When Modeling Social Systems, Models ? the Modeled

Reacting to Wolfram's 'A New Kind of Science'

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Much media attention has been directed to the May 2002 release of Stephen Wolfram's *A New Kind of Science*. What has not been particularly highlighted is that this book is the latest shot in an ongoing dispute between the hard physical sciences and the "soft" social sciences such as sociology, anthropology, and other observation-based studies of human affairs. Wolfram (2002: 9) writes:

> *One will often have a much better chance of capturing fundamental mechanisms for phenomena in the social sciences by using instead the new kind of science that I develop in this book based on simple programs … Indeed the new intuition that emerges from this book may well almost immediately explain phenomena that in the past have seemed quite mysterious.*

He goes on to suggest that "most of the core processes needed for general human-like thinking will be able to be implemented with simple rules … once one has an explicit system that successfully emulates human thinking" (Wolfram, 2002: 629-30). He continues: "no doubt as a practical matter this could be done … by large scale recording of experiences of actual humans" (Wolfram, 2002: 630).[1]

Central to Wolfram's premises is an all too common mistake: the assertion that by the study of models we can directly study the things, systems, or people being modeled. Despite the promise of massive computer simulations and the development of "intelligent agents," tests or studies of computer models, be they cellular automata or quantum computing, remain just that—and are not the direct study of the people or social systems being modeled. John Casti (1999a) has made a similar argument in the pages of *New Scientist*:

> *Large-scale, agent-based simulations … are exactly the sort called for by the scientific method … So, for the first time in history, we have the opportunity to create a true science of human affairs.*

With each reading of that sentence, alarm bells must sound among all who do field studies on actual people. Wolfram's claim is stronger: "Simple programs constructed without known purposes are what one needs to study to find the kinds of complex behavior we see" (Wolfram, 2002: 1185). "The principle of computational equivalence forces a new methodology based on formal models" (Wolfram, 2002: 1197). If Wolfram and Casti are to be believed, economics and most of social science would acquire new meaning, for theory could be actually tested before some governmental official (well-intentioned or not) puts it into practice. All this despite some commentators' warnings concerning the deep theoretical limitations associated with such approaches (see, e.g., Oreskes *et al.*, 1994; Cilliers, 2000a; Richardson, 2002b, 2002c). If only matters were so straightforward. Casti's rhetoric was far more succinct than Wolfram's 1,200-page tome, but both aim guns at the need for field studies. Such persistent polemics are both dangerous and misguided and therefore need further rebuttal.

In recent years new concepts and methods of modeling have combined with substantial computer power to open up the possibility of using them in the social sciences (see, e.g., Goldspink, 2002). From a practical point of view, modeling social systems should be understood as an extension of the way in which we have always dealt with social systems. Models in the computer are extensions of our thinking processes, which use what we think we know to consider various scenarios to help in choosing a course of action—"models are inspirational rather than containers of truth" (Richardson *et al.*, 2000). Models are also extensions of the stories (novels, etc.) we tell, which help us teach others, particularly children, what we have learned from experience. As Pidd (1996: 122) points out, "models are developed so as to allow people to think through their own positions and to engage in debate with others about possible action" rather than "a proper representation of part of the real world." From a scientific perspective, models help us understand the logical consequences of specific assumptions that may or may not have a basis in the real world. This helps us, in a limited sense, to validate or refute assumptions, which is important in developing better models.

Here, agent-based models (of the Santa Fe type) need to be distinguished from the cellular automata models of Wolfram. Agent-based models at least claim to be founded on a logic and a narrative. Wolfram makes no such claim for the automata models. This leaves even more pause for how someone—business leader or policy maker—is to interpret the results.

The opportunities given by new modeling methods are sometimes confused with the idea that models of social systems will work in a similar manner to how they work in describing simple mechanical systems, that is, by predicting precise outcomes, or serving as oracles that seem to tell absolute truths. Hereafter in this article this view shall be referred to as the “merely study models” or MSM perspective. There are scientifically based reasons (especially within the complexity community) to expect that models cannot serve us in the way that representationalist MSM proponents believe. Moreover, achieving models that can serve as transparent and effective aids to decision making requires substantial effort. Learning to use models wisely is as important a part of this process as developing them: “[M]odels are tools that can be used and abused— the best models are worthless in linear hands” (Richardson, 2002b).

Today MSM proponents have the upper rhetorical hand. But the belief in such naïve rhetoric contributed mightily to the financial mess created by the collapse of Long-Term Capital Management in 1999 and to the assorted dislocations caused by rigid adherence to the IMF’s “scientific” models. Even worse, the rhetoric suggests that research funding would be “better spent” on perfecting computer simulations rather than on field studies of actual humans. Despite the widespread demonstrable failures of computer simulation in informing effective decision making, many organizational OA (operational analysis) departments still strive for bigger and better, all-embracing computer models. The illusion of knowledge-based certainty that always accompanies such methods is seemingly preferable to accepting and managing the real world’s inherent uncertainty; we seem to prefer a false sense of security over “an awareness of the contingency and provisionality of things” (Cilliers, 2000b).

When used wisely, models provide a forum for dialog and discourse— between expectations and results, between model and observation, between versions of an ever-being-revised model. Central to that dialog is the recognition that a model without observations of the modeled is a monolog with no audience. The risk of the Wolfram position is that “decision makers” will provide that monolog with a rapt audience and will fail to question the absence of dialogic elements necessary for proper decision making. Merely to study models—without concern for the observed or the modeled—suggests a positivistic belief in the powers of prediction and self-fulfilling cause that are foolish when applied to the “natural sciences” and dangerous when applied to the social sciences.

THE RHETORIC

Wolfram has become the most recent, and John Casti perhaps the most vigorous, proponent of the misconception that the study of models is the study of people (a view labeled herein “strong MSM”). But they have many compatriots. The strong MSM of Wolfram, Casti, and others of their ilk would be well placed if either science or the prediction business were solely concerned with the abstract behavior of large numbers of abstract people. Indeed, this type of science has its place. The issue of studying individuals and their behaviors can be supplanted by the concept of studying large groups and generating descriptive information about the average behavior, deviations from that average, and similar measures of the movement of the group through possibility space. Nevertheless, it is one thing to assert a place for simulations and modeling, and another to assert that such models hold the answers that actual observation cannot supply.

Despite Oreskes *et al.*‘s (1994) masterly critique of the power of rulebased simulation, Wolfram writes, “Systems with extremely simple rules can produce behavior that seems to us immensely complex” (Wolfram, 2002: 735); “… and that look like what one sees in nature. And I believe that if one uses such systems it is almost inevitable that a vast amount … will be possible” (Wolfram, 2002: 840). Casti, writing in 1999, prefigured and amplified these thoughts:

> *I feel strongly that it would be wrong to exclude many important complex systems from the possibility of being explored in simulated worlds just because people are part of a system’s complex interactions. It’s perhaps because I am a mathematician and modeler, rather than a social scientist, that I disagree with the belief that it is impossible to develop any kind of theory about social systems because people are too complex and unfathomable to be encapsulated in equations and rules. The power and versatility of modern computers now makes it possible to use artificial worlds to test out theories about social and behavioral systems which will enable us to get closer to identifying what rules actually drive the relationships between human agents and other system entities.*[2]

Most scientists doubt that; for example, the kinds of issues usually found to be fundamentally important to business—intuition, creativity, response to uncertainty, response to unpredictable events, and so on— are not what get modeled, but are what drive business.

Casti and co. have a further misconception on which their strong MSM is based, namely, the concept that people*are* rule-following creatures:

> *From a philosophical perspective, it is reasonable to postulate that the behavior of human agents in a system can be interpreted as being generated by rules, even if the people do not explicitly invoke rules when they act and find it difficult to define such rules if you ask them. (Casti, 1999b)*

The echoes of Wolfram's simple rule-following systems are obvious; the difference is that Casti proclaims that such rules are based on logic. Wolfram makes no such claim. Many strong MSMers overstate a much weaker claim made by John Holland (the father of genetic algorithms), who argues that we can often learn by treating humans as if they are rulefollowing creatures. The MSM perspective discounts the notion that we exhibit behaviors and that if pushed to explain them we might often explicate a "rule" that finds greater truth in the exception than in the practice. Wolfram throws away claims about such inferred logical rules and begins with his own notion of simple rules. He continues:

> *Inevitably we tend to notice only those features that somehow fit into the whole conceptual framework we use. And insofar as that framework is based even implicitly upon traditional science it will tend to miss … traditional logic is in fact in many ways very narrow compared to the whole range of rules based on simple programs. (Wolfram, 2002: 843)*

If humans were indeed like subatomic particles, individual yet not distinguishable, these modeling notions might hold (see Pesic, 2002, for much more on this). However, while a view of human behavior may work when modeling crowds, traffic, or the stock market (see Wolfram, 2002: 1014), it denies the very essence of being human and of the narratives making up our varied identities. As Pesic writes:

> *In the story of individuality, contrasting visions confront each other. The individuality of each person and macroscopic object is unique, like Hector's shining helmet. Yet that helmet, like each of us, is made of electrons that blend and merge. The world as we experience it calls us to reconcile these views; vast numbers of identical beings can form structures whose complex configurations give them the appearance of uniqueness. Here we return to the question … [of] whether the individuality of persons really touches the individuality of things. I may be like the ship of Theseus, a phantom haunting itself, for on the atomic level I have no individuality. (Pesic, 2002: 148)*

But as Steven Levy (2002) writes of Wolfram in *Wired*:

> *Basically, he's saying that all we hold dear—our minds, if not our souls— is a computational consequence of a simple rule. "It's a very negative conclusion about the human condition," Wolfram admits. "You know, consider those gas clouds in the universe that are doing a lot of complicated stuff. What's the difference [computationally] between what they're doing and what we're doing? It's not easy to see."*[3]

So much for the notions of purpose, narrative, or emotions.

"Scientists," Wolfram told *Business Week* (May, 2002), "should be striving to uncover the underlying simplicity—not just searching for explanations by carving complex phenomenon into smaller and smaller, more digestible pieces." MSM proponents claim to have found such underlying explicants. The strong MSM proponents look at the results of a model, find an analogy in human (or other) behavior, and proclaim that the analogy proves that the underlying model is "simplicity." Wolfram makes the following point repeatedly:

> *Whenever a phenomenon is encountered that seems complex it is taken almost for granted that the phenomenon must be the result of some underlying mechanism that is itself complex. But my discovery that simple programs can produce great complexity makes it clear that this is not in fact correct. (Wolfram, 2002: 4)*

Yet, as celebrated Harvard psychologist Jerry Kagan (2002: 17) notes, "Unfortunately function does not reveal form." Nor vice versa, at least when human behavior is concerned. Just because simple rules can produce complex behavior does not mean that all complex behavior can be explained by simple rules (Oreskes *et al.*, 1994).

Stephen Lansing addresses this concern in a recent Santa Fe Institute working paper (2002). Lansing writes of the "limitations of a social science methodology based on descriptive statistics," and goes on to quote Epstein and Axtell (1996):

> *What constitutes an explanation of an observed social phenomenon? Perhaps one day people will interpret the question, "Can you explain it?" as asking "Can you grow it?" Artificial society modeling allows us to grow social structures in silico demonstrating that certain sets of microspecifications are sufficient to generate the macrophenomena of interest.*

Lansing wisely notes, "one does not need to be a modeler to know that it is possible to grow nearly anything *in silico* without necessarily learning anything about the real world."

The act of interpreting differs from the act of observing, and both may differ significantly from the underlying phenomenon being observed. In their failure to respect this distinction, strong MSM proponents are implicitly suggesting that *the interpretation is reality*

. However, while a good model of complex systems can be extremely useful, it does not “allow us to escape the momentof interpretation and decision” (Cilliers, 2000a). Most well-informed followers of the philosophy and history of science would recognize this perspective for what it is: social constructionism (see Berger & Luckmann, 1966 and Hacking, 1999). The constructionist stance, which underlies the MSM viewpoint, seems to be deliberately overlooked by its followers (who as rhetorical positivists would be forced to reject it).

MSM takes a social constructionist stance for the very reason that Ian Hacking, in his brilliant book *The Social Construction of What* (1999), suggests underlies all forms of such an argument, namely an objection to the idea that “in the present state of affairs, *X* is taken for granted, *X* is inevitable … [but] *X* need not be at all as it is.” Note that Hacking restricts *X* to ideas. In this case *X* is the understanding that human interactions are complex, situated, subject to multiple interpretation, and usually unique. How much simpler the world would be if only we could see and understand that such complexity is but a figment of our minds.

Herein lies the second risk to scientific endeavors posed by proponents of the strong MSM perspective. To the extent that the strong MSMers are believed, decision makers will rely on their simplifying models and analogies. And, over time, the models will be proven wrong. Given enough disconfirming experiences, the general public will reject modeling and simulation as a tool, and both funding and exposure to interesting problems will dry up. In an era where Long-Term Capital Management’s founders can still argue with a straight face that “it was the world not our model” (and have the nerve to operate yet another hedge fund), the rhetorical risks posed by MSM are mighty indeed.

Furthermore, MSM proponents often claim that “simulation is changing the frontiers of science,” just as Wolfram proclaims the study of automata to be “a new kind of science.” If the frontiers of science were only to be found in computer laboratories, perhaps this claim might have merit, but the many scientists doing nonsimulation-based work have every reason to be skeptical. In this regard, the popular press has greatly overplayed the potential value of the computer simulation-based approach of the Santa Fe Institute, among others, with regard to complex systems research. Complex systems thinking is more than merely simulations and their results. Social science is more than any model can deliver, let alone just computer models. MSM’s claims to the contrary are biased at best and pose a risk to the funding of basic research for all engaged in the actual observation of human affairs. The mere fact that a computer simulation can be run does not substitute for a study of human behavior in practice. Nevertheless, try convincing funding sources of that stance while Wolfram, Casti, and others are proclaiming the “virtues” of simulation models for “advancing science” and the popular press is lending credence to the primacy of such a view.

MODELING REDEEMED

Both Suppe (1989) and van Fraassen (1980) suggest that

> *a scientific theory is an attempt to either isolate or idealize a system—usually a physical system—in such a way that its dynamics can be reduced to a manageable number of variables (each of which is usually represented by a theoretical term) related by a mathematical description, so that the model generates a restricted number of likely outcome states. It is often neutral with respect to many of the attributes of the entities and processes it covers (Wilkins, 1996).*

The very complexities of human affairs suggested above are examples of the types of attributes and processes about which a scientific theory may be neutral in its efforts to describe abstract and group phenomena. The act of isolating or idealizing has been termed by Rosen, among others, as the “modeling relation.” The features of this widely known relation are shown inFigure 1.

The model occupies the right-hand side of the picture. The observable real-world system (whatever it may be) occupies the left. In between lies the world of the observer, who not only makes observations about the real-world system but also interprets what those observations imply for the model and what the outcomes of running the model might mean in the real world. The observer has the key role in the modeling relation.

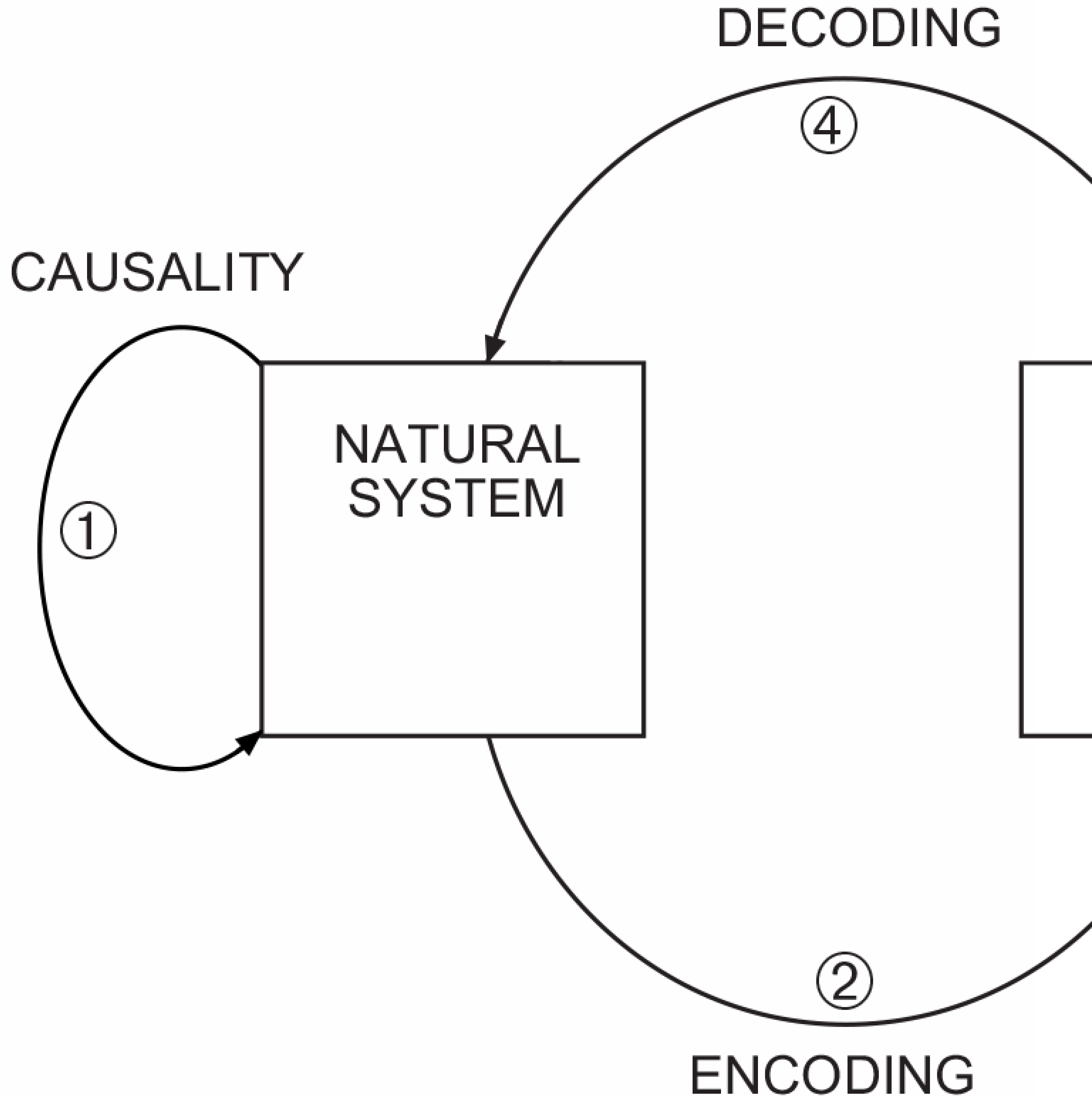

Fig. 1: Two systems, a natural system and a formal system related by a set of arrows depicting processes and/or mappings

The assumption is that when we are "correctly" perceiving our world, we are carrying out a special set of processes that this diagram represents. The natural system represents something that we wish to understand. Arrow 1 depicts causality in the natural world. On the right is some creation of our mind or something our mind uses in order to try to deal with the observations or experiences that we have. Arrow 3 is called "implication" and represents some way in which we manipulate the formal system to try to mimic causal events observed or hypothesized in the natural system on the left. Arrow 2 is some way that we have devised to encode the natural system, or, more likely, select aspects of it (having performed a measurement as described above), into the formal system. Finally, arrow 4 is a way that we have devised to decode the result of the implication event in the formal system to see if it represents the causal event's result in the natural system.