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Discussions

Interactive comment on "Evaluating the strength of the land–atmosphere moisture feedback in earth system models using satellite observation" *by* P. A. Levine et al.

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We thank Referee 1 for their supportive and thoughtful review. The reviewer presented a highly positive perspective on our analysis in their general comments, and provided several constructive criticisms in their specific comments. Below, we address the reviewer's specific comments by quoting each comment in *italicized* font, providing our response in roman font, and quoting our proposed revisions as indented roman font.

Methods: Page 5, line 16: "with temporal gaps filled using linear interpolation." More detail is needed here. How many months in the data record are filled? What was the typical time interval of missing data; for example, were the "gaps" filled predominantly just 1 month or multiple consecutive missing months of data? Is a linear interpolation

reasonable for temporal gap-filling GRACE data?

Missing months in the GRACE record are discussed at the following webpage: http://grace.jpl.nasa.gov/data/grace-months/. We limited the time series to September, 2002 through November, 2014, in order to reduce the number of temporal gaps. Within this time range, there are eight non-consecutive gaps of one single month and one gap of two missing months. Linear interpolation was chosen based on personal communication with colleagues experienced in the use of gridded land data from GRACE Tellus. We plan to clarify this by revising the text as follows:

> We obtained Level-3 TWSA data from GRACE using the University of Texas at Austin Center for Space Research (CSR) spherical harmonic solutions (Swenson, 2012). Global land data at a 1° resolution were scaled using the coefficients provided by Landerer and Swenson (2012). The study period was limited to September, 2002 through November, 2014, in order to minimize temporal gaps. GRACE data during the study period included eight non-consecutive and two consecutive missing months, which were smoothed using linear interpolation.

Page 5, lines 20-22: "...the use of TWS data allows us to include surface storage, canopy storage... all of which may be sources of moisture that are potentially limiting factors for ET". This is just one specific instance of discussing TWS for land-atmosphere analysis, but in general I think the paper would be better served with a more complete discussion of the advantages and limitations of using TWS for this purpose. For example, is the groundwater component of TWS a limiting factor for ET if the rooting depth does not reach the water table? In many temperate agrarian or grassland landscapes this is the case. How do you think these issues could potentially affect the results, or more specifically do you think your findings would be different if you just used (e.g.,) surficial soil moisture or root-zone soil moisture?

These points are well taken. The primary advantage of using TWS data from GRACE is the fact that it is the only multi-year global remote sensing product that includes root-zone soil moisture, unlike AMSR-E and others that only include near-surface soil moisture. The importance of including root-zone soil moisture is mentioned in the Introduction section, which we plan to revise, as detailed below, in order to clarify the use of TWS instead of surface soil moisture.

Regarding groundwater, if the rooting depth does not reach the water table, then groundwater would not serve as a limitation on ET. However, in agricultural and other inhabited areas, aquifer withdrawal for irrigation does provide a connection between groundwater and the atmosphere. Furthermore, the rate of aquifer recharge depends on how much available water is removed via ET. Finally, rooting depths are not always well understood, and the boundary between rooting zone and aquifer may not be known. Each of these points indicates an advantage to including the aquifer component of TWS. The disadvantage of including groundwater is that the metrics could be sensitive to long-term trends in groundwater, but these changes are likely to be relatively small on seasonal timescales and therefore do not provide a strong disadvantage.

We plan to remove segment in question, Page 5, lines 20–22, from the Methods section and to add the following text to the Introduction section in order to clarify and expand upon the importance of including the full TWS column for capturing root-zone soil moisture and other important components:

> Until recently, studies using remote sensing data to look for evidence of land–atmosphere coupling relied on products that provide information about surface soil moisture (Ferguson et al., 2012; Taylor et al., 2012). Consideration of root-zone soil moisture has been accomplished only indirectly via data-assimilated estimates (Guillod et al., 2015). The inability to directly consider root-zone soil moisture has been suggested as an explanation for the relatively weak coupling observed using remote sensing data (Hirschi

> et al., 2014). In order to include root-zone soil moisture, as well as other sources of moisture available across entire seasons, the present study uses remote sensing data of the entire terrestrial water storage (TWS) column.
>
> The metrics we introduce here were designed to utilize the monthly TWS anomaly (TWSA) anomaly product from the Gravity Recovery and Climate Experiment (GRACE) mission (Landerer and Swenson, 2012; Wahr et al., 2004). The GRACE TWSA product integrates soil moisture at all layers along with surface, canopy, snow/ice, and aquifer storage, as each of these components represents a potential source of moisture for fulfilling evaporative demand. For example, in areas where agricultural ecosystems are important, diversion of lake and river water resources and withdrawal from aquifers may contribute to ET. Furthermore, surface storage of liquid water and snow represent sources of water that are available for and potentially limiting to ET. Under these conditions, month-to-month TWS anomalies capture portions of the terrestrial water cycle that soil moisture alone may not.

Results: Page 10, lines 23-26: you find consistently weaker forcing relationships in boreal regions and attribute this to "high levels of climate variability in many high latitude regions" because of AO, NAO, and the like. However, do you think the predominantly energy-limited evaporative regime of many boreal regions contributes to limiting the feedback connection between soil moisture and atmospheric conditions? I would not expect the forcing "limb" to match the strength of the "response" limb in such conditions where evaporative fraction is more a function of incoming radiation than soil moisture availability.

This is a very good point, and we plan to add the following to address this:

> Furthermore, at high latitudes, ET is generally energy limited rather than

> moisture limited, which would lead to weak forcing metrics as moisture availability does not strongly influence atmospheric conditions.

Page 11, lines 28-29: "Furthermore, the well-understood physical mechanisms allow causality to be inferred even when not directly demonstrated." The argument in the previous sentence is well-taken, but I disagree with the premise of this statement, particularly inferring causality in the forcing "limb" in the absence of discussion/analysis of possible confounding effects. At the very least I would like to see some assessment or acknowledgement of the role of atmospheric persistence as a confounding factor when quantifying the forcing limb. For example, is a strong forcing limb caused by the physical constraint of soil moisture on energy partitioning and modification of boundary layer dynamics and thermodynamics (as suggested by the authors), or is it the result of large-scale atmospheric circulation and persistence of synoptic-scale patterns that modify precipitation and atmospheric demand throughout the duration of the TWS draw down season? A correlation coefficient cannot adequately address the question of large-scale atmospheric persistence vs. soil moisture feedback, and indeed this is beyond the scope of this manuscript. However, this does also mean that one cannot infer causality and mechanistic connections between soil moisture and VPD/PPT/SW based on the evidence provided here.

This point is well taken, and we plan to remove the sentence in question so as not to claim any indication of causality. We plan to expand the discussion just prior to the sentence in question to address the issue of persistence:

> The use of correlation coefficients in this study does not enable a direct assessment of whether the relationships are directly causal, as correlation between atmospheric and terrestrial conditions could result from atmospheric persistence and remote forcing from SST (Orlowsky and Seneviratne, 2010). Nonetheless, the satellite-derived metrics provide a meaningful constraint against which coupled models can be benchmarked, as these

> models need to correctly represent the combined effects of persistence, remote SST forcing, and land–atmosphere coupling.

We also plan to address this by adding a paragraph to Section 4.5 Uncertainties and future applications:

> Finally, the issue of causality and the possibility that correlations result primarily from atmospheric persistence and remote forcing from SST rather than land–atmosphere interactions may be addressed using sensitivity experiments similar to those of GLACE and GLACE-CMIP. While the previous experiments have tested the importance of soil moisture interaction with the atmosphere, additional experiments could expand upon these methods by treating SST variability similar to terrestrial soil moisture availability. Such experiments could determine the relative importance of remote SST forcing, including the effect of atmospheric persistence, and local land–atmosphere coupling in explaining correlations between TWS and atmospheric conditions.

Page 12, lines 14-16: "That models and ensemble members with high forcing metrics were also found to have high response metric... highlights the importance of the land surface response in priming the system for subsequent forcing on the atmosphere..." I don't understand how the land surface response "primes the system" for subsequent forcing when your analysis (and Figure 2) suggest the forcing occurs prior to the response. What am I missing? Can you please expound?

We plan to clarify the discussion by replacing the lines in question with the following:

> The inclusion of the response metrics allows the full feedback loop to be considered by recognizing the two-way dependence between the land surface and the atmosphere. The generally higher correlation coefficients in

observed response metrics indicates the importance of the land surface response in priming the system for subsequent forcing on the atmosphere. For example, if TWS response too strongly coupled to atmospheric forcing, a small change in atmospheric conditions could yield an unrealistically large change in TWS. The unrealistically large TWS anomaly would have the potential to impart a larger land surface forcing of the atmosphere in subsequent time steps. That models and ensemble members with high forcing metrics were also generally found to have high response metrics (Figure 10) highlights the need to consider this.

Conclusion: Page 16, lines 13-14: "...which suggests that some of these models may have difficulty properly predicting warming trends and climatic extremes." You include an excellent discussion of the potential links between model overestimation of land surface forcing and warming trends, founded in the body of literature. However, you do not explicitly quantify this linkage in this manuscript. The ability of models to "properly" predict warming trends and climatic extremes is not evaluated here, so this statement should probably be removed.

This point is well taken. We plan to rephrase this section to qualify our conclusion and avoid making conclusions that were not evaluated in our manuscript:

> Modeled feedback metrics are generally found to be stronger than those observed in the satellite record. If this discrepancy is due to models overestimating the two-way feedback between the land surface and the atmosphere, this could bias projections of future warming trends and climatic extremes.

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