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Discussions

# *Interactive comment on* "Modelling the Ruin of Forests under Climate Hazards" *by* Pascal Yiou and Nicolas Viovy

**Anonymous Referee #2**

Review of manuscript "Modelling the Ruin of Forests under Climate Hazards" submitted by Pascal Yiou and Nicolas Viovy

The submitted manuscript describes the application of the Cramer-Lundberg ruin model which is well-established in the insurance sector to tree mortality caused by droughts on 5 sites in Europe. It aims at introducing this model to climate and Earth system science to enable straightforward support for decision-making, something – as the authors claim – the tipping-points lacks. Because it is a simple model of tree mortality, tested at 5 climate stations in Europe, which describes the climate hazard events, I was wondering whether it would be more appropriate to transfer the manuscript to Natural Hazards and Earth System Science. In my view, the manuscript is lacking the feedback and resilience analysis and thus true interdisciplinary research to fit to the

scope of ESD. Furthermore, the manuscript is not well developed that it sets its new idea of applying the Cramer-Lundberg model to quantify tree mortality into the context of existing literature on modelling tree mortality due to drought (the climate hazard) under current and future climate change. It is hastily written and not sufficiently substantiated by the body of literature which is essential when introducing a new concept. I describe my major concerns in the following: 1) The introduction motivates the study with claims that a) the ecosystem service literature ignores the fact that ecosystem services are also threatened by disturbances or hazards, and b) tipping points are mostly qualitative, not providing probabilities, and policy makers make little use of such studies. Several problems arise with these claims. a. For a) the claim is simply not true, the ecosystem service literature does recognize climate extremes, incl. fires and drought, as disservices (see e.g. (Shackleton et al., 2016)). Further, the authors claim that it is a dogma that ecosystems provide services to society. I am not sure if the term "dogma" is a polemic claim or a misunderstanding from not translating it into a corresponding English term. The global IPBES assessment (Diaz et al., 2019) reflects the scientific agreement of an international body of scientists that this is the case. b. For b) Lenton et al. (2008) does provide the time scales at which the tipping points would occur and the literature on tipping points increasingly defines or refines those thresholds, e.g. Hirota et al. (2011) or Zemp et al. (2017) for the Amazon tipping element, or the Antarctic ice sheet (Garbe et al., 2020). Furthermore, it is not explained which limitations the tipping point concept has to answer the questions this paper aims to answer. 2) The introduction of collapsology to the Earth System Science community is not thoroughly done. One 15-year-old citation is provided in the introduction which is not sufficient to introduce the ESD readership to this scientific field which is unknown to this community. Again here, the state-of-the-art of this concept is not well described and the scientific gap not well developed. Furthermore, it is lacking a clear description of why a new concept is needed (things the ecosystem service concept cannot answer and the tipping point concept does not deliver), and why exactly this proposed concept is expected to provide a better solution. 3) The claims on the decision-making literature (from line 25)

is not supported by literature, so lacks evidence. The authors need to provide evidence or overview on how the tools established in insurance and finance provide "all the tools for decision-making", examples must be provided here to substantiate this claim. 4) The paper then later on does not get back to a tipping point/resilience or close collapse analysis nor does it make use of the ecosystems service-disservice concept. The authors do not get back to the issues raised in the introduction. This also applies to the decision-making tools mentioned in the introduction. 5) The general assumption is that the ruin of ecosystems can be captured with tree mortality. And tree mortality does not capture all patterns and processes of an ecosystem. This is an oversimplification that affects the outcome and interpretation of results of the study. Well, it only applies to wooded ecosystems. In addition to the description of forests affected by drought must be accompanied by an explanation on how the collapsology concept can be transformed to Earth system science, specifically ecosystem dynamics. This is the missing link which needs to be explained to correctly set the scene. An ecosystem is more complex than paying something in (GPP) and losing something (due to drought). So, the paper does not provide the evidence why the Cramer-Lundberg model or its extension is a better description of processes leading to drought-related tree mortality. 6) If the model has to produce 104 sample members, and it is shown for 5 meteorological stations only, I doubt its computational costs if applied to the global scale for a range of climate scenarios. 7) It is not explained why a new drought index had to be developed and why not existing and well-established drought indices could be used. This is important and missing in the manuscript. 8) Drought occurrence is not a random process. The assumption for $S(t)$ needs to be revised. Plants have more adaptation mechanisms by which they can avoid carbon starvation, loss of productivity (GPP) due to closed stomata and increased maintenance respiration. They have evolved physiological strategies and physiognomic structures to avoid transpiration loss. It can't be subsumed with having a carbon reserve pool or not. I can understand why this cannot be implemented in a simple model, but some notification of this knowledge is required to justify the model assumptions. 9) Lines 92-94: unclear how this can be

transformed to the tree-mortality application. This needs to be described here. Also, how this can help to advance science wrt drought impacts on increasing tree mortality and the stability of ecosystems. 10) Line 105, NPP needs to be properly introduced. Totally open, and not explained, how $p\_0$ for the investment of NPP to the reserve pool can be justified. 11) Line 104: what is the damage function? $S(t)$ was introduced with a different meaning. 12) It needs to be shown that the climate data, i.e. number of droughts, indeed are Poisson and GPD distributions. 13) Line 155: the authors need to provide evidence that the parameter from their model can indeed be directly measured and evaluated using observations. This statement is not substantiated by evidence. 14) The findings that trees die at the time scale of decades to 100 years, is widely known and evidence is provided. The question is rather, if the model can produce increased drought-related mortality 3-5 years after a severe drought and the authors need to show how their findings compare to other model results or estimates based on drought-indices. There is an ample body of literature that has to be referenced here. Specifically, the result in line 182 indicates age mortality and not something related to a drought hazard. 15) Validation of modelled results is not provided and needs to be included.

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