Hydrol. Earth Syst. Sci. Discuss., 9, C346–C348, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C346/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Assimilation of space-based passive microwave soil moisture retrievals and the correction for a dynamic open water fraction" *by* B. T. Gouweleeuw et al.

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Received and published: 14 March 2012

The authors attempt to link seasonal bias patterns in remotely-sensed surface soil moisture retrievals to a new (and interesting) source – seasonal variations in surface water area. The paper is generally well-written and of interest for HESS readership – however there are a few issues that should be addressed prior to publication.

1) I would suggest that the author's rethink the title – there isn't any formal "data assimilation" in the manuscript and the potential impact of this research goes beyond the use of LPRM retrievals in data assimilation systems.

2) The paper would be greatly-improved by a "back-of-the-envelope" sensitivity calcula-

tion that demonstrates the feasibility of < 3-5 percent uncertainty in sub-footprint-scale surface water inducing (up to) 30 percent biases in remotely-sensed surface soil moisture retrievals. Even if based on very simplistic assumptions (e.g., surface temperature = air temperature, fixed water emissivity, constant VWC and b, the omega-tau model) this analysis would really help the credibility of the paper. Is the magnitude of bias attributed to variations in (sub-footprint-scale) surface water area variations credible?

Lacking this – I don't feel the key manuscript conclusion "The comparison indicates seasonally varying biases of up to 30 percent (relative) soil water content can be attributed to the presence of relatively small areas (< 5 percent) of open water in the (nominal) footprint." is fully justified.

3) In Figure 3, seasonally-varying biases are observed in the "South-Central" Oklahoma domain WITHOUT a corresponding seasonal variation in open water fraction for the same domain (Figure 5). This would seems to contradict the author's assertion that seasonal soil moisture biases arise directly from ignoring seasonality in open water content. If this is true – what causes the observed biases in the "South Central" domain? More discussion on this point would be helpful.

4) The author's argue that the observed seasonality cannot be attributed (at least not completely) to seasonality vegetation optical (VOD) because the seasonal trend of the biases does not align with the seasonal trend of VOD. But it seems like the real issues is the seasonal trend of ERRORS in VOD (and not VOD itself). From this point of the view, the argument concerning the (potential) role of VOD seems slightly off-target. Can the seasonality of VOD errors be assessed somehow?

5) Most of the large water bodies in Eastern Oklahoma are reservoirs – obviously there is some draw-down in these during the summer but Figure 5 seems to suggest there is (at least) a 50 percent reduction in the surface area of these reservoirs within a single year. This seems like a lot and suggests that the seasonal signal is tied to smaller water courses (e.g., farm diversion ponds) that dry up completely in the summer. Some

discussion (even if it's speculative) on this point would help. At present the magnitude of required season variations seems a little implausible. Could the authors show a histogram of water body sizes during summer and winter? Variations in this histogram might clarify where this seasonality is coming from (i.e., what size of water bodies are appearing and disappearing).

6) Figure 7 is good – it would also help to assure the reader that seasonal variations seen in open water fraction shown in Figure 5 are also repeated for other AMSR-E years.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1013, 2012.