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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The effect of climate on timescales of drought propagation in an ensemble of global hydrological models" *by* Anouk I. Gevaert et al.

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General

The paper deals with the very interesting topic of drought propagation through the water cycle, but viewed only from a climatic perspective, with analyses between precipitation and each of three land surface responses (soil moisture, runoff, and routed discharge). The analysis is reported by climatic regions and winter/summer seasons, with the aim to find commonalities in the precipitation-land surface response. The bulk of the work is done based on ensemble mean indices from a range of global hydrological/land surface models, with at the end an attempt to look at the variability in the responses by

individual models.

The subject is very topical and relevant for publication in HESS. However, I regret that the analysis is done : 1) following climatic lenses (precipitation vs land surface; no analysis of propagation between the different land surface responses; summary/ discussion based on climatic regions without attempt to relate to soil/land surface/ bedrock/ catchment size etc... components). This is a shame and a more comprehensive analysis would be more valuable. Note that the title suggest 'effect of climate' but only precipitation (and not temperature/ evaporative losses) are considered, so it is not a full climate analysis that is undertaken ; 2) primarily on a multimodel mean (smoothing out extremely different behaviour; making extremely difficult a physical interpretation of results); 3) without justification of the choice of accumulations periods, which are arbitrary.

I don't feel the manuscript can be published in its current form, but owing to the importance of the subject for the scientific community, I believe it has potential for publications if the following points are addressed appropriately:

1. Undertake a full propagation analysis, by adding correlation between land surface components (soil moisture and runoff; soil moisture and discharge; runoff and discharge), and provide physically-based/ model structure/ parameterisation interpretation of the results. The analysis should also include at minimum catchment size, and if possible information on the land surface fields that should be available for all models.

2. Change the emphasis of the paper to individual models results, with the multi-model mean analysis presented last (if at all) with a justification of what it tells us. I am curious to know how different are the average SIs compared with individual models, and what mean SI represents physically. Understanding how the structure of the models influence drought propagation would be extremely valuable for future analysis. I fully agree with the point made by Referee #1 that there are strong collinearity between the different categories used to divide the models, and this should be considered in the

interpretation of the results

3. Better justification of the choice of accumulation periods, which are very arbitrary: how different would be the results if different / additional accumulation periods were used? Ideally, a sensitivity analysis should be conducted. Are the statistical metrics used appropriate? (Point also raised by Referee #1) Whilst I understand the rationale, I struggle very much with the analysis of the 'difference in ranks' as they are really arbitrary. For example I very much like fig 3 but find fig 4 might be greatly dependent on the arbitrary accumulation periods.

4. I find difficult to understand the rationale and use of the evaluation section, as there are no real links with the rest of the analysis/ discussion/ interpretation. I think it is great to have it, but it should be more prominent. Moreover, as the authors mention, the analysis is extremely skewed with a very unequal distribution of catchments geographically. A filtering, with much fewer catchments in US and western Europe should be done. The drainage area of the model extracted points should also be compared with the catchment one. How do the stations relate to the climate zones?

5. The method section needs to be re-written, especially the section on timescale propagation, and the rationale and description of the difference analysis p5 l9 to 20; what does mean 'statistical significance test does not reflect the relevance of differences between groups'? What is the group mean (mean correlation? something else?) in equation 1 and 2? The section on evaluation of drought propagation also needs clarifying. Are the RMSE done on daily or monthly streamflow? How well the drainage area of the pixel matches that of real catchment? What model results have to be recalculated and why?

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