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Hydrology and Earth System Sciences
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.

T. Ford (Referee)

twford@siu.edu

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General Comments

This manuscript evaluates the strength in the two-way relationship between total water storage (TWS) and atmospheric conditions within a suite of satellite remote sensing, reanalysis, and climate model systems. The correlation method for evaluating the coupling strength is not novel, but the concurrent assessment of both response and feedback "limbs" is. Further, comparison of the response and feedback strengths with model and satellite datasets provides additional evidence of the dichotomy in the apparent strength (and in some documented cases, the sign) of soil moisture-atmosphere coupling between model and observational frameworks. As the authors allude to,

these model system issues can influence estimates of future warming/drying trends. I enjoyed reading this very well written manuscript. The results and implications are international in scope, and it will make a worthy contribution to the land-atmosphere interaction body of knowledge. The outstanding issues I outline below are mostly minor, and the manuscript will be suitable for publication once they are addressed/clarified appropriately.

Specific Comments

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?

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?

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.

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.

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?

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.

Technical Corrections:

Page 11: I'm going to guess the section 4 title should be "Discussion" and not "Introduction"

Page 12, line 21: parentheses should only be around the publication year - Guo et al., (2006)...

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