Responses to Anonymous Referee (1)

The referee **comments** are highlighted in **black** and numbered with **#1-10**, whereas the **responses** are in **red**.

General Comments

The manuscript is well-organized and contextualized, with an extensive set of citations. The results presented are novel in their comprehensiveness and clearly relevant for a range of societal responses to hydrological extremes, as the authors note. There are various places where additional details are necessary to understand why a particular analytical approach was employed, or to further reflect on the implications of the results. However, these are fairly limited in number, and I am confident that the authors will be able to make appropriate adjustments with relative ease.

We thank you for taking the time to revise our manuscript and please, see our responses to your comments below.

Specific Comments

#1 Page 3, line 19: It would seem suitable to mention NGOs as another set of stakeholders that typically have geographically diverse portfolios.

NGOs are now mentioned within the sentence in the revised paper.

#2 Page 3, line 30: Do the authors mean that surface warming is attributed to changes? Or vice versa?

In the sentence we meant that surface warming was considered as the *cause* (or driver) of the observed wet-dry extremes changes. The sentence has been amended in the revised paper.

#3 Page 4, line 14: Given that 2.5-deg resolution is relatively coarse, the authors should somewhere have a sentence or two noting why this resolution is sufficient for their survey, or at least listing some regions where it may pose more of a challenge.

Thank you for the comment. The sc_PDSI_pm has indeed a coarse resolution that over certain regions (e.g. the tropics) and periods of the year (e.g. summer in the mid-latitudes) may not well represent for example local convective precipitation events. However, since our study has a global scope, we believe that the sc_PDSI_pm is the dataset that best suits our needs. We added three sentences mentioning this in the Data subsection and a sentence in the Discussion and Conclusions of the revised paper.

#4 Page 4, line 31: I found point (ii) unclear – over what time/space ranges is the counting done?

The count is computed by summing the number of wet AMAX occurring on the same date (i.e. year-month) from all the grid-cells. For example, in March 1970 a total of 28 grid-cells reported a wet AMAX, in December 2010 a total of 217 grid-cells reported a wet AMAX, etc.. We clarified the sentence in the revised paper.

#5 The co-occurrence discussion (e.g. section 3.2) is highly interesting. I wonder, however, if some sense of the closeness/connectedness of the events should be captured in order to truly reflect impacts, which is the motivation that the authors initially present. For example, it is not self-evidently clear why it matters that floods in Australia and the Northwest US, for instance, occurred simultaneously. Relatedly, some sense of the geographic distribution of co-occurring hydrological extremes might be useful in reflecting the 'widespread, simultaneous' character of event that the authors are trying to measure. I was especially struck by Figure 2b, in which it seems that the global peak is largely driven by drought in eastern Australia, while the rest of the world is in fact similar to normal conditions.

Thank you for your comment. The overall scope of this work is to simply bring to light the possibility that floods and drought can co-occur simultaneously in different regions in the world (see Figure 2). We tried to justify the importance of the impacts of such concurrent extreme events by mentioning the possibility to hedge economic losses with respect to hydropower production, crop yield, planting and harvesting (Page 10, lines 32-34). However, because of the original motivation of the work, we would like to keep further investigations about concurrent wet-dry extremes *impacts* for later studies. With this work, we indeed hope to have stimulated the interest of a range of academic experts and stakeholders. In Figure 2b it is true that the largest extreme dry area is located in Australia, however there are also other regions, such as India, Indonesia and western Africa, affected by extreme dry conditions. At the same time, we appreciate that at least some exploratory analysis in this direction is appealing. We will test a simple index of distance between local hydrological extremes for the globally extreme observations. Depending on the informativeness of the results we obtain, we may then add this to the revised study.

#6 In Section 3.5 (correlations with climate indices), the approaches used aren't capable of proving that these modes of variability explain the results. In other words, there may be a wide range of amount of hydrological extremes associated with similar mode-of variability combinations. Some analysis exploring this issue should be considered.

For a given state of one or more modes of variability, there could indeed be a wide range of possible hydrological extremes, and we did not want to convey the message that the modes of variability explain the entirety of our results. We further note that the correlations of wet and dry hydrological extremes with modes of climate variability is not strictly connected to the concurrent wet-dry extremes. However, it definitely provides new insights with respect to the global distribution of PDSI extremes associated with a given climate mode. There exists an extensive literature discussing the link between regional hydrological extremes and modes of variability. In the revised study, we will add some key references in support of our findings with PDSI. We will also rephrase the relevant section to clarify the limits of our analysis which certainly does not prove causation. We would finally like to note that in our work, three more climate modes have been tested against wet and dry hydrological extremes (i.e. NAO, PNA and QBO), however their results are not shown because lack of statistical significance (Page 9, lines 33-34 and Page 10, lines 1-2).

#7 Page 10, lines 22-23: That the AMO has the largest overall effect is interesting & surprising. What do the authors make of the fact that while the AMO has the largest effect, for the two most extreme wet & dry events (Figure 2) it apparently plays almost no role?

In Figure 5c the spatial patterns of the AMO influence on wet and dry extremes have been shown along with stippling representing statistical significance ($p < 0.05$). It is true that in Figure 2, or during the most widespread wet, dry and wet-dry extreme events, the AMO is not in a strong phase (i.e. AMO=0.21 for extreme wet and wet-dry and AMO=0.05 for extreme dry). The disagreement between the large influence of AMO on wet and dry extremes and Figure 2 can be explained by the fact that the grid-cells showing statistically significant correlations with the AMO (Figure 5c) hardly match with the grid-cells reporting extreme wet, dry and wet-dry events (Figure 2). For example, in Figure 2 part of Australia, India, central Europe, northern South-America, the Middle-East and central-western Russia are affected by hydrological extremes and these regions are not the ones showing statistically significant correlations with the AMO (Figure 5c). We therefore conclude that the patterns in Figure 2 are consistent with the fact that the AMO at the time of the two events was weak. This also points to the secondary role the AMO played in engendering these extremes. We added two sentences mentioning this in the revised paper.

Need for minor methodological comments:

#8 The authors should somewhere add thoughts on the usefulness of soil moisture metrics in addition to PDSI, SPEI, etc. Also, how much do they think that their results might be sensitive to the choice of PDSI threshold?

We understand the concern raised. To some extent, PDSI, SPI and SPEI can be considered as a proxy for measuring soil moisture, since they are derived from variables such as precipitation, evaporation and temperature. However, they have by no means a perfect correlation with soil moisture. In the revised version of the paper, we discuss this in the Introduction. However, based on your comment, we also discuss in Section 4 of the revised manuscript soil moisture metrics such as the ones derived from the ESA Soil Moisture CCI Project (Gruber et al., 2019) and the NASA Soil Moisture Active Passive (SMAP) mission (https://smap.jpl.nasa.gov/).

Concerning the second part of your comment, we replicated Figure 1 with two other PDSI thresholds, i.e. 1) PDSI <= -2 & PDSI >= 2 (Figure R1), and 2) PDSI <= -4 & PDSI >= 4 (Figure R2). Results are generally in agreement with the original Figure 1 (PDSI <= -3 & PDSI >= 3), except for the wet observations (Figure 1a) and for the fact that in Figure R1 the area impacted is larger and in Figure R2 is smaller (because more and less observations available). Here, for Figure R1 the wet land area decreases significantly, whereas for Figure R2 it increases significantly. It is worth nothing that in Figure R1 PDSI observations are not extremes, whereas in Figure R2 they are even more extreme than Figure 1. We added a sentence in the text describing these findings.

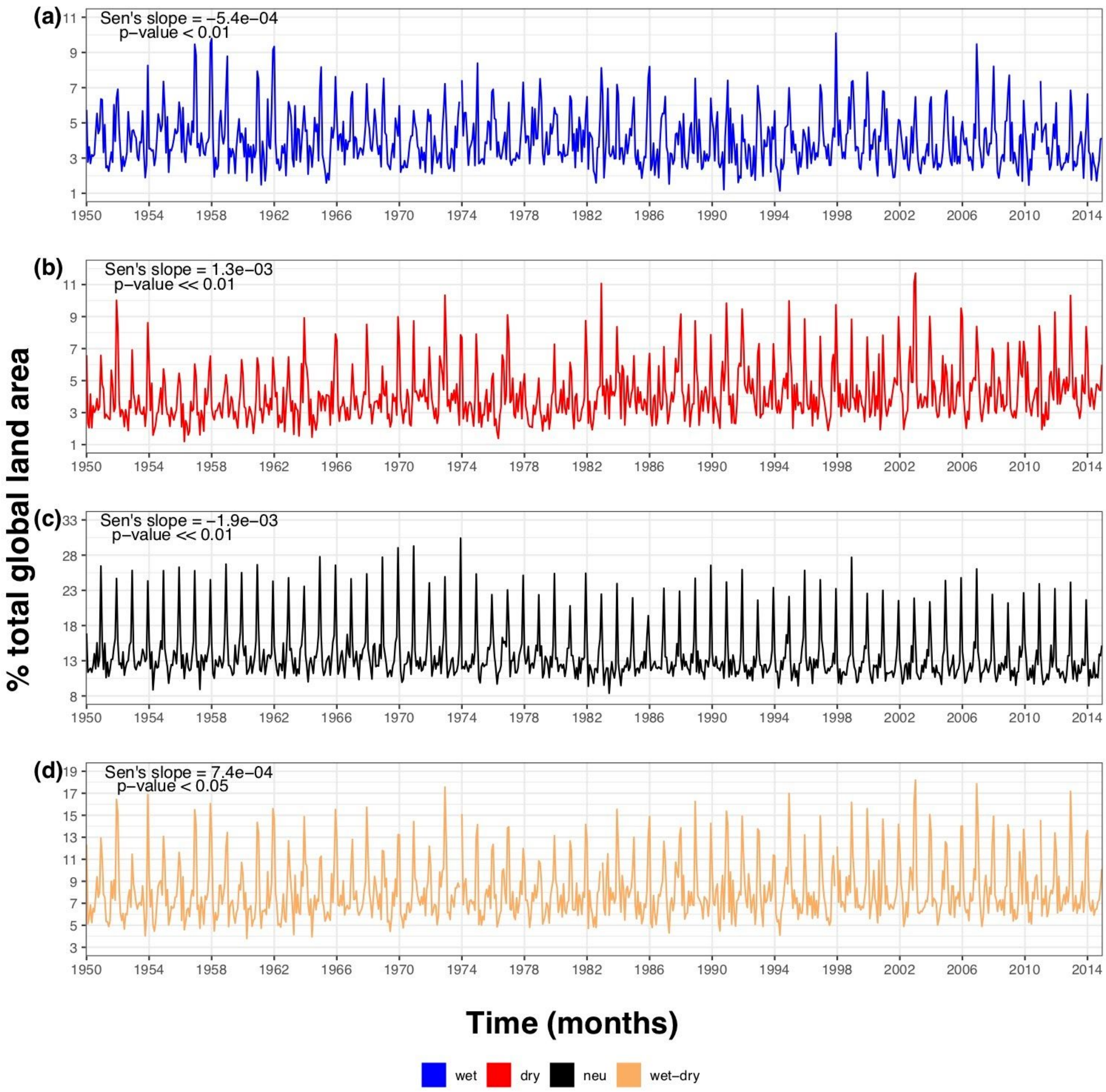

Figure R1 - As Figure 1 in the main manuscript but with wet and dry extreme events defined with a sc_PDSI_pm threshold ≥ 2 and ≤ -2 respectively, and neutral events defined within this range.

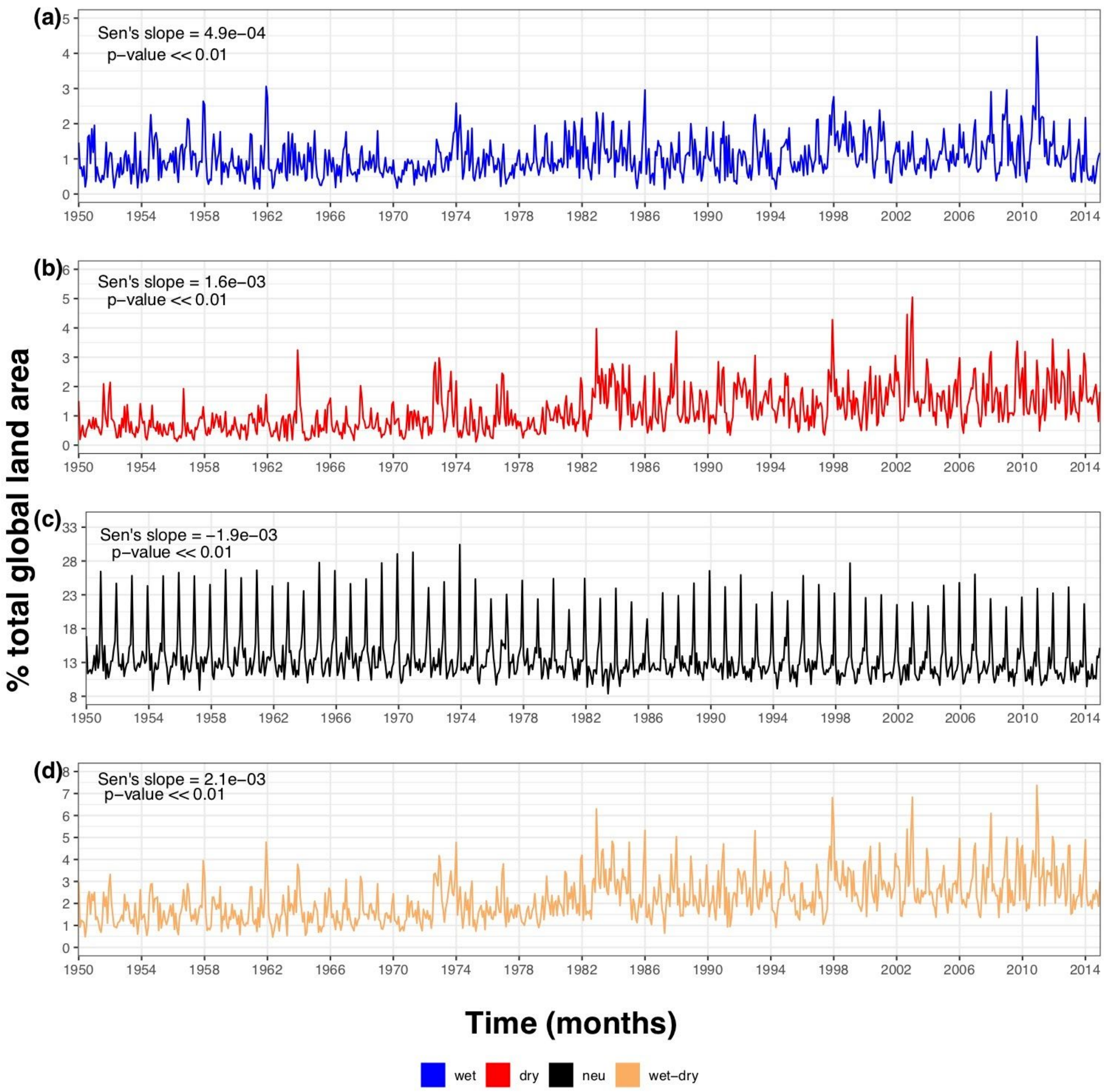

Figure R2 - As Figure 1 in the main manuscript but with wet and dry extreme events defined with a sc_PDSI_pm threshold ≥ 4 and ≤ -4 respectively, and neutral events defined within this range.

#9 Lastly, calculating lagged correlations with variability modes is probably worthwhile to consider, or at the very least a sentence should be added explaining why this was not done/would not provide much more information.

Thank you for the comment. As suggested, we performed lagged correlations with climate modes for 1 and 2 months in advance, i.e. PDSI at t1 and climate modes at t-1 and t-2. Thus, Figure 5 has been replicated with these new lagged correlations (Figures R3-R4). As Figures R3-R4 show, both the correlations and statistical significance patterns are qualitatively similar when compared to Figure 5. We will briefly mention this in the revised study.

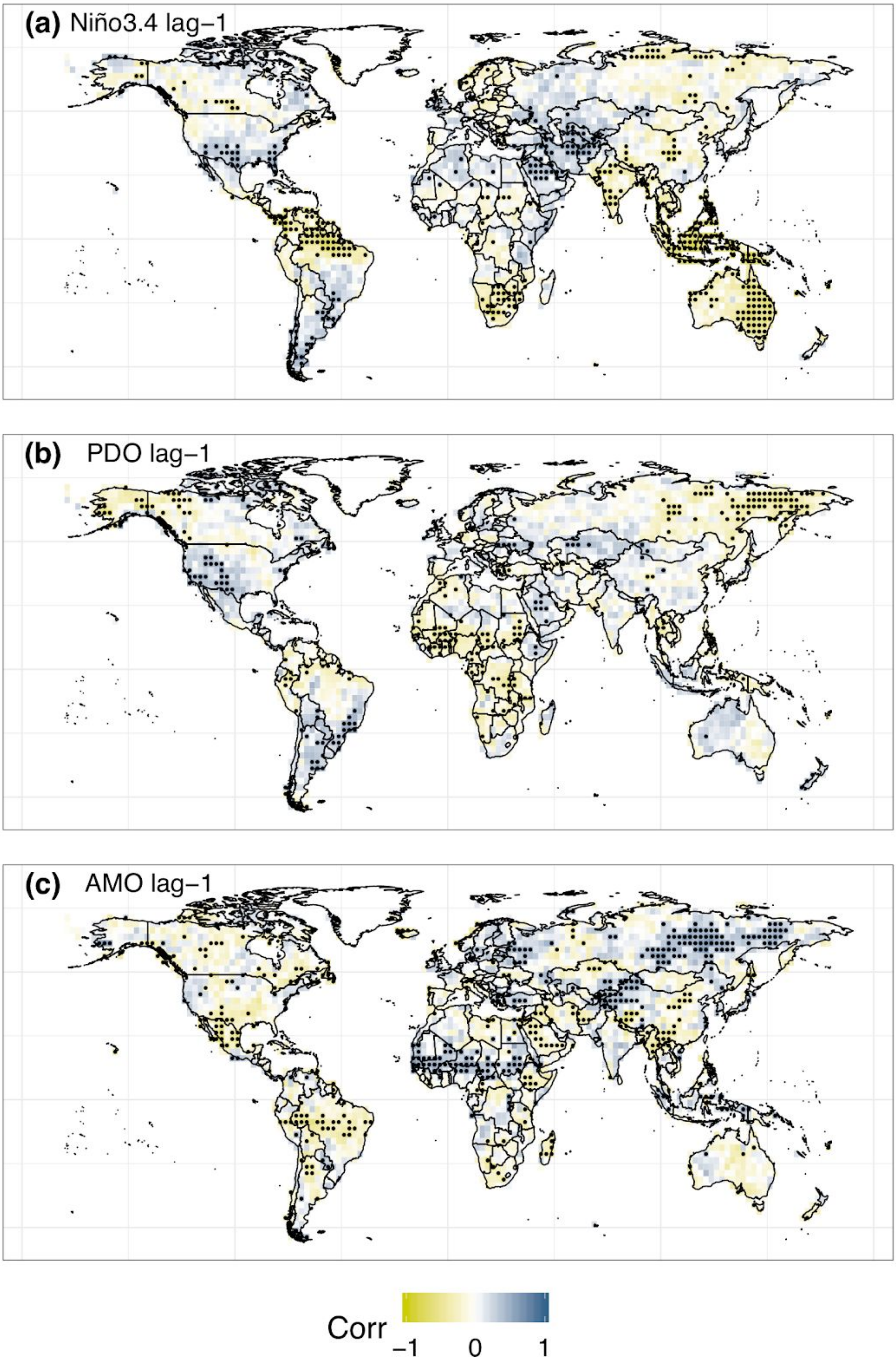

Figure R3 - As Figure 5 but correlations are lagged of 1 month, i.e. PDSI at t1 and climate modes at t-1.

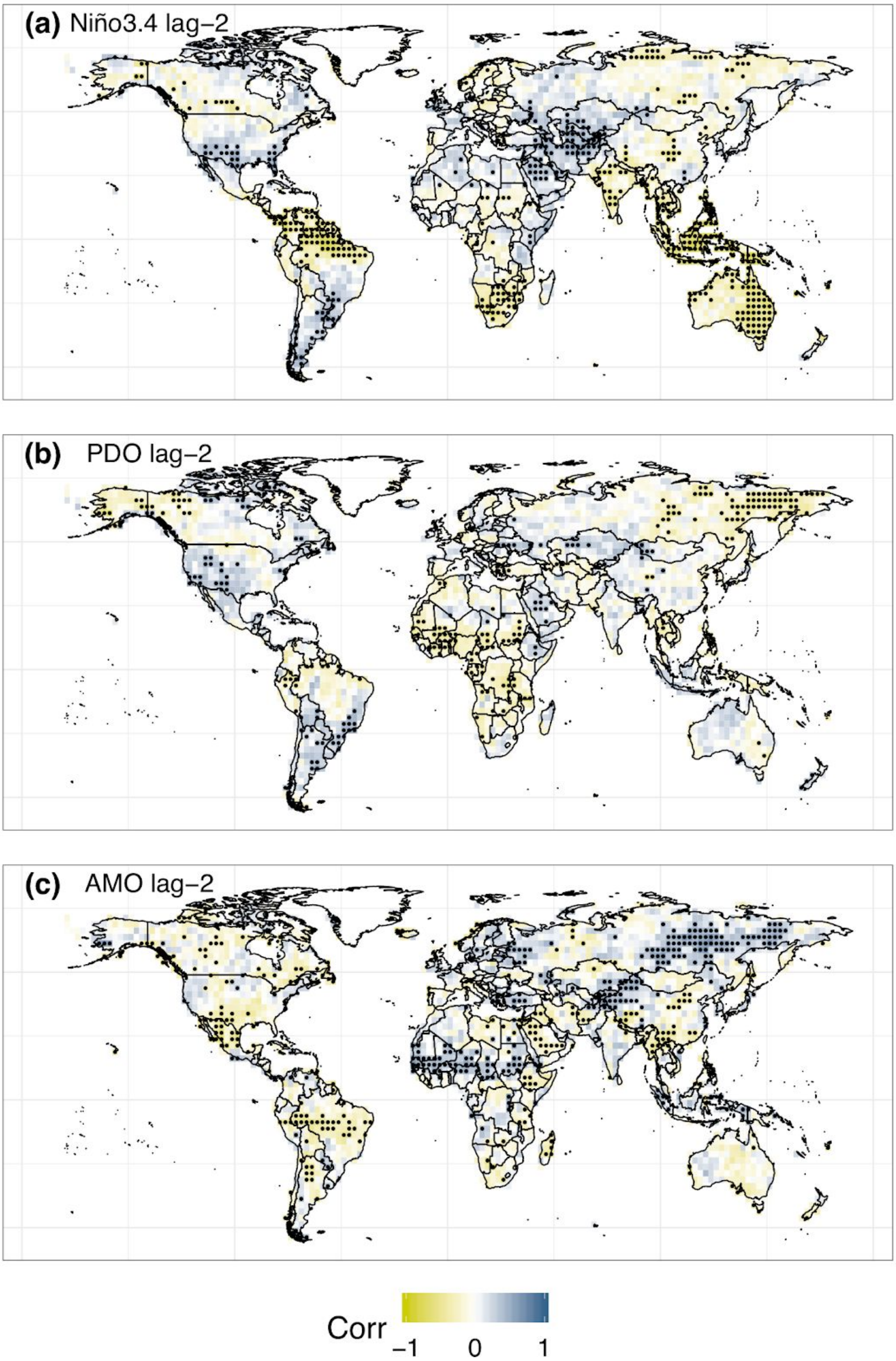

Figure R4 - As Figure 5 but correlations are lagged of 2 months, i.e. PDSI at t1 and climate modes at t-2.

#10 The Figure 4a caption seems to be inverted. As I would state it, ET is plotted as a function of time interval, not total land area impacted. This figure might also be better posed as comparison against the distribution of times as expected from a random Poisson process. While the comparison between the modes of the wet-to-dry and dry-to-wet distributions is easy enough, for instance, it is not straightforward to interpret what the ‘long tail’ means – is this tail longer or shorter than would be expected by chance? A Poisson comparison (or some other such reconceptualization) would help in making this figure more intuitive.

The caption of Figure 4a has been amended as suggested in the revised paper. However, we struggled to understand the Reviewer’s suggestion to compare our results to a Poisson distribution, and to evaluate whether the tails are longer/shorter than expected by chance. A Poisson distribution is informative in the case of wanting to know the probability of a given number of events occurring in a fixed interval. In our case, this could be the number of land grid-cells with Wet to Dry ET above or below a given threshold. However, the curves in Figure 4 show the % of affected land area for a given ET duration in months. Moreover, a Poisson distribution assumes the events occur independently of previous occurrences, which may often not be true in our case. Concerning comparing the tails of the curves to something happening by chance, this would require some assumptions on the parent distribution of the ET transitions, which may well be grid-cell dependent. What one may do is take a Peak over Threshold (POT) approach and conduct an extreme value analysis of the tails of the distributions, but this would not necessarily answer the Reviewer’s point. If we have misunderstood the Reviewer’s suggestion, we would be happy to address this point in a second round of revisions.

References

Gruber, A., Scanlon, T., van der Schalie, R., Wagner, W., Dorigo, W., 2019. *Evolution of the ESA CCI Soil Moisture climate data records and their underlying merging methodology*. Earth Syst. Sci. Data 11, 717–739, https://doi.org/10.5194/essd-11-717-2019