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Earth System Dynamics Discussions

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

Pascal Yiou and Nicolas Viovy

pascal.yiou@lsce.ipsl.fr

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Anonymous Review #2

We thank the referee for the efforts devoted to this review.

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,

0. 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.

We do have a different view of interdisciplinarity (in this paper: putting together ideas from econometrics and earth sciences). This comment from the reviewer on the relevance to the scope of ESD seems based on his/her personal feeling. Our manuscript seems more interdisciplinary than most recently published papers in ESD, including from the editors-in-chief of the journal.

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,

We do not understand this point raised by the reviewer as we never claimed that ecosystem service literature ignores disturbances and hazards. Moreover, our paper is not about ecosystem services.

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.

We will rephrase the introduction. As stated in the title, we focus on trees, not ice sheets. We opted to cite seminal papers, which contain the main ideas, while the newer ones are essentially applications of existing tools and concepts.

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.

We did not mean to write an introduction to collapsology, but merely related our study to that concept. Citing the seminal piece of work, which contains all elements of understanding, seems more efficient than citing recent papers, which are based on the original one. The introduction will be rephrased in order to remove the discussion on collapsology, which can sound far fetched.

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.

The reviewer seems to overinterpret our claim. We obviously do not claim that the insurance sector provides all tools for all decisions: we claim that the insurance sector has developed all tools that are necessary to forecast its own losses and benefits. Such tools seem to be accepted by a majority of humans who are willing to pay a fee to insure their health, house or car. We believe that this fact speaks for itself.

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.

Indeed. This is why the introduction will be rephrased. Thank you for those comments. The paper is not about ecosystem service or disservice.

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.

We never claim that the ruin of ecosystems (in general) stems from tree mortality. We focus on tree mortality (which is called “ruin”), then claim that this concept of ruin modelling can be transposed to other ecosystems, which would obviously require a specific model adjustment.

For example, the paper does not address the question of tipping points in herbaceous ecosystems which have a very different behavior in response to climate hazards. We will make this more explicit in the revised manuscript.

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.

Indeed, reconstructing a probability distribution requires producing a huge number of simulations, which would not be possible with a complex process-based model, but which is not a problem with the ruin model as obtaining the Generalized Pareto Distribution parameters of any variable at a global scale is a matter of minutes on a PC, and a few seconds on a parallel computer (e.g., Kharin, V.V et al. Changes in temperature and precipitation extremes in the CMIP5 ensemble. Climatic change 119, no 2 (2013): 345âĂŚ57), and is done only once. Doing our 10000 Monte-Carlo simulations from those parameters for Europe takes a couple of minutes on a PC. Doing it at a global scale would take a few hours on a PC (at most), a few minutes on a supercomputer, as computations can be parallelized.

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.

A new drought index is indeed marginally important for the ruin model, which is why it was deferred to an appendix. What is important in the ruin model are the parameters of the probability distribution of hazards (intensity, duration and frequency). Well-established drought indices (such as the one of de Martone) are not physically satisfactory, as explained in the appendix. There are other more physically satisfactory drought indices, which do account for physical processes, but they do not go that far into the past and do not allow a reliable estimate of parameters for the hazard model. This motivated this new drought index, which can be computed with basic climatic variables that are recorded over long periods of time.

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.

We do not agree with the preamble of this comment. The phenomenological development of a drought (or any climate hazard) is obviously a deterministic process, but key quantities like frequency, duration or intensity can be modeled by random processes. This is the core of statistical climatology.

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.

Our claim (in those lines 92-94) is that it is possible to determine the probability distribution of ruin time from statistical properties of hazards. The following section (2.2) shows how the Cramer-Lundberg model (and the surrounding probabilistic framework) can be transposed to tree-mortality (that we generically call "ruin"). This will be made more explicit in the text.

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.

An NPP definition will be added. It has been shown that in good condition, the non structural carbohydrate reserves in mature trees tend to reach a maximum value and then bad weather conditions decrease this amount both by increasing use of reserves and decreasing allocation to it (because of reduced total NPP). (Barbaroux, C., Bréda, N., Dufrêne, E., 2003. Distribution of aboveground and belowground carbohydrate reserves inadult trees of two contrasting broad-leaved species (QuercuspetraeaandFagus sylvatica). New Phytol. 157, 605–615. This justifies the assumption of the model of a maximum

11) Line 104: what is the damage function? S(t) was introduced with a different meaning.

S(t) is a "hazard function". We will streamline the terminology between sections 2.1 and 2.2.

12) It needs to be shown that the climate data, i.e. number of droughts, indeed are Poisson and GPD distributions.

Under rather generic mathematical conditions, the probability distribution of the exceedances over a threshold can be modeled by a Generalized Pareto Distribution. This is analogous to the fact that the average of a variable that has a finite standard deviation can be modeled by a Gaussian. The principle is the same for the frequency of exceedances (or inter-arrival times). The simplest statistical model that describes the time interval between exceedances of a threshold is a Poisson distribution. We will recall this fact in the text by citing textbooks that are often used in statistical climatology or hazard models (e.g. S. Coles, An Introduction to Statistical Modeling of Extreme Values, Springer, 2001).

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.

The parameters were initially chosen arbitrarily, in order to illustrate the contrasting behavior of "cash" and "credit" strategies. This has been mentioned in the discussion of the paper. We will re-do analyses that use constraints obtained from the literature, e.g. He W, et al. (Patterns in nonstructural carbohydrate contents at the tree organ level in response to drought duration. Glob Chang Biol. 2020 Jun;26(6):3627-3638. doi: 10.1111/gcb.15078), who performed a meta-analysis of reserve reduction in case

of severe drought.

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.

Thank you for this comment. Indeed, very few trees in Europe live longer than a century. We consider that our study applies to a collection of trees (e.g. a forest), whose lifetime is hoped to be longer than a century. This will be clarified in the text.

15) Validation of modelled results is not provided and needs to be included.

Although we appreciate this comment (also made by referee#1), we would like to point out that since the seminal paper of E.N. Lorenz (Deterministic nonperiodic flow. J. Atmos. Sci. 20 (1963): 130âĂŚ41), many studies, including publications in ESD, have focused on the behavior of idealized models.

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We thank the referee for those interesting recent references.

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