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Hydrology and Earth System Sciences Discussions

Interactive comment on "Evaluation of drought indices at interannual to climate change timescales: a case study over the Amazon and Mississippi river basins" *by* E. Joetzjer et al.

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This well-written study compares different commonly used drought indicators and their ability to reproduce hydrological drought in two large river basins, the Amazon and the Mississippi. Given the large number of drought indicators, this is an interesting study and certainly fits into the scope of HESS. However, although the analysis is well conceived, I find it incomplete with respect to (i) the analysed time scales and (ii) the selection of only two river basins. This compromises the main conclusion, that the SPI is not outperformed by more sophisticated indicators, although it is not so

clear why and where the differences between the two basins come from. I'd suggest a corresponding extension of the analysis, which in my view would allow for a more substantial discussion and evaluation of the comparison. Some suggestions for these (major) revisions follow, together with a few comments on text and figures.

General major comments

1. Could you extend the analyses to more large river basins? Your results differ between your two catchments, but an interpretation of these differences is probably impossible without a larger sample of basins. E.g., in *B. Mueller et al., 2010, Hydrological Processes: New diagnostic estimates of variations in terrestrial water storage based on ERA-Interim data* a nice selection is used. E.g., I'd find it interesting to have global maps with the correlations and CSS-values shown in Fig. 1.

2. Could you also analyse shorter time scales, 6- and 3-months maybe? You mention several times that your conclusions may depend on the time scale (and I'd expect this, too). Depending on the outcome, this could become Supplementary Information with a discussion in the main text.

3. The discussed literature is not complete and not up-to-date. I'll point to a few papers below, but will certainly miss many.

4. The paper would generally benefit from a more detailed discussion of the hydrologies of the catchments and their relation to the different drought indicators. For example, in the Amazon basin, ET and PET have very different trends in the GCM simulations, which hints at some supply limitations. So far, the text is mainly descriptive without providing enough interpretation of the results.

Specific major comments

Abstract: If the authors decide to implement the extended analyses, the abstract would look quite different. Also in its present form, it could benefit from a clearer rephrasing.

Sec. 2.2 and discussion of Fig. 1 in Sec. 3.1: I don't understand the rationale behind the detrending. E.g., Fig. 3 shows quite different trends in the drought indicators, which indicate different evolutions in different parts of the water balance in reaction to global warming. If you remove these by detrending, how comparable are the anomaly time series? How does Fig. 1 look for the original time series? Are the boxplots also derived from detrended time series (it's not clear from the text on P13237L17ff)?
Also, the detrending itself is not reproducible from your description.

Discussion and conclusion: I found it difficult to follow the authors' line of thought. Could you recapitulate what you have done and discuss this in a more stringent way (see some of the minor comments below)?

Minor comments

P13233L1: ... making it necessary ...

P13233L4: There is a lot more literature around, take a look at the SREX by the IPCC and references therein, e.g. *Orlowsky, B. & Seneviratne, S. I. Global changes in extreme events: Regional and seasonal dimension Clim. Change, 2012, 110, 669-696* .

P13233L15: Which drawbacks? That locally derived parameters are used globally?

P13233L18: This is not true any more: *Sheffield, J.; Wood, E. F. & Roderick, M. L. Little change in global drought over the past 60 years, Nature, 2012, 491, 435-438*

P13233L26: Replace 'this new index' with 'the SPEI'

P13234L6: The PDSI is not really meteorological drought only (at least according to your definition), since it considers soil moisture and runoff, too, although indirectly.

P13234L7: 'implicit dominant timescale' – it's true that Burke and Brown, 2008, write about a memory of 12 months, but your formulation suggests that this results from the definition of the PDSI (btw, later, on P13238L3, you mention that there is no specific time scale in PDSI, somewhat contradictory). I have never looked into this, but is this 12-month-memory found everywhere on the globe?

P13234L12: In which way are the basins contrasted and what does this mean for your analysis? Also, a few sentences of table-of-contents would be nice here.

P13235L3: rephrase as '... annual mean values are obtained by averaging monthly values from January to December.'

P13235L10: Could you mention RCP8.5 and CMIP5 etc.?

P13236L1: Could you add a reference for the Clayton skill score? Is it a standard score in hydrology? I read about it for the first time.

P13236L11: And 0 for all-wrong detection?

P13238L12: In fact we have shown in a paper just submitted to HESSD *Orlowsky B.*

& Seneviratne S. I., Elusive drought: Uncertainty in observed trends and short- and long-term CMIP5 projections that time series of SPI12 and soil moisture anomalies in the CMIP5 ensembles show different tendencies in several regions.

P13238L26ff: These statements are too qualitative and should be supported by some quantitative analysis (e.g. regression or correlation). Furthermore, SRI12 areal fractions over the Amazon doesn't change visibly (I'd be very surprised if there is a statistically significant trend), and it thus doesn't make sense to speak of a control by precipitation (unless you have a significant quantitative reason).

P13239L8: The fact that SPEI12_th differs more than SPEI12_hg itself doesn't prove that th is worse than hg. However, there are several papers which make this point and should be cited here.

P13239L11: 'Despite the presumably...' this sentence is contradictory, and no reason is given why a too simplistic PET would cause this large increase.

P13239L21ff: I'd say that in Fig. 3 for the Mississippi, SPEI and, in particular, SPAI agree better than SPI with SRI. Thus the general conclusion that SPI is at least as good as the other indicators is contradicted in one of your two example basins. For general conclusions like this (if possible at all) I think it's really necessary to include more basins in the analysis, otherwise your sample is simply too small.

P13240L1ff: The entire discussion of averaging and normalisation: Is it relevant for your analysis? As far as I see, you don't normalise averaged time series (which is good). It takes up a quarter of the discussion, but doesn't link to the presented analyses.

P13240L2: replace 'of' with 'or'

P13240L8ff: I don't understand this sentence: What do you mean by season-dependent? In Sec. 2 you write that PDSI is calculated for each grid point, only then the basin average is calculated. Why discuss the problem of a basin average water holding capacity here if you don't run into it?

P13240L18ff: Where does the link to precipitation come from? Two different PET formulations give different indicators, but how does this show that precipitation is not the only driver?

P13240L21ff: I don't see the connection of the last paragraph to the text – is this an outlook? It's interesting information, though.

Fig. 3 and its discussion on P13238L14ff: Why do you switch to the affected area instead of the indicators themselves?

Fig. 4: Could you include SRI time series?