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Hydrology and Earth System Sciences Discussions

Interactive comment on "A global analysis of the impact of drought on net primary productivity" *by* T. Chen et al.

Anonymous Referee #3

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Review of 'A global analysis of the impact of drought on net primary productivity' by Chen et al.

The authors investigate the impact of drought on interannual variability of net primary productivity over a short time span using a recently introduced drought index. These two appear to be coupled and in phase when averaged over the globe, but regional variations exists. This study has interesting aspects, like the use of the Köppen climate classification, and is generally well written. However, there are some serious shortcomings in descriptions and in analysis. Overall, my impression is that this work is

rather straightforward - it is actually nothing more than a correlation analysis, where the results are presented in different ways (globally averaged and averaged over regions). The study is intended to add some (spatial) detail to an earlier study, but is seems rather 'thin'.

My advice to the editor is to suggest to the authors to resubmit with *MAJOR* revisions.
Major issues

1. In the abstract, and elsewhere in the article, it is stated that averaged over the globe NPP and SPEI are in phase, while in 'most boreal regions' the opposite occurs. Nevertheless, the authors conclude that the 'strong positive relation' between NPP and SPEI is 'a composite of the positive relation across dry regions and the coherent NPP decline during and after intensive drought event in humid regions'. In my opinion, this is false. An alternative hypothesis is the following. Figure 3 gives the clue to why a positive relation is found, despite the negative relation in some parts of the globe. Fig. 3 clearly shows that the area with negative correlation is roughly above 60°N and there is hardly any area in the southern hemisphere with a negative correlation. This makes that approx. 25% of the globe shows this negative behaviour. Averaging over the globe, the areas with positive correlations will dominate and result in an overall positive correlation. If the authors want to stick to their original explanation, they need to quantify why a simple imbalance in areas with positive and negative correlations cannot explain this observation.
2. The authors use the SPEI as a drought metric. This is a variant of the SPI, where input to the drought index is precipitation minus potential evapotranspiration, rather than only precipitation. There are a few issues with the SPEI the authors need to comment on.
    - There are areas on the globe where the monthly accumulated rainfall is less

than the monthly accumulated potential evapotranspiration. This means that *negative* values are entered into the SPEI calculations. Can the authors comment on the physical realism of using negative net precipitation values in a drought index?

- The SPEI uses the Thornthwaite parameterization for potential evapotranspiration. This parameterization is only related to temperature (and the latitude of the grid square) while radiation is the principal factor in determining potential evapotranspiration. This can be justified by observing that temperature data is much more abundantly available than radiation data, although the Climatic Research Unit also makes the more physically realistic Penman-Monteith-based estimate for potential evapotranspiration available. Nevertheless, the authors should comment on the use of the Thornthwaite parameterization and quantify (or discuss using results from the literature) where this parameterization overestimates or underestimates potential evapotranspiration.
- The SPEI uses *potential* evapotranspiration rather than *actual* evapotranspiration. There is an inverse relation between these two in areas which are moisture stressed (). This will increase the potential evapotranspiration. How does this phenomenon influence the results of this paper?

3. The authors observe the counter-intuitive out-of-phase relation between NPP and SPEI, and relate 1 sentence to the explanation of this (p. 2436, line 20-22) which is in sharp contrast to the amount of attention given to this observation. Can you put this in more balance? Interesting in this respect is how the authors would explain the negative correlations between NPP and SPEI over tropical regions, like parts of Indonesia, African and South America. Is NPP in these areas temperature-limited as well? Note: in the explanation, radiation is mentioned but this should be deleted: no radiation is used in the SPEI used in this study – but this is a side issue.

Other issues to look into

- p. 2433, line 2. I'm not familiar with the ISCCP. The word 'Climatology' suggests that this is not a dataset with observed data for each month and year in the time span considered - or is it? If it is a climatology, then the impact of using a climatology, rather than actual values, needs to be quantified.
- p. 2433, lines 7-14. I have no clue why you use two different precipitation datasets and temperature datatsets here (GPCP vs. CRU TS 3.1 and GISS temperature vs. CRU TS 3.1). Why is that?
- p. 2433, line 20. It is confusing to say that k ranges from 1 to 48, while later you claim to use the SPI1, SPI3 and SPI6 only.
- p. 2434, line 14. The annual values of NPP are used in the correlation. Is that also the case for the SPEI data? This remark also relates to the statement on p. 2436, line 25.
- p. 2435, lines 8-10. Are the correlations calculated by first detrending the data? This should be the case - otherwise the correlation may simply reflect the presence of a trend in both quantities which is rather meaningless.
- p. 2436, line 27. I cannot follow you when you say: '. . . have to last at least two months to impact plant productivity significantly.' can you make it clear where in the results I can deduce this?
- p. 2437, lines 18-20. Are you now saying that trends in NPP are not reliable? Are they not reliably detected? Is there a drift in the instrument? Is there a reference which may corroborate this statement?
- p. 2437, line 27. Add the word 'pluvials' after 'droughts'. Both droughts and pluvials will be relevant.

- p. 2438, line 18. It would be nice to see a comparison with the 1976 drought or the more recent 2010 drought (which struck eastern Europe) to see if this result is robust - but I guess these droughts are not in either the satellite data or the CRU TS 3.1 data, right?
- p. 2438, line 23-24. The claim 'This suggests . . . intensity of drought.' is worded too strongly given that you only have one year (one measurement) to back this up.
- p. 2440, lines 9-10. Remove the part of the sentence where the monitoring is supposed to be done by satellites - that is not the subject of the paper and no comparison between ground-based and satellite observations is made in the paper.
- p. 2450. It would be informative to give the actual size of the correlations (rather than only its sign).

Minor issues

- Abstract, line 1. Explain NPP when this abbreviation is first mentioned.
- p. 2431, line 6 (and p 2440, line 6). The conclusion of Dai (2012) and Sheffield et al. (2012) are at odds with each other. Simply claiming '. . . the occurrence and severity of droughts in likely to increase in the future . . . ' followed by a citation of these studies does not reflect the scientific discussion on this topic.
- p. 2431, line 22. Add 'satellite' between 'recently' and 'soil'
- p. 2432, line 4. Include 'potential' between the words 'calculate' and 'evapotranspiration'

- p. 2433, line 15. To what does the word 'first' relate to? Did you redo this using a more physical realistic method too?
- p. 2438, line 22. Fig 3m should be 4m.
- p. 2439, line 29. What is 'native positive relations'?

Principal Criteria	Excellent (1)	Good (2)	Fair (3)	Poor (4)
Scientific Significance:				X
Scientific Quality:			X	
Presentation Quality:		X		

References

W. Brutsaert and M. B. Parlange. Hydrologic cycle explains the evaporation paradox. *Nature*, 396:30, 1998.