The World Is Not Enough! On the Limits of 'Grand Modelling'

Carsten Herrmann-Pillath

The authors present a laudable effort to enrich existing models of Earth system dynamics by including a much more complex description and analysis of subsystems and their interactions. In a nutshell, the result of such an effort would be a 'Theory of Everything on Earth'. For example, in the conclusion they envisage the possibility to further enhance the complexity of their approach in including a separate socio-epistemic taxon.

This reviewer is not an expert in the field of modelling. Generally, I think that the paper presents a good overview and offers a glimpse at how such type of models might look like, especially in their toy model. Yet, in my view there are several fundamental problems (what I am writing is against the background of my own work in the philosophy of ecological economics, especially https://doi.org/10.1016/j.ecolecon.2018.03.024 and https://doi.org/10.1016/j.ecolecon.2019.106526) . In addition, I commented extensively on an earlier working paper version, and my impression is that some of my points have not been adequately taken into account in this new version, but it seems to me that this is not a question of right or wrong, but a question, well, of different disciplinary worldviews).

The first point is what kind of data and methodologies are considered as 'state of the art' in modelling. The authors clearly recognize that their approach implies the inclusion of a wide range of other disciplines, even the humanities. But these disciplines do not live in peaceful co-existence: Economists and sociologists often sharply criticize each other, and both may consider the humanities more art than science. What are the implications for integrating widely diverging disciplinary approaches in one integrative modelling approach? My impression is that the authors are aware of the issue, but their reaction seems naïve: Just increase complexity of the model, multiply taxa and so on. Of course, they cannot really tackle this issue in this paper, but one would expect a more sophisticated discussion about the methodological implications. One certainly is that model builders themselves should come from different disciplines, and that a part of the 'modelling' would be a precise method for organizing their collaboration. In other words, the true 'model' would include the people, and not just what happens in their computers and what is manifest in the papers they produce. That is philosophically deeper than it sounds (think of actor-network theory, for example): A model of the type sketched in the paper is a dynamic structure of distributed cognition, and a full model description must include this meta-level. Indeed, this is obvious from the fact that the authors present a rich and informative overview of existing models across the disciplines, mostly pointing at their limitations. But what follows from that? Plugging the plethora of models together could be done by a lonely genius, or by a diverse team. In that respect, often I notice in the literature a tendency of 'clubbing together', both in co-author and citation clusters. Thinking systematically about the social organization of modelling must be part of the modelling!

That leads me to the second point, with a vengeance: as far as I can see, the authors do not even mention the concept of 'participatory modelling' which is becoming more prominent in ecological economics. This is about the social organization of modelling, again, but beyond science. In that respect, the authors continue to be overly simplistic about their notion of 'World'. They recognize that there is no 'one world', but what is the consequence? In a nutshell, participatory modelling means that the model must make the worlds explicit in which the agents live who drive the systems, and that must be done in asking them to take part in the modelling. This is mainly small-scale and most fitting to ecological modelling, which, as the authors point out, is mostly not done on the scale on which they conduct their modelling. But participatory modelling even further supports this type of 'fragmented modelling', since worlds are specific to groups, and the groups maintain highly various relationships (even open hostility and war). Therefore, I honestly criticize that the authors only pay lip-service to the insight that there is no 'one world'. If there are many worlds, and these are Uexkuellian worlds of ecological and evolutionary subjectivity, how can we include this in the type of models they envision?

The problem of the 'world' has many facets, and my third point is that I am missing a clear ontological grounding of the suggested taxonomy; or, a clear justification of their particular distinction of levels and systems/subsystems. There was a time when 'general systems theory' was fashionable, and one can learn from that. I mentioned that the authors just hide this issue under the slogan of adapting and increasing the complexity of the model. But why exactly didn't they include a socio-epistemic taxon right from the beginning? That's where the disciplinary debates happen! Just think of the anthropological debates about 'cultural materialism' decades ago (Harris versus Sahlins). After all, what are the primary determinants of 'worlds'? The authors cannot avoid tackling such foundational issues: Is it the ideas, or is it the underlying economics, or what else? This shows that there is a deep property of their models: The model is endogenous to what it describes, apparently from an external standpoint. But once you create a model of that scope and reach, there is no more any external standpoint. That means, if the model explains how, say, ENV and CUL interact, this applies reflexively on their model, since it is part of CULT (the authors mention this!). This is the deeper reason why you need participatory modelling: This breaks the circuit of reflexivity. The scientists will always get stuck in closed loops (just look at the world of economics!).

One aspect of this is providing a foundation for the taxonomy. What could be the alternatives? For example, there are no 'individuals' in the taxonomy. It's all about the larger systems, society, economy, the biophysical systems, and so on. But one could argue that 'agency' should become a taxon of its own. Why? Because the authors aim about 'causal explanations'. That's a big challenge! Can we really build theories of agency on causal explanations? Wouldn't we need a crucial part of the model which is not building on causal explanations, but on theories of agency? In my own work, I have therefore turned to a completely different explanatory framework, semiotics: The stuff of worlds are signs. That would result into a different taxonomy of models. Another example: the entire approach seems heavily anthropocentric. In debates about the notion of Anthropocene, this has been questioned by many scholars. In two of the main taxa we have 'socio'. But there are also good reasons to approach the technosphere as an autonomous domain, analogous to the biosphere. That would also reshuffle cross-disciplinary relations. I believe that the authors should discuss in a more principled way how we can develop scientific standards of taxonomy. Otherwise, what happens is what we see I the paper: They conflate the issue of taxonomy of models with the ontology of systems in their object domain.

Let me end with a personal note. I am deeply skeptical of this type of 'grand modelling', but that is no justification for rejecting the author's approach. The reason is that I work extensively on the relationship between economics and the sciences, not only in the field of ecological economics, but also in the field of neuroeconomics. Neuroscientists rarely build 'grand models' of the brain, it's just too complex. Philosophers of science have developed the so-called 'mechanism approach' or 'constitutive explanations' approach based on this research practice: Neuroscientists study mechanisms, not grand systems. The mechanism approach makes much sense in the social sciences, too, where it is increasingly received. If we regard the brain as too complex to be described in terms of a grand system, why should we hope that this is possible for an even more complex system (if only because it includes brains as parts).

I think that modelers must have the guts to discard claims of integrated ‘grand’ modelling and go for a fragmented, open-ended and incomplete framework of loosely connected, but empirically well-grounded mechanisms, both in the sense of generalized mechanisms and mechanisms operating at specific times and places. Why ‘have the guts’? They are under huge pressure to present forecasts and evaluations to policy makers, and they must at least present the impression that there is ‘progress’ in research, eventually enabling us to control our ‘World-Earth’. That is especially true for those researchers who are really worried about the state of the world. They must make big claims and must suggest that in principle, we could control ‘the system’, if only we accept our responsibility and our moral duties. Just acknowledging that the world is to messy for us to ever catch it with a model, would be self-defeating, and probably even on moral grounds, if that would imply that competing ‘fake news’ would reign the world. So, let it be.