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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Soil moisture and streamflow deficit anomaly index: An approach to quantify drought hazards by combining deficit and anomaly" *by* Eklavyya Popat and Petra Döll

Anonymous Referee #1

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General Comment

The authors introduce two global drought indicators, derived from modeled soil moisture and streamflow, which are based on the approach proposed by Cammalleri et al. (2016) for soil moisture over Europe. The goal of the study is well presented overall, and the analysis is clear. However, in my opinion, the focus on two indicators make the analysis weaker rather than stronger, and the potentiality of the research is not explored in full. For the first index, the authors introduce some modification to the original formulation of the DSI, but they fail in providing a proof that the proposed simplification is better/equal to the original formulation. For the second, there is much more space

for analysis and discussion on the water demand component of the index, which is a key point of the analysis that is not fully explored. In my opinion, the first part of the analysis is not sufficiently interesting, at least compared to the second, and I suggest to focus solely on the novelty of the streamflow drought and expanding this section for a more efficient delivery of the key message. Overall, I think that the paper has a very good potential, but it needs some major reworks to focus more on the strengths of the research.

Specific comments

Introduction The authors highlights how anomaly-based indicators are usually meteorological indicators, whereas deficit-based indicators are usually soil moisture/evapotranspiration indicators. However, they fail to highlight the reason behind this, which is the difficulty (impossibility?) to define a deficit threshold for a meteorological quantity in absence of a clear target (how much rainfall is enough rainfall?), which is instead more straightforward for plant water demand. This is a key point, an very important for the streamflow drought, where water demand can be defined, but is again much more complex than vegetation demand. I suggest the authors to expand this concept in the introduction to give more impact to the introduction of this concept in streamflow drought.

Methods and data More details on the water demand modules of WaterGAP should be provided, since this is a key component of the streamflow drought index. As stated in the general comment, I would ditch completely the analysis on SMDAI. There is not enough novelty in the modified index as it is, and the introduce modification are not sufficiently tested against the DSI to conclude that this proposed formulation is better/equal to the original (few maps on a specific month of 2003 are not enough). There may be still interest in fully analyzing SMDAI at global scale, since the DSI was tested only over Europe, but this can be the focus of a full expanded paper on this topic, where a detailed intercomparison can be performed. Results Removing the analysis on SMDAI will also give more space to the QDAI, which is more interesting in my opinion and truly a novelty. However, the analysis is currently lacking in depth in my opinion. As an example, rather than focusing on a single month globally (August 2003), the authors may show some continental maps for different local events. At the moment, its very difficult to draw any consideration of such large maps in which the interesting data are only on a small region. The analysis of the impact of different ecological flow is also very interesting, but it needs to be expanded beyond a couple of months. Finally, the comparison with the SSFI is very useful to give a benchmark to the new index, but it needs to be expanded as well. Is the number of drought event different for different regions? A

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