

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

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Received and published: 21 May 2013

We appreciate the perceptive comments of all three reviewers and the short comment of Vincente (this issue) and respond here integrally to those. All other minor comments will be dealt in the revised manuscript. We first address issues of the novelty and the choice of the SPEI drought index.

The impact of drought on vegetation is an important topic in terrestrial carbon cycle. As we mentioned in our introduction, a series of case studies for Europe 2003 and Amazon 2005 (Ciais et al., 2005; Reichstein et al., 2007, Phillips et al. (2009)) showed significant impacts of drought on vegetation. At the same time, global scale analyses have started to attribute the decline of plant productivity to drought (Gobron et al.,

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Discussion Paper



2010; Zhao and Running, 2010). These studies generally show that at global scale relationship consists made up of a composite of several regional responses, where not only meteorological variation plays a role, but also the general sensitivity- or adaptation- of the vegetation to drought stresses. Savannah vegetation for instance is likely to be more adapted to periodic drought than a temperate forest that experiences a drought only once in a few years. We used therefore a combination of the Köppen climate classification, together with the CASA derived NPP and the SPEI drought index, to investigate this variability. By doing so, we aimed to improve the understanding of the relation of drought with vegetation (using the Köppen classification) and also detect whether our hypothesis of regionally varying responses is correct. We believe that this present sufficient novelty, even though the tools we use for analysis are based on correlation and time series analysis.

By using the SPEI index, we believe to have made the appropriate choice to study drought in a more meaningful way than with for instance the PDSI, or other static drought indices. As demonstrated by (Heim, 2002) and see also the comment on the Discussion paper by Vincente (this issue), over 10 different drought indices have been developed during the twentieth century, of which SPI and PDSI are the most widely used. PDSI is more physical based but SPI is easy to calculate and has different time scales. This time scale characteristic of SPI is very important to represent different kinds of droughts (McKee et al., 1993) and we noted this in the introduction by referencing van der Molen et al (2011). These authors present a review on drought effects on vegetation and in particular, note the response time as an important factor. SPI is also recommended by the World Meteorological Organization as a standard drought index. Zhao and Running (see Text S4 in (Zhao and Running, 2010)) also need to point out the limitations of PDSI in their use. SPEI has similar features as SPI but includes evapotranspiration, therefore providing a more meaningful parameter to detect the impact of drought on vegetation (see also comment by Vincente Serrano, Discussion this paper). As suggested by Reviewer 3, we used also applied the Penman Monteith equation based estimate of potential evaporation by using latest SPEI v2.2 (also recommended

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by Vincente in this issue, available at <https://digital.csic.es/handle/10261/72264>) in our analysis, rather than the Thorntwaite equation as in the original. Figure 1-3 show the main differences in the spatial patterns of correlation. We hope that by using SPEI as an index we improve on the static analysis of Zhao and Running (2010).

Figure 1-3 show the results of this change. Although there are some changes in the precise location of the correlations, overall the differences are minor and do not affect our conclusions. Because the PM equation is more physically based, we use this in the revised paper throughout.

We choose NPP as an indicator for drought sensitivity, so as not to get involved into separating several ecosystem type responses of heterotrophic respiration, R versus GPP. We appreciate that respiration is also sensitive to drought and soil moisture, but this field is only just evolving and we did not wish to further complicate matters. We also note that these components are usually calculated from NPP. Therefore NPP tends to be more useful for our study than either R or NEE.

We find in our analysis that the relationships are different in dry regions (arid and seasonal dry) and boreal regions. The general global scale relationship is thus composed of a composite of the positive relation across dry regions (note, not just the extreme drought cases) and a coherent NPP decline during and after intensive drought events in humid regions. We did not comment much on the regions where no strong relation was found, although, as reviewer 1 suggests, this is also a noticeable feature. In the revised version, we do comment on this and the consequences for the variability of the global carbon cycle.

To improve this discussion we plotted the correlations against latitude and calculated the contributions to global NPP of these latitudes. This is shown in the accompanying plot, which we will also use in the revised version. Figure 4 clearly shows that the dominant contributor to NPP is the tropics. The correlations coefficients show generally a mixed pattern, averaging around zero. Below 20 S we observe the strong positive

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correlations that are also visible in Figure 2-3. The negative correlations are above 60N, primarily in northern temperate and boreal forest. We discuss this further in our analysis in the paper using the biome classifications.

Further replies on the main comments of the reviewers.

Reply to the main comment of Reviewer 1

The reviewer mentions “It’s generally accepted that the main environmental stresses governing light use efficiency are temperature and moisture conditions, such as CASA and MODIS plant productions. However, different models are using various empirical expressions to describe temperature and moisture impacts”.

CASA employs a sub-model to calculate the soil moisture balance. The model keeps a running water balance where the main impact of soil moisture on NPP is given by water stress factor ( $W(\epsilon)$ ) which is calculated as  $W(\epsilon) = 0.5 + 0.5 \cdot P/PET$ , where PET is the potential evapotranspiration and P is the precipitation. This equation, though arguably simple, contains the primary responses of NPP to soil moisture. The factor 0.5 is chosen to incorporate the effect that in the fPAR data used in CASA, a soil moisture effect would also be visible.

Reviewer 1 mentions that “. . . .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’).”

The reviewer points out a very important issue about the drought index. All drought indices usually have such a limitation in the cold season at high latitudes. Therefore, we presented these results at annual scale not at seasonal scale. In the results section, we added “Significant values occurred at 1,3-month scales and the absolute values are much higher than that of 6-month scale, which may be caused by the very short growing

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periods during summer.” At the annual scale, we note the drought scale remains quite independent of the precipitation seasonality.

Reply to the main comment of Reviewer 2

The reviewer mentions the controversy surrounding the decline in NPP. We note that our CASA estimates suggest indeed a smaller overall decline than Zhao and Running. We did some work particularly on the perceived decline in the Tropics in 2005, but could not find an unequivocal response using FLUXNET data. Because of this we preferred to concentrate on the variations rather than the absolute decline. In the revised version we make this choice more explicit and mention the controversy. In figure 5 we show the behaviour of NPP and SPE over time per 0.5 degree grid box. A few issues stand out. In 2003 in Northern mid latitudes we note a decline in both SPEI and NPP, indicative of the large droughts experiences for instance in Europe (Ciais et al., 2005). In 2005 we note a similar phenomenon in tropical latitudes. This suggest that our analysis is indeed capable of picking up these “sever” events. On the larger timeframe, it appears however that for the tropics these events do not influence our correlations. We have extended our discussion of these issues in the revised paper and will include this plot.

Reviewer 2 states :”You mention NPP in Southern Hemisphere appeared to be more sensitive to variability in droughts. Did you calculate these correlations?” The reviewer 2 questions the relation for the tropics and correctly point out that these refer to the humid equatorial regions only. In that paragraph, we wanted to link our analysis to the larger scale. Both NPP and ENSO are related to the atmospheric CO2 annual growth rate because ENSO controls tropical precipitation and therefore impacts tropical NPP. We found indeed that NPP variations in the equatorial humid regions, are closely related to the drought index, and this contributes to this ENSO-precipitation-NPP mechanism. We modified this paragraph as the reviewer suggested in the resubmission.

The reviewer 2 questions the issue of sensitivity and notes that arid and semi arid systems may be better adapted to drought, while still show large sensitivity. Vicente-

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Serrano (2013) also points out this issue: “It is noteworthy that the highest influence of drought on vegetation identified in arid areas does not imply necessarily that plant communities from those areas are more vulnerable to drought than those dominant in humid biomes. In arid and semiarid regions, drought impacts usually result in decreased vegetation activity and plant growth, but rarely cause plant mortality or long-term damage”. In general, drought vulnerability is much larger in humid biomes than in arid ones, although we found a lower response to drought in the former. ” We note that this is supported by our earlier analysis (van de Molen et al., 2011),

We agree with the view that high sensitivity does not mean that plants in (semi) arid regions are more vulnerable (as defined as impact times sensitivity). Our point here is about the general climatic conditions that generate variation in the availability of water in these regions. Water availability conditions dramatically change with precipitation when the average climate states are very dry. We modified our text to reflect this position more clearly.

Reviewer 2 notes that we do not comment about the role of evaporative demand (potential evapotranspiration), which is calculated by SPEI. We agree with the idea that evaporative demand is an important role and that this is also one reason we chose SPEI rather than SPI. Potential evapotranspiration (PE) is the main difference between SPEI and SPI. We do note however, that in a recent analysis of global evaporation Jung et al (2010) indicated that particularly in the Southern Hemisphere this was caused by declining soil moisture rather than changes in potential evaporation. Showing declining soil moisture from satellites backed this up. This is in contrast to the suggestion made by Zhao and Running (2010). However, in the modified version we used SPEI with a Penman Monteith equation to calculate evaporation. This includes a humidity component that would generate a demand trend if there were one in the data used. We have noticed before that the use of this evaporation product does not affect our main conclusion and produces marginal changes only in the observed correlations. In line with Jung et al (2010) we conclude that soil moisture is the key to the observed

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decline rather than warming and increased demand. We extended our discussion in the revised version to comment further on this issue.

### Reply to the main comments of Reviewer 3

The reviewer states that “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. . . . . 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.”

We agree with this opinion that the positive related areas are much larger than negative areas, leading a positive global pattern in consequence. There is no conflict at this point. The purpose of our original expressions (‘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’) is to highlight the category (dry and humid region) of the positive contributions to such a global pattern. We made appropriate modifications in the revised version: our findings suggest that the strong positive relation between global average moisture availability and NPP is mainly composed of a composite of the positive relation across dry regions and the coherent NPP decline during and after intensive drought event in humid regions.

The reviewer 3 notes some issue with the SPEI and evaporation estimates. We refer to the accompanying comment of Vincente (this issue) who gives more reasons to consider why “the use of PE estimates in the calculation of a drought index is very useful, has a theoretical justification, and it is an efficient and easy way to include the

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effect of evapotranspiration demand on a variety of systems". We also note that it is not necessarily our purpose to compare drought indices, but rather use the one we consider appropriate to study the relation with NPP.

"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."

This can be explained because, in temperate ecosystems without very strong water limitations, higher temperature or dry conditions lead in general to more carbon uptake by a deciduous forest study (Goulden et al., 1996). However, NPP exhibited a sharp decline with SPEI in 2003 Generally in tropical rainforest, radiation appears as the primary limiting factor (see for instance Nemani et al., 2010). Therefore, we find no spatial unique relationship between NPP and SPEI, both negative and positive correlations were found in these regions, such as Indonesia, Africa and South America, except for the dry year such as 2005.

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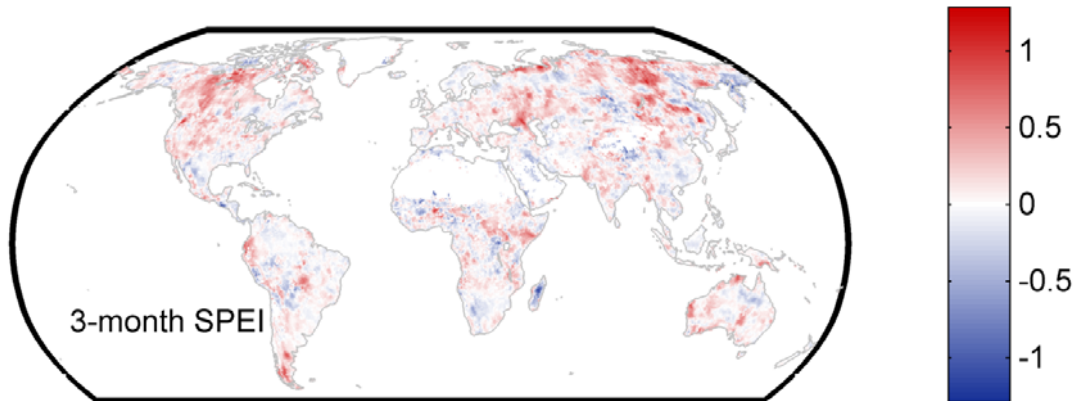
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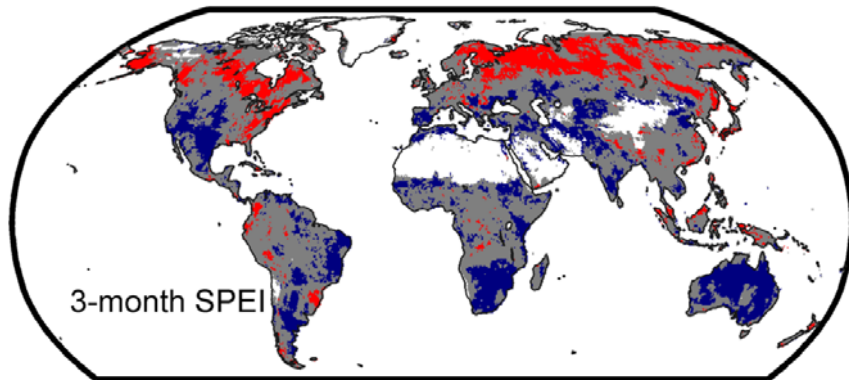
**Fig.1.** The spatial distribution of the difference between the correlation coefficients calculated by  $CC_{(SPEI(Penman-Monteith))} - CC_{(SPEI(Thorntwaite))}$  of 3-month SPEI and NPP.

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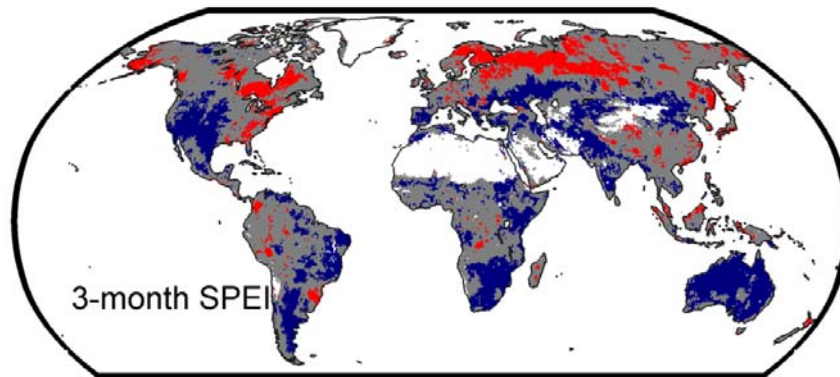
**Fig.2.** Original figure 3 in our paper with SPEI<sub>(Thorntwaite)</sub>

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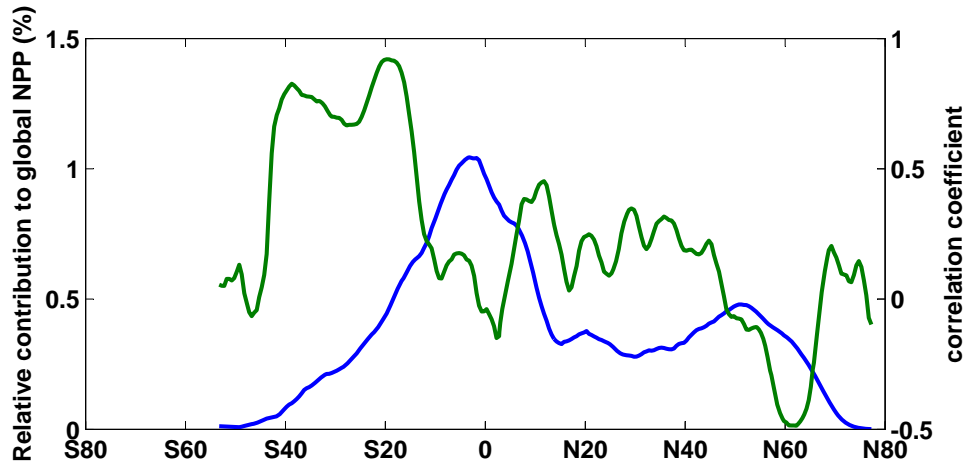
**Fig.3.** Original figure 3 in our paper with now with SPEI (Penman Monteith)

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**Fig. 4.** Correlation coefficients of latitudinal zone averaged NPP and SPEI (green) and NPP contributions as a percentage of global NPP. 5-degree moving averages were applied to all 0.5-degree steps along lati

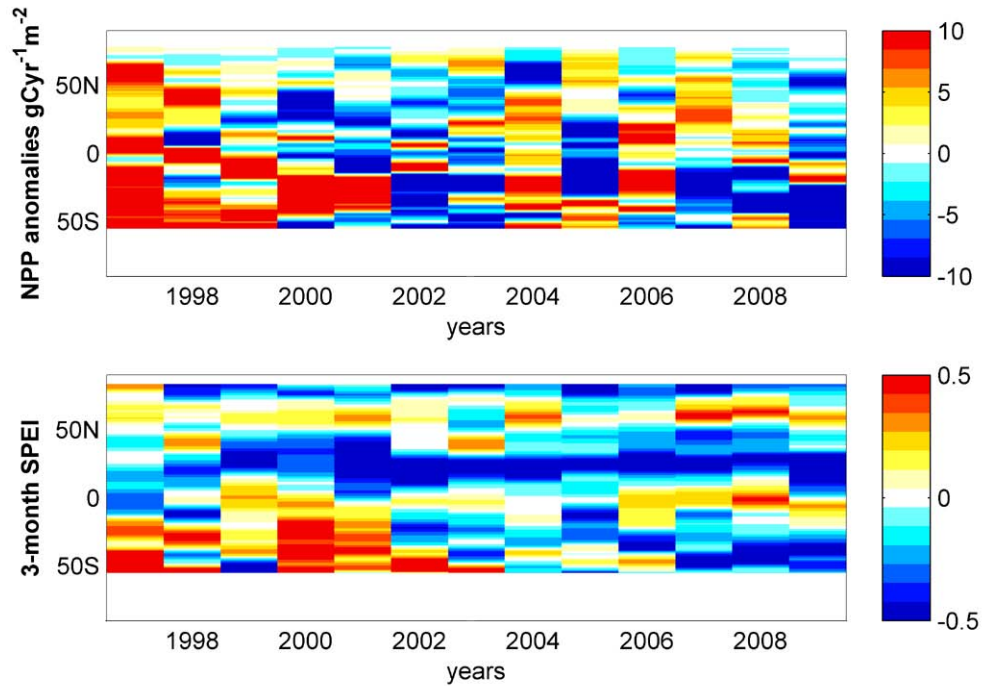
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**Fig. 5.** Latitudinal time-series of annual NPP anomalies SPEI.

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