The Scientist Personality of System Dynamics

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Abstract

System dynamicists frequently seem to defend the "scientific" status of their models by recurring to scientific principles. This is not surprising since the philosophy of science has been the usual place to look for the philosophical standing of System Dynamics. However, SD typically aims at designing artifacts of different types, e.g. models, policies, plans, organizational schemes, etc. that address a specific situation that is wanted to be improved. Such an attitude is the trademark of engineering, a stance that is easy to see in the underpinnings of the field that Jay Forrester shaped. This paper delineates some issues that show why the philosophy of engineering provides a more suitable ground for SD. Once such a ground is acknowledged, the questions of the "scientific status" of SD, with all the demands that come from such a concern, e.g. validation, confirmation of knowledge, truth of statements, scientific method, predictability, generalizability, replicability, empirical basis, etc. become truly irrelevant.

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1. The Scientist Personality

Science is frequently taken as the paradigm of academic activities. This tendency is puzzling when it is applied to the arts, humanities and engineering. The scientific goals, its principles of reasoning, methods, validation criteria, among other elements, sometimes become examples to follow. I see such type of attitude in several academic engineering initiatives, including diverse modeling and simulation approaches. System Dynamics and its critics are no exception. Consider for instance questions of validation, identity ("what is System Dynamics?"), legitimation, the discussions on the "theory" of System Dynamics, among many other topics, e.g. "the modeling process *must* follow the scientific method", "hypotheses *must* be testable", "modelers *must* build theory", "models *must* be built on theory", "models *must* be scientifically evaluated", "we *must* have scientific credibility", etc. The problem that I see is that along the way such demands contradict, without noticing, the engineering heritage of System Dynamics.

But it is not a matter of history or genealogy. Engineering is, in fact, very different from science. With "different" I mean that the principles of reasoning of engineering are different from the ones used for generating scientific knowledge. To recognize the engineering "personality" of System Dynamics helps to dissolve various SD debates and to guide System Dynamics to "get its job done". I say this without implying that System Dynamics cannot contribute to science or to the generation of scientific knowledge, e.g. Sterman (1994) highlights the enhancement of scientific reasoning via SD virtual worlds. Moreover, SD can be also used as the basis for scientific activities, e.g. it is very powerful for model-based research in social sciences, but this is only one of many possibilities. I do not imply either that SD models should not be rigorous and done with the highest standards, of course not. But what kind of rigor? What kind of standards? I do not mean either that scientific rationality can not be beneficial for System Dynamics practice; scientific reasoning can be useful for building certain models in many situations. But usefulness is one thing. To elevate the principles of scientific reasoning as values or ideals to follow in SD modeling, for the sake of it, is quite another.

For example, with the goal of having confident models for decision making, Eckerd, Landsbergen and Desai, (2011) clarify that the results of more scientifically "rigorous" models will be seen by users more confidently. However, apart from being a pragmatic requisite, they see the generation of scientific knowledge as the ideal to pursue: "modelers should consider the scientific validity of the model results. Ideally, models should contribute to knowledge and theoretical understanding of the system in question. If a model is applicable to only a few specific scenarios, then the results are not generalizable and do not provide us with a deeper understanding of the system in question." (p. 8). For these authors, the goals of System Dynamics *are* and *should be* the same goals of any respectable scientific enterprise: to generate theoretical and generalizable knowledge that permits to understand phenomena.

The previous example shows how sometimes the scientist personality is explicitly shown. I suspect that this attitude is very common in the unconscious mind of the SD community. If we believe that SD modeling has a scientific nature, then we answer some important questions in a particular way. But if we believe that SD is primarily an engineering activity, intrinsically different from scientific activity (even regarding "academic SD"), then our answers change.

2. The Engineering Personality

The trademark activity of engineering is design (Pitt, 2011; Van de Poel & Goldberg, 2010). A design shows a "know-how", as opposed to the scientifically valued "know-that". Engineering expresses a distinctive type of knowledge (Mitcham, 1994). However, the prevalent bias towards *knowledge-that* undermines engineering's *knowledge-how*, i.e. "engineering knowledge is practice-generated... it is in the form of 'knowledge-how' to accomplish something, rather than ´knowledge-that' the universe operates in a particular way" (Schmidt, 2012, p. 1162). Engineers know what to do in non-ideal situations, engineering knowledge is defined by such a know-how (McCarthy, 2010). Knowledge-how is not concerned with the truth or falsehood of statements that concerns knowledge-that, "you cannot affirm or deny Mrs. Beetons recipes" (p. 12). Engineers *know how to do* things.

Justification philosophy—the search for epistemic authorities—has been the dominant style of Western scientific philosophy. This is the view of knowledge as *justified true belief* that looks for "well-grounded" (positive) knowledge, that is, "knowledge-that". This popular position supports most of current Western thinking about what science should be: it is rational to accept only those positions that have been justified according to the rational authority. Lately such authority is "empirical evidence". Another popular authority is the collective endorsement of a knowledge claim by a scientific community. But the epistemology of engineering does not need epistemic justifications. The intentional creation of artifacts is done by experimental *methods* that are more fundamental than (and not derived from) any type of *theory* (Doridot, 2008). The origin of designs is irrelevant, they do not necessarily have to be *a priori* supported by anything, including theories or data. They can be freely generated with the help of any procedure, sourced from reason, or guided by previous expectations ("theoretic" or not) (Stein & Lipton, 1989), guided with the help of a model, or guided just based on imagination or instincts. "Empirical evidence", or any other indirect mechanism of representing the world, is just another option, but it is not a requisite. For instance, "the inventor or engineer... can proceed to design machines in ignorance of the laws of motion... These machines will either be successful or not" (Petroski, 2010, p. 54). An artifact is not false or true (or closer to), simply it works, or it doesn't. If it works, engineering succeeds. The popular notion of knowledge as "*justified* true belief" means nothing in a pragmatic approach in which knowledge is *unjustified*. In the words of Pitt: "If it solves our problem, then does it matter if we fail to have a philosophical justification for using it? To adopt this attitude is to reject the primary approach to philosophical analysis of science of the major part of the twentieth century, logical positivism, and to embrace pragmatism" (2011, p. 173). In engineering "what works is what counts", justification is optional and dispensable. Consequently, its method it is not the "scientific method" (on any of its variants or interpretations). Engineering uses *heuristics*, that is, fallible and unjustified means to address any problem (Koen, 2003).

The previous characteristics bring special criteria that actually oppose to scientific "principles". Several SD discussions take a different light if we see them through such engineering glasses. I will list some of them.

The Relevance and Primacy of Design

The significance of design for engineering and for the Industrial Dynamics that Forrester envisaged is straightforward. For instance, Sterman (2000) underscores the main purposes of SD modeling for management: organizational design. In fact, design is ubiquitous in System Dynamics literature and practice.

Validation and purpose

Forrester (1961) stated that the "validity" of a model is a question of usefulness for a purpose. Let us consider the paper of Epstein (2008) entitled "Why model?" in which he expresses that "I can quickly think of 16 reasons other than prediction...to build a model", for instance:

1. Explain (very distinct from predict)
2. Guide data collection
3. Illuminate core dynamics
4. Suggest dynamical analogies
5. Discover new questions
6. Promote a scientific habit of mind
7. Bound (bracket) outcomes to plausible ranges
8. Illuminate core uncertainties.
9. Offer crisis options in near-real time
10. Demonstrate tradeoffs / suggest efficiencies
11. Challenge the robustness of prevailing theory through perturbations
12. Expose prevailing wisdom as incompatible with available data
13. Train practitioners
14. Discipline the policy dialogue
15. Educate the general public
16. Reveal the apparently simple (complex) to be complex (simple)

Many purposes, many uses. If the goal is to develop scientific theory then perhaps some of the principles of scientific reasoning might apply. But this is not "the" sole possibility. A design process can be supported by a model that can be used in many ways. A model is not like a scientific theory. Its purpose is not necessarily to generate a scientific theory either. A model is fallible, uncertain, and many different (useful) models can be built for the same situation (there is no single "best" theory, as science pursues). Given the pragmatic philosophy of engineering then if the model works for the purpose in hand, then the engineer succeeds. It does not really matter if there is no justification for a model that works.

Methodology: Heuristic Trial-and-Error

The questions on the "scientific method" address the possibility of generating scientific theoretical knowledge, that is, abstract, generalizable, confirmed, justified knowledge. The issue of scientific validity rests mainly on the method. However, any system dynamicist knows that there is no one "best" or "standardized" method for building SD models. In fact, since there are many possible purposes, then the methods *should* vary according to them. Moreover, given the heuristic nature of design activities, then *any* procedure is welcome. Modeling is a *creative* activity. It is largely a matter of trial-and-error. This heuristic quality is easily recognized in some well-known recommendations for building SD models, e.g. "there is no cookbook recipe for successful modeling... Modeling is inherently creative... Modeling is iterative... Models go through constant iteration, continual questioning, testing, and refinement (Sterman, 2000, p. 87). Homer (1996) recognizes that model development is "a process that is iterative, involving a certain amount of trial and error, and often requiring significant time and effort to come to fruition" (p. 1). System Dynamics, as any engineering enterprise, is experimental.

Methodology: Problem-Oriented

For engineers the first issue to consider is a problem to solve. Indeed, the very first step of the "Industrial Dynamics" approach of Forrester (1961) is "to identify a problem" (p. 13). Sterman also underscores the task-oriented primacy in SD: "The most important step in modeling is problem articulation. What is the issue the clients are most concerned with? What is the real problem, not just the symptom of difficulty? What is the purpose of the model?" (Sterman, 2000, p. 89). Indeed Sterman makes the important warning: we should model problems, not whole systems. Engineers are very aware of such a risk.

Critics: "SD has no Theory"

Over the years various critics have expressed the accusation that System Dynamics has no social theory behind. However, it should be unmistakable that SD, as engineering activity, does not pretend to build theories of human behavior or alike. Moreover, from an heuristic stance, SD does not need theories to build models either. Theories *may* be useful, but that is another matter. SD is not interested in individual action, furthermore, it does not assume that structures determine human behavior either—the sort of determinism that Burrell and Morgan (1979) oppose to free-will. This type of criticism has already been answered and clarified by Lane: SD is concerned with aggregate social phenomena and not with individual meaningful actions (2000). Moreover, SD does not propose invariant causal laws, as Lane (2001) already showed. SD's engineering personality should help to dissolve these issues.

Critics: "SD is Unscientific"

Some critics utter that SD has abandoned (or diverges from) the scientific method. A variation in this sort of "attack" is that SD lacks scientific rigor. Connected with these criticism is the charge of lacking empirical evidence (which is scientifically used for justifying knowledge). In the light of the points above, it should be straightforward that an engineering enterprise is not concerned with empirical evidence as such (again, it *may* be useful). The alleged scientific rigor of the "empirical evidence", for guaranteeing "legitimate" models or recommendations, is not even relevant within engineering activities. And that is a good thing (I do not mean that data is not useful, for example for testing behavior reproduction, but that is quite a different use for data). Akkermans and Romme (2003) make the closest point to the fact the SD is about design, though for them it is a "design science" enterprise. Their invitation is worthwhile, although the differences between a "design science" and engineering can be a matter of debate. I find the "unscientific" criticism highly misplaced. Instead, SD could be charged, if ever, of not following the engineering method. But then the verdict would be: not guilty!

Identity Crisis: What is System Dynamics?

It is not uncommon the concern about "the identity of System Dynamics", e.g. (Vanderminden, 2006). In fact SD has been labeled as a theory (Flood & Jackson, 1991; Jackson, 2003), a method (Coyle, 1979; Lane, 2001; Meadows, 1980; Sterman, 2000; Wolstenholme, 1990), a methodology (Roberts, 1978), a field of study (Coyle, 2000; Richardson, 1991), a tool (Luna-Reyes & Andersen, 2003), a paradigm, among other nouns. I want to highlight that the "engineering roots" of SD address the question of identity in a way that unmistakably discards various scientific traits that I see as a source of identity confusion. In fact Homer recently expressed his concern regarding the "lack of progress and success" of the field of System Dynamics (p. 124), which for him it is "a problem with how we think of ourselves and how we project ourselves to others. Perhaps the right metaphor here is a psychological one. In particular, it seems to me that SD has for many years suffered with an identity crisis". I can't agree more. But I disagree in the nature of such *identity*. The pursue of a scientific credibility for SD undermines the engineering character of SD. The full potential of SD will no be found in meeting the "scientific" demands made by some academic communities but in its actual power for supporting the design and redesign of complex systems.

What is a Good Model?

That is always a central SD question. And I guess the answer is straightforward (and not simplistic at all): A good model is a model that *works*, for its given purpose. To establish if a model "works" is not an uncomplicated matter. But the ultimate razor for judging the "goodness" of (engineering) models should be, unmistakably, effectiveness.

SD for Engineering Education

Goldberg (2008) states succinctly the problem in his article *Bury the Cold War Curriculum*:

> Pushing science and math at the expense of design may have worked once but is now doing students a disservice... The global economy places a premium on more creative engineering activities at home. Furthermore, the death march of math and science disillusions some otherwise able students, causing them to drop out. Disproportionate numbers of the departing are minorities and women, whom engineering schools should instead be attracting. Moreover, students who come to engineering to be entrepreneurial, socially responsible or both wonder why business and ethics are merely bolt-on topics. When design is finally taught, students are unable to solve other than rote problems and struggle to communicate their results.

Engineering faculties are fertile soil for SD because of its modeling and simulation-based truly creative, integrative and design possibilities. But paradoxically SD is rather scarce in engineering curriculums. This means a significant diffusion potential for SD. In the past there have been convincing claims in this direction, e.g. (Caulfield & Maj, 2002; Saeed, 1997). Perhaps the strongest one is reflected in the title of an article of Radzicki and Karanian (2002): *Why Every Engineer Student Should Study System Dynamics*.

3. Perspective

Milestones of engineering over extremely challenging problems, like the flight of the Wright Brothers, the Chilean mine rescue, or the landing of the NASA rover Curiosity on Mars, are often attributed, erroneously, to science (Petroski, 2009, 2011, 2012). I believe that this is a matter of historic and cultural prejudice that associates "knowledge" (to solve problems) with "science", and "engineering" with mere "application". Goldman, in his text "The Social Captivity of Engineering" makes the case that:

> Engineering is today captive of society... to a cultural prejudice that denies the very existence of a theory of engineering—that is, of a distinctive conceptual framework, a *theoria*, or perspective on the world, of engineering's own—by reducing engineering to devising applications of the products of scientific theorizing... [which] provides a rationale for concluding, quite incorrectly, that all of the 'serious' intellectual problems... attach to science, the principles of whose practice are supposed to comprehend the practice of engineering as well" (Goldman, 1991, p. 121)

Since Plato, Western culture value universals over particulars, theory over practice, thinking over making and doing, and representations as copies over representations as models (Floridi, 2011). Our culture favors the elegance of values such as certainty, truth, universality, abstraction. The engineering way of doing things works under undervalued principles that favor uncertain, context-dependent, contingent, practical solutions (Goldman, 2004). Indeed thinking or acting by "trial-and-error" has been traditionally used in a pejorative sense. However, the engineering way of facing the world represents a truly *effective* enterprise. Engineering, SD included, is not "applied science". Perhaps the situation is the other way around, as Goldberg (2010) expresses: "science is merely the application of engineering method to the evolution of models or concepts" (p. 8), as in the Popperian trial-and-error sense. The same iterative framework that system dynamicists have been developing for the last 60 years.

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