Review of "Regional co-variability of spatial and temporal soil moisture-precipitation coupling in North Africa: an observational perspective"

General comments

This study uses both spatial gradients and temporal anomalies to investigate the relationship between soil moisture and precipitation over the Sahel. It is very well-written and well-presented, and I appreciate its focus on a single region and the thorough meteorological and geographical understanding brought to bear.

My primary concern is about the statistical rigor of the significance testing: "Significance is represented by a percentile of the [difference in mean metrics] in a bootstrapped sample." Presumably this is done for each grid point individually, but these grid points are not statistically independent and one is almost guaranteed to have Type I errors (erroneous rejections and overstated results). This statistical dependence *between* spatial and temporal metrics is discussed on page 13, but not acknowledged *within* the individual metrics. The more rigorous manner of constructing Figures 4 , 5, or 9 would be to use field significance as in Wilks 2006 JAMC *"On Field Significance" and the False Discovery Rate*. This would better validate the stated "strong preference for convective rainfall over spatially drier soils" or that "temporally negative SMPC dominates". However, it would require substantial reworking of the manuscript in its current form, so this matter is left to the discretion of the authors and editor. Otherwise I have clarifying questions and suggestions:

Specific comments

I understand that the statistical framework being used is described in Taylor et al. 2012, but more clarification would still be helpful in reading through the methods:

- Page 7, Line 3 – Why is the accumulated precipitation threshold prior to the rain event lowered to 0? This seems overly stringent to me.
- Page 7, Lines 12-13 – The climatological mean is subtracted from *S'* prior to L_{max}-<L_{min}> gradient calculation. The entire current year is excluded so that the rain event does not impact this climatological mean, but are other years also excluded if a rain event occurred then?
- Page 7, Lines 15-17 – It is confusing to use Y_e as both the spatial gradient and temporal anomaly. The S'_e (L_{max}) is calculated in this temporal anomaly exactly as within the spatial gradient, right? A sentence stating this explicitly might also be helpful.
- Page 7, Lines 19-20 – Again I am curious whether the calculation of Y_c accounts for rain events in years other than the one under consideration.

Page 8, Lines 22 to 24 – Although they do not come from this study, I would have appreciated more explanation of the anomalous positive spatial correlations in Figure 5. The mention of "lower consistency of PERSIANN precipitation and soil moisture variability in time" is not entirely clear.

Page 9, Lines 31 to 32 – From Figure 4, I think these values for positive δ_e are listed backwards. It seems that <3% are positive in the 5° grid, while 6% are positive in the 2.5° grid.

Figure 6a – I do not think this Figure is necessary, as it simply illustrates a basic statistical concept.

Figure 6b – Could you please add a colorbar or mention what the colors mean within the caption?

Page 10, Lines 14-15 – This sentence is a bit vague. Which "initial hypothesis"?

Page 10, Lines 22-33 – It is not clear to me why this linkage of extreme soil moisture gradients to flooded areas is done only for L_{min} locations. Would one not be even more interested to pinpoint the locations of intense rainfall (i.e. L_{max}) where the drying rate and high soil moisture have low correlation?

Page 10, Line 33 – In my opinion, Figure 11 could be referenced in the wetland breeze discussions because it is quite useful to understand what is being proposed here, even if comes later in the manuscript.

Figures 7c and 7d – The Hovmöller diagram is of rain rate in [mm h^{-1}] (not accumulated precipitation in [mm]), right? Why are there two diurnal profiles one after another?

Page 11, Lines 26-27 – I am surprised by the finding that MCS are "short-lived and smaller" in the Western domain. This is not in agreement with some past literature. For example, Zipser et al. 2006 BAMS find some of the most intense thunderstorms globally in West Africa, and studies like Jackson et al. 2009 MWR focus exclusively on the high frequency of West African MCS. Is there a reason for the discrepancy?

Page 12, Lines 9-14 – This result suggest to me that the "choice of a later accumulation time than in T12" stated in Section 3.1.1 has a large impact on, for example, what is shown in Figures 4 and 7. It would be good to mention the potential impact of adjusting accumulation time in the discussion of these two figures.

Figure 9 – It might be nice to do one sensitivity test of the robustness of the temporal anomaly to the definition of "pre-event" (i.e. for what period of time preceding the rain event does this mostly negative anomaly field persist?).

Page 15, Lines 11-12 – This explanation is a nice mechanistic way of understanding the results, but should adjust with a different soil drying rate (which varies and sometimes depends on initial moisture from Figure A1). This adjustment could be described briefly.

The final point leads to a more general question I had: it seems to me that the current temporal anomaly definition from a climatological mean may not well account for the fact that certain locations simply have much greater soil moisture variability than others. To me, a kind of "soil moisture z-score" normalized by a variability would make more sense. Could the authors comment on this?

Boreal spring and autumn are the African rainy season, and I suppose that isolating these months from the others should change the fields. Have the authors investigated seasonality at all? Could some discussion be added, even if new Figures are not?