

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.

Anonymous Referee #1

Received and published: 7 November 2013

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

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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.]

L40: Siqueira et al., JHM 2009 also focuses on this negative feedback.

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.

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.

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.

L70: Good place for that Siqueira reference here.

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?

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

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other forcing variables might not be.

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.

L187: What is the typical measurement error and/or closure for ECOR fluxes? 20% that I've seen cited?

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.

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).

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?

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.

L319: 'daytime'

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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. . . . ?

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?

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?

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)

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.

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.

L796-on: Very important point!

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

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furthering understanding of complex systems.

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

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.

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.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29137, 2013.

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