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Response to reviewer comments on "Soil moisture – precipitation coupling: Observations from the Oklahoma Mesonet and underlying physical mechanisms"

Response to comments, Dr. Gentine

We thank Dr. Gentine for his comments and suggestions. As recommended, we have made substantial revisions to our introduction, results, and discussion sections in order to compare and contrast our findings with previous studies. We outline our responses below:

1) ***Pg. 3208 lines 1-3: what is missing in this discussion is the stability and dryness of the atmosphere see (effect of surface vs. entrainment) Ek and Holtslag (2004), Westra et al. (2012), Huang and Margulis (2011), Gentine et al. (2013),***

We have rewritten this section (lines 51 – 75) to include a discussion of the stability and dryness of the atmosphere and review the results of the previous studies that you recommended.

2) ***Pg. 3208 line 4: Moist convection is generated not deep, you should mention shallow convection cumuli – the sentence with less CAPE is not always true, ex: Sahel***

This section (lines 64 – 75) has been rewritten to reflect your comments.

3) ***Pg. 3208 line 27: There are also observations of no feedback in the region (Phillips and Klein, 2014). You might want to mention observations in the region and different results obtained (Findell and Eltahir, 2003).***

This section (lines 87 – 102) has been rewritten to better summarize previous studies that focused on this region. We now mention the observations in the region and the results of past studies.

4) ***You mention the Guillod et al (2014), which evaluated Findell et al (2011), but the sentence says that soil moisture changes EF, isn't it obvious? Maybe this is not what you meant. I would reformulate. Also you should define EF. I would change this with a chronology of results (and mention the datasets since it is useful for your later points).***

This section (lines 87 – 102) has been rewritten. We include a summary of several of these past studies, including the datasets used.

5) ***Pg. 3209 lines 8-10: You might want to talk about atmospheric stability in the region and the role of the LLJ. Also mention mesoscale convective systems and squall lines in the region. You talk about it later but I would change that.***

This section (lines 103 – 118) includes a brief summary of the LLJ and MCSs that can complicate any soil moisture feedback signal. However, we have kept the majority of the discussion of synoptic-scale features in the precipitation identification section because it directly pertains to our decision to focus on identifying unorganized convective systems.

6) ***Pg. 3210 line 10: Mesonet are high quality: reformulate***

This sentence has been rewritten to improve clarity.

7) ***Section 2.2: for the majority of rain you should mention MCS***

This sentence has been revised to mention MCSs as suggested.

8) ***Pg. 3211 lines 5-6: You should definitely mention Phillips and Klein***

Thanks for the suggestion. Their results are very applicable to ours in this case. We have added a discussion of Phillips and Klein (2014) in this section (lines 146 – 148).

9) ***Pg. 3211 line 21: You might want to mention the work of Guillod et al (2014) who also used NEXRAD in the US***

Thanks again for the suggestion. We have cited the Guillod paper here (line 174).

10) ***Pg. 3214 line 10: You should add a word on which parcel you use for CAPE and CIN definition.***

We used the non-virtual surface parcel to calculate CAPE and CIN, we now mention this in the section.

11) ***Pg. 3214 lines 13-14: You might want to talk about Findell and Eltahir (2003) analysis with radiosounding and CTP-HI framework. You talk about it later. Also Aires (2014) and Guillod (2014).***

Because we have a section (3.4, lines 383 – 442) dedicated to replicating the CTP-HI framework of Findell and Eltahir (2003), we feel it is most appropriate to discuss their work there.

12) ***Pg. 3215 lines 16-17: Could be interesting to compare with Findell et al (2011), Guillod et al (2014, 2015)***

We have added a discussion of the Findell and Guillod papers. The differences between our results and theirs highlight the importance of EF datasets.

13) ***Pg. 3215 line 23: Some of that is described in Guillod et al (2014)***

Thanks for the suggestion, we have added the Guillod *et al*. (2014) paper here (line 283).

14) ***End of section 3.1: Would be interesting to link that to the stability and humidity of the atmosphere, as discussed in Huang and Margulis, and what was called thermodynamic vs dynamic advantage in Gentine et al (2013)***

Thank you for the suggestions. We have linked our findings to those of Gentine *et al.* (2013) and Huang and Margulis (2011) in this section (liens 273 – 276).

15) ***Section 3.2: Might want to cite Avissar and Pielke***

Thanks for the suggestion, we now cite this important paper (line 301).

16) ***Pg. 3220 lines 9-11: Yes see discussion in Gentine et al (2013) and the role of slope of theta, actually in this framework you should be able to differentiate early morning and surface conditions. In fact it would be really interesting to see if you fall in the same delineations of regimes as the one described in Gentine et al (2013) in terms of atmospheric stability and soil moisture/EF. Given all the data you have that should be straightforward analysis but very interesting to see if there is clear delimitation of the phase space.***

Thanks for the suggestion. We have integrated our results within the framework proposed by Gentine *et al.* (2013) throughout the manuscript. We have also included the analysis suggested by Dr. Gentine (lines 501 – 514), and replicate Fig. 5 from Gentine *et al.* (2013) using the 17 convective events over Lamont (now Figure 11).

17) ***Pg. 3220 lines 23-25: Higher CTP is the same as lower stability in the free troposphere Huang and Margulis (2011)***

Thanks for the suggestion. We have provided a clearer explanation in this section, and cited the Huang and Margulis paper (line 424).

18) ***Pg. 3222 line 5: Yes described as well in Huang and Gentine et al (2013)***

We now describe the thermodynamic effect of the deep boundary layer in this section (lines 461-479). This addition better situates our results in the context of the previous studies

19) ***Pg. 3222 lines 11-12: Over strong stability direct moistening by increased EF and ET is the reason for the increased MSE, might want to make that more explicit***

Thanks for the suggestion. As noted above, we have added a description of the dynamic effect and its application to our results (lines 471-472).

20) ***Pg. 3223 line 5: yes discussed in Ek and Holtslag (2004) and Gentine et al (2013)***

We now cite both of these papers in this sentence (line 494).

21) ***Pg. 3224 lines 4-5: With strong stability***

Corrected as suggested.

Thank you Dr. Gentine for your helpful comments and suggestions!

Response to comments, Anonymous Referee #2

1) ***Introduction: The introduction is brief and could include a little more background. For instance, soil moisture-precipitation interactions processes via mesoscale circulations (e.g., Taylor et al., 2011) are not mentioned. Also relevant could be the recent paper by Guillod et al (2015), which compares spatial and temporal perspectives on soil moisture-precipitation coupling. Interestingly, Oklahoma is a region that displays negative temporal relationships in that Guillod et al. study (compared to overall positive values elsewhere), which is consistent with that study's results.***

Thank you for the reference suggestions. In response to this comment, and those from Dr. Gentine, we have rewritten the Introduction section to provide a more comprehensive review of the literature and to place our study in context.

2) ***Pg. 3209, line 1-3: Although I agree with this sentence, the editor's comment on this highlights the need to clarify (by explaining what, other than soil moisture, can induce a relationship between the evaporative fraction and precipitation; I think atmospheric controls on evaporation, through potential evaporation, is what Guillod et al. 2014 have mentioned, is that what the authors had in mind)?***

We have rewritten this section (lines 87 – 102) to better clarify our explanation of soil moisture connections with EF and precipitation.

3) ***Pg. 3210, line 14: "for a given month" leaves open whether this includes all years or is done separately for each year. Change, e.g., to "for a given calendar month".***

This change has been made as suggested.

4) ***Section 2.1 and results section: it is often unclear which depths (soil moisture data) are used in the analysis, apart from Fig. 3. Is always surface (5 cm) soil moisture used? If so, this can be made clear in Section 2.1. Otherwise, it would be useful to state this clearly in the figures and in the respective results/discussion sections.***

Thanks for the suggestion. We now include a sentence at the end of the soil moisture data section (lines 139 – 140) that more clearly states the 5 cm soil moisture is used for all analyses.

5) ***Pg. 3211: Guillod et al (2015) have also applied the same event detection methodology.***

We have added a citation of the Guillod *et al.* (2015) paper when describing the CMORPH event detection methods (line 174).

6) ***Section 2.2: It is not always easy to describe a methodology which involves manual identification. I think that the authors have done a very good job here.***

Thank you!

7) ***Pg. 3215, line 3-4: Confusing sentence, please reformulate***

This sentence (lines 258 – 261) has been rewritten to improve clarity.

8) ***Pg. 3216 lines 12-19: The example (98%, 2%) and the actual result (99%, 1%) are very similar and can lead to confusion. This paragraph could be a bit clearer.***

We agree that this paragraph was confusing. We have rewritten it and deleted the theoretical example because it detracted from our results (lines 268 – 273).

9) ***Pg. 3216, line 8: "Wang et al., in review". This reference is not listed in the references at the end. Does HESS accept references to work in review?***

This is a good question. We could not determine whether HESS accepts citations for papers in review. We have added the Wang *et al.* work to the list of references, and would like to keep the citation. However, if HESS does not accept references to papers under review, we will remove it.

10) ***Section 3.2: The methodology used here (distribution of nearest neighbor distances) is in fact very simple, but its description is a bit difficult to follow. A few additional details on how this analysis was conducted would facilitate a quick understanding and interpretation from the readers.***

This methodology is more difficult to describe than to implement. We have rewritten the description to provide additional details of the analysis (lines 303 – 314).

11) ***Pg. 3217, line 15,17: The panels are top/bottom, not left/right. Moreover, it would be easier and clearer to use the subfigure labels.***

Thanks for the suggestion. We now use subfigure labels in the text description of Figure 5 (lines 326 – 331).

12) ***Section 3.3: This section does not clearly mention that the fact that wet soil events tend to occur over predominantly on wet years (and vice-versa) can be expected from the impact of precipitation on soil moisture alone: On wet years, soil will be overall wetter than on dry years, leading to a higher fraction of events to occur over wet soils even in the absence of a causal role of soil moisture in triggering these events (simply because soils are generally wet that year). For example on page 3218, lines 1-6, this seems obvious: wetter years (high precipitation total) will have wetter soils and therefore more wet soil events. This analysis is interesting, but the paper will gain in quality if the authors can make clear that this might simply depict the impact of precipitation on soil moisture, rather than that of soil moisture on precipitation. This also applied to other parts of section 3.3***

You raise a good point. We have rewritten section 3.3 (lines 333 – 381) to include a discussion of how precipitation influences soil moisture. This is an important aspect of the findings presented in this section, as we would expect wet soil events to occur more often when soils are homogenously wet, and vice-versa, without any soil moisture feedback.

13) ***Pg. 3218, line 6-9: Is this simply because of the selection of events? I.e., perhaps wet versus dry years mostly differ in terms of frontal precipitation, or organized convection rather than unorganized convection? This would also support my previous comments.***

Yes, good point. We do expect these results are due to our selection of unorganized convective events. We include a sentence making this clear in this section now (lines 371 – 374).

14) ***Pg. 3220, line 2: "HI" was termed "HIlow" in Findell and Eltahir (2003). This could be confusing.***

Thanks for the suggestion. We have changed HI to HI_{low} in all text, figure captions, and figures.

15) ***Pg. 3221, line 14-15: "convective temperature" should be defined.***

We have added a definition of convective temperature in this section (lines 447 – 449).

16) ***Pg. 3221, line 20: The authors have defined CIN as being negative, which makes the discussion confusing, and sometimes not strictly correct (e.g., while the absolute value of CIN is indeed smaller for clusters 1 2, the actual value is larger). See also, e.g., p. 3222 line 25-26, which highlights that the chosen negative sign of CIN render the discussion difficult (if a decrease in CIN is a decrease in stability, then shouldn't CIN be defined positively?).***

Thanks for the suggestion. We now define CIN as a positive value. This makes it easier to interpret the results and figures.

17) ***Pg. 3222, line 26-28: This sentence is confusing***

This sentence has been rephrased to make it clearer (lines 481 – 487).

18) ***Pg. 3223, line 15 "total volumetric precipitation (mm)". How is this defined and computed? Why does a volumetric variable exhibit linear units (mm)? Does it relate to average event accumulation, event size, …?***

Total volumetric precipitation is the same as total event accumulation, which is a function of event size, intensity, and duration. We now refer to precipitation only as "total accumulation" to make this clear (line 517).

19) ***Figure 5: Highlighting Oklahoma on the maps would be helpful to non-US readers.***

We have added an Oklahoma label on the maps.

20) ***Figure 8: Mention that CTP and HI are taken at 6 LST in the caption***

This addition has been made.

Thank you to Reviewer 2 for your helpful comments and suggestions.

Response to comments, Anonymous Referee #3

1) ***Page 3210, Line 10 to 20: does this method for soil moisture conditions and anomalies only take into account the "local" temporal variability? What if the convective precipitation is caused by the spatial heterogeneity in surface fluxes linked with soil moisture? Does this method lead to smoothing of spatial variability? Is it possible that events with drier soils are from spatially relative wetter regions (compared with their neighboring regions) in a drier year, or vice versa?***

Using soil moisture percentiles from individual stations does only take into account the "local" temporal variability, and cannot account for spatial heterogeneity of soil moisture or surface fluxes. We have added this caveat in the data section describing soil moisture (lines 137 – 139). Aggregating point-based measurements to a grid may smooth spatial variability; however, the majority of grid cells contain only 1 observing station. It is possible that events with drier soils are from spatially relative wetter regions, or vice versa.

2) ***In the decision tree of precipitation events, the minimum event size is 6 km by 6 km. The precipitation data are at 4 km resolution. Then how is the event size determined? E.g. continuously adjacent data points?***

The 6 km x 6 km was adopted from Gallus *et al.* (2008); however because the precipitation resolution was 4 km, we mandated the system size be at least 2 x 2 grid cells, meaning 8 km x 8 km. We have since updated Figure 1 reflecting the more accurate description of the event identification.

3) ***Page 3213, line 13: why 3 a.m. was chosen for the cut-off time? The average diurnal cycle of precipitation observed during warm season at ARM SGP site shows its primary peak around 3 a.m., which means there are significant portion of rain events passing by during the nighttime before 3 a.m.***

We selected the time 3 a.m. as our cut-off based on similar methods by Taylor *et al.* (2011; 2012) when identifying precipitation events. This time is chosen in an attempt to eliminate (as best as possible) the confounding effects of morning precipitation on surface-boundary layer interactions. It is true a large portion of precipitation falls over the SGP region during the nighttime; however for this study we focus on isolating a signal of soil moisture feedback on afternoon precipitation.

4) ***Still related to 3 in above, what if there is precipitation accumulation less than 3 mm before 9 a.m.? How would you define your initiation time and location then? Will such data be used to relate with daily soil moisture at 9 a.m.?***

The 3 mm cut-off was also adopted from Taylor *et al.* (2011, 2012) to account for the confounding factor of morning precipitation. Events occurring before 1200 LST are not included in the analysis, as our focus in on afternoon precipitation. Therefore, we would not define an initiation time and location for any precipitation event before noon.

5) ***How much perturbation will be observed in soil moisture roughly if there is 3 mm accumulated precipitation?***

This is quite difficult to estimate, as the efficiency of soil moisture recharge is dependent on overlying vegetation density, soil texture and organic matter, and the rate or intensity of precipitation. If we were to assume all 3 mm of precipitation were to infiltrate the top 10 cm soil layer, this would increase the volumetric water content of the layer by 3% (i.e., 25% to 28%).

6) ***Is there a precipitation rate limit, e.g. 0 or 3 mm/hour, for you to determine precipitation initiation?***

One of the decisions in the event identification scheme is if the event precipitation rate was at least 3 mm/hour. Any grid cells showing precipitation rates less than that were not included in the event identification.

7) ***On Page 3223, Line 19 to 20. It is NOT the PBL growth that leads to shorter and smaller events. As you have shown on Page 3220 Line 20 to 25, the atmospheric moisture is also low over drier soil cases. It is indeed the atmospheric lower RH results in shorter and smaller events. PBL growth larger due to delayed cloud development with lower RH leading to higher lifting condensation level (LCL).***

This is a good point, we have updated this sentence to accurately reflect the impact of decreased relative humidity and not necessarily PBL growth resulting in shorter and smaller events (lines 521 – 523).

8) ***Page 3208, Line 15: put "." Between U.S. and "These"***

This correction has been made.

Thank you to Reviewer 3 for your helpful comments and suggestions.