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Interactive Comment

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.

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Reply to the review by Anonymous Referee #2

We thank anonymous referee #2 for the review, which usefully points to aspects in the manuscripts for which improvement is required. These suggestions are highly appreciated. Answers to the comments are included in blue font right under the unmodified comments from the review.

REVIEW

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

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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.

We appreciate the overall positive tone of the reviewer's comments as well as the relevant points raised, for which we mention our intentions for the revised manuscript.

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.
   Thanks for this good point. We are aware that the manuscript is rather long. Following the reviewer's suggestions below we hope that it will improve the readability and we tried to be more concise. We also spell out the implications in the abstract. We are working in the direction of condensing the content without compromising the integrity of the message.

2. From what I understand, GLEAM is a simple model of evaporation driven largely

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by remotely-sensed data (precipitation, soil moisture, microwave vegetation optical depth, 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.

We agree with the referee. Evaporation cannot be directly observed with satellite sensors, because it does not have a clear direct impact on the surface emission or reflection of radiation (e.g. McCabe et al., 2013).

GLEAM is the satellite data-driven ET model that uses a wider range of satellite information than other ET products, partly due to its Priestley and Taylor formulation, which appears well suited to the range of satellite observable variables. It is also the only satellite-data driven product of ET that assimilates observations of surface soil moisture, and that estimates the temporal dynamics of root-zone soil moisture based on observations of rainfall and soil moisture, and (observation-driven) estimates of evaporation. Therefore, while it is not assimilating real measurements of evaporation, it does apply a wide range of observations of variables that are critical for evaporation and combines them within traditional equations that are known to derive accurate estimates of evaporation under a wide range of conditions (like the Priestley and Taylor model, or Gash analytical model of interception). We also note that GLEAM estimates of ET have been extensively validated in past years and compared to other methodologies for estimating surface heat fluxes (Mueller et al., 2013; Liu et al., 2013; Miralles et al., 2011a,b;

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Trambauer et al., 2014; Miralles et al., 2014). The method has been run with a wide range of inputs (Miralles et al., 2014), and its error has been characterized using triple collocation (Miralles et al., 2011a). Latent heat flux estimates from GLEAM have been applied to a large number of studies over the past three years (Mueller et al., 2013; Liu et al., 2013; Miralles et al., 2011a; Trambauer et al., 2014; Miralles et al., 2012; Reichle et al., 2011; Fersch and Kunstmann, 2013), including also a study in Nature (Jasechko et al., 2013) and a recent paper in Nature Climate Change (Miralles et al., 2014). Within this latter study, GLEAM was successfully validated using measurements from 163 eddy-covariance stations and 701 soil moisture sensors all across the world (see Supplementary Information of Miralles et al., 2014).

Nevertheless, we will use “observation-driven” rather than “observation-based” to designate GLEAM in the revised manuscript, to emphasize that it comprises a model (like most of the remote sensing-based datasets of climatic and environmental variables).

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?

   These are very good points! The assimilation of soil moisture is done at the end

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of the time step (i.e., end of the day, at 9LT) and thus we assume that precipitation occurs before the night-time soil moisture data. This is reasonable given that rain mostly occurs in the afternoon or evening in summer.

However, for interception, we recognize that the way we include it in GLEAM might not always be appropriate as the timing of the rain event within the 24h does not influence the estimated interception. In fact, because of the fast rates of interception loss and the limited vegetation storage capacity, vegetation will be wet in a summer morning only if it rains shortly before or especially during the morning. We realize that, since rain mostly occurs in the afternoon/evening and since days with morning rainfall (6-12LT) are removed from the analysis as these are likely of synoptic nature, the presence of remaining interception storage at the time of the estimated EF (9-12LT) on analyzed days is very unlikely. With our approach, however, there will be plenty of intercepted water in the morning, independently of when it rained on the previous day, because we did not assume (contrary to the original GLEAM and to related Gash interception model) that vegetation dries out within one time step. In the revisions of the paper, we revise this assumption and thus remove interception from the GLEAM EF estimates as a default methodology (i.e., this default version is like the current right panel of Fig. 11). We then introduce interception estimates in Fig. 11 as a sensitivity test and as a way to compare to the signal from NARR. We are confident that this change provides us with more realistic estimates. Note that, in spite of leading to different TFS in some regions (see Fig. 11), the estimates are very similar independently of what approach is chosen (high correlations with each other), which preclude a thorough evaluation of which of the two approaches is more realistic.

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?
   This is a good point. We cannot use EF anomalies relative to the seasonal cycle due to the short record length of the FLUXNET data, but we did it globally for GLEAM (with CMORPH precipitation in a follow-up work in progress) and did not find strong impacts in North America. Following the referee’s suggestion, we have computed TFS for each month separately (Fig. 1 for GLEAM-NEXRAD, without interception in GLEAM for consistency with the previous comment): some noise appears due to shorter time series in computations for individual months, but overall the region with significant coupling in the Southwest remains based on individual months. See also Berg et al. (2013), who mention that the estimated TFS signal is contributed equally by each month and that seasonal covariability of EF and precipitation appears to play little role. Given the current length of the paper, we would like to avoid adding more material, but we add a sentence about

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this on Page 29159 (L23).

Minor concerns

Page 29139

1. Line 18 “Investigating these differences... these relationships”. It’s not clear which differences and relationships are being discussed
   “These differences” refers to the differences in TFS patterns between the different datasets, and “these relationships” refers to EF-precipitation relationships. We make this sentence clearer in the revised manuscript.

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)
   Ok, thanks a lot, these sentences have been rewritten for clarity as the reviewer suggests.

3. lines 27-8. This is written assuming that the statistical relationship implies causation (“the impact of EF on convection triggering”)
   True. We correct this using e.g. “EF-precipitation relationships”.
   Page 29141

4. line 15 “in most cases” Is this phrase needed?
   True again, we agree that “in most cases” is not needed and have removed it.

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5. line 26 I don't recall Seneviratne 2010 including cloud-resolving simulations
The citation of Seneviratne 2010 refers to the whole sentence (statement that most modelling studies that find negative coupling are single-column models or cloud-resolving simulations). We make this clearer in the revised text.
Page 29143

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

7. Section 2.1 on NARR. I'm unclear whether NARR assimilates screen-level temperature and humidity to constrain surface fluxes, or not.
Thanks for this important point. NARR assimilates screen-level humidity but not temperature. This is added to Section 2.1.
Page 29145

8. line 3 "and remain thus tied to the more vigorous model-induced water cycle" I don't understand this phrase.
This last part of the sentence can be removed. The main point here is that evaporation and moisture convergence are not corrected by the assimilation of precipitation and thus include bias of model (not assimilated) precipitation.

9. line 16 "data are"
Thanks for catching this.
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10. line 5 "robust" or "similar"?

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Similar is indeed better, thanks.

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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.
    This is true, but in fact GLEAM also assimilates soil moisture from SSMI sensors before the AMSR-E era (mid 2002). This was overlooked but it is added here.

Page 29152

12. "i.e. no possible overlap even if E_Q60-EF_Q40". How could these quantiles be identical?
    For instance in the case of interception, when e.g. 70% of the data are 0. In that case all percentiles up to the 70th are equal to 0. This does indeed usually not happen for soil moisture or EF.

13. line 14 I'm not sure I agree that cloud-free mornings are an indication of potentially convective days
    We agree that this criterion may not always be adequate. However, our criterion is not very restrictive (requiring 2/3 of potential radiation) and allows for some clouds in the morning, while removing days which are very cloudy. For instance, a day with full, thick cloud cover (low incoming shortwave radiation) in the morning is likely stratiform and not convection-driven, while an hour of clouds between 9-12LT may be convective but will not be sufficient to exclude a day. Finally, we recall that our analyses are not very sensitive to the choice of criteria for the selection of potentially convective days. Note that, as we have been looking for a simple criterion which can be easily applied at many locations, the unavoidable

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drawback is that it may in some cases not be the most appropriate.

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).
Agreed. We keep this subsection but we try to make it more concise. In particular, the points discussed in depth in Sec. 4.2 or other sections are drastically shortened here. "Different coupling behaviour" referred to a coupling that may be model-dependent, but in fact one may also say that it is eventually the reflection of all other points (EF data, time series lengths/noise, selected days, etc). Therefore this point is removed.

15. Figure 5 Can the authors indicate which locations have a significant correlation at a level of 95 or 99%?
This is a great idea. We will add significance of the correlations on Figure 5, but we prefer the use of a 90% level to stick to the significance level used in the TFS computations.

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"?
True, we remove "possible".

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17. Line 14-16 “In addition...” I don’t understand this sentence.
    This was a minor point and the sentence will be removed in the revised version.

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)?
    See our answer to major comment 4.

19. Lines 26-28 “Conversely. . .” Repetition.
    Thanks for catching this.
    Page 29163

20. lines 5-6 “. . .it is of high relevance for the results presented here” I’m not so sure how high the relevance of what follows is, and wonder if it could be shortened.
    We remove this statement. Nonetheless, we believe these results are highly relevant as they emphasize different possible time scales of the feedback (see also the discussion in the paper). That said, we agree that it could be shortened and we do so.
    Page 29164

21. line 5: Can this substantial part of the signal be quantified?
    We realize that “substantial” might have been a bit too strong there, but it is difficult to quantify due to the non-linear interactions between E_{pot} and the water storage terms in determining EF. We reformulate this sentence in a more cautious tone.

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?
Yes, this is in fact very likely and it was what we meant. We mention that point directly there.

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.
By “observed coupling” we meant observed in the figure, but we realize this was not clear. We replace “observed coupling” with “identified relationship” to avoid confusion.

24. Line 29 “entrainment” is introduced here without explanation. Is it needed?
True, mentioning entrainment without explanation may be confusing. We remove it as it is a possibility among others.
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.
True, thanks. It is replaced by “In the Central and Southwestern US, soil moisture (surface and root zone) drives the relationship, which would be consistent with

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the existence of a positive soil moisture-precipitation feedback".

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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. Good point, the last part of that sentence was a bit overstated. We remove it and keep only the first part of the sentence "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."

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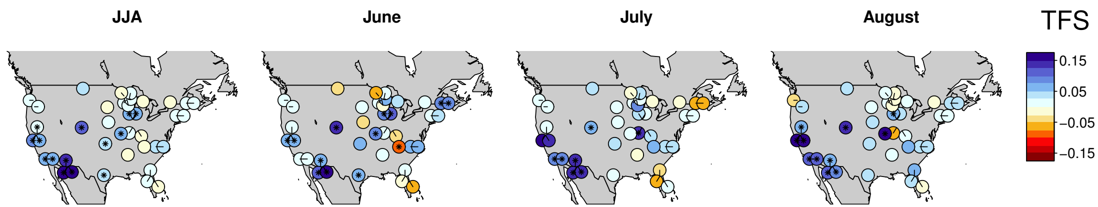

Fig. 1. TFS*(EF) for GLEAM-NEXRAD computed for JJA and individual months. Note the inversed scale compared to the originally submitted paper.

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