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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Soil moisture–precipitation coupling: observations from the Oklahoma Mesonet and underlying physical mechanisms" *by* T. W. Ford et al.

Anonymous Referee #2

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General comments:

This study examines the relationship between soil moisture and the initiation of convection in Oklahoma, using in-situ soil moisture and radar precipitation data. The authors find that precipitation is initiated most frequently over dry soils, and its location is not related to land use and land cover. The dry/wet event ratios are also analysed per year and month, highlighting large year-to-year variability. The authors then focus on a subset of events where atmospheric soundings are available, and can identify different conditions for wet versus dry events as well as contrasting impacts on convective indices.

Overall, this study is of high quality, and the manuscript is enjoyable to read. The authors deserve credit for having kept the length of the manuscript reasonable, and for having presented most results in a clear way. I have listed my few comments below. A major point is that the authors do not discuss the possible role of atmospheric persistence. Indeed, Taylor et al. (2012) compare precipitation at two locations (rainfall maximum vs minimum) in order to mitigate the impact of different atmospheric conditions. In this study, by simply considering soil moisture at the location of initiation, differences (e.g., in dry vs wet soil events) might in part be due to large-scale synoptic conditions rather than local soil moisture. This should be at least mentioned, in particular in Section 3.3 (see also comment below). In spite of this issue, the clean analysis, in particular with respect to the identification of unorganized convection events, might avoid some issues in other, more general analyses - a strong aspect of this manuscript.

Specific comments:

- Introduction: The introduction is brief and could include a little more background. For instance, soil moisture-precipitation interaction 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.
- Page 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?).
- Page 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".

- Section 2.1 and results sections: it is often unclear which depths (soil moisture data) are used in the analysis, apart from Fig. 3. Is always surface (5cm) 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.

- Page 3211, line 12 (also page 3213, line 10-11): Guillod et al. (2015) have also applied the same event detection methodology.

- 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.

- Page 3215, line 3-4: Confusing sentence, please reformulate.

- Page 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.

- Page 3216, line 8: "Wang et al., in review" (see also page 3224, line 19). This reference is not listed in the references at the end. Does HESS accept references to work in review?

- Section 3.2: The methodology used here (distribution of nearest neighbour 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.

- Page 3217, line 15 17: The panels are top/bottom, not left/right. Moreover, it would be easier and clearer to use the subfigure labels (Fig. 5a, Fig. 5b).

- 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 (i.e., with high precipitation totals, due to external factors such as sea surface temperatures), 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.

- Page 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.

- Results from section 3.4 are very nice. These could be summarized in the abstract as well.

- Page 3220, line 2 (and other instance): "HI" was termed "HI_{low}" in Findell and Eltahir (2003). This could be confusing.

- Page 3221, line14-15: "convective temperature" should be defined.

- Page 3221, line 20 (and other instances): 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?).

- Page 3222, line 26-28: this sentence is confusing (see also above comment on the sign of CIN).
- Page 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, . . .?
- Figure 5: Highlighting Oklahoma on the maps would be helpful to non-US readers.
- Figure 8: Mention that CTP and HI are taken at 6 LST in the caption.

References:

- Ford, T. W., Rapp, A. D., and Quiring, S. M.: Does afternoon precipitation occur preferentially over dry or wet soils in Oklahoma?, J. Hydrometeorol., doi:10.1175/JHM-D-14-0005.1, in press, 2015.
- Guillod, B.P., Orlowsky, B., Miralles, D.G., Teuling, A.J., and Seneviratne, S.I.: Reconciling spatial and temporal soil moisture effects on afternoon rainfall. Nature Communications, 6, 6443, doi:10.1038/ncomms7443, 2015.
- Taylor, C. M. et al.: Frequency of Sahelian storm initiation enhanced over mesoscale soil-moisture patterns. Nature Geosci. 4, 1–4, doi:, 10.1038/ngeo1173, 2011.
- Taylor, C. M., de Jeu, R. A., Guichard, F., Harris, P. P., and Dorigo, W. A.: Afternoon rain more likely over drier soils, Nature, 489, 423–426, doi:10.1038/nature11377, 2012.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3205, 2015.