

***Interactive comment on* “Tropical drought risk: estimates combining gridded vulnerability and hazard data” by Alexandra Nauditt et al.**

Anonymous Referee #1

Received and published: 6 December 2020

This manuscript describes the evaluation of drought risk in four tropical catchments of Brazil, Colombia, Costa Rica and Cambodia/Vietnam.

Drought in tropical areas has been scarcely evaluated; therefore, this paper makes a substantial contribution to the assessment of drought considering hazard and vulnerability to achieve a compound drought risk index.

Overall, the manuscript is clear and well written; it is concise and easy to follow. The tables and figures are descriptive enough to communicate the methods and results.

The Introduction deals with the main aspect to place the study in context and contains relevant and updated citations to other published works. A minor observation is in line 69, where I suggest using “livestock” instead of “cattle” grazing, as livestock is a

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more general word to include the production of domestic animals, while cattle refers only to large ruminant animals, mainly with horns. Unless cattle are the only type of domestic animals for production in the four catchments, “livestock” seems to be a more appropriate term.

In general, the methodology is scientifically sounded, but I have some comments: The assessment of drought hazard seems to be straight forward. However, it would be useful if authors provide the time periods of analyses for each studied catchments, then the reader can have an idea of whether the periods are long enough to reflect the variability of the precipitation. It is not clear if they used the same period from all the catchments. Related to this, in line 144, authors mention they used a period from 1980-2018 to analyze CHIRPS data, but in line 156, they say they considered a period from 1980-2017.

Why did the authors only use the vegetation condition index to assess the vegetation condition within the drought hazard component? Why didn't use the Vegetation Health Index (VHI) which integrates the temperature condition (which can also be computed from MODIS data) in addition to vegetation index?

Vulnerability was computed using different freely available data sources which have different spatial resolutions. I suggest including these spatial resolutions in Table 2 to provide more information regarding these different data sources. Combining different sources with different spatial resolutions always represent a challenge for any spatial analysis. I am aware that authors used the best available spatial datasets regarding livestock, cropland and population densities, as well as data on roads and GDP. However, authors should consider adding some lines in their discussion regarding the way these different spatial resolutions may affect their results. I am aware that authors make a discussion regarding that more detailed data would be necessary to obtain better results at a more local scale (but may not be available or existent). Although they state in lines 218/219 that all grids were resampled to a common cell size, this does not mean that all grid layers contain the same level of detail. Thus, my suggestion is to discuss

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briefly about the implications of integrating different scales on their results.

There are some errors of omission of reference (e. g. lines 192, and 201/202 and 231). I guess they may be caused during the conversion of the original file into a PDF file. Please check and amend.

The results section contains enough description of the main findings and all maps, tables and figures are adequate.

In the Discussion section, authors start saying that they get “plausible” results. I suggest removing “plausible”, as it seems that they were comparing their results to real data for validation, which is not the case. Authors did this only for the Muriaé catchment; therefore, I suggest following the same style they used to report results for the other catchments (in which they did not use “plausible”).

Although the Discussion is rich in the way the authors analyze and interpret their results, I found a lack of references to put their study in a more general context. For example, how do this approach to the evaluation of drought risk is similar to other studies? Does this study have advantages or disadvantages compared to other drought risk studies? This would outline the utility of the approach presented in the manuscript.

I hope the authors and editor find my comments useful.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2020-360>, 2020.

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