

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

Anonymous Referee #1

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General comments

This manuscript uses gridded Standardized Precipitation Evaporation Index (SPEI) and CASA Net Primary Production (NPP) data to study the relationship between water availability and NPP on the global scale as well as on regional (climate zone) scale. The results show that on a global scale there is a significant impact of water availability on global NPP. When studied on regional scales, this global signal appears to be composed of a significant positive relation between SPEI and NPP in (summer) dry regions and a significant negative relation in boreal regions. A result that is hardly mentioned is that no significant relation is found between water availability and NPP at the majority of the land, including tropical forests as well as mid-latitudes natural vegetation and agricultural landscapes, which is very relevant in the perspective of drought impact on

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the global carbon cycle and climate change.

My opinion about this manuscript is ambiguous. On the one hand the manuscript reports about a robust piece of research, resulting in clear results, which are relevant for understanding the land carbon cycle. The paper is well-written and comprehensive. On the other hand, the manuscript only poorly differentiates itself from studies with similar objectives. The longer time-series and the finer grid resolution as added value over Zhao and Running (2010) is hardly sufficient innovation to authorise publication. Important questions remain unaddressed: why do the authors use the SPEI index, and what difference does it make relative to the SPI index or the PDSI index? What is the advantage of using CASA in this study (there are many alternatives)? What is the physical explanation of the regionally varying positive, negative or non-significant correlations between NPP and SPEI. There is some speculation about light, temperature and/or water being limiting to NPP, but this is not substantiated. It would be relevant to know what processes are limiting, and how this could change in the future. Why do the authors only study NPP, as ecosystem respiration is also affected by drought.

My conclusion is that this manuscript is relevant and robust, but it's scope is too narrow. In order to be acceptable for publication, the innovation brought by the manuscript needs to be specified better and several aspects of the manuscript (mentioned above) need to be placed in broader perspective.

1. Does the paper address relevant scientific questions within the scope of HESS? yes
2. Does the paper present novel concepts, ideas, tools, or data? needs attention
3. Are substantial conclusions reached? yes, but needs to be better explained physically
4. Are the scientific methods and assumptions valid and clearly outlined? yes, see minor comments
5. Are the results sufficient to support the interpretations and conclusions? yes
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes
8. Does the title clearly reflect the contents of the paper?

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yes 9. Does the abstract provide a concise and complete summary? yes, but see minor comments 10. Is the overall presentation well structured and clear? yes, but see minor comments 11. Is the language fluent and precise? yes 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? yes, tables 1, 2 and figure 4 contain too much unsorted information 14. Are the number and quality of references appropriate? excellent 15. Is the amount and quality of supplementary material appropriate? n/a

Minor comments Abstract: explain what the positive/negative correlation implies: +ve cf = more NPP in wet conditions Line 8: The ‘however’ suggests controversy, which I cannot find, however. Abstract: add that not finding a significant correlation in many parts of the world is at least a relevant as finding both +ve and –ve relations.

Introduction: The questions ‘what is new’ keeps coming up. Please declare your innovation explicitly

Methods, equation 1: You explain each term in the equation, except $f(\epsilon)$, which as CASA’s drought impact modifier, could be essential in this study. How is this term prescribed, what is it based on and what is its relative impact in the total NPP, and in the results of this study?

Methods, page 2433: SPEI is proposed as the ‘one and only drought’ index, whereas in reality there are many, each with their own pros and cons. There is nothing wrong with using SPEI, but I miss a discussion about the sensitivity of the results to other drought indices or observations.

Methods, page 2435: The bridge for going from global to regional study is made in different words in lines 12-13 and again in lines 20-21. These two paragraphs confused me, and should be better organized.

Methods, page 2435: The authors compare the regional correlation coefficients with

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the global ones. They give two examples (DFEA and CWEA) which imho make no sense. In DFEA (boreal Eurasia) the authors observe no correlation between NPP and 6 months SPEI. In these cold regions, 6 months SPEI always contains a few months of winter season, when NPP is close to zero, and precipitation is snow. The poor correlation of 6-months SPEI with NPP should not be surprising. Similarly, for winter dry Eurasian CWEA, which I only find in South-East Asia, the seasonality is probably small, and consequently there is hardly any seasonal influence in 6-months SPEI. The two examples would seem to suggest that seasonality in precipitation and evaporation has a large impact on SPEI and its correlation with NPP, but I am not convinced that this proves that the drought impact on NPP changes with the duration of the drought ('NPP is only sensitive to droughts for a narrow range of time scales'). Note: I do not mean to say that the statement is wrong, instead I mean to say that the proof the authors claim to give is not valid.

Page 2436, line 26: The 1-6 months SPEI changes from -0.46, -0.61 to -0.75. The authors state that this 'indicates that in this region, droughts may have to last at least two months to impact plant productivity significantly'. The SPEI index only expresses the severity of droughts, how can you make statements about plant productivity based on SPEI alone? You would need to look at the correlation with NPP.

page 2437, line 3: 'anti-phase changes' do you mean 'anti-phase variability'?

page 2439, line 24: 'Using the drought index is an effective way to estimate drought impacts, compared to using precipitation data only'. What is the basis of this statement? Did you also do estimations based on SPI? How would you know which one was more effective?

Tables 1 and 2, figure 4: These tables contain a lot of information, the authors cannot realistically assume a reader is going to digest this. Please reduce the amount of information, or organize it in such a way that agreements/disagreements/conclusions present themselves.

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