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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Land surface controls on afternoon precipitation diagnosed from observational data: uncertainties, confounding factors and the possible role of vegetation interception" *by* B. P. Guillod et al.

Anonymous Referee #2

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This study brings forward some important new analysis of the topic of land-precipitation coupling in the US. The starting point is the study published by Findell and colleagues in Nature Geoscience in 2011, which used reanalysis data to identify a strong role for land surface fluxes in the triggering of convective rain across large regions of the US, particularly in the east. In the current work, the authors use independent observational data to explore the sensitivity of the original results to the choice of dataset. To examine the behaviour of the surface flux partition between sensible and latent heat, they use both site-based flux measurements from 39 sites across the US and Canada, and

outputs from a simple global land evaporation model driven by remote sensing. They also explore the impact of the precipitation dataset on the results.

The authors find that major uncertainties arise in the calculation of the "Triggering Feedback Strength" (TFS) from the different surface flux datasets. They go on to identify a strong rainfall persistence effect in the Eastern US. This result suggests that the TFS signal detected in the original study might primarily be a manifestation of atmospheric persistence on a daily time scale. This raises questions about the original interpretation of the signal as being driven by the land surface.

The authors have taken on an important and challenging topic, and deserve great credit for comprehensively exploring so many avenues in the datasets. I think this study provides new insight into tackling the land coupling problem, and the work needs to be published. However, I have several concerns and suggestions which the authors need to address before the work is publishable in a full journal.

Major concerns

1. Although the material was very interesting and many sections were well-written, overall I found the paper very hard to read. I think the discussion and conclusions section is 2 pages too long and, along with the abstract, needs to present a clearer description of the results and their implications. For example, the abstract states "we find that much of these relationships can be explained by precipitation persistence alone, with ambiguous results on the additional role of EF in causing afternoon precipitation." Can the authors not spell out the implications? I recognise that there are many nuances associated with using observations in the way the authors have, and as a result, conclusions must be expressed with an appropriate level of caution. All the same, I think this work will reach a wider audience (and deservedly so) if the authors are able to make it more coherent.

2. From what I understand, GLEAM is a simple model of evaporation driven largely by remotely-sensed data (precipitation, soil moisture, microwave vegetation optical depth,

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radiation). The observations used in GLEAM are only indirectly related to evaporative fraction. The assumptions made to compute the evaporative stress S are similar to some of the assumptions which underpin more complex land surface models. Whilst GLEAM provides a useful independent dataset for the purposes of this paper, its description as a "remote sensing product" is a bit misleading. It is a simple model driven by remote sensing data.

3. I'm not clear about the representation of interception in the GLEAM product described here. It has a daily time step, and precipitation from 0900 (local time) on the previous day to 0900 on the day of interest. Is the depiction of interception (and the modified stress factor in equation 3) sensitive to the timing of the rain event within that 24 hours? It certainly should be, given the short time-scale of re-evaporation during summer daytime conditions. Similarly, in the assimilation of the overnight soil moisture data in the model, is any account taken of whether the prescribed rain has occurred before or after the satellite overpass?

4. The authors assume that monthly signals (within the June to August window) do not affect the results. (Page 29159 "Precipitation persistence might also arise from seasonality in precipitation; however, this effect is less relevant for our study as only summer is considered"). I wonder if this is really true for the North American Monsoon region. The stations in the South-West of the US (which are highlighted in the conclusions and abstract) will usually be extremely dry in June, with little evapo-transpiration (see Scott et al references in the paper). This contrasts strongly with much wetter August conditions. Can low frequency atmospheric variability (both the seasonal cycle and intraseasonal variability) explain "the existence of significant relationships between EF and convective triggering" in all the datasets?

Minor comments

Page 29139

1. Line 18 "Investigating these differences... these relationships". It's not clear which

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differences and relationships are being discussed

2. There are several very long sentences with multiple clauses. These sentences need to be rewritten for clarity (e.g lines 21-26, Page 29151 lines 8-11, page 29155 lines 19-24)

3. lines 27-8. This is written assuming that the statistical relationship implies causation ("the impact of EF on convection triggering")

Page 29141

4. line 15 "in most cases" Is this phrase needed?

5. line 26 I don't recall Seneviratne 2010 including cloud-resolving simulations

Page 29143

6. line 20: "we quantify the effect of. . ." Effect or correlation? This sounds like causation

7. Section 2.1 on NARR. I'm unclear whether NARR assimilates screen-level temperature and humidity to constrain surface fluxes, or not.

Page 29145

8. line 3 "and remain thus tied to the more vigorous model-induced water cycle" I don't understand this phrase.

9. line 16 "data are"

Page 29147

10. line 5 "robust" or "similar"?

Page 29148

11. line 10: "between 1:30 and 6am depending on satellite". This study uses AMSR-E, so presumably the overpass time is closer to 1:30.

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Page 29152

12. “i.e. no possible overlap even if EF_Q60-EF_Q40”. How could these quantiles be identical?

13. line 14 I’m not sure I agree that cloud-free mornings are an indication of potentially convective days

Page 29155

14. Sub-section beginning line 25. The authors should consider the impact of this lengthy discussion (at this point) on the readability of the paper. I’d suggest cutting it down and avoiding any repetition with sections 4.2 and 7. Also I don’t really understand what is meant by “different coupling behaviour” (point ii).

15. Figure 5 Can the authors indicate which locations have a significant correlation at a level of 95 or 99%?

Page 29161

16. lines 12-14 “Nonetheless, for days following rain-free days the clear weakening of the signal suggests a possible strong role of precipitation persistence” Is the “possible” necessary given the “suggests”?

17. Line 14-16 “In addition. . .” I don’t understand this sentence.

18. Line 19-20 “Over the Southwestern US, the signal is less sensitive to precipitation persistence and TFS_ remains significant over most sites for both datasets.” Is this remaining signal robust when considering the sub-seasonal variability at these North American Monsoon sites (see above)?

19. Lines 26-28 “Conversely. . .” Repetition.

Page 29163

20. lines 5-6 “. . .it is of high relevance for the results presented here” I’m not so sure

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how high the relevance of what follows is, and wonder if it could be shortened.

Page 29164

21. line 5: Can this substantial part of the signal be quantified?

22. Line 6: I find this dominant control on EF via EF_pot confusing. Is it also possible that the temperature and humidity of the airmass (presumably the factors responsible for changing the partition between sensible and latent heat) are a proxy for the likelihood of the airmass to produce rain that day, irrespective of surface fluxes?

23. Line 22 “observed coupling” What is meant here? Firstly, GLEAM is not observing EF, and secondly, you have already shown how significant relationships can emerge without causation.

24. Line 29 “entrainment” is introduced here without explanation. Is it needed?

Page 29165

25. lines 1-3. “In the Central and Southwestern US, soil moisture (surface and root zone) drives the relationship, suggesting the likely occurrence of a soil moisture-precipitation feedback.” I think that final phrase (“suggesting the likely occurrence”) is out of keeping with the cautious tone of the rest of the paper.

Page 29167

26. lines 4-7: “Hence, the confounding impacts of precipitation on soil moisture and EF may preclude conclusions on the existence of a land-precipitation coupling in this region, as precipitation persistence could either be induced by a coupling or reflect the impact of large-scale forcings.” I agree that atmospheric persistence makes it very difficult to isolate a surface feedback effect. However, I disagree with the authors in their suggestion that the coupling could be so strong that the land (rather than the atmosphere) induces precipitation persistence in this case.

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