

## ***Interactive comment on “Sustainable use of renewable resources in a stylized social-ecological network model under heterogeneous resource distribution” by W. Barfuss et al.***

### **Anonymous Referee #1**

Received and published: 2 November 2016

Dear Editor,

General comments:

This manuscript satisfies your editorial criteria as described at [http://www.earth-system-dynamics.net/peer\\_review/review\\_criteria.html](http://www.earth-system-dynamics.net/peer_review/review_criteria.html).

The authors of this manuscript extend an existing model to explore the implications of resource heterogeneity, and social interactions, on the sustainable use of resources.

This manuscript makes substantial contributions to methodological developments that

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are required to advance science, however some of its fundamental definitions and arguments need to be reconsidered or reinforced, and some of the results have to be clarified and put into context. Therefore I recommend inviting the authors to carry out a major revision. Since this paper seems to have a seminal intention, I would suggest the editors to be generous in terms of space.

Specific comments:

The manuscript make substantial innovations, explaining the potential to extend the ideas presented into integrated models, although the latter do not have their main focus on private renewable resources, as described here, so a cautionary note should be added in the discussion or conclusions.

The theoretical example to which this model is applied has been chosen with certain misfortune, there are very few examples that follow the description of the resource. I can only think of a fisher's club who own the right of fishing in a river, with different amounts of fish in different portions to which the club members have restricted access, and even in this case the fish would migrate. Can you think of more examples? They should be mentioned to make this exercise sensible, and to add sense to the potential knowledge transfer towards integrated models, which seems to be a side goal because it is mentioned a number of times.

The manuscript makes a very well detailed description of methods; however, in the definition of the resources, I would like to suggest clarifying a number of points: is it a common pool resource? Does it have private access to its sections as accessed by agents? Would not it be more appropriate, in the context of a socio-ecological interaction, to talk about heterogeneity in the access to the resource?

More on confusions on the resources: you say “maximum stock (commonly referred to as carrying capacity)”. You here likely refer to the “maximum sustainable yield” or to the maximum stock that “can be extracted” per unit of time without compromising the future of the resource (commonly referred as sustainable use, but not always). If used

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alone, maximum stock could be understood as being the resource base.

You mention a “preference formation process” in a “social preference network”, can you provide an extended rationale for the process? Why preferences are formed in the way you describe and implement in the model, is there a theoretical paradigm you follow, and which is the evidence for it? This should be detailed and crystal clear because it is a fundamental basement of the whole contribution, from the current 5 lines at the beginning of section 2.1 is not possible for the broad climate community to see whether there is a rationale for your model. For instance, one of the questions to be considered in such suggested extended rationale is “how this model follows Traulsen’s results on strategy updates”, but not only mentioning it as in sentence 13th of page 4.

In Figure 3, the homophily parameter is referred as “rewiring”, but that term exist only after section 3 starts. Why to use such term without mentioning it before, please be consistent over the terms and definitions used all along the paper.

Section 3 (Results) is not accessible to the broader climate research community. This section should be rewritten, with a focus on making the results accessible to climate researchers with diverse backgrounds, from impact to pure circulation modellers. This comment alone justifies the need for a major review.

Page 8, line 4: “In this setting” helps little to understand where to refer, to some of the displays in Figure 3? To the entire set of results?

Page 8, line 4: it can be argued that the fact that in the model “here, social interactions and thereby the comparison of harvest rates typically happen when the logistic resource has been harvested for a sufficiently long time” is an unrealistic assumption with no basis that conditions too much the results and therefore makes them invalid and its interpretation worthless. Can you explain such assumption as incorporated in the model and why the results are valid?

Page 9: “Non-sustainably harvesting agents exploit their resources exponentially”. I

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wonder if you got confused here by the definition of the distributions used: exponentially means in this sentence that each time considered for the extraction the agents extract more resource according to a (large?) exponent, which seems far even from unsustainable behaviours (e.g. if acting as described, any country would finish its natural stocks in very few years).

Figure 4 and comments about it seem to be overvaluing the differences, which are visible but not portentous; can you provide a statistical measure showing the significance of the differences observed?

The results are seemingly contradictory to reasonable expectations on the following point, I suggest justifying why. Interactions faster than resource growth lead to unsustainable outcomes; this can be learnt from each of the graphs you present: shorter social interaction time scale brings the average fraction of sustainable nodes below 0.5. Overall these results imply that higher exchange of knowledge between resource users is bad(!). Why is that reasonable? The paper does not make a strong case on this item and I am left wondering whether this is just the result of the assumptions made while constructing the model.

Furthermore there is no discussion in Section 3. No logic or context is provided for it beyond a purely mathematical interpretation of the results. I would suggest making this section meaningful and functional by including a discussion of the results in the light of the existing literature. I perceive this could make the paper more impactful and meaningful to the broad community working on similar issues.

Technical corrections:

Overall, well presented and written.

Page 3, the references for homophily do not need internal round brackets, a “,” suffices.

You use these terms often: strategy update and social update, if referring to the same, use only one (strategy), otherwise clarify.

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What is a World-Earth model? Please provide a reference explaining it, or explain clearly for all climate-research audiences when first mentioned.

Last paragraph of page 7 could start referring to Figure 3. (. . .observed" in Figure 3":)

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Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-15, 2016.