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A Skeptic Embrace of Simulation

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Abstract

Skeptics tend not to be the first to jump on the next bandwagon. In quite a few areas of science, simulations and Complex Adaptive Systems (CAS) has been the bandwagon in question.

This paper intends to reach out to the skeptics and convince them to hop-on; take over the controls and make the wagon do a U-turn and aim for the established scientific theories.

The argument is that simulation techniques, such as Agent-Based Modelling (ABM), may possibly be epistemically problematic as one sets out to strongly corroborate theories concerned with our overly complex real world. However, using the same techniques to explore the robustness of (or to falsify) existing abstract and idealised mathematical models will be to be epistemically uncomplicated. This allows us to study the effects of reintroduction of real-world traits, such as autonomy and heterogeneity that was previously sacrificed for mathematical tractability.

Introduction

Simulations of Complex Adaptive Systems (CAS) have provided answers to questions previously deemed as out of reach for science, questions whose level of complexity would not ever allow for classical formal analysis. The methodology of computer aided simulation is a recent addition to the toolbox, especially in the social sciences. Hence, it should come as no surprise that there are some tendencies among both its enthusiasts and skeptics, that can come across as immature.

Complexity economics, that is the study of economics using CAS and computer aided simulation, has both been described as almost a panacea for economics (Beinhocker 2007; Axtell 2007), and as a method unfit in most application to give us any knowledge of the real world causalities.

Enthusiasts are at time overly keen to regard their simulations as giving a relevant, if not the causal explanation of a phenomena. (Grüne-Yanoff 2009)

Skeptics on the other hand tend to disregard simulations all together, arguing that it is a method that is somewhere in between experiments and theoretical modelling, and carrying the drawback of both.

This paper is directed at those with an overall skeptical lean. Not necessarily to convince them that they are all wrong about questioning simulation methodology or its practitioners. Instead it sets out to give a convincing argument for the use of simulation methodology as a skepticist's sound power tool.

Economics will be given a certain focus, but the pondering wisely transfered, will most often apply equally well to other application.

If one is a complete anti-realist, in economics that would be something like adopting Milton Friedman position (Friedman 1953), that the only thing we need to consider is a theory's proven predictive power, this paper will not be of much interest. If one however has the slightest realist tendency, and is interest in a deeper understanding of phenomena, this paper will hopefully be of some value. No matter if the interested solely is motivated by aversion to getting beaten up by black swans, or if one has more intricate reasons.

A sober skepticism to the epistemological quality of the abstraction and idealisation that is conducted in mathematical modelling of complex phenomena is assumed.

Now, corroboration by simulation may be regarded as problematic as one need to have perfect knowledge of the relevant "natural laws" of the world and the initial values of our overly complex real world.

However instead of setting out to corroborate a theory of some aspect of the real world, the suggestion here is that the skeptic instead could use the same tools to demarcating the area of application, or even falsifying, existing theories.

By the nature of theories, they give us full knowledge of their "natural laws", as well as the relevant initial conditions, and hence they lend themselves very well to simulation. Simulations that allow reintroduction of aspects of the target phenomena previously abstracted for mathematical tractability and elegance. Hence, it tests the idealised theory's robustness and applicability to previously excluded aspects of the target phenomena, possibly falsifying it.

Epistemically: What are simulations?

According to Stephen Hartmann simulations have five areas of applicatio, namely as: a technique, a heuristic tool, a substitute for experiments, a tool for experimentalists; and as a pedagogical tool. (Hartmann 1996)

Only when we consider simulations as either a technique or a substitute for experiment, we laden ourselves with the need to consider the epistemic quality of the simulation results.

As we consider simulations as a substitute for experiments the fact that the the simulation is a theoretical model ran on a computer makes it necessarily different from the phenomena it is intended to imitate.

Our experiments will thus risk to lead us astray unless the dynamics of the original and imitation processes are equal in all relevant aspects. Experiments conducted where the laws of the dynamics and the inital conditions are well-known will however yield epistemically sound results.

A computer simulation is however nothing more than the rules of the specified dynamics unfolded from some inital values and parameters. Its nature is a (complex) deductive process from the assumptions made, no more, and no less.

Simulation are thereby internalistic, but not to a human subject, but to the computer. As Paul Humphreys writes, "scientific epistemology is no longer human epistemology." (Humphreys 2004)

A related debate on whether simulations introduces novel epistemological issues (Humphreys 2009; 2004), or not (Frigg and Reiss 2009; Maki 2005), will only be noted here this paper, but could be of importance to the argument of this paper.

In many areas of science theories are most commonly manifested as a mathematical models. Models are theoretical abstracted and idealised descriptions of the phenomena one which to understand, a relevant analogy if you will; relevant in relation to the aspect of the phenomena the modeller is interested in.

Idealisation and abstraction is often conducted in the name of mathematical tractability and elegance. This sometimes lead to easily ridiculed assumptions that we know are false. It may also lead to an adoption of a higher level of abstraction (LoA), the construction of concepts that aggregate autonomous and heterogeneous components of our target phenomena, or we might even invent proxy concepts that have no counterpart in the target system, but which we argue captures some functional aspect of the target, and make the model fit existing data well.

In many areas of science there are certain LoA that can be argued to be more natural than others. In economics on such LoA, is the one of the individual actor, at least if you are willing to grant the individual a grain of free will.

This was the LoA in the Smithian analysis, but since then the neo-classical school of economics, via the classical one, has made simplifying assumption in order to make the theories' calculus more fruitful.

Simulations and CAS allows to a greater extent scientist to choose the most natural of LoA:s, in economics it also promises to allow formal analysis without the straitjacket of equilibrium, homogeneous and non-autonomous agents, and so forth. A big promise indeed.

However as Till Grüne-Yanoff concludes,

> "the generative richness of agent-based models is thus not an asset, but an embarrassment, as it in fact reduces their explanatory power." (Grüne-Yanoff 2009)

If we manage to create a simulation of a model that grows in such a way that it fits our data, it may suggest that it is a process that at least is a part of an explanation, but if we consider, what is often a wast multitude, of other ways we could have grown the data, the "explanation candidate" might lose its significance as explanatory power.

As the models are allowed to become more complex, we encounter even starker than before modelling problem of what aspects we consciously and unconsciously choose to include and exclude. How do we rest in an certain isolation set up in a complex model of an economy?

How do we beat a theory?

In many sciences, not least economics (Stigler 1983; Axtell 2007), the dominant philosophy is that you "need a theory to beat a theory". Imre Lakatos called this sophisticated falsification. (Lakatos 1970)

In economics it is not easy to point at one research programme, and convincingly argue that it will beat all other. The programmes range from Miltion Friedman's positivist economics, where nothing but the predictive power were deemed important, to traditions like the Austrian and Marxist, that set out to have some claim on scientific realism.

The latter are less prone to set up them selves for predictions, but offer more of deeper understanding and possibly appropriate humbleness. It is far from trivial how one makes the programmes commensurable.

The best theory is therefore not objectively determined, but is left as an excercise to the scientific community.

Kudos to Paul Feyerabends provocative epistemological anarchism, and the cynical aspects of Thomas Kuhns paradigm shifts, but without giving up on the Popperian tradition we may still grant that science is not always directly guided by the pursuit of truth.

Economics is probably worse of than most other sciences in the sense that what is relevant is up to those who use economics as a tool, that is politicians, bureaucrats and businesses.

Another relevance bias stems from what Rachman calls "physics envy". (Rachman 2010) Peter Boettke is far from alone in claiming that there is an over-appreciation of mathematical methods in economics. (Boettke 1996)

The "envy" of the harder sciences, and its manifestation is spread well beyond economics. Include a few non-trivial equations in your academical paper and it will look serious.

In the 1920s when Knut Wicksell was pondering on mathematical economy, he states that his colleagues mostly consider mathematical economics as a curiosity rather then a practice that carried any true value. He goes on to state that he considers its main contribution an aid for our thinking, as it helps us to be precise and fixate our concepts. (Wicksell 1925).

It interesting to note that it would be fairly uncontroversial to echo that statement today, replacing mathematical economics, with complexity economics.

Lastly, some scientific results just come across as more interesting. While running data mining, regressions, and experiments, the results that are more likely to get published are the searched-for results, and the unexpected anomalies. (Thaler 1988; Young, Ioannidis, and Al-Ubaydli 2008)

Biases, or not, when the "it takes a theory to beat a theory"-slogan is applied to a science that has a research culture that fail to acknowledge negative and demarcating results as great achievements lead to an implicit disqualification of Fredrich Hayek's idea[1], that,

> "the curious task of economics is to demonstrate to men how little they really know about what they imagine they can design." (Hayek 1991)

And if it refuses to get beaten?

John Stuart Mill made a distinction between the inquiry of practical men and theorists. The first Mill claim to be using the methodology of induction, while the latter use "a mixed method of induction and ratiocination".

The latter is deemed as the scientific mode of investigation for economics, and that the former is not of great value of discovering truth, but of verifying it. (Mill 1874) Coming back to the two uses of simulation that we interested us of, as a technique simulations are a theoretical tool, and as such one can argue that it is a theoreticist's tool.

As a substitute for experiments, we find it difficult to apply it to explain real world phenomena. It is however most useful for unfolding the consequences of abstract, well-defined situations. This is still what Mill would consider the inquiry of practical men, and hence not a way to discover truths, but of verifying them, and more importantly post Popper and Lakatos, falsifying them.

The Lakatosian "you need a theory to beat a theory" does not give guidance on how to handle the different level of corroboration the preferred theory has.

For Lakatos there is no evidence or tests that could justify the use of a statement in practice nor in theoretical inquiry, as he holds that there are no better evidence for one non-falsified proposition than for another.

John W. N. Watkins, whom Paul Feyerabned referred to as the stern janitor of the Popperian temple (Feyerabend 1982), seem to be the one in this tradition that have opened up for corrboration after all.

Within science it might still be quite sound to talk about a preferred theory only if it is the non-falsified one (again in Lakatos "sophisticated" sense), but as soon as one considers practical matters we might want to make distinctions between more and less well corroborated theories.

The article on "Advanced Directives" on Standford Encyclopedia of Philosophy states that,

> "there is a rough consensus in medical ethics on the requirement of respect for patient autonomy." (Jaworska 2009)

[1] Unless economics would embrace philosophical realism as part of its standard.

An autonomy that, under the assumption that the doctor and patient has the same goals, only is interesting in the face of uncertainty.

Medical science, just as other sinces has had to make itself relevant, it has been required to supply its customers with what they are shopping around for, i.e. answers. Practical men faced with a problem concludes that something needs to be done. He looks into science for "something", and who ever supply "something" becomes relevant.[2]

If one allows oneself to transfer the above, or Fyodor Dostoyevsky stark depiction of his contemporary medical guild, to economists, and other scientists. It might be worth to ponder if similar autonomy should be granted as we are subject to policies, and the like, grounded in deep scientific uncertainty.

Falsification by simulation

Simulation of complex systems is, as has previously been mentioned, often epistemically problematic. Instead of taking on the difficult task of finding explanations or predictions by simulating the overly complicated real world, the intention in this section is to replicate the dynamics of existing already abstract models.

As we consider systems where all the laws and relevant parameters are well-known, simulations become epistemically uncomplicated.

What was considered problematic as one set out to simulations the real world becomes an asset in this setting. The possibility to tweak initial conditions is now turned into a possibility. One may plot a map of the robustness space of a given model, which thereby demarcate under what set of parameters and assumptions where the model stays true to its target phenomena.

Now, as the simulation makes complex systems tractable, we may reintroduce aspects of the target phenomena that did not make the cut as the mathematical model was constructed.

These simulations are experiments that will be able to explore the mathematical models robustness, the conditions under which it is robust and applicable, as well as possibly falsify it.

Conclusion

First, it is noted that simulations often are epistically challenging if we try to corraborate a theory, or conduct simulations as substitute for a real world experiment. Simulations are deductive processes that unfolds the consequence of a model's rules and the given parameters. In order for the simulation to provide explanation or corroboration it therefore need to sucessfully immitate the relevant rules and parameters of the target process. This is in many cases impossible to achive.

Next, the focus is moved to mathematical modelling. Here we note that the abstraction and idealisation that is conducted to create theories with a fruitful calculus often are questionable.

Now, the nature of theories, such as represented by mathematical models, is to supply full specification of its laws,

[2] Parfrasing the BBC series "Yes minister"

and hence they lends themselves well to simulations. It is therefore possible to explore the parameter-space by simulation. We may also reintroduction excluded aspects of target phenomena in the theories, such as heterogenity and autonomity, and thereby demarcate where an existing theory actually

Maybe this methodology of falsification by simulation could help us to honour Hayek's curious task; in economics and elsewhere.

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