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Earth System Dynamics Discussions

# *Interactive comment on* "Societal breakdown as an emergent property of large-scale behavioural models of land use change" *by* Calum Brown et al.

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This manuscript presents a modelling experiment using a large-scale behavioral model of land-use change over Europe.

Overall, the research done is very valuable. In itself, it is not a breakthrough, as it builds on many earlier modelling efforts and explores one additional aspect of what can be done with this modelling framework, but it reveals a series of interesting insights.

However, in its present form I don't think that the manuscript is ready for publication. It misses details on some important methodological aspects as well as on some of the results that are at the core of the added value of the paper. These two issues are interlinked, in the sense that without these methodological details it is not really



possible to appreciate the value of some of the results, and these results themselves justify some of the methodological progresses.

So my main comment is to clarify key methodological aspects related to the ecosystem services demand and supply calculations, as well as the land use decision-making process, and to better present the results in particular related to the behavioral aspects. Most of my substantial comments are related to this. I return to this more in details below, and add two other substantial comments, on the evaluation of the scenarios based on "shortfall", and on the differences between standard economic models and this effort, as well as some minor ones.

1/ On gaps in methodological description and results presentation:

In general: I understand that the two modelling frameworks (Crafty and IAP) have been described elsewhere, but in order to understand the added value here more explanations are needed.

The behavioral part of the scenarios is very lightly, and unclearly, described. There's 3 lines on p.4 (l.24-26) to introduce the fact that the Agent Functional Types (AFT) have different behaviors, and then 10 lines p.7 (l.11-20) which describe very vaguely these different behavioral parameters. Beyond this, the basic behavioral and decision-making framework of the agents is not clearly described. The sentences on p.5, l.24-31 are very unclear to me. The description in the Appendix, in particular p.31, l.6-10 (which re-explains, but much more clearly, the p.5 l.29-31), and the caption of Table A4 p.38, are much more clear to me. This should be in the main text. Basically, we need to understand how agents make decision to either maintain or change land use, or how they are outcompeted by others. Just as two examples, in the present version it is impossible to understand how an agent can be "outcompeted by other agents" (p.7 l.16) or why "the more extreme behaviours [are] being selected out by a competitive process" (p.13, l.29).

This is similar when it comes to the results of this exploration. The results are introduced on p.9, l.25, but this is in fact referring to the Supplementary Material, and to Table 2 which is just a narrative summary of the results of the different scenarios, including the sensitivity of the behavioral parameters. But no actual result is presented in the main text. To me, this is insufficient.

As this is part of the title of the paper and one key aspect in the paper is to argue that such behavioral models are important to explore potential decision-makings that differ from monetary optimization, this would deserve more details. I do understand that one key conclusion is that is is not the behavioral parameters in themselves that matter so much, but rather their basic existence in the model, so that outcomes do not differ so much depending on the behavioral parameters but do differ between this model and others based on neoclassic economics. But still, if you want the reader to buy that idea, you really need to explain much more clearly how do the agents in this model, behaviorally-speaking, differ from basic monetary-optimizing agents that are implicit in many other LU models. And you should find a way to present, in the main paper, some of the results of the behavioral exploration. Currently, this is a set of graphs in Appendix 2 which will be, in essence, totally inexistent for most readers. I understand that this si lot of graphs to summarize in perhaps one or two Figures in the main text, and it probably requires some creativity, but I really think that discussing these results without presenting any of them in the main article is not correct.

The discussion on this (e.g., p.11 l.5-7) is itself very thin, and sometimes not very clear (e.g. p.13, l.5-11)..

2/ In addition, I have two other substantial comments:

2.1/ On the evaluation of the scenarios based on "shortfall": One key outcome on which the scenarios are discussed is the relative shortfall between demand and supply. This notion brings ambiguities. As discussed by the authors themselves (p.12), if supply really crashes because of socio-economic collapse, then at some point the demand will fall too, in a way that is not captured in the model. OK, but my concern also goes in the



other direction: The scenarios that correspond to high socio-economic development most likely generate a higher demand. If there is a shortfall in these higher demands, does that really mean that society's well-being is harmed? Is it possible to consider that some shortfall in a high-demand society reflects something more like a reasonable supply, which may lead to sufficient consumption? To formulate this in a less normative and more technical way, is it appropriate to only evaluate the outcomes in terms of the shortfall between demand and supply, or would it be reasonable to also evaluate the outcomes in terms of the overall (absolute value) of the supply?

2.2/ On the differences between standard economic models and this effort: The discussion, p.12 l.9-12, suggests that these economic models would be unable to represent such a collapse. I'm not totally clear about all the reasoning here. These models would indeed (l.10-12) display rising food prices, and thereby some maintenance of food production, but I'm not totally clear on how this would be so different than the results presented here - noting that the demand isn't adjusted here, as acknowledged by the authors. p.8, they say: "Conversely, where these capitals declined substantially, widespread extensification and abandonment of land occurred...": Yes, that makes sense, but you would expect this to also occur in standard economic models. What is precisely the argument?: - That this model predicts a much stronger decline in food production than standard economic models, then this has to be substantiated by numbers, - Or that this decline is more realistic than the lower decline in standard economic models, then this has to be justified convincingly. The idea in standard economic models that with production shortfalls, prices would rise, which would thus somehow buffer the production shortfall by mobilizing more capital towards agriculture is reasonable, especially considering that at some point anyone would have to admit that food is a basic need.

3/ A few more minor comments:

* Abstract: "economic irrationality": This is an ambiguous formulation. If one sticks to monetary profit optimization (I agree that this can be called "standard" economic mod-

els, but this needs to be explicit), lots of behaviors are irrational, if an "enlightened" economist expands a utility function to encompass pretty much anything, then it is hard to find any irrational behavior, and so on. (without entering into the whole discussion, things like imitation, sticking to one's behavior, and so on, can be perfectly rational under a given set of information and agentic capabilities). Better rephrase without such a connotation, or perhaps at least talk about irrationality in regards to monetary profit maximization. Next sentence, "this theoretical optimum" bears the same unclear connotations to me. This notion of "irrational" agent comes back later on and is misleading to me.

* p.2: "... where they are most required; when socio-ecological processes break down...": Yes, but this is only one example, any other situation of regime shift / systemic change / land-use transition brings similar challenges for basic land system models, be they based on economic rules or on statistical calibration.

* p.8: "..., which were not substantially reduced...": who is this "which"? The following sentence seems to suggest that you refer to the divergences in land system outcomes, but the sentence is odd as it is not correct to write that "divergent land systems (...) were not substantially reduced".

* p.9: l.1-15: This is described qualitatively. It would be good to find a way to present quantitatively the differences between these scenarios, in a way that would convince the reader of some of the points made, for example that SSP3 has such an impact compared to the climate scenario.

* p.11: l.14-17: Maybe yes, maybe no. This depends on how is the actual balance between ES, compared to your own way to balance them. But still I agree with the conclusion of the following lines that better understanding how these trade-offs are actually formulated in reality is crucial.

* p.11: l.23-24: and this would likely reinforce the shortfalls, right?



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