Interactive comment on "Regional co-variability of spatial and temporal soil moisture - precipitation coupling in North Africa: an observational perspective" *by* Irina Y. Petrova et al.

Irina Y. Petrova et al.

irina.petrova@ugent.be

The authors highly appreciate the editor's work in organizing the fast and smooth review procedure. The authors are also grateful to the two reviewers for their overall positive evaluation of our work, for their time and useful, concise comments and suggestions which will certainly help us to improve the quality of our paper. The comments of every reviewer are addressed separately. The authors' response is given below every reviewers' remark. The already implemented corrections/ changes are highlighted in the attached pdf file using latexhdiff tracking tool.

The author's response to the Referee #2: Benoit Guillod ——————————————————

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The authors thank Benoit Guillod for his willingness to review our manuscript, for the detailed assessments and valuable suggestions. We also appreciate Benoit's kind decision to forgo anonymity. It is especially relevant for us to get an evaluation of the manuscript by Benoit, since our work among others is built upon the paper of B. Guillod and his co-authors from 2015.

General comments REV#2 - This paper describes a detailed analysis of soil moisture-precipitation coupling over North Africa. Building upon the work from Taylor et al. (2012) and Guillod et al. (2015), the authors conduct an analysis at a higher resolution which allow them to identify the driving mechanisms in more details than these previous global studies. Among others, they highlight the role of wetlands and irrigated areas, and also study mesoscale convective systems (MCS) (both their impact on the statistical analyses and the impact of soil moisture on these systems). The manuscript presents a useful study that deserves publication in HESS. It is overall well written, clear and concise, with a few exceptions that deserve improvements listed below. Most of my comments below are minor but there is a number of them, hence I recommend major revisions although they should not be difficult to account for. I have also listed below a number of typos or edits (e.g. removal of commas). Being myself not a native english speaker, the authors can feel free not to implement these if they are confident that their version is more correct. I am also happy to forgo anonymity. Benoit Guillod

Specific comments (given as PX,LY for page X, line Y): REV#2 - - P3,L5-7,L16,L25-26: The mention of the 5 degrees resolution of T12 and G15 analyses (and 1 degree in this study) is somewhat misleading. All three studies analyse events at 0.25 degree, and subsequently aggregated their statistics to 5 degrees boxes (or 1 degree in your case). Please make this clearer at these lines to avoid confusion for readers who are not very familiar with those previous studies.

AR: Thank you for bringing this point out. We were aware the resolution verses aggre-

gation may cause confusion. We replaced the word resolution to either horizontal grid or scale consistently throughout the text. The resolution is only referred to data sets. We hope this can solve the confusion.

REV#2 - - P6, L7-9: The event identification and spatial metric (point i) is from T12 but the temporal metric (point ii) is from G15.

AR: The sentence was corrected following the remark.

REV#2 - - P6, L29-30: "a negative rainfall gradient between Lmax and its adjacent four pixels must be present". I do not understand what the authors mean: if Lmax is the pixel where is rained most, isn't a negative gradient with the neighboring pixels already ensured? Or perhaps I misunderstand what is meant here, in which case some clarification would be useful.

AR: Thank you for pointing it out. In fact, identification of a local maximum does not exclude the chance of having similar cum. rainfall value in a neighboring pixel. Minima locations are not necessarily the neighbors of Lmax. Therefore, an additional criterion is required to proof that Lmax is an absolute maximum within a box. As it is stated in the following sentence (P6-L30/31), such a criterion also helps to eliminate number of events identified within or at the edge of squall-lines. Following the reviewers' remark, it was decided to exclude this sentence from the paper, as it is rather a technical detail, and does not add much to the understanding of the results.

REV#2 - - P8,L20-22. "As in G15, the weakest negative coupling signal in the Sahelian domain is obtained with the PERSIANN (Precipitation Estimation from Remotely Sensed Information using Artificial Neural Networks) data set (Hsu et al., 1997)." I do not fully understand this sentence since the authors did not use PERSIANN. Do you mean perhaps not "As in G15" but rather "The PERSIANN estimates from G15 exhibit weakest negative spatial coupling from all..." or something along these lines?

AR: Indeed, your interpretation is correct. We modified the sentence following your

suggestion.

REV#2 - - P8, L24-25: It could be stated that the first part relates to the grey lines on Fig. 5 while the second and last part of the sentences is not shown.

AR: The Figure and the text were corrected.

REV#2 - - P10,L2: The chosen range (Q25-1.5$\times$IQR, Q75+1.5$\times$IQR) is somewhat complicated to understand. Why wasn't an easier range such as a percentile (e.g. Q01-Q99) or a fixed distance to the mean (e.g., +/- 2 std deviation) chosen? I understand that the choice restricts the selection to values that are very far from the mean and this might not happen at every pixel, but it is not straightforward to understand.

AR: Following the reviewers' advice, the definition criterion for an extreme value using varying percentile thresholds have been tested. Unlike the original extreme value definition, application of percentile thresholds will always result in the identified outlier in every grid box due to the way the percentile limits (1st and 99th percentile) are calculated. In that case, we would need to justify somehow an additional offset selection. Differently, originally chosen Q25 – 1.5*IQR and Q75 + 1.5*IQR thresholds on the contrary identify the values that are anomalously "far" from the sample, and hence lead to identification of only outliers and extremes. Therefore, we decided to preserve the original definition of extreme value in the study, since it is also a rather commonly used and justified extreme value definition. Yet, to support the text explanations and to make the approach clearer, an additional schematic was added to the Figure 6.

REV#2 - - P10,L5-10: This result might indicate that the use of the median Delta rather than the mean might be more appropriate, i.e. less affected by those extreme values?

AR: Indeed, it is so. Using median instead would reduce the magnitude of delta and hence, the amount of significant boxes, though the spatial pattern will remain the same.

REV#2 - - P12,L7-9: This is encouraging and supports the methodology of T12/G15 which was primarily aimed at detecting newly created systems rather than existing,

advected MCS. This might be worth noting.

AR: It is a good idea, but it seems that the sentence was a bit misleading. We did see that the majority of strong negative gradients is attributed to the first rainfall at the earliest time step, but we did not further analyze weather these rain systems were formed locally. Hence, we would be careful making a statement on the nature of rain systems. The sentence was reformulated a bit to avoid the confusion.

REV#2 - - P13,L9-19: The description of LCL results confused me initially, because Fig. 10b shows the height in hPa but the authors implicitly refer to the height as a distance above ground, both of which are of opposite sign. Hence I was first confused when reading "A slight increase of the LCL in the South" while Fig. 10b shows negative anomalies. I support the implicit use of height above ground in the text, but I suggest the addition of a short sentence that highlights that increase of the LCL height is shown as a decrease, in red, of LCL in hPa - or something along these lines.

AR: Thank you for the careful evaluation. Indeed, it reads confusing. We modified the text now following your suggestions.

REV#2 - - Section 5.2 (role of rainfall persistence): This section is useful and I like the concept behind Figure 11. However, the authors do not discuss explicitly whether rainfall persistence may partly reflects an effect of the land-surface or whether it only reflects atmospherically-driven persistence (the latter implying that the observed statistical relationship would be due to confounding factors). This is, of course, impossible to disentangle from observations alone and it is out of the scope of this paper to fully address this issue. Nonetheless, I feel that it deserves to be at least briefly discussed here. Numerous papers address this topic (e.g., Salvucci et al., 2002; Guillod et al., 2014; Teuling et al, 2005; Seneviratne et al., 2010).

AR: Thank you for pointing this important difference out. We plan to elaborate on the section 5.2, and following your remark we will add a brief clarification on the nature of persistence and references to the discussion text.

Figures REV#2 - - Figure 2: This is a very useful diagram.

AR: Thank you for sharing a positive comment.

REV#2 - - Figure 4: "The percentile values lying outside the significance range (10-90

AR: The sentence was rephrased.

REV#2 - - Figure 5: This figure is slightly confusing, although the content is useful. My understanding is that the upper dots are the fraction of negative SMPC and the lower dots are the fraction of positive SMPC, if that is correct this should be stated clearly. However I would suggest to use another way of displaying these, for example as a bar plot and one colour for positive SMPC, one colour for negative SMPC, both of them shown as values above 0 (technically it is the percentage of grid boxes so it cannot be negative). Also, the mean and ST.DEV are not clearly defined: is this the mean/stdev of all the dataset combinations of T12, G15 and your study? Why not show, for instance with light blue lines, the same for positive SMPC?

AR: We thank the reviewer for his suggestions. The figure was replotted accordingly, and hopefully looks much simper and clearer now. The data from the figure was additionally summarized in the Table A1, which was placed in the appendix section for the moment.

REV#2 - - Figure 6: "flood planes" -> "flood plains"? Also, why are there grey boxes? Is this where no extreme value is reached?

AR: Indeed, the grey boxes are indicative for all the other grid boxes, where no extreme values was identified.

REV#2 - - Figure 7: "ERA-Interim temperature and specific humidity profile and surface pressure data" -> "ERA-Interim temperature, specific humidity profile and surface pressure data". Also, "their typical state" is unclear, perhaps replace with "their climatology"?

AR: The suggestions were implemented. Thank you.

REV#2 - - Figure 8: This is a nice illustration, but it could be improved. Among others: (i) the X axis is not "[DAILY RAINFALL]" but "[TIME]". (ii) The Y-axis is not only soil moisture but also rainfall. (iii) Rainfall appears twice, once as "rain events" in grey bars and once as a solid black line (rainfall sums). Shouldn't it appear only once? Also, I am not sure why rainfall sums follows a sinusoidal shape here, I would favour the grey bars rather than the solid lines. (iv) More generally the caption should better explain the diagram. If some of these suggestions do not make sense, it probably points to something being unclear which led to a misunderstanding from myself. . .

AR: Thank you for the detailed suggestions. We will reevaluate the complete Section 5.2 following both reviewers' comments first, and then will also elaborate on the Figure 8 and will include your suggestions.

Technical corrections REV#2 - - Page 1, line 2: "1 degree horizontal resolution". This is somewhat confusing as the analysis was done on 0.25 degree but the statistics were aggregated to 1 degree. - Page 1, line 20: "1 to 3-D" -> "1-D to 3-D"? - Commas (",") are a little over-used in the manuscript. I suggest the authors to check these, here is a non-exhaustive list of where I think should be removed: P2,L2: "Both, observational", P2,L34: "wet soil, can favour...", P7,L11: "To estimate, whether". - P2,L12: TMPA is used as an acronym but is defined only later, perhaps refer to section 2.3. - P3,L4: Add a comma before "respectively"? - P3,L5: "no attempts were made" -> "no attempt was made"? - P3,L15: "in North African region" -> "in North Africa"? - P3,L18: "First we focus on identification" -> "First, we focus on the identification"? - P3,L23: "inter-relate" -> "relate to each other" or "interact"? - P4,L3: "inset rectangular" -> "dashed rectangle"? - P4,L5: "2016) and one of the" -> "2016), and as one of the"? - P6,L8-9 and P6,L26-P7,L3 and P9,L3 etc...: "-" often appear after (i),(ii) etc which could be removed. - Title of subsection 4.1: replace "." with ":"? - P12,L30: "anywhere" -> I think the authors meant "everywhere" (or perhaps "almost everywhere").

REV#2 - - P13,L13: "typical value" is somewhat unclear, perhaps only mention "climatological mean"? - P13,L13: "would imply presence of a stronger..., which can easier..." -> "would lead to stronger..., which can better..."? - P13,L21: "is shown" -> "has been shown"? - P16,L9: "Benoi Guillod" -> "Benoit Guillod"

AR: Thank you for the thorough evaluation Benoit and the suggestions. Your comments will be implemented, and an additional check up on the grammar and punctuation will be done.

Please also note the supplement to this comment: https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-530/hess-2017-530-AC2-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-530, 2017.