**Response to interactive comments on "Sustainable use of renewable resources in a stylized social-ecological network model under heterogeneous resource distribution" by W. Barfuss et al.**

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# Review 1

### General comments:

This manuscript satisfies your editorial criteria as described at
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 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.

We thank the referee for taking the time to review our manuscript. In the course of the upcoming revision we plan to follow the suggestions on extending fundamental definitions needed to comprehend the purpose of the reported modeling framework. We will also discuss in more detail the implications of the obtained results to make the results more accessible to a broader audience.

We are confident that such procedure together with a response to the issues raised below will greatly improve the work presented.

### 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.

We are thankful for being pointed at this possibly misleading issue. In fact, our main focus is not to present a model that aligns with concepts from integrated (assessment) models, but rather to illustrate possible conceptual alternatives. One core ingredient of integrated models is the observation of averaged quantities under equilibrium conditions which is a standard way to study established macroeconomic processes. Our main intention here is to highlight the importance of understanding (i) the possible ways to reach the above mentioned equilibrium conditions and (ii) to assess the role of individual actors on the formation of such an equilibrium state. Our approach is neither comprehensive nor applicable to real-world processes yet, but it serves to illustrate that individual actions, which are usually suppressed in integrated models, may allow to drive a system into different equilibrium states. We will clarify our intentions in the course of the revised

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manuscript.

> 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.

Our intention is not to model a real-world process in detail. The idea behind the model design results from the fact that on a conceptual level, human-environment interactions are either happening in a common-pool setting (e.g. climate change) or a private setting (e.g. some forms of land-use such as agriculture). On the one hand common-pool dilemmas have been extensively studied in the recent past. On the other hand agents can exchange harvesting information over the common pool itself, i.e. interactions between the agents happen via the ecological submodel. This is why on the contrary, we chose agents interacting with private resources. This was done to emphasize that these interactions between the agents are purely social-cultural, to make the case that this is an important domain of processes for further investigation.

The logistic resource function was chosen as the simplest and well established resource function, that may represent fish stock, but is also used in the context of forest growth.

> 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?

The resources exemplary and stereotypically studied in this work are not of a common-pool type but individually assigned to each of the agents. With that respect no discrimination has to be made whether parts of the resource are accessible only by certain agents or whether there exists some distinct partitioning as each agent thus harvest its own resource exclusively in private. We will clarify this property once more in the course of the revised manuscript.

> Would not it be more appropriate, in the context of a socio-ecological interaction, to talk about heterogeneity in the access to the resource?

Since we model private resource harvesting (see above), the size of the resource capacity denotes the amount of resource an agent has access to. Thus, heterogeneous capacities can be interpreted as one operationalization of heterogeneous access to resources.

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

Exactly, "maximum stock" was supposed to denote the carrying capacity. We will clarify the respective terms in the manuscript's text.

> You mention a "preference formation process" in a "social preference network", can you provide an extended rational 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.

We will make the motivation of the chosen social process more clear in the respective sections.

> 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.

We are thankful for being pointed towards this inconsistency in our manuscript. In the course of the revision, we will take care to clarify such terms when introducing the model components in Section 2 and also ensure consistent usage of the respective terminology in the remainder of 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.

In fact, in its current state the manuscript may seem more tailored to an audience with a background primarily from (socio-economic) physics or social-ecological modeling. Furthermore, the model does not exhibit any climatic component, but focuses primarily on dynamics related to social interactions in conjunction with  local ecological dynamics.  We therefore acknowledge that in its current state a solid knowledge of the related terminologies from physics and social-ecological modeling is required to comprehend the results put forward in the manuscript.

To satisfy the interdisciplinary self-understanding of "Earth System Dynamics" we will first thoroughly revise all definitions of model components and give approprie real-world examples wherever possible in order to put the model setup itself into a comprehensive perspective. Secondly, we will add to the technical discussion of the results more vivid and illustrative explanations that puts our work into relation with observed real world systems. We are convinced that these two steps will make the results more accessible also to a broader audience by reducing the need for prior technical knowledge to a bare minimum.

> 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

Originally, this phrase was intended to refer to the specific set of parameters that was used to obtain the results that are discussed in the respective section. We will carefully check for misleading statements and add such references in the course of revising the manuscript to ensure that all results are discussed consistently with respect to the correct initial setting of parameters.

> 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?

We compare the social interaction rate (tau) from faster to slower rates with respect to the resource growth rate. This was done to gain a system's understanding of the interaction processes. We do not intend to make claims how these two timescales relate to each other realistically.

> Page 9: "Non-sustainably harvesting agents exploit their resources          exponentially". I 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).

Exponentially here refers to the time period the resource gets extracted, not a distribution among the agents. We will clarify this misleading formulation.

> 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?

Thank you for pointing this out. We certainly see the need to strengthen the evidence our discussion is based on. Each point in the 4 parameter space sections shown Figure 4 summarizes 250 ensemble runs. It therefore constitutes a statistical measure. Nevertheless, in the revised manuscript we will update Figure 4 to emphasize more on the differences between the log-normal and the normal based distributions.

> 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.

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We too find this an interesting result. Yet it is explainable since it is rather a myopic imitation of successful behavior than an exchange of knowledge. Our agent are not capable of estimating the consequence of their actions, they are myopic. We do not intend to recover a specific real-world case. Instead we constructed this model of social-cultural with biophysical interaction and examined this systematically. We understand the fact that unintuitive results emerge as evidence for the value of a thorough system's understanding.

> 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.

We gratefully acknowledge the observation that Section 3 would greatly benefit of a deeper embedding into the relevant literature. For example we think of extending the discussion on sustainable first movers with the large resource capacities. Also how our modeling paradigm brings in novel aspect could be considered. We will extend Section 3 towards this regard in the revised manuscript.

### 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.

We thank the reviewer again for taking the time to revise our manuscript. All technical corrections proposed in the last paragraph will be implemented in the course of the revision. We are confident that by revising our work with respect to all above issues our manuscript will ultimately be suitable for publication in "Earth System Dynamics".

# Review 2

We thank the referee for taking the time and effort in completing the review of our manuscript. It will result in additional clarification, especially in terms of methods and notions that will greatly benefit the presentation of our work.

### General comments:

> I think this manuscript presents a very important step forward in the analysis of social-ecological networks by considering explicitly resource heterogeneity. I endorse all

comments made by Referee #1, and will provide observations on topics not covered in said previous review. In general the paper is very well written and results are clearly presented. Finally, I also will recommend inviting the authors to carry out a major revision.

**Specific comments:**

In page 3 the idea of a Poisson process driving the social update times is presented, with parameter tau being the mean and std. dev. of the associated exponential distribution of said times. According to this distribution, tau can get values in the positive real line. This means that network rewirings can happen on a continuous time line. Meanwhile, rewirings can imply an update in the strategy such that a harvest rate for a given agent can change immediately. This implies suddenly updating the -Ei*si term in the differential equation for stocks (Eq. 3, pg. 4). While nothing is said about the numerical method used for solving these equations (and that needs to be provided) I will assume you are using some form of Runge-Kutta method (perhaps simply 1st. Order Euler). Depending on the software package utilized, you need to set the desired accuracy to the method and the algorithm should adapt the Time Step accordingly, or you set the Time Step by hand according to some criteria (which should be made explicit). In any discrete-time numerical approximation method, interrupting an integration step

with a sudden update to the equation (as discussed above) can be tricky. If you didn't develop the integration method yourselves, many out-of-the-box numerical packages will silently update your equation only at the beginning of the next integration step (and NOT exactly at ti) when the update is required to happen at a social update time ti that is not an exact multiple of the method's selected Time Step (with the latter coincidence bearing a theoretical probability of zero !). This is a common phenomena (an error) that might or might not alter your numerical results. What is for granted is that this could become a numerical artifact that artificially synchronizes your emergent system's behavior to the solver's Time Step, and that could be a problem, because sometimes you are not in control of this TIme Step, or simply did not pay attention to it. E.g. if the Time Step is in the order of magnitude of the average tau value, you can get noticeably biased behaviors. Please provide all required information to understand how your simulation code deals with these equation updates (called "time-event detection and handling" in the domain of continuous systems simulation). Also provide details for the numerical method adopted, its parameters (e.g. accuracy and/or Time Step), software used, etc.

Thank you for this careful analysis. We will clarify this methodological issue in the revised manuscript.

In fact, between two social updates (and thus at constant effort per agent) the growth or decline of each individual resource stock is analytically integrable. This makes the corresponding following update processes easily solvable and circumvents the need to integrate any differential equation numerically.

In page 6, you state that the model will always converge to a consensus for the given set

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We thank you for pointing out that our notion of "consensus" was chosen under a certain misfortune. What was actually meant with "consensus state" is a *subgraph consensus state*. To make things more clear we will change the term "consensus state" to "steady state" in the revised manuscript.

The article that is referred to above presents a very interesting model examining the steady state in an opinion formation model under the initial fraction of undecided agents. Certainly one could also move forward in this direction with the model presented in our manuscript. However, we think, that this is beyond the scope of our work, also because in the mentioned paper no social network effects are considered.

The focus of our article is the model dynamics under resource heterogeneity. Other model parameters (the initial social network structure, initial conditions of harvesting strategy, initial stock values, harvesting rates) were chosen to be either constant or uniformly at random (Erdös-Renyi network, initial conditions). Furthermore, by this randomization we explicitly did not impose any correlations between different states variables or between state variables and network structure which in turn could alter the results significantly.

> In page 7, the name of 2.3 should be "Model parameterization and simulation protocol" as the system is not modeled here but is only parameterized, together with making experimentation decisions like the number of runs.

We will change the respective section name accordingly.

> In page 8, when talking about critical values for phi and tau, it gives the impression that the observations made apply for all possible cases, regardless of the heterogeneity sigma (lines 5 to 15). It is obvious that this is not the case, as you elaborate on the impact of sigma in 3.2 Please make it more explicit what scenario lines 5 to 15 apply to (perhaps for sigma=0.01 in Fig. 3 ?)

Indeed, you are right. We will clarify the respective section.

We are grateful for the time the reviewer put in to revise our manuscript in much detail. We are confident that our revised manuscript will benefit greatly in terms of clarity from this review, such that it will eventually be suitable for publication in "Earth System Dynamics".

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