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The Methodological Challenges of Complex Systems

Commentary on Dirk Helbing

1 Introduction

In this rich and insightful paper, Dirk Helbing addresses a large number of methodological problems regarding the modeling of complex systems. Some of these problems concern models of complex systems in general; others concern particular models of socio-economic systems. The latter are the most complex systems we can think of, and it is by no means clear (and accordingly controversial) whether the modeling approach can succeed here at all. However, there has been a lot of progress in this field in the last years. This progress is made possible by the increase in computer power and the highly interdisciplinary nature of this research, which led to a variety of different modeling approaches. Helbing has tremendously illuminating things to say about all this, and I recommend this paper highly to all philosophers of science interested in modeling.

In this short commentary, I will discuss two central methodological points that Helbing makes: his plea for pluralistic modeling (Sec. 2) and the possibilistic approach that he advocates (Sec. 3).

2 Pluralistic Modeling

Helbing explains that we find a whole spectrum of types of models in the new field of socio-economic modeling. While some of these models are more detailed, others are simple (so-called ‘toy models’). Whereas some models use a physical approach, others use an economic, sociological, or psychological approach. It is important to note that all types of models have various merits and shortcomings. Even if we consider different models of the same type, we find that some models work well for a certain class of applications, while other models work well for other applications. No single model gets everything right, which makes it hard or even impossible to identify the one true model (if there is one). Besides, different models have different functions. For example, while some models allow us to make accurate predictions, others give us insight and understanding. Interestingly, no single model fulfills all desired functions. For these reasons, Helbing advocates a pluralistic approach to socio-economic modeling according to which the scientific community should study a variety of models for a certain phenomenon even if the different models are not consistent with each other and presuppose different "world views". Here are a few remarks on these claims:

I think that Helbing is right to stress that different models have different functions and that the scientific community values and desires all these functions (Frigg and Hartmann 2006). However, it may happen that different scientists give different weights to a certain function. For example, one scientist might find it most important that a model makes accurate predictions. Such a scientist will presumably favor a more detailed model to a simple model, as more detailed models typically lead to better predictions. Another scientist, who is interested in gaining understanding, will prefer a simple model which is easier to grasp, and from which she hopes to identify the essential features that bring about the phenomenon under consideration. It seems that both scientists are rational in their choice. Consequently, there does not seem to be a unique model choice, which is independent of the (arguably equally rational) epistemic preferences of the scientists. However, if two scientists agree on their epistemic preferences (i.e. on the weights they assign to different functions), they might well (and perhaps even *should*) agree on their model choice.

Our previous discussion has presupposed that the different functions of models are independent and mutually irreducible. That is, we assume, for example, that a predictively successful model does not automatically also provide good explanations. Moreover, the different functions are not entailed by a common goal such as truth. That is, we follow Cartwright (1983), who famously argued that "the truth does not explain much". Cartwright rightly observed that simple explanatory models are often far from empirically adequate, while detailed models do not provide much insight (Hartmann 1998). And so we have to make a choice. This typically means that some members of the scientific community explore simple models, while others explore more complicated models. Again, others explore some models in-between.

Despite Cartwright's skepticism, it is controversial amongst philosophers of science whether, and to what extent, the various functions of models can be reduced to the goal of truth. For example, some authors have presented ingenious arguments to show that unification (Myrvold 2003) and simplicity (Swinburne 1997) are truth-conducive. And while a lot of progress has been made here, not everybody is convinced. Consequently, I assume that a number of different epistemic and pragmatic values are associated with scientific theories and models, which we take to be irreducible, to the effect that different scientists (or different parts of the scientific community) endorse different values. There is another reason why the goal of truth is not privileged. After all, why would the scientific community care about a true but otherwise useless (e.g. too complicated) model?

To proceed, let us assume that two scientists agree on their epistemic preferences and ask whether a proliferation of models is methodologically advisable. Two considerations come to mind here: First, one may say that a proliferation of models is advantageous as the availability of alternatives helps finding the best model that satisfies the agreed-upon epistemic preferences. The belief here is that there is a best model, and that it is only a matter of effort to find it. Exploring several alternative models at the same time will then speed up the process of arriving at the best model. This is the *epistemically optimistic* view associated pluralism. Alternatively, one may believe that there is no best model, and that all we can do is to entertain a number of alternative models, explore them, and apply them as good as we can. This is the *epistemically pessimistic* view of pluralism.

It is not quite clear which of the two options Helbing favors. He presents some polemics against the 'one-true-model' view, but I am not sure whether his arguments also apply if we agree on the functions (and their respective weights). Helbing also does not say much about the conditions of adequacy of an acceptable model. Are all models equally acceptable? Or are there certain conditions that a model has to satisfy to be acceptable or (at least) entertainable? Does a model have to score high on at least one function? And: What if a model conflicts with some data? Shall we then wait and see if it accounts well for other data, or shall we reject the model right away in this case? It is difficult to find answers to these questions and to formulate reliable criteria. Much seems to depend on the details of the specific case, and on the judgments of the respective scientists.

Let us turn to the relation between different models for the same phenomenon. Helbing observes that different models of the same phenomenon make different assumptions about the world. Thus, it is interesting to ask whether these assumptions (and hence, the models) are compatible with each other. Helbing seems to be content with different models being inconsistent with each other. I have two comments on this: First, the situation in socio-economic modeling is not at all special in this regard. We also find alternative and seemingly incompatible models in more traditional parts of science, such as nuclear physics. There, we have a similarly rich spectrum of models ranging from, e.g., the liquid drop model to the various shell models. These models make fundamentally different assumptions about their target system and appear to be contradictory. However, there does not seem to be a reason for concern about these apparent inconsistencies. Firstly, all nuclear models are just models and, as such, involve idealizations. They are strictly speaking false and do not tell us the whole truth about the object or system under consideration. The different models rather complement each other, and each model provides (metaphorically speaking) a certain perspective on the phenomenon in question (Giere 2006). It is also interesting to note that the various models can often be approximately derived from (or at least be made plausible on the basis of) a more fundamental theory (Hartmann 1999, cf. Morrison 2011). Besides, the different models often have different domains of applicability. One model may explain certain aspects of a phenomenon, another model may explain others. If one adds the domain of applicability of a model in a ceteris paribus clause, the apparent inconsistency of different models disappears. And so I conclude that we should not worry about apparent inconsistencies. They only become a problem if we take the models too seriously (Hartmann 1996).

3 Possibilistic Modeling

Many of my above claims may be too liberal. Is it really false to assume that truth plays a privileged role in scientific theorizing? Should we not instead require that a model is at least approximately true? This seems to be a plausible requirement, as giving up truth altogether and focusing only on the pragmatic functions of models does not seem to do justice to the scientific endeavor. It seems reasonable to think that a model is only possible if it is approximately true, and so Helbing's (not worked out) possibilistic approach seems to require an explication of the notion of 'approximate truth'. To do so, several proposals have been made in the literature, see Festa et al. (2005) and Niiniluoto (1999).

On a related note, it is also hard to assign non-vanishing probabilities to models if we accept that the latter involve idealizations (i.e. false claims). Should we then not assign a probability of zero to the model? And if we do not assign a probability of zero to such a model, what does the probability assignment actually mean? Does it measure the usefulness of the model? But if it does, it is not clear why these (effective) utilities should follow the axioms of probability theory.

These are important questions, which any Bayesian philosopher of science who wants to account for the practice of science (in which models play an important role) has to address. Here, we can only sketch a proposal for how non-vanishing probabilities can be assigned to a given model. The basic idea is that idealization-involving models may nevertheless help us to make good predictions and account for given data *within a certain margin of error*. It seems that, given such an error margin, it does not matter whether we use a highly idealized assumption (such as "the Earth is a point mass"), or a more realistic assumption (i.e. that the Earth has a certain shape and mass distribution) if we want to calculate, e.g., how long a rock needs to fall down from a certain height. Replacing the true assumption by an idealized assumption is justified in this case. Clearly, this idea needs to be made more precise. But if we do so, it appears possible to assign probabilities to idealized models for a specific application, and given a certain margin of error.

With these probabilities, along with epistemic utilities that weigh the different functions of a model, an individual scientist (or a group of scientists) can then calculate the expected utility of different models and choose the one which maximizes expected utility. This scientist (or group of scientists) will then explore the model further, apply it, and study its domain of applicability. Given the provisional nature of the various models, it is however important that other members of the scientific community focus on other models. And if predictions are expected from the scientific community (as, for example, in the case of climate models), some average of the predictions of the various models should be chosen. But which average? One option is to simply use the straight average, i.e. to give all models the same weight. As Professor Helbing stresses, this strategy uses "the wisdom of the crowds" and often leads to much more reliable predictions. Alternatively, one could weigh each model prediction with its validity (as Professor Helbing suggests). However, I doubt that this works. Firstly, there will not be a consensus on these validities across the scientific community. All we have is subjective assessments of the true validity value. Secondly, to reach a consensus on the validity value, some kind of deliberation has to take place. However, Professor Helbing himself has shown that this procedure often leads us away from the truth (Lorenz et al. 2011). And so I think that taking the straight average of the various model predictions is the best strategy. It is also very easy to implement.

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