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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.

**E. Joetzjer et al.**

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Revision of hess-2012-428 entitled "Hydrologic benchmarking of meteorological drought indices at interannual to climate change timescales : A case study over the Amazon and Mississippi river basins" by E. Joetzjer et al.

B. Orlowsky

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.

We agree that it would be interesting to extend this analysis to more river basins (cf Introduction). Nevertheless, this is beyond the scope of the present study and we claim that focusing on the Amazon and Mississippi basins is sufficient to support our main conclusions as explained in the revised manuscript (Introduction : L101 to L 111 ), namely that 1) meteorological drought indices based on both precipitation and temperature do not outperform the SPI at capturing the interannual variability of annual hydrological droughts (a result which is corroborated by Table 2 showing globally averaged scores calculated over all land grid cells), and 2) meteorological drought projections are strongly index and PET-calculation dependent (as illustrated by the CNRM-CM5 scenarios over both river basins).

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.

As emphasized in the original manuscript, the focus is indeed on relatively long droughts as characterized by the selected 12-month timescale (cf Introduction). This is a deliberate choice which guarantees a meaningful comparison between meteorological and hydrological drought indices. For shorter timescales, there might be a significant time lag before meteorological droughts translate into hydrological droughts and our SRI (surface runoff index) benchmark becomes questionable. Moreover, potential (here marginal given the size of the selected basins) impacts of dams are less significant on an annual basis than at shorter timescales. Nevertheless, additional analyses have been conducted in order to address this comment. Please find hereafter a box and whisker plots of sliding correlations and CSS20 scores calculated for the five members of the RCP8.5 scenario from 1850 to 2100 over a 49-yr time span for the Amazon and Mississippi watersheds for SPI, SPEI_th and SPEI_hg cumulated over 3 and 6 months respectively (fig.R1). Note that scores were computed against SRI3 and SRI6, without accounting for any lag between precipitation and runoff at the basin scale. Results show that the SPI is systematically but slightly outperformed by the SPEI_hg. Though not shown, this finding is discussed in the revised manuscript (cf Discussion) in order to temper our conclusion about the scores obtained for annual droughts.

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.

Please,find at the end of the response a list of added references in the revised manuscript.

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 sim-



ulations, which hints at some supply limitations. So far, the text is mainly descriptive without providing enough interpretation of the results.

Right, cf. Below (commentary P13234L12), also the revised manuscript have been rephrased.

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. The abstract has been totally rephrased, as the introduction and the discussion in order to clarify the objectives and temper some of the conclusions.

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?

Given the surface warming (and the possible precipitation trends) observed since the beginning of the 20th century and simulated in the historical and 21st century simulations of CNRM-CM5, it is indeed important to detrend all timeseries before focusing on inter-annual variability (section 3.1). This is particularly important for the 21st century simulations given the severe GHG scenario considered in our study. As shown hereafter (figure R.2 to be compared with fig. 1 of the manuscript) this is less important in the instrumental record: even if some trends appear over the Mississippi river basin, they remain relatively weak compared to the magnitude of inter-annual variability.

Are the boxplots also derived from detrended time series (it's not clear from the text on P13237L17ff)?

Yes, this has been clarified in the revised manuscript.

Also, the detrending itself is not reproducible from your description. As it is specified in the revised manuscript (cf part 2.2), cubic splines have been applied with 2 and 4 degrees of freedom for detrending over a 49-yr and 250-yr time span respectively. While such a choice is somewhat arbitrary, spline functions are known as the best approximation for smoothing a random timeserie and there is no reason to use a simple linear fit given the non-linear aerosol and GHG radiative forcings used in our climate simulations (cf. Voldoire et al. 2012). More details about splines can be found in the following reference, which will be added in the revised manuscript: Wahba G. 1990: Spline models for observational data. Society for Industrial and Applied Mathematics (SIAM).

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)?

cf. our response to the minor comments and the revised section 4 which has been totally rephrased.

Minor comments

P13233L1: ... making it necessary ... ok

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 ok included (section1)

P13233L15: Which drawbacks? That locally derived parameters are used globally? Yes, this is indeed the main drawback that was addressed by Wells et al. (2004). The PDSI underlying water balance model is quite empirical and has been tuned using a limited number of instrumented sites in the US. This limitation has been addressed by the development of the scPDSI (Wells et al. 2004) where the empirically derived climate parameters and duration factors are automatically calculated based upon the



historical climatic data of the selected location. This has been clarified in the revised manuscript (section1).

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 Thanks. The original manuscript has been modified in this respect and the suggested reference has been added in the revised manuscript (cf section 1, 3.2 & 4)

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

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. Since the PDSI is computed only from precipitation and temperature inputs, it is generally classified as a meteorological drought index (e.g., Dai 2010). Note that comparisons between the PDSI and soil moisture (Sheffield et al. 2004) suggest that the PDSI might also give some indication of agricultural drought, but the Palmer moisture anomaly index (Z-index) is more widely used in this respect.

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?

We agree that this formulation might be misleading and that there is no specific timescale in the PDSI even if the prescribed soil water field capacity has a possible impact on the index persistence. This sentence has been suppressed. The main motivation for the focus on the 12-month timescale is the possible direct (without a river routing algorithm) comparison between meteorological and hydrological drought indices. This has been clarified in the revised manuscript.

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.

The features of the Amazon and Mississippi river basins can be summarized as follows:

Vegetation: The vegetation density and rooting depth (and therefore surface evapotranspiration during dry spells) is much stronger over the Amazon than the Mississippi.

Hydrology: Both Amazon and Mississippi are among the world's largest river basins and are only marginally directly influenced by human activities (i.e., dams, irrigation). In contrast with the Amazon, the Mississippi hydrology has a significant snow component which is not accounted for by the empirical meteorological indices. Climatology: More importantly, the amplitude of the annual cycle and the magnitude of interannual variability are much weaker over the Mississippi than the Amazon for precipitation and vice versa for temperature. Such contrasted climatological features explain why droughts are dominated by precipitation anomalies over the Amazon, while they are more strongly modulated by the evaporative demand over the Mississippi. This is illustrated by the figure R3 which represents the 1951-2006 climatological annual cycle of precipitation (P), temperature (T2M), Hargreaves potential evapotranspiration (PET_hg) Moisture convergence (P minus PET_hg) are also shown. Note that the green (blue) line represents the Amazon (Mississippi) basin-average.

The introduction was rephrased and now emphasizes the reasons why the two selected basins are relevant a testbed for our analysis. A table of content was added.

P13235L3: rephrase as '... annual mean values are obtained by averaging monthly values from January to December.' Ok cf section 2.2

P13235L10: Could you mention RCP8.5 and CMIP5 etc.? Ok cf section 2.2

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

We are aware that the CSS is not so popular in hydrology, but it is a common metrics in rare-event situations. As a number of other metrics derived from a contingency table,



it includes two measures of performance to account for the two degrees of freedom present in the 2 by 2 contingency table. It is here used to measure the probability to observe an hydrological drought (according to the SRI) knowing that the meteorological index predicts such a drought minus the probability to observe an hydrological drought knowing that the meteorological index doesn't predict such a drought. This skill score is well adapted to measure detection and does not account for the false alarm ratio : B/(A+B), which is here less relevant as a meteorological drought doesn't necessary imply an hydrological drought. More details can be found in the following reference that will be added in the revised manuscript: Wilks S., 2004, Statistical Methods in the atmospheric sciences second edition (international geophysics series), 624pp

P13236L11: And 0 for all-wrong detection? Random detection would lead to CSS=0, and for all-wrong detection CSS=-1.

A table (table 3) has been added in the revised manuscript for more clarity.

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.

Ok thanks, included in the discussion.

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).

We agree with reviewer #1. These statements have been modified as there is no significant SPI trend over the Amazon. Quantitative statements are not necessary here as fig. 3 is sufficient to illustrate our main conclusion whereby the multi-decadal evolution of drought is clearly index dependent in our climate scenarios.

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.

In this study, the SRI is considered as a benchmark for all meteorological drought indices over both basins. SPEI_th overestimates the area affected by drought at the end of the century much more than SPEI_hg compared to the SRI projections. References have been added in the revised manuscript.

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

In the original PDSI calculation, potential evapotranspiration (PET) is diagnosed according to Thornthwaite's equation which is based only on temperature. The rationale of this equation is the strong connection between surface temperature and incoming solar radiation for monthly mean present-day climate conditions. Under enhanced concentrations of greenhouse gases, the surface warming is mainly caused by enhanced downwelling longwave radiation. Surface temperature is no more a reliable proxy for solar radiation available for photosynthesis and PET_th is no more a reliable estimate of potential evapotranspiration. This is the reason why meteorological drought indices using the simple PET_th formulation are probably too sensitive to climate change.

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.

The SPAEI cannot be considered as a proper empirical index since it is calculated using the actual ET simulated by our land surface model (rather than only from temperature



and precipitation inputs). The motivation for introducing this additional diagnostic is just to illustrate that P-ET (i.e. SPAEI12) is a better proxy for the long-term evolution of annual mean runoff (i.e. SRI12) than P-PET (i.e. SPEI12) given the overestimated sensitivity of PET to global warming (especially when using the simple Thornthwaite's equation). This conclusion holds for both selected basins and we don't see any reason why it would not hold for other basins as well.

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.

We feel that the methodology discussion is important since it might help the reader to replicate our results. Doing the spatial averaging before or after normalization has an obvious impact on the results given the implied non-linear transformation. Such a choice is somewhat arbitrary, but it must be clearly described and motivated. In order to address this specific comment, the discussion has been however shortened and moved to the section 2.

P13240L2: replace 'of' with 'or' ok

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?

This part of the final discussion has been removed and partly transferred to the methodology discussion in section 2 of the revised manuscript.

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?

If precipitation was the only driver, changing the calculation of PET would not affect the SPEI (which is based on P minus PET).

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

Given the discrepancy among the proposed meteorological drought indices, we propose the off-line land surface models as a more physical alternative for monitoring and forecasting droughts at the global scale.

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

Both spatial mean drought indices or drought-affected areas would lead to the same conclusion. Nevertheless, it was chosen to show the areal fraction affected by drought in % to have a more telling illustration of the index-dependent sensitivity and to avoid a partial cancellation between grid cells with increasing versus decreasing drought indices.

Fig. 4: Could you include SRI time series?

It does not seem very useful here as the aim is just to show that the SPEI response to global warming is strongly dependent on the calculation of PET.

List of added references

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Wilks S Statistical Methods in the atmospheric sciences second edition (international geophysics series), 624pp

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13231, 2012.

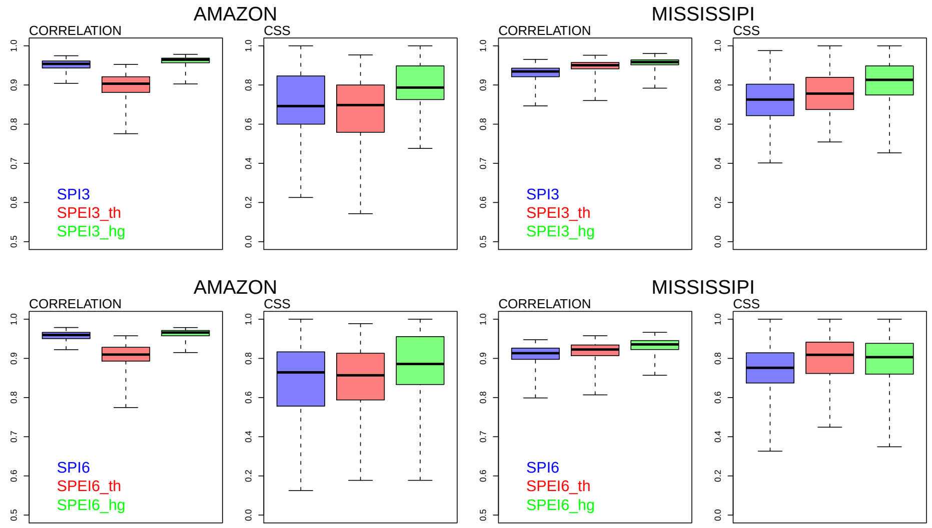


**Fig. 1.** R1 same than fig 2 in the manuscript but for SPI et SPEI cumulated over 3 and 6 months



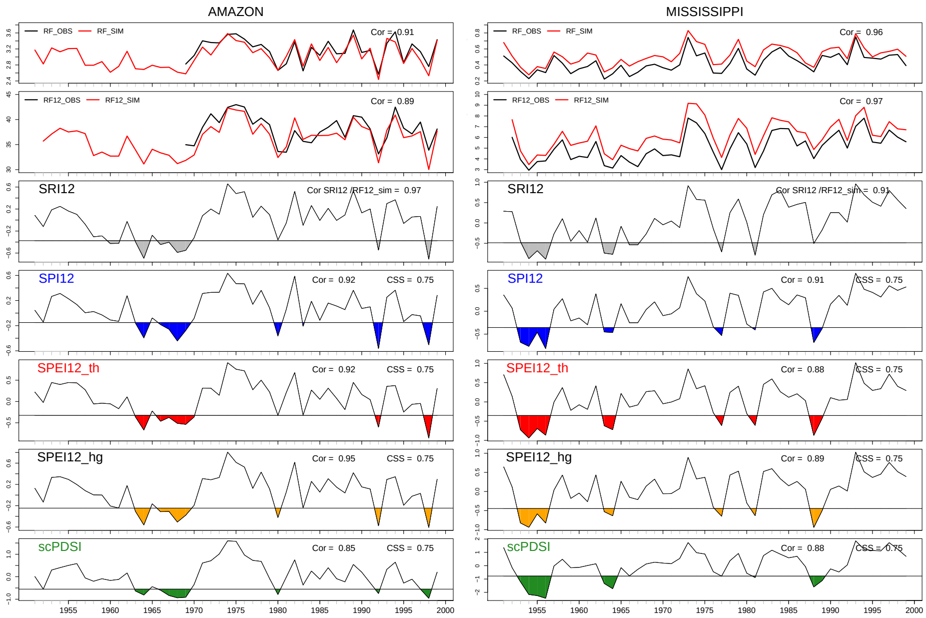


**Fig. 2.** R2 same than fig1 in the manuscript with raw timeseries

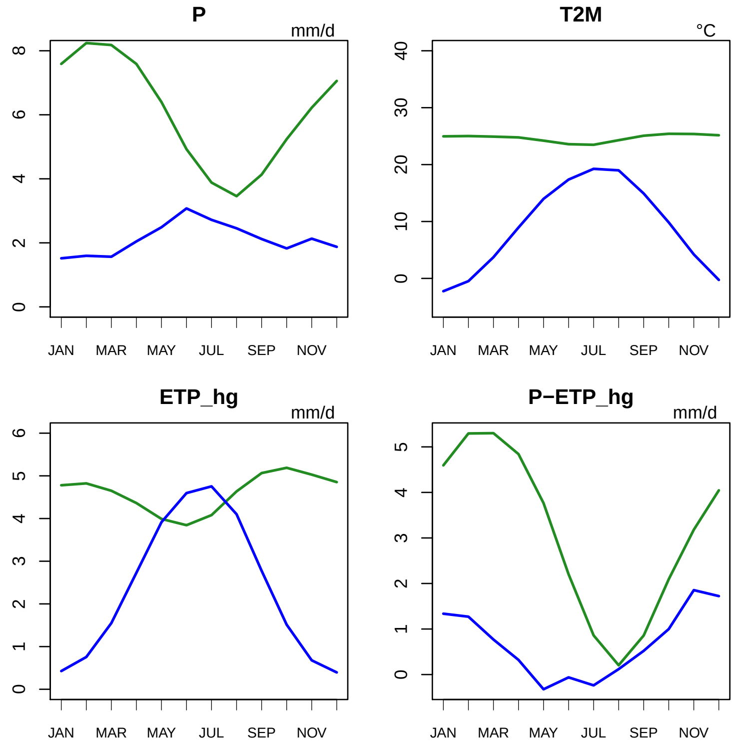


**Fig. 3.** R3 1951-2006 climatological annual cycle of P, T2M, PET_hg and P-PET_hg. green (blue) line represents the Amazon (Mississippi) basin-average.