Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-530-AC1, 2017

Hydrology and Earth System Sciences Discussions

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.

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Received and published: 10 December 2017

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 #1

Printer-friendly version

Discussion paper

The authors thank anonymous reviewer for a thorough evaluation of the manuscript and especially for the valuable remarks on the structure and clarity. For details, please find our response to every item further below.

Major comments REV#1 1. Presentation: I found the paper a bit disjointed to read. I think there are lots of interesting ideas, but it jumps a bit from one thing to the other. I wonder if the results could be reorganised to make it easier to read. One suggestion is to first describe the feedbacks, i.e. show both the spatial and temporal soil moisture – precipitation coupling (figures 4, 5, 9, maybe 10), and then have a section talking about processes. The description of processes should then be consistent with both the spatial and temporal feedbacks you observe. The language throughout could be improved a bit as well (I have included some suggestions below, but my comments are not comprehensive). Finally, some figures I think could also be improved (my suggested changes are in the minor comments).

AR: We thank the reviewer for his careful evaluation and proposed solutions. After weighing the arguments of the referee carefully, we believe that restructuring the paper does not fit with the structure that we have foreseen. That being said, we realize that we have not clarified the structure well enough. We chose our ordering, because the analyses presented in Sections 4.3, 4.4 and related to a processes part are applied to the spatial SMPC only. The temporal SMPC is originally meant to be a secondary result, and is used here as one of the criteria to approve/ reject consistency to the mechanism of local breeze-like circulations on moist convection development.

REV#1 2. Wetlands (S 4.3): Of the results, this is the part that I found most confusing, and therefore least convincing. Some points: - Your definition of extreme is very confusing, it would be easier if it were expressed simply as the values above/below given percentiles (which is more or less what is done here, as far as I can tell, but with a fixed offset as well).

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#1 Wetlands (S 4.3): - I agree that features such as wetlands are likely to lead to extreme soil moisture gradients, but you might also expect the temporal variability to be low (at least over the wet part). Given you calculate temporal statistics, why not use this as an additional criterion? i.e. find locations with high spatial and low temporal variability. - The map (fig 6a) is not all that useful, as it is hard to see the exact locations and extent of topographic and wetland regions. It is therefore hard to tell if the conclusions in the last two paragraphs are really substantiated. For example, the region of extremes in the south East is very large, and I can't really tell how much of it falls on actual wetland. Similarly, the Gezira scheme in the map is quite small, and it's quite hard to tell what points on the significance contours you are linking to it.

AR: We agree with the reviewer that the explicit quantitative link from the identified location of extremes to the wetlands is missing, and would add a value to our conclusions. We elaborated on the reviewer's suggestions and some of our earlier tests to get an estimate of locations likely covered by wetlands (See Fig. 6c). Based on this analyses we will modify the paragraph in Section 4.3 and Fig 6 accordingly. Interestingly, all of identified areas of extremes including the large region in the East resembles pretty well distribution of wetlands obtained by Matthews&Fung from the pilot observations (see Fig.6b) The exception only comprises the floodplains south of Chad lake for unknown reason.

The added analysis is based on the linear regression estimates of 1-day soil moisture drying rate (SM at day 0 "minus" SM at day -1) and a starting soil moisture value (i.e. SM at day -1) climatologies. The climatologies are calculated for every Lmin location, (for same month as the event but for the non-event years). As the output, we consider the Lmin locations, where climatological values of the drying rate are always small and do not vary much with the initial soil moisture, as being representative for a water body or a wetland. Finally, 1x1 deg boxes on the Fig 6c, which contain the identified Lmin locations, are marked with a back cross. The detailed explanations will be added into Appendix.

REV#1 - Following on from this, in Fig 6b the dots (where the median and mean are opposite signs) do not really match the extremes (colors). I'm therefore not sure if the statement is justified that "extreme [soil moisture gradients] . . . in some cases appear to predefine its significance".

AR: We modified a bit the wording in the paragraph, so we hope that the link between the extremes and significance of the coupling is clearer now (P9-L13). Generally, the extremes will predefine the significance of 1x1 deg boxes not because of their effect on the sign change but due to their influence on the sample mean. The change in the sample mean in turn will affect the magnitude of the departure from the control (climatology), and hence the coupling significance. That is why simple removal of extreme values from the samples leads to a 30% reduction in the amount of 1x1 deg boxes with significant SMPC (P10-L8). The sign change is rather considered here as another conclusion stating that in most of the cases presence of extremes in a sample does not affect SMPC parameter sign (difference between median and mean).

REV#1 - More mechanistically, could the wetlands not be partly the cause of the covariability between the spatial and temporal feedbacks? The wetlands are approximately spatially and temporally constant, therefore when the soils surrounding it are drier than normal, this will always represent both a temporal and spatial negative soil moisture anomaly.

AR: It is very hard to answer this question with some degree of certainty for a number of reasons: 1) If the wetland location is always wet, then a presence of negative gradient is guaranteed. Yet the latter does not exclude the possibility of a drier location to be represented by a small positive SM anomaly, i.e. a positive temporal SMPC. 2) correlation of temporal and spatial coupling does not reveal particular signature over wetlands (Fig 9c). It is more likely that the cause of the co-variability is primarily a negative gradient itself.

REV#1 3. Reasons for co-variability of spatial and temporal SMPC: The discussion here is again a bit muddled. In the conclusions you state "the drying of the soil for several days . . . may play a role in the opposite sign of the temporal coupling as compared to the positive relationship in wetter climates". It is worth noting that in G15 the temporal coupling is positive across most of the globe, including in other arid and semiarid regions (northern India, Australia, Saudi Arabia, etc.). The 3- 4 day variability of rainfall in West Africa driven by African easterly waves is a factor, as is pointed out. I think, however, that the primary factors is probably that this is a high CIN/high CAPE environment, therefore anything that helps overcome the CIN will enhance rainfall. This is mentioned in the text when comparing the southern and northern part of the domain, but I suspect it can also explain the differences with other areas of the globe.

AR: We thank the reviewer for being objectively critical. 1) We may not fully agree on the first point stating that ". . . in G15 the temporal coupling is positive . . . including in other arid and semiarid regions". From their suppl. Fig.3 it is seen that the semi-arid regions like the Great Plains and the Sahel have expressed negative temporal coupling. Most of the positive temporal SMPC weirdly is identified in the deserts, inc. Saudi Arabia, S. Africa and Australia. Australia, inter alia, was not identified as a SMPC "hot-spot" in the studies of Koster et al., 2004 and Dirmeyer et al., 2011. Northern India might still express an orography effect. 2) It is indeed worth mentioning the relevance of BL recovery linked with building up CAPE and depleting CIN. Thank you for the comment. Yet, to our perception main point here is rather in the effect of cyclicity of rain systems and soil drying periods on the temporal coupling sign in the Sahelian semi-arid environment verses role of synoptic system and rainfall persistence in wetter climates.

Minor comments REV#1 P2, L8: ‘have a direct effect on the resulting sign’. It would be useful to explicitly state what the sign they find is (i.e. positive temporal, negative spatial) AR: The sentence was reformulated.

REV#1 P4, L9-10: ‘on the northern flank. . .’ In the diagram it looks like the gradients go across the ITCZ, as opposed to just being on the northern side? AR: The sentence was reformulated.

REV#1 P6, L29-30. You say you need at least three values of Lmin – do you take the three lowest? (presumably, unless Lmin is 0, there is only one minimum value). I am also confused by the criterion that ‘negative rainfall gradient between Lmax and its adjacent four pixels must be present’. If Lmax is the maximum, then the gradient with the 4 pixels surrounding it will always be negative – I suspect I am misunderstanding this line.

AR: 1) Indeed, in most cases if Lmin is non-zero, then it is likely that there will be only one Lmin value around given Lmax. Hence, most often values of aft. accum. rainfall in Lmin locations will be 0.0 mm. The clarification sentence was added in the text following your remark (P6-L30). 2) It is indeed a confusing sentence. Thank you for pointing it out. In fact, identification of a local maximum does not automatically 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 was 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 your remark, we decided to exclude this sentence from the paper, as it is rather a technical detail, and does not add much to understanding of the results.

REV#1 P8, L9: '. . .orography mask applied in this study'. Do you mean the maxima are produced by the fact that you are masking the region around the maxima? This sentence is not very clear. AR: The sentence was reformulated.

REV#1 P8, L31: I wonder if the different datasets agree more also because precipitation retrievals themselves are more consistent over flat terrain, while they are likely to disagree more over complex topography.

AR: In general, the areas of strong geographical gradients are masked out in the study. This however would not necessarily mean that we are left with flat terrain only. The (dis-)agreement between experiments is surely a combination of uncertainties coming from both, soil moisture and rainfall data sets. More complex terrain and hence more recurrently flooded areas towards East are expected generally to complicate both, the accuracy of soil moisture and rainfall data sets, as well as ability to isolate surface effects on rainfall. It is therefore likely that orography influences the results. Yet, at which degree and if it could be a dominant factor for a (dis-) agreement between the experiments is hard to answer without carrying out more analysis.

REV#1 P9, L19: might be worth mentioning that the significant correlations lie exactly on the semiarid transition zone between forest (in the south) and the desert. Also, have you considered the impact of vegetation (where, presumably, you do not get soil moisture retrievals) on your results? You do get some significance extending down to 8N, where it is quite vegetated.

AR: 1) We deliberately decided to not mention the link to the land cover or the transition (Sahel) zone. Mainly because the high correlations partly reach quite far south as you mention, but also because the high correlations appear as a dipole rather than a zonal feature. 2) By method, the soil moisture pixels are excluded if vegetation optical depth goes beyond 0.8. It is a common threshold that is usually used to filter out effect of vegetation on soil moisture quality. Effect of vegetation on the SMPC results was not assessed within the current framework. It is expected however that vegetation (especially in the south and during wettest months of July-August) will influence SMPC relationship via its effect on turbulent flux partitioning. In this way, application of soil moisture parameter instead of e.g. surface fluxes is a definite drawback of the given method, which can be explored in the future studies.

REV#1 P9, L23-28: While the explanation offered here sounds plausible, I would be wary of drawing conclusions from a few 'blue' points – as you say, this is likely not statistically significant. Also, are you sure less than 0.1% of points have a positive delta(e)? In the 5 âŮę domain there is less than 100 points (and one 'blue' point).

AR: 1) In the lines (now L28-33) the link between the "blue" points and location characteristics is rather meant to be a hypothesis to prove in the following section 4.3 (extremes). Then, in the section 4.3 as well as in the newly added analyses on wetland locations we actually illustrate that extreme positive soil moisture gradients indeed emerge in this concrete location and coincide with wetland positioning. As a potential solution we could suggest adding a sentence after the L28-33: "... The potential link between the land surface characteristics and SMPC parameter will be explored in more detail in the following section 4.3. " 2) Thank you very much for checking on the numbers. We seem to have forgotten that the table values are in % for these few experiments. The values have been corrected now.

REV#1 P12, L23-26: have you looked at the seasonal variability? I wonder if June (or years which are particularly dry) behave differently.

AR: We did check very briefly if the sensitivity of the SMPC signal to the choice of summer month is consistent with the result presented in the study of Taylor et al., 2011 (suppl. FigS4). However, in order to preserve maximum sample size, all further calculations were carried out for JJAS months jointly.

REV#1 P13, L25: I imagine the point here is that boundary layer moisture in the north is less tied to (local) soil moisture, and depends more on the larger scales (i.e. monsoon intrusions), which is why you don't see a wet advantage even though moister conditions do increase rainfall.

AR: Indeed, that is also how we understand the result.

REV#1 P13, L30: might be worth mentioning that in some cases soil moisture gradients can determine the location of convection, even if the trigger is provided by cold pools or the larger scale (Birch et al 2013)

AR: The remark has a good point, but in our opinion it falls out a bit of the context of the paragraph if added (now P13, L22). The paragraph and the result indicate that the mean number of 10 dry days in the northern latitudes (>15N) will unlikely lead to any strong soil moisture heterogeneity. Therefore other triggering processes in combination with increased moisture advection will likely favour moist convection development. In this sense our result would be more consistent with the listed studies of Barthe et al., 2010 and Cuesta et al., 2010. The MCS case study of Birch et al,. 2013 lies exactly at the boarder (∼15N) of the increased BL moisture pattern in our Figure 10b.

REV#1 P14, L13-24: I find this whole section quite speculative, and as I say in the major comments, doesn't really address the differences in Sahel with the rest of the globe (what about tropical areas? Or other semiarid ones that are different?). Also, I'm not sure I agree with "the above relationship is consistent with the negative spatial but positive temporal SMPC". I can see the link with the positive temporal relationship, but why a negative spatial one?

AR: We agree with the reviewer's remark on the link to spatial coupling. The negative spatial coupling was probably mentioned there by a mistake. We also plan to elaborate more on the section 5.2 (rainfall persistence) and place it out of the main results story line to a discussion section.

REV#1 P15, L1: what's the explanation for this conclusion on predictability?

AR: The assumption on predictability is based on the identified negative spatial SMPC and on the conclusions made in the Section 5.2. The positive temporal SMPC in wet climates is likely to reflect rainfall persistence linked to persistence in synoptic situations. Hence, it can be expected to provide some predictability to rainfall. In the semi-arid African region, negative temporal coupling is tightly linked to the drying of the soil in time. Therefore, it is unlikely that temporal SMPC alone provides any information on the future rainfall as the soils experience drying cycle all the time. Yet, the identified significant negative spatial SMPC hints on a possibility that next rain will happen in the vicinity of the previous, therefore providing some predictability potential for rain.

REV#1 P15, L3-4: ‘supports the relevance’ I don’t understand this sentence. As far as I can tell all of this could be explained just with the spatial relationship.

AR: Indeed, the spatial relationship alone would be consistent with the mechanism of “breeze-like” circulations. Yet, in combination with a positive temporal coupling in the conditions of the Sahel, formation of local circulations in our understanding would be less likely. Positive temporal coupling in the Sahel environment typically means that it rained 1, max 2 days ago, and the soil (in Lmax location) is wet. Hence, a smaller spatial negative gradients in soil moisture and a higher (lower) moisture (heat) flux can be expected. This altogether would theoretically lead to an additional cooling, less vigorous updrafts, and hence a decreased likelihood to form thermal rolls. The combination with the temporally drier soils (i.e. negative temporal SMPC) in Lmax location is on the contrary expected to result in a higher buoyancy flux, stronger spatial gradients and hence facilitate likelihood of breeze-like circulations. In general, the reviewer’s remark is a valid and an open research question, which can be addressed in the future. Following existing model experiments (Avissar&Schmidt 1998), higher mean sensible heat flux conditions would also require stronger spatial gradients to form thermal rolls.

REV#1 P15, L7: wouldn’t a positive temporal and negative spatial relationship maximize the moisture flux?

AR: We would not think it would be the case for the studied semi-arid region because of the argumentations brought above. For wetter latitudes, the results from the paper of e.g. Taylor et al., 2015 also support higher likelihood of “breeze-like” circulation mechanism in the conditions of less antecedent rainfall so that the soil moisture limited regime can be archived.

REV#1 P15, L29-32: I don’t understand this point. The reason for filtering water bodies and topography is to isolate the role of soil moisture, because it is very likely that water/mountains are much stronger controls.

AR: It seems that the sentence was not well formulated as it caused confusion. We have edited it, and removed the reference to orography. Overall, main point here was about the gaps in the filtering of water bodies. The water body mask in the present method is static and does not take into account variability in flood plains between and within years. That is also the main reason why we identify a prominent link of the rainfall and extreme soil moisture gradients to the location of wetlands. In the future, application of existing dynamical wetland products like the one from C. Prigent (https://lerma.obspm.fr/spip.php?article91&lang=en) may be used to eliminate the effect of water bodies on moist convection development.

Figures REV#1 Figure 1: I don’t understand how/why you regrid the wind data to a finer grid. In any case, you only plot the streamlines for every âĹij5 âŮę , so this seems redundant. I suggest you delete the last line. L2: change ‘indicates’ to ‘shows’. -L3: state the longitudes for the zonal mean. Change ‘rectangular’ to ‘rectangle’.

AR: ERA-Interim wind data was re-gridded using bilinear interpolation method of the CDOs (Climate Data Operators) tool to keep consistency to observational data sets applied in the study. We agree with the reviewer, that for the purpose of this plot the re-griding step could have been omitted. As follows, the above suggested wording changes have been implemented.

REV#1 Figure 3: the ‘golden shading’ is very hard to see. I suggest you replace it with stippling. Also, is it necessary for panel a for the contours to go up to 3000? A smaller range would highlight more detail.

AR: The figures were replotted and a mask presentation was improved.

REV#1 Figure 4: ‘rectangular’ to ‘rectangle’.

AR: The word was corrected everywhere throughout the text. Thank you for the careful review.

REV#1 Figure 5: This plot is not very clear, as it’s very hard to match specific runs to the symbol (as there are so many, and they are so small). My suggestion would be to move this information into a table. Potentially, you could also include a box and whisker plot, with one box and whisker for T12, one for G15, a dot for your study, and potentially a dot for T12 TRMM/merged and G15 TRMM/GLEAM (as these are the closest set of observations to what you use). I think this would give a better overview of how your results compare with the literature.

AR: Following both reviewers suggestions, the figure was replotted in a more clear manner, as well as the data from the figure was additionally summarized in the Table A1. The Table A1 was placed to the appendix section for the moment.

REV#1 Figure 6: see my comments on the wetlands above regarding panel a.

AR: Additional analyses will be done to identify potential wetland locations (see in the above comments on wetlands), and the Fig 6b presenting an extreme gradients will be updated and improved. The Fig 6a will be removed.

REV#1 Figure 9: it is not clear from the caption what is the difference between panels a and b?

AR: The caption text was changed and is hopefully more clear now. In general, the panel (a) shows soil moisture anomaly values averaged over 1x1 deg boxes, while panel (b) indicates the departure of these averaged anomaly values from their typical (non-event) conditions.

REV#1 Figure 11: provide a bit more detail on what you are showing in the caption (e.g. Se). x axis is time, not daily rainfall (as far as I can tell), and you should give some measure of what timescales you are showing. Y axis is both soil moisture and rainfall. ‘rainfall sums’ over what period? In the bottom panel, are these many short rainfall events, or persistent rainfall (it looks like the former as it is presented)?.

AR: The figure will be modified following the reviewer’s comments

REV#1 Figure 12: I like the idea of including a conceptual diagram, but at the moment I don’t really follow its logic (particularly the drawing on the left). It doesn’t really explain the coexistence of the two mechanisms either; in the second step (‘soil dries out in A’), presumably you return back to where you were in step 1 before it rains (it won’t get drier than when it is dry), so why do you get ‘stronger than usual SM gradient’?

AR: We thank the reviewer for questioning some of the proposed concepts. The figure was improved now, and it will additionally be elaborated following the modification to the Section 5.2 (persistence).

Editorial REV#1 P2, L15. Delete ‘recognized’. P2, L18: change ‘subtle’ to ‘less clear’. P3, L4: ‘anomaly’ to ‘anomalies’ P4, L3: change ‘inset rectangular’ to ‘dashed rectangle’. P4, L25: ‘into’ to ‘in’ P4, L27: ‘few studies HAVE’. P5, L1: ‘still JUST A few centimetres’ P9, footnote: ‘statistics’ to ‘statistic’ (or ‘is’ to ‘are’) P9, L1-2: ‘does not exclude or even favour higher’ to ‘is not expected to affect the’ (if I understand it correctly). P9, L9: ‘coherence’ to ‘agreement’. P11, L23: ‘AN additional area’ P11, L30: ‘increase in THE amount. AR: All correction were implemented.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-530/hess-2017-530-AC1-supplement.pdf

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