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Discussions

Interactive comment on "Global-scale drought risk assessment for agricultural systems" *by* Isabel Meza et al.

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Veit Blauhut Referee comment:

Dear authors,firstly, please excuse my delay submitting this report. secondly, congratulations to avery well written piece of work. The papers reads very fluent and only leaves space for few questions.

The authors are exploring agricultural drought risk on a global scale, comparing rainfed against irrigated agriculture in the frame of a conceptual model. I highly appreciate this unique attempt, especially the preceding expert survey. My major concern is a lack of validation e.g. with other global agricultural drought risk models and the "lacking"verification of the relevance of selected indicators. More data exists. Also quantita-

tive approaches for validation exist. I please you to apply something that is not "visual comparison" (This really lowers the quality of your, apart from that, high quality paper)

Response: Many thanks for the positive overall summary. To our knowledge, the analysis presents the first attempt to assess drought risk for agricultural systems at the global scale. Existing global analysis focus on drought risk in more general terms (e.g. Carrao et al., 2016, Dilley et al., 2005). Hence, a direct statistical comparison might be misleading - due to the different foci of the studies. We have visually compared our results to the work done by Carrao et al. (2016) and mention this in the discussion. An in-depth comparison and validation, while needed, would be a paper of its own. We will add a few lines in the discussion mentioning this as an outlook for future research.

1. Please find my more explicit comments in the PDF https://docs.google.com/document/d/19w7_cn6r4t3rKJxqq51H6I6veY6G5vZBLkgN-zUNbcs/edit

Response: Many thanks, kindly find our responses below.

[a] P1 L20-21: A glimps if the assessment is of general risk information or applicable for early warning would be nice.

Response: Thank you for the comment. We will make it more clear that the aim of this paper is to conduct a risk assessment for agricultural systems and not to focus on early warning.

[b] P2 L49: I assume that you know what are you talking about BUT: please consider to cleary define risk relevant terminologies. Many terms are interpreted differently by schools, scientific field or even authors. Maybe a list/table in the appendix could do the job.

Response: Thanks for the comment. This comment has been raised by the other reviewer as well. We follow the latest IPCC AR5 WGII (IPCC 2014) definitions of risk. We will add a line in the methods (chapter 2) on the risk concept that is used.

[c] P2 L65: Since you are setting a point on exposure I recommend to define this. Exposure is treated differently in drought risk community (from landuse to drought frequency)- thus a clarification is of need. Please see De Stefano et al. 2015 & Gonzales Tanago et al. 2016.

Response: Many thanks, this comment is also related to the previous comment on definitions and has also been raised by the other reviewer. We follow the IPCC (2014) definition. We will add a line in the methods (chapter 2.1) on how exposure is defined.

[d] P2 L65:Partly true. Dilley et al. 2005 and Li et al. 2017 also investigate at global scale. But using different/ and less vulnerability factors

Response: Thanks for the suggestion, we will add the paper from Dilley et al. (2005) to the introduction. The paper by Li et al. (2017) has a geographic focus on Northwest China. Hence it was not mentioned in the introduction. We decided not to add it since the analysis is not global.

[e] P3 L72-75: You might also bring their "validation" schemes into play?

Response: Thanks for the suggestion, Carrao et al. (2016) evaluated the robustness of their analysis to changes in indicator weights. Their model is based on an internal validation procedure that chooses the best model as the one giving regional vulnerability ranks that approximates the median of the ensemble among all models tested. Nevertheless, in this case the absence of reference data for performing an independent validation reduces the lack of effective testing options. A statistical validation of the sensitivity of the results towards changes in the input parameters (indicators, weights, normalization, aggregation methods), while needed, goes beyond the scope of this paper. We will add a few lines in the discussion to mention this as a possibility for future research.

[f] P3 L77: I see the point, but not the matter for your research.

Response: A social-ecological systems (SES) perspective is relevant when assess-

ing drought risk for agricultural systems, which by definition have a strong social and ecological coupling. This point was also highlighted in a recent assessment of social-ecological systems vulnerability of deltaic regions confronted by multiple hazards - including droughts - by Hagenlocher et al. (2018).

[g] P3 L82: I question why a conceptual model should be "better" to analyse agricultural drought risk then a global analysis based on crop- models (such as Li et al 2009, Yin et al. 2014, Zhang...). Please explore the caveats of outcome related vs. conceptual models with the background of your, now published, drought risk review.

Response: Here we used an integrated drought risk assessment approach based on the risk concept put forward by the IPCC WGII in their 5th Assessment Report (IPCC 2014). The advantage of such a conceptual model over an outcome oriented model (that estimates vulnerability indirectly by looking at losses) is that our approach allows for revealing drivers of risk (incl. vulnerability drivers) and hence entry points for vulnerability reduction whereas an outcome-based assessment approach does not allow for that. One strong advantage is that our approach provides comprehensive, aggregated, comparable and data-driven information on the actual vulnerability conditions and patterns at the global scale.

[h] P3 L82: What is an integrated drought risk assessment?

Response: An integrated drought risk assessment combines drought hazard, exposure, and vulnerability by bringing together data from different sources and disciplines. We will make that more clear in the text.

[i] P4 L100: ??

Response: The answer to this point is linked to the comment on P3 L77, which was already addressed.

[j] P6 L140-143: This indeed is a little arbitrary. Of cause, most thresholds are subjective like this one. And I actually do not have a better idea, bit maybe you could

provide some explanatory box/violine plots. Are there regional/ continental specifics? (appendix is fine)

Response: In total there are 37,265 grid cells of the size 0.5 x 0.5 degree containing rainfed cropland. With the threshold of 10% selected for the present study, no drought at all would be observed in the period 1980-2016 in 3,999 grid cells (10.7%), mainly and very humid or cool climate. The number of grid cells without any drought would increase to 11,879 (31.9%) when using a threshold of 20% and to 20,803 grid cells (55.8%) when using a threshold of 30%. We decided, therefore, to keep the threshold of 10% but will add some descriptive statistics on the effect of this threshold to the supplementary information.

[k] P7 L170: Why?

Response: Many thanks. This comment was already raised by "Reviewer 1" (see comment #13). IH describes the volume of irrigation water needed additionally in drought periods. In most grid cells these volumes are relatively small but there are also some grids with extremely high values. In 569 out of the 26,478 irrigated grid cells the additional irrigation water requirement per drought event is lower than 100 m^3; in 1,450 grids it is lower than 1,000 m^3. These are grids with very small irrigated areas. However, there are also 95 grids where the additional irrigation water requirement per drought event is larger than 100,000,000 m^3. The logarithmic transformation accounted for the value distribution.

[l] P8 L212-214: Again, please refer to Gonzales Tanago et al. 2016 and add some cons of this practise

Response: Thanks for the suggestion. We have carefully read the paper by Gonzales Tanago et al. (2016), and it was already cited in the initial version of our manuscript. However, the authors do not mention specific limitations of index-based approaches. Hence for this point we decided not to refer to the paper. But we will add some cons about this approach following suggestions by Naumann et al. (2018), Beccari (2016),

and de Sherbinin et al. (2019).

[m] P9 L231: Similarity? Did you test this in the frame f a similarity test? Or Did you do cross-correlations? Please be more specific and consider to show your pre-selection criteria/ similarity tests, cross correlations. (again, appendix is fine)

Response: Thank you for the comment. We agree that the sentence is not clear. The decision of which indicators to combine was not based on statistical similarity tests, but on "logical reasoning" due to what these indicators represent. For instance Agriculture (% of GDP) and Dependency on agriculture for livelihood (%) were combined under one income indicator and the variables GDP per capita, PPP and Population below the national poverty line (%) both refer to poverty and therefore combined in one integrated indicator. We will reframe the sentence and add which (and how) indicators were combined in the text.

[n] P11 L288: I'm aware that only few global datasets on drought impacts are available. With respect to the EM-DAT database regions like Europe are not well represented in the database. E.g. Spain and Northern Europe did not suffer a single drought event, Portugal maybe 2 ,etc. Thus, I question the reliability of this source and wonder why you did not compare to other global, maybe impact based, drought risk analysis.

Response: We agree that the number in EM-DAT depends highly on data availability and the size of the country is important. We discussed these limitations in the manuscript (lines 397-402; 492-502) and countries are mentioned as an example. Regarding the comparison with other drought risk analyses: To our knowledge, the analysis presents the first ever attempt to assess drought risk for agricultural systems at the global scale. Existing global analysis focus on drought risk in more general terms (e.g. Carrao et al., 2016, Dilley et al., 2005), and thus a direct comparison could be misleading due to the different foci. An in-depth comparison of existing global drought risk assessments goes beyond the scope of this paper. It could however be an interesting piece of work for future research. We will add this as an outlook to the discussion. We also agree that calling this approach “validation” might be misleading, and will hence change it to “comparison”.

[o] P11 L292: Coming back here (just read the end): Your validation is of "visual nature". With respect to your own research (review paper) please consider to at least try to get some numbers behind your statements. Naumann et al. 2014 showed an easy approach to compare/validate relative pattern of vulnerability. This could also be done with the results of Carao et al.This way you could have to "different" datasets for validation

Response: A direct numeric comparison is difficult due to the lack of an actual validation data set that represents the ground truth with adequately high spatial or temporal resolution. We decided to use EM-DAT because it systematically collects reports of drought events and drought impacts from various sources, including UN agencies, NGOs, insurance companies, research institutes and press agencies (lines 288-289). We will add more about the point the referee is raising on the discussion, and mention the advantages and the need to have an in-depth statistical validation of drought events for future research.

[p] P16 L379-381: This seems counter intuitive for me. E.g. a country for Iran is heavily depending on irrigation. The absurd overuse of groundwater within the last decade led to an extreme drop in groundwater levels, which in a next step, increases the vulnerability of the farmers. Of cause, this comes back to the lack of knowledge on the dynamics of vulnerability.

Response: We agree with the comment. We will add a line on vulnerability dynamics on the paragraph.

[q] P20 L455: Did not see to many ecological vulnerability factors, only "Terrestrial and marine protected areas", soil erosion and fertilizer could also be shifted to technical and landuse, or?I would recommend not to highlight your research as social-ecological. (but it might be a matter of taste)

Response: Thank you for the comment. Even if we moved them to another category, they still represent the environmental susceptibility of the country. Especially when assessing drought risk in the context of agricultural systems, which are by definition SES (Kloos and Renaud 2016), a SES perspective is of relevance. This point has been raised by the same reviewer before (see our response to comment P3 L77).

[r] P20 L461: I expected a more intense comparison

Response: Many thanks. Our analysis has a distinct focus on agricultural systems, while Carrão et al. (2016) present a more generic drought risk assessment at the global scale. An in-depth statistical comparison of the findings hence might even be misleading due to the different foci of the analyses. We have, however, conducted a visual comparison of our findings and their findings.

[s] P21 L493: But there are more data (modelled but..) Since you are not satisfied with EM-DAT, I do not understand why you did not use others.

Response: This point is related to the one in the [o] P11 L292. We decided to use EM-DAT because it systematically collects reports of drought events and drought impacts from various sources, including UN agencies, NGOs, insurance companies, research institutes and press agencies (lines 288-289). We will add more about the points the referee has been raising on the discussion, and mention the advantages and the need to have an in-depth statistical validation of drought events for future research.

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