

Interactive comment on “Taxonomies for structuring models for World-Earth system analysis of the Anthropocene: subsystems, their interactions and social-ecological feedback loops” by Jonathan F. Donges et al.

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This is a timely and much-needed effort at creating more realistic and inclusive/integrative models of the Earth system that meet the challenge to model the complexities of human action in various systemic contexts (such as economy, society and so on). It is far too easy to criticize such endeavours because they necessarily offer many points of attack, being the first steps in this direction. This would be unfair. In my review, I try to find a proper balance between necessary criticism on the one hand, and endorsement and encouragement on the other hand. I am writing from the position

of an economist working in the field of evolutionary, institutional and ecological economics (hence, clearly 'heterodox') who always injects a strong dose of philosophy in his work. That is, I am an outsider to the modelling community in the Earth and Climate Sciences. I will always combine general observations with specific comments on the discount model. Yet, my focus is on principled issues, hoping that this might provide different perspectives than those within the modelling community. The authors explicitly recognize the philosophical dimensions of their work; hence I start out from there, and I congratulate them for showing the grit to use the notion of 'World-Earth system' (p. 2). However, this immediately raises many philosophical questions. 1. In which sense can we separate 'Earth' from 'World'? Although we must assume that there is an 'Earth' interacting with 'World', the 'Earth' is only accessible via the sciences which construct it as a 'World'. That means, we must recognize the fundamental fact of our incomplete knowledge about 'Earth'. In other words, a complete model of the 'World-Earth system' must be reflexive in the sense of including the Earth Sciences (and others) as endogenous generator of our scientific conceptions of 'Earth'. There are direct implications for the subsystems: For example, our knowledge about the biophysical mechanisms will impact on public perceptions of policy issues, and so on (which the authors aim at modelling explicitly, indeed). These will feedback on funding Earth Systems sciences, and hence will determine how 'Earth' will appear to us in future times. I think that science is an essential part of the 'social-cultural taxon' and cannot just be imagined as being exogenous. But if it is endogenous, 'Earth' is endogenous. Looking at the discount model, this seems implicit to the role of the parameters p and q . Although these are essential for driving the specific model dynamic in determining the probability of switching discount rates, they are not explained in any way. To be frank, I have difficulties in understanding their meaning. What exactly is 'curiosity' of a country? What is 'myopic rationality'? Many people would just think that the latter means, well, steep discounting of the future. Another definition would be the length of the time horizon, which differs from discount rates applied within that horizon. Thus, besides from the need to give precise definitions these parameters and explain how

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they could be measured, I suggest that they refer to the endogeneity of science. Just consider Trumpian America: the recent issue of 'The Economist' has an article about 'Swamp Science' at the EPA. This is actively reducing 'curiosity', it seems to me. In other words, I think that if one uses the 'World-Earth' duality, one faces the challenge of treating human knowledge about 'Earth' as endogenous. There is no external standpoint of the model-builder. Another excellent example for this problem is the treatment of 'damage' and 'welfare' in the discount model. It seems that the authors think that there is an objective measure of welfare and damage. But we know that this is one of the most difficult and disturbing aspects of IAM, namely that the damage function is endogenous and depends on the discount rate. That is why some economists now even take the very radical step to build their models without damage function (Llavador, H., J. E. Roemer und J. Silvestre (2015): Sustainability for a Warming Planet. Cambridge University Press). But behind this is the simple, but deeply philosophical problem that neither welfare nor damage can be assessed from the standpoint of an external observer. Apparently, the authors are aware of this, as on p. 17 we find the expression: $s(C)$ ($ENV \rightarrow MET \rightarrow CUL$). But that implies that social learning may not only happen via imitation of discount rates, but also via diffusion of valuations, or, 'worldviews'. 2. The next foundational question is whether there is 'ONE World', which seems implicated by putting both World and Earth together into one 'system'. Obviously, this is not just annoying sophism: There are many philosophers who cast doubt on that (just mention the German philosopher Markus Gabriel with the provocative book title 'Why there is no World' 'Warum es die Welt nicht gibt'). The serious argument behind it ties up with the previous: Science constructs worlds, and there is no necessity that these are just 'one'. This is even more evident if we consider human worlds: Actually, it is the CUL taxon that creates the 'worlds'. I think that it would be most helpful for the authors to look at Bruno Latour's recent work on Gaia and the more general work on 'modes of existence' (see <http://modesofexistence.org/>). Latour distinguishes between different ways to bring 'worlds' into existence, such as religion, economics, or law. The philosophical backing is different criteria for truth. Personally, I do not fully endorse his

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approach, but it connects with many other philosophical streams that, most generally, analyse ‘social ontology’. One of the most concise approaches is Searle’s distinction between ‘mind dependent’ and ‘mind independent facts’. Well, ‘mind dependent facts’ are – facts. That means, they have the same ontological status as other ‘facts’ conventionally treated as such by the sciences. Turning to the discount model again, a core question is the ontological status of institutions. From the viewpoint of institutional economics and Searlian philosophy of institutions, the authors appear to be imprecise in treating institutions because they seem to suggest that a clear boundary can be drawn between CUL and MET. Granted, there is the overlap in the diagram, but what does that mean? I think the problems crystallize in the question how to deal with the economy: Is the economy a ‘world’ of its own? It is a very specific and very comprehensive institutional structure that creates ‘realities’ to which we need to adapt, in the eyes of many (the infamous TINA principle). Accordingly, some economists think that the discount rate should follow the market interest rate, as this is the only way to define a ‘collective discount rate’ apart from individual time preferences. Markets are real, everything else is ‘subjective’. This ‘objectifies’ markets just in the Searlian sense. Indeed: Which other way do we have to generate a ‘country’ discount rate in an empirically meaningful way? The authors introduce this with a sleight of hand, but this is a very strong ontological projection! I think the discount model cannot simply take a ‘country discount rate’ for granted – unless that would be the discount rate that governments apply in their policy making framework. In my own work, following dystopian statements by experts such as Pyndick (Pyndick, R. S. (2013): Climate Change Policy: What Do the Models Tell Us? Journal of Economic Literature LI (3), 860-872), the discount rate is the central parameter that manifests the mutual irreducibility and incommensurability of economy and ecology, which becomes manifest in the methodological troubles of IAM. The authors refer to the fact that economic models always refer to monetary variables (p. 8) – that is what constitutes a ‘world’ in the Searlian or Latourian sense. Thus, I wonder whether a minimum requirement for building a discount model is to include a model of the economy that generates a reference interest rate as a ‘social fact’. 3. I

can only hint at these issues here, but the central question arising from this is how to justify the assumption of an integrated 'World-Earth system'. I would bet on a 'Multiple Worlds-Earth ...' – system? Again, Latour has a strong point in rejecting the very notion of a 'system' as that would imply integration, coherence, and so on. In more practical terms, that leads us to consider the question why the authors did not follow a more traditional (hence probably outdated) approach in distinguishing between different 'systems', such as 'the economy', 'the society' and so on, which are not integrated, but may stay in fundamental tensions and contradictions with each other. Again, this is not merely a philosophical issue. In the Anthropocene literature, many critics point out that it is misleading to confront an abstract notion of 'human system' with 'Earth system' as this papers over the fact that the 'human system' is deeply fragmented and conflict ridden, and hence fundamentally politicized in a most general sense (as an exemplary work, see Bonneuil, Christophe and Jean-Baptiste Fressoz. 2017. *The Shock of the Anthropocene: The Earth, History and Us*. Verso London New York). This is the real challenge that the authors must meet: Catching this complexity in ONE model. Given these caveats, I think that a comprehensive approach would indeed be well advised to go back to ontological fundamentals. Mario Bunge's formal ontology seems still unsurpassed to me (Bunge, Mario (1979), *Treatise on Basic Philosophy, Volume 4. Ontology II: A World of Systems*, Dordrecht: Reidel). Perhaps this would allow for formalizing 'multiple worlds'. I am not sure how a model would look like. Perhaps it would be a set of modules that are only loosely integrated, with certain thresholds that block interaction below them, under normal circumstances. That means, for example, the 'economic world' would generate a discount rate that would be isolated from other worlds, until a catastrophe happens which triggers sudden spillovers. Modules would run separately for longer periods, until connections become activated that would generate sudden changes across the modules. The discount model might consist at least of two modules, one economic, the other cultural. The latter would include public opinion, value changes, and so on. I think this is what we observe in reality: Public opinion might shift towards de-carbonizing the economy, but the economy seems to move towards

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sustaining it endogenously (think of the continuous process of postponing depletion of fossil fuel reserves) (see Covert, T., M. Greenstone und C. R. Knittel (2016): Will We Ever Stop Using Fossil Fuels? Journal of Economic Perspectives, 30(1), 117-38). Then, everyone is shocked learning that Germany even increases CO2 emissions! 4. Another issue is the principles how to build a taxonomy. The authors work with causal concepts, such as 'mechanisms' and 'feedback loops'. This is a basic methodological requirement, for sure. But it implies that the taxonomy must be based on theories and hypotheses about causality. This creates a very, very high benchmark. Taxonomical work is often more modest, such as biological taxonomy that is based on notions of descent, similarity and so on, without classifying patterns of underlying causalities. This is one advantage of working with old fashioned systems categories: They offer simple criteria of building taxonomic classes, such as treating the economy as a system in which money and monetary values are coordinating media, and then subdividing this in other systems, such as sectoral, regional or developmental. That would imply for the discount rate, for example, that one would minimally distinguish between 'North' and 'South' as economic systems which have different reference rates (maybe North can afford to be more 'patient', South not). But if one refers to causality, every single example given by the authors is a Pandora's box out of which theoretical controversies and cross-disciplinary battles emerge (in fact, the authors often mention that many disciplines deal with the various mechanism that they subsume under their categories). Just take one: there is the ENV-CUL taxon, and cultural identity as 'sense of place' is mentioned. I assume that many cultural geographers would immediately protest. Does that mean that there is environmental determinism regarding culture? I do not think that the authors believe that, and after all, there is also the CUL-ENV taxon. But how can we deal with this in building a model for one cultural element, 'sense of place' (which certainly is important in the context of modelling migration, induced by climate change)? In this single example, very complicated theoretical issues regarding causality, its direction, and divergences between different disciplines are involved. Which of the competing positions in the literature would be selected to model this single ele-

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ment? Clearly, that would directly affect the taxonomy. In other words, the taxonomy is built on a minefield of theoretical and cross-disciplinary controversies. The difficulties multiply if one considers larger complexes, such as ‘the economy’. I appreciate very much that the authors recognize complex feedbacks such as the cultural shaping of preferences, or their technological determinants. But all this is object of deep controversies between the different disciplines. ‘The economy’ looks very different in the eyes of the economist or the sociologist. Which position should be included in the ‘World-Earth-System’ model? Even the most basic assignments to taxa would be affected by this. For example, many sociologists would assume that ‘the economy’ belongs to the socio-cultural taxon, and that repercussions on the other taxa work via technology. My comment is already very long, so I stop here. I am afraid that my comments appear overly destructive, but this is not my intention. I hope that the authors take it as creative stimulus. Yet, my general point is serious: If the taxonomy is about causal patterns, it needs to be built on theories. The authors assign various elements to different taxa with levity, without considering that all this is subject to many competing and often contradictory theories across the entire universe of disciplines, not only the sciences, but even the humanities. Evidently, their model must also be a model of cross-disciplinary relations, to avoid unjustified essentialization and hypostasis of assumed mechanisms, loops etc. This is, well, the ultimate ‘Theory of Everything’ coming along in the disguise of a model? I hope that we will continue with this discussion and hope to learn from the authors’ response, as well as from the comments of other reviewers.

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