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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 #1

We are very grateful to the anonymous referee #1 for the constructive and positive review. We include our answers to the comments in blue font right under the unmodified comments from the review. We note that the line numbers provided by reviewer #1 refer to the originally submitted document and do not correspond to the published discussion manuscripts. When referring to a particular line in our answer here, we provide line numbers for both documents to avoid confusion.

REVIEW

This is a well-written manuscript that achieves its goals of using different observation datasets with the TFS metric to compare against established model-based (NARR) results. More importantly, this work shifts from the results of the TFS to really dig into the confounding issues underlying our current ability to assess the EF-P relationship and causality. As such, this paper highlights some very important issues and limitations for current L-A coupling studies. The confounding issues are many and complex, and there are significant limitations in observing the coupled L-A system in terms of soil moisture, surface fluxes, PBL properties, and precipitation. We cannot rely on models alone, yet are forced to bring more uncertainty when introducing observations with inherent errors and scale limitations.

I am supportive of this paper primarily as a discussion piece to spur community thinking and focus, and recommend publication after minor revisions. Most notably, the length could probably be reduced, as there are some redundancies and wordiness. Much of the discussion in the latter half could be tightened up a bit as well. [Also, please bear with my comments/suggestions below in terms of lit review and citation suggestions. I am quite interested in this topic, and want to make sure the current state of knowledge is presented as best possible to benefit other readers.]

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Thanks for this positive review. We agree that the length should be reduced and in particular we try to shorten parts of the discussions, while making sure to do this not at the expense of required explanations. We also cut back on the introduction - in this case, some of the suggested works to cite might become less relevant.

- L40: Siqueira et al., JHM 2009 also focuses on this negative feedback.
  Thanks for this addition. We will cite it at L70 as suggested below.

- L45: Might also reference the process-chain defined in Santanello et al., JHM 2011. Basically is identical but includes a bit more explicit description of the inherent processes.
  Agreed. We cite this paper there.

- L59: There are a host of studies focused on ‘B’ that the authors might want to at least touch on. For example, the RH-Tendency approach of Ek and Holtslag (2004), and the sensitivity studies of van Heerwaarden (2009), which look explicitly at how the PBL and free-atmosphere processes modulate B.
  Thanks a lot, we add these two citations at the end of that sentence.

- L66: Also, Dirmeyer et al., JGR 2011 and JHM 2012 (modeling studies) define a Land surface Coupling Index (LCI) to assess SM-EF that goes a bit further than simple correlations, and a ’2-legged’ approach to assess the combination of ’A’ and ’B’. Would be nice to make that connection here.
  Good point. We add a sentence and reference to Dirmeyer’s work there.

- L70: Good place for that Siqueira reference here.
  Ok, thanks.

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- L112: How is A addressed in this study without the use of SM observations? Looks like only B is really the focus here since EF and P data are the focus?
  This is correct, the focus is on B but soil moisture is brought in to help interpretation (Section 6). We will adapt this paragraph and mention the use of both EF and soil moisture in NARR.

- L144: Is it really the case that NARR forces the L-A component 'more accurately', if radiation and other components are biased? P is better (assumed so), yes, but the other forcing variables might not be.
  Good point. In fact, NARR was originally designed to better force the land surface in terms of precipitation input, but we acknowledge that the point the reviewer is making is relevant. We include this point in the revised manuscript.

- L156: I know from the abstract that interception will be a focal point of this analysis. Can you say, generally, what is the percentage of evaporation from interception relative to bare soil or transpiration? Is it on the order of 10% of the total (or less), or more significant? I realize it depends on vegetation type and amount, but just to put ballpark figure on it at this stage of the manuscript might be useful.
  Individual fluxes are not provided in the NARR output, only storage terms are available. It is therefore not possible to quantify the percentage of evaporation that is provided by interception in that dataset. Estimates from other studies are provided in the conclusions at L709-710 (L27-29 on p. 29167 in the online published discussion paper) and suggest a global average of over 10%, with 20-50% over forests. We will not include numbers in the NARR description but we propose to include these estimates around L570 (L15 p. 29162), where we think this

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becomes relevant.

- L187: What is the typical measurement error and/or closure for ECOR fluxes? 20% that I've seen cited?
  The sum of H and λE typically underestimates the available energy by 10-30% (e.g. Wilson et al., 2002; Mauder et al., 2006; Hendricks Franssen et al., 2010). We add this there.

- L202: 20km seems quite a large footprint compared to the plot-scale of a flux tower. How representative of the greater area around them are the flux sites themselves (e.g. in terms of land cover)? SM-EF interactions happens locally, but the 'B' processes happen on PBL scales up to 50-100km. Maybe should be discussed.
  Good point, thanks for noticing this. Although the size of this footprint (NEXRAD) is much larger than the footprint of the flux towers, we note that it is comparable to the scale at which EF-P coupling is expected to occur. It is also comparable to the scale of GLEAM (0.25°). FLUXNET measurements, on the other hand, are available at a smaller scale. Usually the land cover around the tower is rather representative of the mesoscale, but this is not always the case. This issue is discussed in Sections 4.1 and 4.2, where we also mention that this may partly explain the different signal found with FLUXNET.

- L227: How does the typical behavior of EF over the daytime factor into these estimates of 9-12am EF? EF typically 'flatlines' at a near constant value in midday (10-2).
  Good point. Values from 9-12LT are used precisely because they are close to

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midday and EF is considered most stable at that time. Moreover, we are most interested in the day-to-day variability of (before-noon) EF, and we use for each day the same "before-noon" hours. Therefore, we are confident that the diurnal cycle of EF does not strongly impact our results.

- L238: G has been shown to be a large (up to 50%) percentage of Rnet in some studies, and G/Rnet is a relationship that has been shown to evolve in time (higher in am than pm during the daytime). Also G/Rnet has been shown to be a function of vegetation amount (LAI) and soil moisture. Are you certain this approach, given the importance of 'before noon' EF, is the right one for estimating G?
  The treatment of G in remote-sensing based evaporation algorithms usually involves crude simplifications of how this flux behaves in nature. G is either neglected by these algorithms (e.g. Fisher et al., 2008) or estimated as a constant function of Rnet and land use type (e.g. Miralles et al., 2011b; Su, 2002; Zhang et al., 2010). GLEAM in particular uses constant G/Rnet values as a function of land cover. While we acknowledge that this is a simplification, we also note that the diurnal variability of G/Rnet is known to be larger than the multi-day changes at the same time of the day (originated for instance by the multi-day variability of soil moisture, see e.g. Kustas and Norman, 1999; Gentine et al., 2011, 2012). Therefore most of the multiday-variability in before-noon available energy will be given by Rnet. In this study we are interested in the before-noon periods only, and GLEAM uses average Rnet/G taken from field campaigns over different land use types corresponding to this time of the day (see Kustas and Daughtry, 1990). Here we want to stick to the original GLEAM methodology as much as possible.

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Also, note that the study focuses on the partitioning between sensible and latent heat flux with the goal of identifying soil moisture stress and thereby the treatment of G of secondary importance relative to λE and H.

- L282: Can you say something about the accuracy/uncertainty and scale of the GLEAM estimates of ET? This product or approach hasn't been validated from what I can tell, and there are a host of uncertainties and even more assumptions introduced in this study (see this whole section above) that make the accuracy of GLEAM highly suspect. Ultimately, the authors are using this as an observationally-derived product, but there is still a model at the core as well.

  Thank your for this comment. The GLEAM estimates of latent heat flux (λE) used in the calculation of EF, have been extensively validated in past years and intercompared to other methodologies to estimate heat fluxes (Mueller et al., 2013; Liu et al., 2013; Miralles et al., 2011a,b; 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).

  We understand that reproducing average patterns of heat fluxes is different from representing morning fluxes accurately. However, we cannot conceive a more deep insight into the uncertainty of GLEAM morning EF estimates than the one obtained by straight comparison to FLUXNET already shown in our study (see Fig. 5 in the discussion paper). The results of this comparison show substantial differences, not just due to errors in GLEAM and its forcing variables, but also due to the discussed uncertainties in EF estimates from eddy-covariance. However, we note that the purpose of our paper is in fact to uncover these disagreements between EF estimates, the pitfalls of TFS calculations and the subsequent uncertainties in current soil moisture-precipitation coupling estimates.

Nonetheless, together with the two previous referee comments, we see that the section describing GLEAM needs clarification. The scale is 0.25° and is mentioned at L207 (L9 p. 29147). We agree that GLEAM is not a direct observation but is "observation-based", hence we will take care not to mention "observational product" but rather refer to it as an "observation-driven product".

- L319: 'daytime'
  Thanks.

- L319: Every day has 'daytime PBL evolution', so convection is the result a particular kind of evolution that allows for LCL/LFC to be reached. . ...?
  Good point, we change this.

- L338: Is convection completely inhibited when it is cloudy (always)? What about an hour of clouds and 2 hours of clear skies (9-12)? Why did you take Findell's screening of P to a more stringent level based on clouds as well?
  Thanks for this comment. We agree that the proxy used here may not always

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be adequate, but in most cases we think it is. Findell et al. (2011) use a second criterion (in addition to no morning precipitation) based on the convective triggering potential (CTP) to select days. We also think that removing days based on morning precipitation alone may not be sufficient and therefore introduce our radiation-based criterion. A day with 1 hour of cloud and 2 hours of clear skies will likely not be removed by our filter due to our threshold based on 2/3 of maximum expected global radiation, and our selection leads to similar results to Findell et al. (2011) (see the Supplement to our discussion paper).

- L358: Will the scales of FLUXNET vs. GLEAM be addressed? One is gridded and dependent on coarse data (GLEAM), the other are point measurements from flux towers. NEXRAD is a 20KM average. Which is the better combination then based on scale alone?
  Good point. Yes, this is addressed in Sections 4.1 and 4.2 (see also answer above).

- L430: With all these confounding factors and uncertainties, how can one make any conclusion on the relative agreement or accuracy of each combined dataset approach? I see later - that is what the rest of the paper tries to untangle! (Disregard comment)
  Thanks!

- L676-on: This is the key conclusion of this work from my perspective. Having EF at the core of any diagnostic is currently problematic, due to the scarcity of suitable observations (even in-situ flux towers have large errors), and representativeness and uncertainty in derived ET products from RA or satellite.

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We agree that this is an important point. In addition, we think that other findings are very relevant as well, in particular with respect to the current lack of information about the role of interception loss in the triggering of afternoon storms in nature as well as the difficulties in statistically isolating the coupling.

- L786: Indeed, MERRA had to conduct a land-only rerun due to issues with canopy interception in the original MERRA product (which employs Catchment LSM). The new MERRA-Land product adjusted an interception parameter and is more accurate, but also highlights that this an issue that offline/coupled and weather/climate models need to pay close attention to.
  Good point! We mention that and cite Reichle et al. (2011). Note that MERRA-Land was validated and benchmarked using GLEAM interception (Reichle et al., 2011).

- L796-on: Very important point!
  Agreed.

- Overall: A point the authors could emphasize is that addressing confounding issues/uncertainties/assumptions is a non-trivial task, and even in-depth investigations of these issues such as those presented here do not always result in definitive conclusions. That does not mean such investigations are not warranted and important to furthering understanding of complex systems.
  Ok, we appreciate the reviewer's perspective and include this in the concluding section.

- Table 1: Might not be necessary to include. The 4 sources of data are easy to

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remember.

Ok, we remove that table.

- Table 2: There are major limitations in AMSR-E soil moisture estimates, the implications of which should be mentioned. Resolution is coarse (50km), and accurate only for top 1cm (max) of soil and for sparse vegetation.

  We assume this refers to Table 3. We agree that there are some limitations, but we note that (i) AMSR-E is available at 0.25° (i.e., the resolution of GLEAM), (ii) the Supplement of Miralles et al. (2014) highlights small improvements brought by the soil moisture data assimilation in GLEAM, given the usual higher quality (and therefore weight in the assimilation) of the rainfall-derived model soil moisture estimates compared to soil moisture observations. In that article, the quality of GLEAM root-zone soil moisture estimates was validated against 701 soil moisture sensors all across the world from the International Soil Moisture Network (Dorigo et al., 2011), showing average correlation coefficients of around 0.7.

- Figure 1: Similar schematics of L-A interactions (with additional processes) are seen in van Heerwarden (GRL 2009) and Santanello et al. (JHM 2007). All three basically highlight the same mechanisms, just want to make sure the authors are aware of them and the context of each.

  Agreed. We prefer to stick to a very simple representation here, see also our reply to 2nd comment, but we mention the work from van Heerwaarden et al. (2009) and Santanello et al. (2007).

- Figs 2-on: The TFS maps that dominate the results of this paper are a bit heard to interpret and ultimately result in a qualitative eye examination of differences. The

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difference plots and the binned (regional) plots that follow are therefore critical and most welcome. I'm not sure if there is a better way to do it overall. Also, I'm wondering what a fully distributed spatial plot of GLEAM-NEXRAD might look like to compare vs. NARR, or likewise the Fluxnet gridded product that was recently released (not sure how well NEXRAD is spatially distributed though). I have no problem with the plots as they are currently presented, just brainstorming on other potential angels as well.

Good point. Ongoing work investigates the coupling using global data at a 0.25° resolution, but in the current paper we choose to consider only FLUXNET sites for ease of comparison, and also because the used NEXRAD data are available at individual radar sites rather than as a gridded product over the whole US and collecting/processing all data would be very time-consuming. We agree that the results require comparison of different maps and that the regional boxplots are useful additions.

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