scientists often speak of using models but seldom pause to consider the pre-suppositions and the implications of their practice.*
>
> Max Black (1962, p. 219)

> *The half-life of an empirical proposition may be great or small. The more open a system, the shorter the half-life of relations within it are likely to be.*
>
> Lee J. Cronbach (1975, p. 123)

> *The practitioner does not event test* ***things****, such as tools or drugs, save in the extreme cases: he just uses them, and their properties and their efficiency must again be determined in the laboratory by the applied scientist.*
>
> Mario Bunge (1967, p. 128)

1. Introduction

The novelty of system dynamics in the applied sciences like public health, social work, and medicine forces us to revisit questions in philosophy of science and technology that are central to the applied sciences. For example, in asking, "What is the utility of system dynamics models?" or "How do we know a model is valid in system dynamics?" we are often asking questions about system dynamics as a method, and by implication, also raising more general questions about utility and validity in social research in general.

In contrast, widely used methods rarely if ever get this type of scrutiny. For example, we rarely hear a serious question asked about the utility of regression analysis or the use of validated measurement scales. We might ask specific questions about what is the best approach or the suitability of the approach in our research design, but we are not re-examining fundamental assumptions at each turn. This is not a bad thing. A scientist who routinely revisited the foundations of every concept in their toolkit would hardly have any time to get on with science and its application.

However, this does not mean that there are no discrepancies that warrant re-examination of assumptions in our underlying philosophy of science and technology in our respective fields. It is just that these discrepancies tend to take a back seat until a crisis leads to a scientific revolution (Kuhn 1962) or the emergence of a technology that disruptive an industry (Christensen 2003). What this does suggest is that working out these more fundamental issues, while necessary to advance system dynamics as a method, might not be all that unique to system dynamics and that the solutions can sometimes have general implications for entire fields. And, in some ways, this is part of the intellectual pleasure of studying and working with novel methods, i.e., not to find oneself in some esoteric niche of methodological research, but to be at a place of discovery that might have generalizable implications.

It is in this spirit that this paper takes up the issue of developing a clearer statement about the application of system dynamics to solving real world problems, both for the field of system dynamics, but more interestingly as it speaks to a much broader set of issues that pervade much of the applied sciences today (e.g., in the translational, transdisciplinary, and implementation sciences in medicine, public health, and social work). The main argument put forward is that system dynamics was developed to address the ontological and epistemological challenges posed by what Herbert Simon (1996) called the artificial sciences where the main outcome is a set of (improved) technological rules (Bunge, 1967; van Aken, 2005) for the practitioner. Simon (1996) drew a distinction between this and the basic sciences, where the basic sciences were about explanation of some phenomena, and the design sciences were about the creation of something new (e.g., see Akkermans and Romme 2003). But this alone, it is argued, isn't sufficient for resolving the dilemma. In particular, simply asserting something as scientific doesn't make it so. What is missing is a more objective construct that we can use to build a progressive program of research (Lakatos

1970) for the discovery and evaluation of methods for the discovery and evaluation of technological rules using system dynamics.

The paper is organized as follows. The background section begins with an overview of the argument followed by a summaries of prior work on mental models in system dynamics; distinctions between science, applied science, and design sciences; and, theories of action. We then formally introduce the notion of technological rules followed by typology of changes in technological rules in system dynamics modeling and proposed research agenda. The paper closes with a summary of the implications for the field of system dynamics practice, research, and implementation science.

2. Background

The issue of the application and utility of system dynamics has been a topic of discussion over the last several decades, not so much as whether it should have application, but why the application of system dynamics has not had more visible impact in the world. System dynamics was founded by Forrester in the late 50's as a method for solving problems. But, even Forrester of late spoke to the direction of the field as straying away from practical problem solving toward a more abstract practice in seeking greater academic legitimacy within universities (Forrester 2007a, b).

Others had a different explanation (e.g., Homer 2013). The move toward more scientific modeling, grounded in data and the extant literature, helped establish system dynamics in academic fields that trained future generations, and in turn, provided the basis for expansion into other fields like public health, health services, and social work. From this view, the problem of not having more of a visible impact was due to the prevalence of "unscientific" modeling. The policies that have stemmed from this view range from limiting the visibility of low quality work to creating certifications and accreditations of academic programs. Yet many of the efforts to anchor system dynamics modeling in more scientific work often appear to conflict with consulting practices that deal more directly with the pragmatics of meeting clients' needs and have demonstrated value to the client. The push to do more scientific modeling has therefore had the unintended consequence of often leaving practicing consultants feeling excluded from the field, and pushing further in this direction is likely to narrow rather than grow the field of system dynamics.

The dilemma for the field of system dynamics has been whether we need to return to our pragmatic ("unscientific") problem solving roots or move forward toward more "scientific" modeling as defined by rigorous academic standards of peer reviewed research and scholarship. This dilemma is significant for the field of system dynamics because it cuts to the core in how we see and attribute the outcomes, evaluate the impact of our projects, and ultimately communicate the value of system dynamics to a wider audience including our clients and our colleagues in academia. It's also a dilemma that has persisted for decades despite ardent efforts by various to settle the debate. And, it has implications for how we develop and promote the field of system dynamics in the future. However, most have largely ignored that underlying issues are of a more general questions concerning the philosophy of science and technology that appear across the applied and design sciences.

Historically, we have placed the most emphasis on trying to answer these questions by more clearly documenting the process and model (e.g., Andersen and Richardson 1997, Hovmand et al. 2012, Martinez-Moyano and Richardson 2013, Rahmandad and Sterman 2012, Vennix, Andersen, and Richardson 1997), evaluating the impact of our work (e.g., Rouwette et al. 2011, Rouwette, Vennix, and Mullekom 2006, Rouwette, Vennix, and Felling 2009), and focusing on evaluating changes in mental models (e.g., Scott, Cavana, and Cameron 2013, Gary and Wood 2011, Doyle and Ford 1998). Of these, evaluating changes in mental models has proven to be one of the more vexing problems to study and bring into the routine evaluation of system dynamics practice.

2.1 Mental Models

Forrester (1990) was quite clear that the purpose of system dynamics was to help improve the mental models of decision makers over what they would have otherwise used. Yet despite this focus and clarity, the notion of mental models as a way of evaluating the utility and impact of system dynamics has proven much more difficult beginning with a basic operational definition of mental models.

Axelrod (1976) was perhaps one of the earliest scholars to bring attention to the role of cognitive representations that decision makers used to represent situations, but the term 'mental models' as cognitive representation that problem solvers used was introduced and developed by Johnson-Laird (1983). Within system dynamics, Richardson et al. (1994) sought to define the foundations of mental models research in cognitive psychology, while Doyle and Ford (1998, 1999) developed more formal operational definitions of mental models that could be used to evaluate the impact of system dynamics interventions. However, these efforts have largely been difficult to implement and scale because the approaches are too resource intensive for regular practice and tend to be reactive measures[1] in constructing mental models through an elicitation process. For example, asking questions to elicit an underlying mental model can lead the respondent to construct a mental model much like the way that asking someone to explain their behavior often leads to the construction of reasons after the action. Hence, the reactivity of mental model measures have

[1] Measures are reactive when the individuals alter their response as a consequence of measurement. Problems with the reactivity of measures are common in areas involving surveys that seek to measure changes in learning and reasoning.

therefore posed a challenging threat to the external validity of studies to seeking to assess changes in mental models.

Most recently, Gary and Wood (2011) were able to address some of these limitations through a combination of interactive learning environment and survey tools that were able to empirically test a set of propositions about mental models. While promising for advancing research on mental models, the approach doesn't lend itself directly to clarifying the target of system dynamics interventions in regular practice. That is, one might be able to rigorously demonstrate the utility and scientific basis of system dynamics for changing mental models in a research design, but this does not address the larger concern driving the field around the external validity of these findings in regular practice. Specifically, these approaches may help us clarify the potential efficacy of system dynamics interventions, but say very little about the overall effectiveness of system dynamics interventions in regular use.

Part of the problem, it seems, is that the notion of mental models, however rich and well anchored the term is within system dynamics and systems thinking, rarely functions as a clearly defined term with a corresponding operational definition that can be used to evaluate change. As a consequence, we stand as a field on soft foundations when trying to clearly articulate the benefit of system dynamics to our clients and scientific colleagues. The problem isn't that mental models don't exist, but that a field based on the central argument of changing mental models still has not adequately developed a scientific way of ensuring the correspondence between our intervention, mental models as the main target of our intervention, and the outcomes. As a consequence, we are often find ourselves attributing outcomes of our interventions without a rigorous explanation that would satisfy external audiences.

2.2 Science, Applied Science, and Design Sciences

The problem of grounding an applied field in science is not new, nor is the despair that can arise from realizing the ontological difficulty of the task. In his presidential address to the American Psychological Association, Cronbach (1975) lamented the problem of how fast empirical reality changed relative to the discovery and implementation of scientific analyses. The civil engineer and historian Petroski (2011) noted something similar when describing how rare it was to see scientific advances translate into applied technology and the length of the delay when this happen as being on the order of 20 years or more. Petroski attributes this to a fundamental misunderstanding about the sequence of discovery between science and engineering. The prevailing view, at least in the United States, has been that basic scientific discoveries precede engineering applications in technology, a point that is illustrated whenever someone attributes putting a man onto the moon to science as opposed to solving a series of engineering problems.[2]

For Petroski, the relationship is reversed. Engineers work to solve a series of problems, often grounded in what is known in terms of basic principles. However, discovery often happens when there are some limits to a new technology that generate novel scientific questions (e.g., what's the explanation for lift in powered flight?) that are necessary to understand in order to further advance the technology, and that is where the real breakthroughs from science back to engineering happen.

While the engineer may innovate and raise new questions, it's important to stress that, at least for the modern engineer, the practice is grounded in science. What arguably distinguishes the engineer from the hobbyist isn't the years of formal training or professional certification, but an underlying

[2] This is arguably changing both current popular skepticism and challenge to scientific knowledge and in the rise of the technological hero to solve problems.

understanding and commitment to scientific principles and methods. But, the essence of engineering isn't the science, but a focus on problem solving using scientific methods.

The connection between basic science and problem solving in engineering is often easier to see in these principles, and hence there is a tendency to see engineering as an applied science where basic scientific laws are extended through application. This can certainly work and be productive in many situations. For example, behavioral economics is largely organized around finding law-like patterns of human behavior and applying these results to shape subsequent human behavior. However, it becomes much more limited when we move outside the realm of natural phenomena that are the object of study in the natural sciences.

Organizations, for example, while having to still conform to basic principles from natural sciences, have many more degrees of freedom through the socially constructed terms, relationships, and categories that can emerge through social interaction, institutions and culture. Simon (1996) described this distinction as the difference between the basic sciences explaining some natural phenomena, the applied sciences seeking to apply that knowledge, and the design sciences that focused on creating something that did not yet exist.

2.3 Theories of Action

The notion of theories of action as distinct from theories of explanation or theories of prediction have been around for some time, but took on greater interest in the late 1960's and 1970's as scholars sought to understand what distinguished the effective from the ineffective professional. Schön (1983) was among the first to systematically study and try to develop more effective professionals by drawing the contrast between their espoused theories of action and theories in use. In particular, he noted that the average professional tended to apply more of the same in responding to a discrepancy between goals and outcomes (which he called single loop learning), while the higher performing professionals also reconsidered the goals they were pursuing (double loop learning).

In business, this work catalyzed scholarship on the learning organization and organizational learning. In social work, medicine, nursing, and other helping professions, it influenced how professionals were being trained. For example, the use of process recordings in social work education, once common, asked students to record the behavior of the interactions, their espoused theory of action, and then analyze the recordings to describe their theory in use. By the 1980's and 1990's, however, reflective practice was gradually replaced by a move toward more empirically based theories of action including evaluation research and evidence based medicine, or more generally, evidence based practice in social work, public health, and education.[3]

However, much of the debate within system dynamics about how to improve the quality and impact of system dynamics modeling to solving real world problems has largely focused on the scientific rigor underlying the models. It's not that scientific models are not valuable in their own right as contributions to knowledge, but this alone does not solve the problem of how to conceptualize and evaluate the impact of a system dynamics intervention in a more rigorous way. To become more of an evidence based practice, we need to develop an approach where we can design and conduct the research and evaluation designs to determine what works, for whom, and with what kind of

[3] Note that this shift to evidence based practice also appears in management science. In particular, van Aken (Huff, Tranfield, and van Aken 2006) suggests that evidence based medicine might be a good model for management science to look to in the development of technological rules. However, some caution is warranted here as the effort to build evidence based theories of action, while effective within a biomedical research paradigm, tend to fail and be more difficult in the design sciences. In fact, Simon (1996) was quite explicit in drawing a distinction between the practice of medicine as an example of a design science and academic medicine, which he saw as having fallen into the same trap as other professional schools in seeking greater legitimacy within the academy by positioning medicine as an applied science.

impact. And here, we argue, we are limited because of our reliance on the notion of mental models as one of the main constructs in our theory of change. Specifically, if we see mental models as the target of our intervention efforts, then the methodological problem we face is that we have few options for rigorously assessing to what extent and in what ways mental models changed as part of our intervention. This becomes critical if we are going to have a progressive program of research where we can disentangle the nonspecific intervention effects that might stem for attributes of an intervention such as the reputation of the system dynamicists, group facilitation skills, or content expertise of the team from the more specific intervention effects such as the quality of the model, analysis, and interpretation of the results.[4]

If we can't rely on the notion of mental models for such assessments, what might be an alternative? In this paper, we situate the problem more broadly within philosophy of science and technology, and argue that one path forward is a program of research organized around the notion of Bunge's (1967) notion of technological rules as distinct, but deriving from basic scientific laws. Hence, the next section introduces the more formal notion of a technological rule as the main outcome in the design sciences for informing theories of action (van Aken 2005).

3. Technological Rules

A technological rule is a statement within a theory of action, that is, a theory about what to do in order to achieve some outcome (Bunge 1967). Technological rules generally take the form of "If you want to achieve A then do B" or "If the goal is C and one is currently in a situation D, then do E." Technological rules are prescriptive and conditional. Bunge (1967) notes that technological

[4] This notion of nonspecific versus specific effects is drawing a similar type of methodological problem that arose earlier in the design and development of evidence based treatments in psychology (Lohr, DeMaio, and McGlynn 2003).

rules are rules, not scientific laws, and formalizes the distinction as one where scientific laws imply a set of technological rules that can be effective, ineffective, or indeterminate in their outcomes. Specifically, if one has a scientific law of A → B, then this implies a foundation for a set of technological rules of the form, B per A (read as, "To achieve B, do A"), but also the various combinations and their negation (e.g., "To avoid B, don't do A" or ~B per ~A for short). Hence, the scientific law A → B implies the following set of four technological rules: B per A, ~B per ~A, ~B per A, and B per ~A.

Although a technological rule can be embedded within a cultural convention (e.g., "Chicken soup is good for the common cold"), a technological rule normally implies a remedy that is claimed to be better than common practice. However, the effectiveness of a technological rule can range from helpful to harmful, so the focus on evaluating technological rules often centers on assessing their foundations. In a scientific community, a technological rule based on science is superior to one based on superstitious beliefs. Hence, the expression or discovery of a technological rule does not, by itself, say much about its validity in a scientific sense. What the expression of a technological rule does do is provide a means for evaluating the rule scientifically.

Technological rules in the way we are using the term are then logical implications from a model. A model, in this sense, logically entails set of formal statements or technological rules.[5] Specifying the immediate outcome of a system dynamics intervention in terms of changes in technological rules then provides a way that we can compare the changes in technological rules with evidence from real world interventions. That is, we can both document how the technological rules changed,

[5] An interesting question to consider is how large the set of technological rules is from a given model, whether the set is finite or more likely infinite in nature, and how the utility of system dynamics might vary with the size of the models and, by implication, the set of technological rules entailed by a model.

and compare how those changes correspond with real world changes in performance. In doing so, we create the conditions where we can both find support in our claims that system dynamics interventions led to a change in system performance, *and* more importantly, reject claims when the changes in technological rules do not empirically align with changes in system performance.

Moreover, the form of technological rules closely aligns with people in a system talk about planned action like a treatment, policy, or program design.[6] The customer or audience for a model is generally preoccupied by some question about what to do for some situation, and it is relatively easy in practice to ask questions about how they conceptualize a problem or issue in these terms. In seeking advice, for example, we are already using the form technological rules. Hence, we expect that technological rules will not suffer from the same problems of reactivity as eliciting mental models does, and be relatively easy to implement both pre and postintervention to evaluate change.

4. Typology of Changes in Technological Rules

So far, we have argued for the use of technological rules as the primary construct of interest as a replacement the more elusive construct of mental models. In this section we illustrate the application of technological rules by presenting two cases where the project led to changes in technological rules, and then propose a typology of changes in technological rules. In doing this, we want to be clear that we are retrospectively coding the technological rules as they arose in the work as opposed to applying them prospectively or concurrently within a project. However, we do not see this as a serious limitation insofar as illustrating the feasibility of the approach. To the

[6] In fact, the utility of technological rules came from noticing the different way a basic scientists and medical researcher were talking with respect the same basic physiological system around a system dynamics model, and noticing the differences in how the model was being used.

extent that a retrospective analysis would threaten the validity of an analysis, one would simply need to consider the design of the coding procedure and build in established methods to minimize the threat (e.g., multiple coders working independently from the same transcripts or notes check for interrater reliability).

Perinatal Health Policy Simulation. This 3-year project consisted of a series of group model building workshops to inform the design of a results-based financing scheme to improve the health outcomes for the lowest 20% in poverty in a low-income country. Participants included policy makers, hospital health directors, physicians, public health workers, and technical experts familiar with policy design, health systems, and quality improvement. The initial country level policy was focused on maximizing institutional delivery ("To improve maternal and neonatal survival, focus all efforts on increasing institutional delivery"). One of the key insights from an early version of the model was that by doing this, the system was not going to reduce mortality, but only "shift where women and children were dying." This led to a recognition that they would have to investment in multiple places in the system simultaneously, for example, by improving the quality of institutional delivery ("To improve maternal and neonatal survival, increase both access to institutional delivery and quality of institutional delivery.") Toward the end of the project with a more complete and tested model, it became clear from the model analysis that this would not be sufficient for meeting the intended targets. Analysis of the model led to the discovery of a weak balancing loop of quality improvement that would need to be strengthened in order to achieve the desired outcomes, which led to the conceptualization of what was later termed quality improvement plus or QI+ ("To improve maternal and neonatal survival, invest in quality improvement in institutional delivery as a dynamic capability while improving access to institutional delivery.")

Changing Systems Youth Summit on Education Equity. This involved a 4-day workshop with approximately 40 area high school students in a peer led model to develop causal maps and identify potential actions/recommendations for improving education equity. At the start of the workshop, given that young people are often excluded from both the process of generating ideas about how to improve education equity and oversee their implementation as stakeholders, participants started with a few or no ideas about what could be done. However, by the end of the 4-day workshop, participants had generated a large and diverse set of ideas for improving education equity that ranged from including students in school governance ("To increase education equity, have students involved in school governance"), establish a regional education equity commission that would have equal representation of adults and students ("To increase education equity regionally, establish a regional commission on education equity with equal voice between adults and students", and more sharing of resources and innovations between school districts ("To increase education equity regionally, created and distribute a regional pool of resources for innovation in education").

In both cases, we are able to tie the changes to technological rules to process of using system dynamics as the changes should correspond to insights from the modeling process and/or analysis. As we have considered a number of projects, we have found it helpful to have a typology of changes in technological rules in mind as we work with clients and audiences by drawing a distinction between generation, shifts, refinements, reductions, and alignments of technological rules (Table 1). We don't see this types as mutually exclusive. For example, one can have a change that involves both a generation of one technological rule and refinement of another. Or, one can have an alignment of technological rules that happens through a reduction of technological rules being considered. However, these types are useful as they appear to have good face validity with early conversations with clients in determining what they want to see happen as a consequence of a system dynamics intervention.

Table 1 Typology of changes in technological rules (TR)

Type	Change in TR	Example
Generation	Generating new TRs	Using a model to create new strategies; eliciting actions ideas from a group of participants; generating a set of options for changing the structure of a system
Shifts	Shift from one TR to another TR	Shifting the focus of intervention from one set of existing alternatives within a model to another set of existing alternatives; shifting from a low-leverage intervention point to high-leverage intervention point in a model
Refinements	Refining an existing TR	Refining understanding of how an a change in the system needs to be implemented in order to achieve desired outcomes, e.g., sequence and timing of interventions;
Reductions	Reducing the number of potential TRs	Eliminates TRs that are being considered
Alignment	Alignment of TRs	Aligns a set of actors around a subset of TRs, e.g., through consensus deciding to focus on a specific part of the system

5. Discussion

There are some limitations in technological rules, but we generally see these as solvable. The first is a question about the with the addition of concept like technological rules is really only a more complicated way of stating something that already exists in regular use. For example, van Aken (2005) describes technological rules as a heuristic and the examples we have provided appear to resemble recommendations. If that’s the case, then this is simply another jargon term that should be avoided. However, it is in the more precise formulation of technological rules from Bunge (1967) logically tied to scientific laws that seems especially productive and puts the applied science of system dynamics on stronger foundations.

A second limitation is that while the form of technological rules is easier to implement than the concept of mental models with respect to the reactivity of measures, we have found some indications that eliciting technological rules can also be reactive when respondents do not have a clear goal in mind, but only a set of actions. In these cases, eliciting questions about technological rules is also prompting the formation and likely refinement of actions. As this may be higher in groups with a low efficacy, there is a risk of attributing the changes of technological rules to a system dynamics intervention when it may have more to do with simply eliciting goals. This might be addressed in future work by developing a version of technological rules that relaxes the logical form or developing instruments that pay attention to the sequencing or measure goal setting as a control variable for analysis.

Both of these issues we see as being addressed within a broader research agenda on the efficacy and effectiveness of system dynamics interventions framed around the study of technological rules.

1. *Develop and validate measures for technological rules*. This involves develop instrumentation and testing the instrumentation for reliability and validity with specific attention to the reactivity of measures, test-retest reliability, parallel forms reliability, and face and construct validity. Ideally, we would have a range of instruments from self-administered paper and pencil tools and online surveys to structured interview guides. Equally important would be establish the feasibility of using the measures for research and routine evaluation of system dynamics practice.

2. *Develop formal methods for generating technological rules from quantitative simulation models*. This is will likely involve defining the formal grammar of technological rules in a manner that support the efficient implementation in software and identifying equivalence classes for sets of technological rules that are essentially the same with respect to system dynamics. Ideally, this would lead to a unique set of technological rules that can be used

for comparing the equivalence of models (e.g., within a project over time or between projects).

3. *Extend formal methods of generating technological rules from simulation models to qualitative models*. System dynamics has a rich set of structure-behavior relationships that should make it possible to define a more general form of technological rules based on qualitative models (i.e., without simulation). For example, while it is generally impossible to reliably infer behavior of a causal loop diagram and predict where the leverage points are in a system from the graph analysis alone, it may be possible to generate a maximum possible set of technological rules or devise ways of ruling out technological rules. This would allow for a more formal way to evaluate the contributions of modeling at different stages, from initial conceptualization of the system to formal simulation and policy analysis.

4. *Determine the origin of changes in technological rules*. We are assuming that the changes in technological rules occur from the system dynamics intervention, and hence we expect that the changes would be tied to the process of conceptualizing the problem and building a model, or the model analysis and results. But, once we have a way to track changes in technological rules more precisely, we should also be able to get more specific as to the precise origin of the change within a process or analysis. For example, is the origin of a specific change due to a comment made during a presentation by the modeler, something a participant or client shared in a meeting that came from reflecting on the process, or an idea that was already formed outside the intervention, but system dynamics provided a forum for bringing that idea into the session? The specific answer to this can help determine

what it is we might want to do and emphasize more in both system dynamics practice and training.

5. *Evaluate whether the perceived utility of system dynamics improves with the implementation and evaluation of change in technological rules*. The overarching reason we first became interested in the idea of technological rules was to improve the perceived utility of system dynamics interventions. Indeed, that itself is a technological rule: "To improve the perceived utility of system dynamics practice, focus on identifying and changing the technological rules of interest to the client." Well, that can be empirically evaluated as well.

6. Conclusion

In this paper, we have argued that in order to better position the utility of system dynamics as scientifically rigorous, we need to reconsider some of the underlying presupposition of system dynamics within the context of system dynamics as a design science, and recognize the more general problem of translating scientific explanations into statements that can be implemented in practice. To do this, we have argued that we need to move from a focus on changing mental models through the process and analysis of system dynamics to a more explicit focus on changing technological rules.

In making this argument, it is important to be clear that we are not dismissing the value or foundational concept of a mental model. Rather, we are arguing that focusing on shifts in mental models is both too impractical to research and routinely evaluate to improve practice, and ultimately leaves the client and audience with a poorly defined outcome from a system dynamics project. Modeling at any level will surely promote what Richardson (2011) defined as the mental effort to think about systems or systems thinking, but this is typically too distal an outcome for most clients seeking a more fundamental systems insight. We argue the salient features of mental models in terms of utility are not the mental models themselves, but the technological rules we apply to achieve some outcome. Technological rules in this sense are both easier to identify before system dynamics interventions and more closely tied to the impact we expect in terms of changing how people act in a system.

The practical implications of this work are that it has the potential to offer a new path forward in how we frame and articulate the practical relevance of system dynamics to wider audiences without comprising scientific principles that should underlie system dynamics modeling, and instead strengthens the foundations in how we articulate our research, understood in the broadest sense from product development to applied and basic scientific research, to a wider audience of scholars, scientists, policy makers, and other decision makers, especially in the applied and design sciences. Future research in this area would focus on developing and validating practice and research tools for eliciting technological rules (e.g., in a pre-post surveys, interviews, group model building scripts) and tracking their discovery and development in a model building process.

What we must recognize, however, is that these distinctions are neither unique to system dynamics or more generally computational modeling or software intensive sciences (Symoms and Horner 2014), but they are timely and made more visible by role of computing in science today. Questions of trust, reliability, and replicability—all central to the practice of science—get more complicated when one requires supercomputing resources that rely on massive collections of CPU cores that in turn are sufficiently complex in both their design and manufacturing to raise questions about their basic reliability for drawing scientific inferences. And, more generally, they apply to the translational and implementation sciences insofar as both revolve around the idea of translating some proposition to an application involving behavior, whether that behavior be an individual organism, team, organization, or some larger artificial system.

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