

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2  
<https://doi.org/10.5194/hess-2022-124-RC2>, 2022  
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## Comment on hess-2022-124

Mariana Madruga de Brito (Referee)

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Referee comment on "Global assessment of subnational drought impact based on the Geocoded Disasters dataset and land reanalysis" by Yuya Kageyama and Yohei Sawada, Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-124-RC2>, 2022

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In this manuscript, the authors use the new GDIS data and link it to hazards (i.e. soil moisture). The contribution is timely and innovative. I enjoyed reading the paper. Currently, we have little understanding of the relationships between impacts and hazard data. Hence, the proposed approach provides a way to understand these linkages. I particularly appreciate that instead of the traditional approach (e.g. hazard x vulnerability x exposure = risk), the authors go from the disaster itself, and from it try to identify the hazard drivers.

The manuscript is well written, yet, some issues need to be addressed:

- My main criticism is that the authors claim to have assessed the “regional vulnerability to drought” (see line 258). Figure 5 shows that the SDI alone cannot explain drought disasters. Indeed, in Sub-Saharan Africa and South Asia, the SDI is not as high as in Europe and North America, yet, many disasters are observed. One explanation for this is indeed, the socio-economic vulnerability in these regions is higher. However, it could also be that it is the population exposure. Since you have not accessed any of these, you cannot make claims regarding the vulnerability.
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- You mentioned that you disregarded drought events that lasted for less than 2 months. However, I assume most of the EM-DAT data only has a year, and few have a start and end date. In such cases, you mentioned that you considered the whole year as a drought year. That can be very problematic, as probably many of these events could actually be 2 months. It would be great if you could bring this limitation in the discussion section, where you already mentioned the limitation of the impact data.
- Why was landcover data from 2020 used (line 127) if the drought events go from 1964 to 2018? Likewise, why did you consider soil moisture data from 2019-2020 (line 132) when your impact data is from previous years?
- Item 3.3 and 4.2. Similar to the first comment, stratifying your SDI data by different regions is not equivalent to “understanding the vulnerability in each region”. What you did here was to analyze the SDI according to different regions. This is definitely an

important analysis, but it is not a vulnerability analysis. You can hypothesise that the vulnerability causes these differences, but you cannot confirm this with the present data. The statements in line 255 are pretty strong and cannot be made without considering the socio-economic vulnerability. I suggest toning them down and writing that "the vulnerability could explain these differences". However, for this, further analysis is required.

- Item 3.4. It is not clear to me what the advantage of clustering is. Why would the results be different with or without clustering? Could you please elaborate?
- In line 259, you say that your findings are consistent with previous ones that used GDP, however, you have not used GDP here. Please rephrase the sentence, or do the analysis
- In line 266, you write that the vulnerability is explained by the lack of structural measures. Sure, these are contributing factors, but socio-economic variables and exposure could be even more important. I understand the author's point of view, being an engineer myself. However, "command and control" approaches in which drought risk is controlled solely with conventional engineering measures is an outdated view. The authors cannot ignore the vast literature on socio-economic vulnerability and risk perception. You do not need to cite these papers, but have a look at the GAR chapters on vulnerability and droughts:

[https://gar.undrr.org/sites/default/files/chapter/2019-05/Chapter\\_3.pdf](https://gar.undrr.org/sites/default/files/chapter/2019-05/Chapter_3.pdf)

<https://gar.undrr.org/chapters/chapter-6-special-section-drought>

- Line 282: Spain is not shown in Figure 5. Hence, you cannot make this statement based only in the Figure. It could well be that the other European countries are "robust" and push the mean up, whereas Spain pushes the mean down. I suggest removing the sentence or doing the analysis for Spain only.
- Line 301: I find it good that you can find good linear correlations. However, I was surprised as soil moisture and drought impacts usually do not have a linear relationship. Indeed, there are non-linear relationships (there is a lag between soil moisture and forestry impacts for instance). Furthermore, you have multi-year drought events, which add complexity to the interpretation of spatial and temporal differences in drought prevalence. Indeed, it could be that a drought is not so severe in biophysical terms. However, because they last so long, the impacts are high. I assume you got good correlations because EMDAT usually do not have long term impacts but rather focuses on immediate ones. Please add a discussion of these issues in your discussion section.

Some minor comments:

Line 9: What is meant by "drought information" here? Do you mean Drought impact information (i.e., the GDIS data?). Please be more specific.

Line 55-57: I do not understand the purpose of these 3 last sentences. When you write about vegetation growth, do you mean forest or crops? Regarding the last sentence. Why do we need to treat drought impacts as they are socially perceived? If you think the section is relevant, please expand it to make it clearer to the reader.

Line 95: This information is repeated in the introduction. I suggest removing it: "GDIS is the geocoded...."

Line 122: You do not "show the drought vulnerability". Land cover is an exposure data, not vulnerability.

Line 207: "significantly higher": could you please provide the test results that made you reach this conclusion.