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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The 2018 northern European hydrological drought and its drivers in a historical perspective" *by* Sigrid J. Bakke et al.

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Thank you very much for the positive and constructive feedback on our paper "The 2018 northern European hydrological drought and its drivers in a historical perspective". Hereby, we would like to respond to your comments:

All reviewer's comments regarding language corrections will be accounted for in the revision. Other more substantial comments are responded to below, with the original comments marked by 'AR#1' and our response paragraphs marked by 'Authors'.

AR#1: Adding a small subchapter at the end of Section 5 with parts of the conclusion, where all the results are placed together, would help to connect the different discussion parts already earlier and leave more space for an even more concise conclusion.

Authors: The structure of the discussion chapter was something we discussed extensively during the writing process, in particular the discussion related to drought propagation. Currently the discussion regarding 2018 drought propagation is embedded in Sect. 5.2. Following your suggestion, we will consider adding a new subsection at the end of Section 5 bringing together the key results in the context of drought propagation by moving parts of the content from the conclusion and Sect. 5.2.

AR#1: In the introduction: An additional elaboration on the other methods included and the reasoning behind using them would help prepare the reader for the following analysis and results and would strengthen the introduction and emphasizing why this paper is special in its own way and closing current research gaps. Adding more information on this and mentioning more similar studies might also help setting the scene for a deeper discussion later on.

Authors: We agree on including a more complete presentation of the methods applied, including their motivation as well as potential similar studies not already mentioned in the introduction. We will embed this in our revised version.

AR#1: I personally think including some more lines on the results and observation in early spring until the end of the year, besides the extreme events observation in the period of May-August 2018, would create an even better base to start a wholesome discussion. (. . .) Extending the results and discussion to months where drought characteristics were also observed in April and autumn months. (. . .) This in the end might help to create an even stronger discussion and to put the work into more context by being able to connect it to other drought studies of 2018 throughout Europe, bringing together other strains of research and closing the picture of the drought 2018.

Authors: We agree that it is a good idea to include some more lines about the spring and autumn to make a more wholesome discussion, and we will do that in our revised version. If you are aware of other studies of the 2018 drought in Europe, we are happy to receive a note on that. Regardless of already existing studies, we find your

comment to extend the discussion beyond May-August 2018, valuable in that potential future studies on the 2018 drought can more easily connect it to our paper if accepted.

AR#1: Table 1: adding an additional column for the observed impact category (e.g. agriculture, energy sector, etc.) would make table even more complete and could reduce effort to write all examples out in text.

Authors: We agree it is a good idea to include the impact category in Table 1, and we will do so in our revised version following the EDII categorisation provided in Stahl et al. (2016; doi:10.5194/nhess-16-801-2016). We will further ensure that the format of the Table fits nicely to one page, despite its extension, and potentially reduce details on the impacts in the text.

AR#1: p5 line21: 3 stations within mountain regimes mentioned which were highly influenced by glaciers, were they treated differently in the analysis or just included in the average?

Authors: We are not sure which average you refer to here, but the regimes highly influenced by glaciers were not treated differently from the other regimes in the analysis. Accordingly, the stations are included in the total percentages of stations affected and in the EOF analysis in the same way as the other stations (also reflecting widely different regimes).

AR#1: (...) to what extend was climate change reconsidered in the analysis and the trend that might have been included automatically in the datasets used?

Authors: Climate change was not considered explicitly in the analysis of the 2018 drought, and accordingly, potential trends in both the average and extreme conditions are automatically included. The main purpose of the ranking maps was to investigate the extremeness of individual months in 2018 compared to previously observed. The paper do not address the cause of the extreme ranking itself, rather whether or not it was an extreme year as compared to the historical record. On the other hand, the

purpose of the EOF analysis was to detect main patterns in summer streamflow variability, and linear detrending of the JJA streamflow time series was conducted prior to the EOF calculation (ref. p9, line 27-28).

AR#1: Results and discussion section in general: also include beginning and end of the year results next to extremeness of summer months if mentioned later on in discussion (for example HGT500 from April might already indicate how situation in May could look like);

Authors: We interpret this comment as being related to the third comment, both which we agree to.

AR#1: Fig. 4 and Fig. A3 using the same range for HGT500 values for all months presented would allow to compare values between months more easily. Additional question toFig.4: why aggregate over May-August (as most other results presented are shown separately per month)?

Authors: We did not seek to have the same scale on the HGT500 axes; rather focus was on depicting the relative variability for each month using standard deviation (thus allowing direct comparison of the variability as such). Figure 1 below shows the same figure as Fig. A3 in the paper, except that the same range is used for HGT500 values in the right panel. Accordingly, the time series shifts its location (up or down) along the HGT500 axis. We do not see any advantage of presenting the results in this way, and prefer to keep the figure as it is. Nevertheless, we will add a remark that the range is different, to ease the interpretation for the reader. Figure 4 is provided to emphasis the extreme overall large-scale atmospheric situation in the period May-August. Combined with separate monthly plots in the Appendix (Fig. A3), we think this provide an informative overview for the reader.

AR#1: (...) additional information and figures on mean historical temp vs 2018 temp would help to put this into place in regards to absolute values, also helps to understand precipitation observations as not that many low extremes were recognised but in SPI3

drought is indicated;

Authors: We agree that this is interesting additional information. We will make anomaly maps of temperature and precipitation and add these to the appendix or supplement.

AR#1: Fig10: what was the reasoning to switch to months June-August for this analysis, compared to the other results that have been heavily focused on period May-August?

Authors: The main reason to use June-August instead of May-August in the composite maps, was to use the same period of the year as used for the EOF analysis of streamflow that the composites are based on. The main reason for using June-August instead of May-August in the EOF analysis was to avoid the effect of high flow in May caused by snowmelt. Furthermore, EOF analysis and composite maps are traditionally done on a seasonal basis, making the results more easily comparable to other studies. We will make the reasoning behind the choice of June-August more clear in the text in our revised version.

AR#1: Discussion, section about annual hydrological cycle: more information and figures about initial conditions (e.g. snowfall) in supplement (e.g. annual averaged time series and 2018 situation, similar to Fig.1 and 2) and citations would support and help to follow the explanation of the specific observations and putting them into more context (some good starting information was already given in introduction about the hydroclimatological characteristics, streamflow and groundwater regimes);

Authors: Observed annual average time series plotted along with the 2018 time series for each streamflow and groundwater station, were made as part of the initial analysis, but not included in the paper itself due to the article already being relatively long. We agree that they can help support the interpretation and discussion and suggest to include such figures in the appendix or supplement. We will not include data of initial conditions, such as snow and soil moisture for each catchment, as this would require using modelled data.

AR#1: p16 line2: citations or other examples to underline this assumption?

Authors: This is a speculation based on the known important role of groundwater in Denmark and the less extreme potential ET losses in the west compared to the east (mentioned in the previous sentence, ref. Fig. 6 in the paper). We will include references to underline this assumption in the revised version.

AR#1: p16 line14-16: could you elaborate a bit more (e.g. references to figures where this is observed). If I look at Fig A9, A8, A7 for example I see overlapping areas and stations with indicate drought occurrence?

Authors: We will refer more specifically to figures used in this assessment, and clarify what we mean in the text.

AR#1: p16 line24: would you say this is already the effect of drought propagation one can observe (with the ongoing dry conditions until the end of the year (e.g. seen in SPEI3 results)?

Authors: We interpret your question as to whether the below normal groundwater levels at end 2018 /start 2019 are a response to the summer 2018 event or caused by the dry conditions towards the end of the year. It is probably a combined effect of the two. Following the inclusion of the situation in the spring and autumn (ref. third comment), we will embed an assessment related to this comment in the revised version.

AR#1: p17 line8-9: maybe include this reference already in introduction to set the stage for the discussion;

Authors: We will try to find a natural place for this reference in the introduction.

AR#1: Appendix: A1 mountain regime: why not include December as winter month for classification criteria for streamflow regime?

Authors: The reason for this is that none of the stations have minimum or a second minimum flow in December (the same is true for November). We will add a note about

this in Appendix A1. November or December is typically the beginning of the winter season, whereas lowest flow for stations with winter low flow regime typically occurs towards the end of the winter season (most winter low flow regime stations have minimum flow in February and March).

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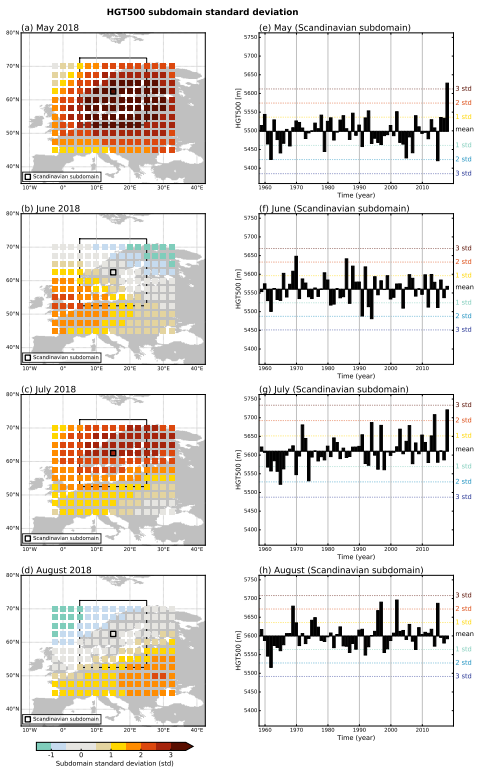

Fig. 1. Same as Figure A3 in the paper, except that the same range is used for HGT500 values in the right panel.