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Hydrology and Earth System Sciences Discussions

HESSD

Interactive comment

Interactive comment on "Global change in flood and drought intensities under climate change in the 21st century" *by* Behzad Asadieh and Nir Y. Krakauer

Anonymous Referee #1

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Review of "Global change in flood and drought intensities under climate change in the 21st century".

This paper aims at understanding how climate change will manifest in changes in flood and drought conditions globally. To pursue this, the authors collected ISI-MIP streamflow projections (which are based on bias corrected output of five global climate models (CMIP5), and five hydrological models) at a 0.5x0.5 spatial scale. Climate scenario's that are explored include RCP8.5 (which assumes emissions continue to rise throughout the 21st century) and RCP2.6 (which assumes emissions peak between 2010-2020, with emissions declining substantially thereafter).

To quantify changes in flood and drought conditions, the authors calculated the "normalized change" which they defined as the difference between mean percentiles (5th for low flows and 95th or high flows) of 20th century flow conditions compared to 21st century flow (normalized by the sum of 20th and 21st flow percentiles). Calculations are performed for areas where streamflow >0.01 mm/d, and exclude Greenland. Global maps of changes are provided.

The results suggest that: (i) Globally, both floods and drought are expected to intensify. (ii) in some regions (especially several highly-populated areas), both flood and drought intensity will intensify (iii) Especially, in northern high latitudes flood intensities will increase. (iv) RCP8.5 leads to nearly 2 times higher impact on changes in flood and drought intensities compared to RCP2.6. (v) Hydrological models have a larger contribution to uncertainty in the projections, compared to the effect of different climate models.

General comments

Understanding the nature of future floods and drought, and the effect of changing climate conditions on these hydrological extremes is very relevant for HESS. The analysis is rather straightforward and reasonably well-explained.

However, I do have several (major) concerns that need to be addressed before I recommend publication of this manuscript in HESS:

1) What makes the metric you define to characterize changes in flood and low intensities appropriate for the problem that you address? While the 5th and 95th flow percentiles are surely representing higher and lower flow conditions, they are not real hydrologic extremes. For example, if the 95th percentile represents flood conditions, every grid cell would experience ∼18 flood days per year. Would it not be much more useful to quantify changes in the more extreme conditions (e.g. 99th percentile?). It seems that this choice arises from Giuntoli el al (2015) (except that they use the 10th percentile for low flow), but does that warrant that it actually is an appropriate metric to

use?

2) A lot of literature is already available on the topic of projected flood and drought (/low flow) changes. While you cite several papers in the introduction, it remains unclear to me which knowledge gap your paper fills compared to earlier work. I made a list of detailed suggestions in the comments below, which may help to address this issue. However, right now when I read the abstract I do not see understand knowledge gap you address, when I read the introduction no I get some idea of what has been done before but still no clear niche is identified that you fill. Any discussion is a description of the results rather than how we better understand global flood and drought changes, and in the conclusions, you summarize findings, but still I have difficulty how this relates to the vast amount of literature on previous flood changes (also what's new compared to Arnell et al. 2016, Alfieri et al. 2015, 2017, Giuntoli el al. 2015?). While I do not doubt that are new things in your paper, they need to be clearly identified.

3) Related to the previous points: the rationale behind some choices in the analysis is missing. Can you answer: (i) why we would be interested in flood and drought changes simultaneously. (ii) why the metric you choose are appropriate to characterize what you do (iii) why correlating population density and change in flood hazard is meaningful. (iv) why looking at "intensity" is novel and important compared to "frequency (which is already available using a similar approach). (v) why do we also need to look at changes in median flow (when the purpose seems extreme flows)

I do not try to imply that these choices are not well thought out and relevant. However, I do think you need to take the reader by the hand in why such choices are made

4) 0.5 x 0.5 degrees and daily forcing seems like very large spatial and temporal scales to resolve the hydrology of flood processes. I understand it is "the best you can work with" right now if you want to understand flood changes globally for the entire 21st century. However, can you better reflect on how this actually affect the degree to which you can resolve flood changes? Many different flood generating processes cannot be

represented at these scales. For example, flash floods (e.g. I expect you need sub-daily P for this?) or snowmelt driven floods (e.g. I expect that you need to parametrize the sub-grid heterogeneity of snow conditions) seem challenging, while in many places these processes are very important (e.g. see Berghuijs et al., 2016). Since there essentially is very little science in the paper (basically there are no rejectable hypotheses you test, and the paper provides a summary of available data), I think you need to say a few useful things on this topic, such that it still meets the standards of a journal like HESS.

5) I suggest to be careful with the using low flows and drought interchangeably. Low flows and droughts are not the same. You quantify changes in low flow conditions, not in drought conditions (which reflect some deviation compared to the normal flow conditions of a catchment during a particular time of the year). These two different concepts should not be mixed. (e.g. see Van Loon, 2015). To some degree the same applies for high flows and floods. However, this difference seems less important, since those two concepts are more closely related.

6) The changes in flood and drought indices per latitude intrigue me (Fig 2). How can they virtually be the inverse of another (when organized per latitude)? Is this the physical reality or an artifact of how this study quantifies changes in these aspects? Surely, floods and drought within one area can be connected (since they are part of the same climate and landscape system). However, (in my opinion) this almost one-on-one inverse pattern seems to require some attention.

7) You quantify the percentage of land area that undergoes significant streamflow change. However, the two-sample t-test that you use for this (as explained in the supplementary material) seems inappropriately used. Why would scaling this relationship by the streamflow time series variances be appropriate here? Should they not be scaled by some measure of (e.g. between year) variability in the hydrologic extremes flow?

8) By presenting only mean values of results (e.g. the mean magnitude of flood and drought changes) the reader has no idea from what distribution of changes these mean values are derived. This seems rather basic information that can easily be presented in will provide valuable insight for the reader. Right now, some of the numbers you use in key points of your paper are difficult to interpret because we have no idea if they represent many small changes and a few extreme ones, or because they represent consist medium size changes.

9) Please address the list of comments I provide below. (Several of these comments go beyond small technical details.)

detailled comments

Page 1 Line 11-12. Consistent with my main comments above, it would be very useful to have a transition sentence that actually introduces the knowledge gap in flood and drought risk projections that the paper aims to fill. Preferable reflect on that goal in the end of the abstract.

Line 16: without a definition of what aspects of these hydrological extremes you actually look at (e.g. duration, or magnitude, or both)) these precise percentages not very useful. Being more specific in line 12 may resolve this issue. The same problem applies to all other percentages provided in the abstract. (or the statement in lines 22-24)

Line 17-19: “the averaged rates of increase” (can) suggest that you exclude places where it reduced? Or is this the average of all increases and decreases globally?

Line 19: “potential risk” or “are projected” (since I guess all areas are under the “potential risk”?)

Line 20: “rate” or “change” (or “increase”)?

Line 21. Semi-column or just start a new sentence?

Line 26-29: It seems odd to me that the paper suddenly talks about changes in streamflow (I guess that means mean runoff?) while the rest of the paper is about extremes?

Page 2

Line 15-18: Sure: more extreme P can lead to more extreme runoff. However, there is so much more going on that dictates runoff response (e.g. antecedent moisture conditions in floods etc). Would it be worth to say one or two things about other mechanisms that underlie floods? In many places, there is a disparity between extreme rainfall and flooding, or between lowest P and lowest Q, since so many other factors are also important (e.g. seasonal moisture conditions). Emphasizing which other processes are important may help to understand the reader what the added value is of adding the GHM's to the game (since they at least theoretically should represent all these processes that go beyond extreme P). Nor can I logically connect more extreme high P to more extreme drought (without some extra information about changes in dry spells, or hydrologic partitioning)

Line 19-29: I do not see why the paper needs to talk about changes in "mean streamflow conditions" since it distracts from what you're really interested in (which are the hydrologic extremes)

Line 31-33: Sure, that a decrease in P can decrease runoff. However (like you give with the following example) you can also think of conditions where this does not apply.

Page 3

Line 1-5: Ok, I understand that there may not be many studies that use ensembles. However, still that does not answer the question of what knowledge gap you can fill with your approach. What do we not understand because we haven't run particular ensemble projections yet?)

Line 4-5: The detection of areas that are expected to experience both more floods and drought sounds interesting at first, but what is again the knowledge gap that the paper fills compared to earlier work, and what is the merit in identifying these at the same time (there are reasons why this can be valuable, but they need to be presented to the reader).

Lines 5-17: put these findings into context of the novel thing you're going to expose/test. Right now, it reads like a random list of previously reported streamflow changes, which are unclear why they're directly relevant to the paper.

Lines 18-20: Maybe a reference (or two) can help to support this statement?

Line 20: remove "trend" (since there may not be one)

Page 4

Line 13-14: Be very explicit to the reader what the difference between "frequency" and "intensity" is, and emphasize why this difference is relevant.

Line 13: "this study" may be unclear because it can refer to your own work or the work of Giuntoli

Line 15: It may be worth to start state "Here we" and then list the "goal", rather than directly go into the "methods". This will make the list of subsequent steps outlined in the rest of the introduction much more logical. For example, right now it sounds fun that you also investigate the link with human populations, but I have no idea (or at least it's up to my own guess!) why you'll be doing this.

Page 5

Line 13: why are these five GHM's selected? (after line 15-17 that question still stands)

Line 18-19: do you have any references that show this, or did this only appear in your own work?

Lines 19-21: consider rewriting this sentence

Lines 18-27: it seems a bit confusing to justify normalization before you define the metric that you adopted.Âă

Line 28 – Line 2: Is “no change” also an option?

Page 6

Line 3- 14: Why did you choose the 5th and 95th percentile and not “real extremes” (see earlier comment above)

Line 16: 200?

Lines 26- 29: And what about any places where there are insignificant changes?Âă

Lines 30-3: why don’t you use the absolute value (and then you don’t need to separate by quadrant).

Page 7

Line 3-7: Why would you even bother to try that method? It seems like this method is just less logical at the start (because it is very sensitive to absolute runoff changes between models), and hence should not be considered at all?

Lines 8-13: Your results suggest that 95% of the projected flood changes are significant, but what does that really mean? Does that imply that for 95% of the grid cells you are very certain about the projections? Or does it mean that model projections may show a significant change, but all other biases and uncertainties not accounted for may lead to much lower certainties of projected change?

Also (in the supplementary material), why would “streamflow time series variances” be a relevant scale of variance here (rather than something like the variance of annual maxima or Q95).

Line 16-18: you need to show the distribution of changes, rather than just the mean value. Right now I have no idea if the mean results from consist small changes, or a few very big changes.

Line 19: why bother with median flows? I thought this paper was about the extremes?
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Figure 2: The changes in flood and drought indices per latitude intrigue me. How can they virtually be the inverse of another (when organized per latitude)? Is this a physical reality or an artifact of how this study quantifies changes in these aspects?

Figure 5: it is impossible to read the scale bar in the far bottom left (on a printed page).

Page 10

Line 30: you were not interested in decreases?

Line 31 because people live in a grid cells where floods increase does not mean they are affected. That depends on many other factors.ÂăCorrect?

Page 11

While I appreciate, you repeat all the main results of the paper, I think the paper really needs to reflect on what we learned compared to earlier work, rather than list what came out of some modeling exercises.Âă

Table 1: without information on the distribution of changes, I have no idea about what these mean values of change represent.

Reference list: what does :"(80-.)" do in several references?

References

Arnell, N. W., & Gosling, S. N. (2016). The impacts of climate change on river flood risk at the global scale. Climatic Change, 134(3), 387-401.

Alfieri, L., Burek, P., Feyen, L., & Forzieri, G. (2015). Global warming increases the frequency of river floods in Europe. Hydrology and Earth System Sciences, 19(5), 2247-2260.

Alfieri, L., Bisselink, B., Dottori, F., Naumann, G., Roo, A., Salamon, P., ... & Feyen, L.

(2017). Global projections of river flood risk in a warmer world. Earth's Future, 5(2), 171-182.

Van Loon, A. F. (2015). Hydrological drought explained. Wiley Interdisciplinary Reviews: Water, 2(4), 359-392.

Giuntoli, I., Vidal, J. P., Prudhomme, C., & Hannah, D. M. (2015). Future hydrological extremes: the uncertainty from multiple global climate and global hydrological models. Earth System Dynamics, 6(1), 267.

Berghuijs, W. R., Woods, R. A., Hutton, C. J., & Sivapalan, M. (2016). Dominant flood generating mechanisms across the United States. Geophysical Research Letters, 43(9), 4382-4390.

Trigg, M. A., Birch, C. E., Neal, J. C., Bates, P. D., Smith, A., Sampson, C. C., ... & Ward, P. J. (2016). The credibility challenge

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