# Alife as a Model Discipline for Policy-Relevant Simulation Modelling: Might "Worse" Simulations Fuel a Better Science-Policy Interface?

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## Abstract

Policy-relevant scientific models are typically expected to make *empirically valid predictions* about policy-relevant problems. What are the consequences of shaping our science-policy interface in this way? Here, it is argued that the theoretically insecure simulation modelling pioneered within artificial life is emblematic of an important alternative approach with significance for policy-relevant modelling.

The 21st century brings a raft of significant *systemic* challenges: global finance, global climate, global technology, global security, global governance, global sustainability, etc. Meeting these challenges will involve understanding and managing complex systems that comprise many interacting parts (Gilbert and Bullock, 2014). Computer simulation is emerging as the key scientific tool for dealing with such systems (e.g., Farmer and Foley, 2009). As a consequence, simulation science is increasingly political and has the potential to be critical to our future well-being and quality of life (e.g., Edwards, 2010). To what extent do our current simulation modelling practices measure up to the responsibility that they must now shoulder?

The assumed gold standard for simulation modelling is that our models should achieve a (pseudo)-empirical status (Peck, 2004) that allows their results to be understood as secure forecasts, or valid predictions about their real-world target systems (but see Epstein, 2008). Where simulations have real-world political significance, it is expected that their predictions and forecasts can inform the decisions of policy makers, stakeholders, etc. Such models are analogous to the simulated wind tunnels within which new designs of cars or planes are trialled before deciding whether they should be constructed and sold in the real world (e.g., Bullock, 2011).

Why would we want a science-policy interface structured in this way: one in which the flow from policy to science defines "challenges" and "impact" while the flow in the other direction takes the form of predictions and forecasts? Here, the merits of an alternative science-policy interface will be considered; one in which simulation models that do not rely upon empirical validity are built and explored in order to generate *insights* into our understanding of policy-relevant target systems (Di Paolo et al., 2000).

I argue that, in the same way that *Drosophila melanogaster* and *C. elegans* have played a useful role within biology as model organisms, Artificial Life has the potential to be a *model discipline* within academia, epitomising the use of simulation to deliver *insights into what may be* as opposed to *predictions of what will be*.

There are several points in favour of this position. First, the field is explicitly located at the boundary between the artificial and the real (Silverman and Bullock, 2004); second, the research community has explicitly debated the role of simulation and its epistemological status (e.g., Wheeler et al., 2002); and third, crucially, it is generally accepted that most artificial life models lack theoretical security in the following sense. Theoretically secure models are underwritten by mature and consensually agreed upon theory (e.g., the Navier-Stokes equations for fluid flow). Insecure models do not benefit from such a mature theoretical underpinning, but rather, are exploratory attempts to generate or progress such theory. Perhaps uniquely, the field of artificial life deliberately courts insecurity of this kind, by tackling counterfactual questions ("life-as-it-could-be") for which there is, almost by definition, little theoretical basis. Similarly, the related field of complexity science can be regarded as an effort to address questions concerning emergent phenomena that lie outside the security provided by reductionist science.

Research into the policy-relevant systemic questions with which this paper opened currently tends to be deeply insecure in the sense described above. It may not always be this way since systemic problems are not necessarily inherently insecure. Indeed, it is to be hoped that progress on the problems of climate change, financial stability, etc., might provide more secure foundations for policy making in the near future. However, presently, it is not only more facts and data pertaining to these problems that is required but more and better theory with which to marshall and make sense of these data. Consequently, it is important to learn lessons from modelling and simulation efforts that may appear "worse" in terms of empirical validity, but may be "better" in terms of generating the insights and understandings that fuel new and better theory.

In outlining this style of simulation, I will draw on an analysis of Charles Babbage's simulation model of miracles, the first example of a simulation model (Bullock, 2000), and its subsequent impact on attempts to build machines capable of automatic economic reasoning (Bullock, 2008), and analysis of Levins' position on the trade-offs inherent in modelling living systems (Levins, 1966; Bullock, 2014).

As a concrete example of this type of model, consider Schelling's (1971) well-known model of urban segregation, which demonstrates that an initially well-mixed population of city dwellers of various types, each with a tendency to relocate at random unless they are adjacent to at least a few similar individuals, tends over time to become strongly spatially segregated by type. The insight here is that real-world segregated populations may not be made up of individuals with explicit preferences for segregation; the "micromotives" of the agents may not be reflected straightforwardly in their aggregate, population-level patterns. Schelling's model is not empirically valid, makes no forecasts, and is not intended to reresent a specific city or population. Nevertheless it delivers an insight that is compelling, transparent and, consequently, capable of helping to shape better social policy.

Conversely, one might imagine many more sophisticated and more data-driven models of, say, Chicago's economy (including representations of its housing market, land prices, geography, etc.) and population (including accurate demographic information, and analogues of relevant psychological and social processes, etc.). Such models might be able to achieve a degree of empirical validity in the sense of being able to "hind-cast" Chicago's historical patterns of segregation to some degree of accuracy. Nevertheless, despite the ready ability of such models to generate specific forecasts and point predictions with the potential to strongly determine associated policy decisions, they would (given our relatively poor understanding of the fundamental systems involved) be both insecure and opaque, and, consequently, problematic as policy tools.

The groundwork outlined above enables us to engage with three key questions: Might a re-examination of simulation modelling enable a science-policy interface to be restructured in order to allow insights to pass across it? To what extent are accountability and democracy impacted by the current character of the science-policy interface? Could epistemically insecure simulation models of the kind pioneered within artificial life research offer a paradigm within which an effective reassessment of impact-driven science takes place? To which the answers argued for are, respectively: "yes", "badly", and "perhaps".

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