

Taxonomy paper by Donges et al., for Earth System Dynamics, 2nd revision, response letter

Editor report

Dear authors,

based on the completion of the latest round of independent peer review I am very pleased to inform you that I would like to accept your manuscript for publication – subject to minor revisions. After you have made these revisions I will review them. There will be no further requirement for independent peer review.

Review 1 report contains some very valuable observations and in places recommendations. I do not think there are any specific issues that need to be addressed as the reviewer says most of these are largely foundational. I would recommend considering their comments about participatory modelling as this may be a good and specific issue with which to at least acknowledge some of the reviewer's concern. I do not think any substantive changes or additions are required here.

Reviewer 2 report contains some specific and easy to address issues. I reproduce them here: [...]

Once you have made your revisions and upload, I will do my best to ensure a speedy route to publication.

*Best wishes
James Dyke*

Dear Editor,

See below our specific responses to the reviewers.

Reviewer 1 (Carsten Herrmann-Pillath)

*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).

We emphasise again that our taxonomy is diagnostic, not prescriptive! It provides categories and principles to clarify and structure discussions about model combinations in expanded (whether actual, proposed or imagined) Earth system models. The reviewer imbues this with the idea that the end objective of modelling is a unified ontology or ‘theory of everything’, where all the boxes should be filled. It is precisely that kind of Frankensteinian mess that we want to help avoid, through enabling a more systematic discussion that goes beyond techno-methodological aspects of model combinations.

We welcome the reviewer’s suggestion of bringing a deeper philosophical approach to bear on this challenge. In Section 2.4 of our revised text we have now referred to the articles on the function and information aspects of the technosphere (which in the taxonomy mostly spans the MET and CUL taxa) and the philosophical grounding of ecological economics, where arguably more attention to CUL aspects would advance the co-creation of the field.

On p10, a reference has been added to line 6, and text added in lines 15-18: “... in general terms, their view of society contains aspects of our MET taxon, while “the economy” is

more restricted than MET. Hermann-Pillath (2020) argues that the field of ecological economics would benefit from more attention to the creative processes of 'art', which we would frame as CUL aspects that are largely absent from current conceptualisations (as also argued by ...)."

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?

Here too, our intention is much simpler than the reviewer suggests in the question above. We are not promoting efforts in the direction of "one integrative modelling approach"; we are observing that Earth system analysis already is integrating models. Model builders may come from different disciplines – and this may be desirable for many reasons, not least relating to the social realities of academic life and the place of science in society – but we would not argue that they necessarily "should" do so (eg, economic modellers don't work with physicists despite the physics origins of key modelling concepts.) When insights, representation techniques, algorithms etc are taken from multiple fields of knowledge, it is helpful for model-makers, model users and model critics alike to be able to categorise those domains not by discipline but by the kinds of interactions. Our taxonomy is not a structure for a model; it is a structure for enabling meta-analytic (if not necessarily metatheoretical) conversation about models and modelling.

We now make this clearer:

- in added text in the abstract (“combine and critique model components...”);
- on p2: replacing “the World-Earth system” with “World-Earth systems”, to indicate we envisage the same kind of diversity and pluralism in these new models as in current Earth system models and integrated assessment models;
- also on p2, adding clauses “should now be included on equal terms in a new family of models to conduct systematic global analyses of the Anthropocene”; and “nor do we intend it to serve as a universal blueprint for models of essentially everything”.
- on p10 line 24-25 (also in response to reviewer 2), adding “The taxonomy approach means that things that were previously included in models as opaque and unquestioned systems can be unpacked and critically examined. This would be of particular benefit to model users who were not the model builders.”
- adding reference in the conclusions to earlier discussions by Schellnhuber and Steffen et al about how World-Earth systems understanding and modelling efforts can evolve
- adding a sentence to the concluding remarks: “By supporting the development and discussion of new family of models, and not by pushing for a rigid and universalising model of everything, the taxonomy promises...”

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!

This would be a fascinating exploration for a future study. The individual contributions and social organisation of modelling have been examined for several other contexts (a prominent example is Nordhaus and DICE). For Earth system analysis, arguably the field has been one of international collaborative strategic design more than a tradition of a lonely genius plugging bits together, making it harder to see this as a tractable approach now.

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 Uexküllian worlds of ecological and evolutionary subjectivity, how can we include this in the type of models they envision?

We tackle the rich themes raised in this paragraph in reverse order. We now refer to “World-Earth systems” in plural, not singular, throughout the paper, so the text now indicates more explicitly that many framings and representations are possible in World-Earth modelling.

Any given model will (likely) represent one framing of the system, just as they do in Earth system modelling. For example, in ESMs, the different representations of atmospheric processes in energy-balance models and general circulation models enable or constrain coupling of different subsystems – ocean, land, cryosphere, etc. Thus choices about representations in the different taxa will have consequences for the design and viable combinations of components of World-Earth models. A purely subjective ‘world’ might not be one of the many worlds that can be integrated in World-Earth models, for instance if (say) it obviates the analytic or predictive power conferred by ‘objective’ things such as the specific heat capacity and freezing point of water.

We disagree that all world-modelling must ask the agents to take part in the modelling. We view participatory modelling as an example of how a system can come to be represented in a model, not what is represented (although in some applications the participation becomes the model, such as in real games). However, our taxonomy might be useful in participatory processes, so we add the following to our conclusions:

p21 lines 8-10: “It can help with operational model development as is illustrated by the work reported in the companion paper (Donges et al. 2020). It can also help in interdisciplinary communication, model critique, and potentially even participatory modelling processes by providing an organisational scheme and a shared vocabulary to refer to the different components that need to be brought together.”

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 in the paper: They conflate the issue of taxonomy of models with the ontology of systems in their object domain.

We reiterate that the taxonomy is NOT a model blueprint. Of course there are ontological aspects to this taxonomy – and we deliberately do not want to narrow their possibilities, just make them more readily visible. For instance, Figure 1 helps to show that we do not view ENV and CUL as necessarily incommensurate, despite their very different ontological foundations.

To make this more concrete: "secularization" does not figure at all in the materialist positivist ontology of ENV representations of climate and the water cycle, just as the specific heat capacity of water (to return to that earlier example) does not have a place in models of the social construction of the spiritual meaning of sacred springs. Yet an World-Earth system analyst may actually choose to combine these entry points in a model. Such models are currently given the expansive umbrella labels of "hybrid" or "integrated" models, but we argue that it would be useful to be able to describe such an effort more explicitly and systematically as, say, a hybrid CUL→ENV model.

We do not expand on the socio-epistemic sub-taxon idea for the reasons we give when we mention it, namely, we see science & technology as a small part of wider culture, and the taxonomy is focused on a compact set of "higher-level" taxa.

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.

We are actually coming from the same position of skepticism about ‘grand modelling’ ambitions and claims. The taxonomy provides a way for scholars to expose, structure and discuss the differences between modelling efforts, not a structure to promote this universalizing approach. We hope that it will help people to recognize when and in what ways their model toolkit is incomplete or inconsistent, because it is in those contexts of use and discussion that we have found it useful ourselves.

Reviewer 2 (Birgit Müller)

End of page 19 on the version with track changes: “Overall the DISCOUNT model...”. It seems that sentence is not fully written out (see “...”). Please check.

Thank you for noticing this mistake. We have completed the point we were trying to make! The revised text now states: “Overall, the DISCOUNT model provides a first test of the taxonomy’s guiding principles. It demonstrates the taxonomy’s operative capacity to trace links between established dynamical systems methodology and macro behaviour; it is compatible with diverse research fields, here linking carbon cycles and social learning; and it has appropriate compactness, since tracing the loops and flows between taxa in this World-Earth model do not make us need to rethink the whole structure of the taxonomy.”

Suggestions to the authors:

2. *With respect to an answer of the authors to reviewer 1: “The taxonomy approach means that things that were previously included in models as opaque and unquestioned systems can be unpacked and critically examined. Model users who were not the model builders would really benefit from knowing (to take the example above) whether the representation of an education decision process in a model is in MET or CUL.”*

Suggestion: Perhaps this illustrative short example on education could be added to the manuscript. It points out the additional value of clarifying the assignment of submodule to a certain taxa by the modeller.

We have added this point as an additional paragraph on page 10, section 2.4:
“The taxonomy approach means that things that were previously included in models as opaque and unquestioned systems can be unpacked and critically examined. This would be of particular benefit to model users who were not the model builders. For example, education may be explicitly linked to demography (as in various integrated assessment models), so typically would be treated as a quantifiable and accumulable process in the MET taxon: i.e., investment in women’s education results in a lower birth rate and

therefore less future land use. In CUL, education would perhaps be treated in a more relational way dealing with the spread of ideas, development of communities, changes in power structures etc.”

3. *With respect to an answer of the authors to reviewer 2: “Since to our knowledge it is the first published model which endogenizes the choice of discount factors used in climate policy.”*

Perhaps this is also worth to mention as novelty in the paper?

Thank you for pointing this out. We agree and have emphasised this important point more in the abstract, introduction, model description and conclusion sections:

- p2 lines 1-2 “As an example, we apply the taxonomy to a stylised World-Earth system model that endogenises socially transmitted choice of discount rates describing how much societies value the present relative to the future) in a greenhouse gas emissions game.”
- p3 lines 24-25 “...we study an example of a novel World-Earth model that seeks to overcome the long-standing challenge of endogenising the choice of discount factors in climate mitigation studies.”
- p17 lines 5-7 “The novelty of this model is that it endogenises socially transmitted choice of discount rates in a greenhouse gas emissions game to illustrate the effects of social-ecological feedback loops that are so far typically not considered in current climate economics and IAM modelling efforts.”
- p22 lines 3-5 “We use the Copan:DISCOUNT model to demonstrate the value of the taxonomy for tracing how dynamics and feedbacks loop through different taxa, enabling better model design and communication about path-breaking approaches to World-Earth modelling.”