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Hydrology and Earth System Sciences Discussions

Interactive comment on "A global analysis of satellite derived and DGVM surface soil moisture products" *by* K. T. Rebel et al.

Anonymous Referee #3

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The authors performed a global analysis of a microwave-based soil moisture retrieval based on the LPRM model (using AMSR-E Tb) and modeled soil moisture provided by the ORCHIDEE model. The article is well written, however, several issues are present which need to be addressed.

1) Why was the ORCHIDEE model chosen for this analysis? Although it is a well received LSM, the authors make a special point that this analysis is necessary before a full assimilation methodology can be implemented, a point that is correct. However, the structure of the soil layers in ORCHIDEE may present a very difficult implementation of a soil moisture data assimilation system because of the dynamic surface layer in ORCHIDEE. The authors make this point in the conclusions, yet I believe it may need

to be addressed earlier in the paper and also the authors should provide an opinion on how these necessary structural changes may affect the findings in this study.

2) The references to AMSR-E in the analysis should probably be changed to LPRM, while still making it clear that the LPRM is based on AMSR-E Tb in this study (for example, in Sec 2. 1).

3) What is the reasoning for the application of the low pass filter? The filter is going to act to dampen the soil moisture signal from LPRM, which when C- or X-band is used has an effective sensing depth of around 1 to possibly 2 cm. The use of the filter needs to be better justified.

The revisit time of AMSR-E is more on the order of 1 to 2 days (dependent on latitude), not 16 days.

4) The manuscript cites the availability of 300 FLUXNET sites, why are only 15 chosen for the analysis? If it is a data availability issue (i.e. some sites do not measure soil moisture), I still think it would help the reader accept the decision of 15 sites. I think it may also be helpful to provide information about the depth of the soil moisture observations at the sites which were included in the analysis.

5) The manuscript appears to reference several different time periods of available observations:

ORCHIDEE – 2000 to 2008 AMSR-E (LPRM) – 2002 to 2008 FLUXNET – 2000 to 2008

Considering that this analysis would have benefited from using all available years, why was the analysis only performed for 2003/2004? The reasoning needs to be addressed in the manuscript.

6) How is the monthly precipitation interpolated to daily values from the CRU dataset? Why was this dataset chosen during the analysis periods, were better precipitation datasets not available? Was any attempt made to temporally correct the monthly pre-

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cipitation, so that the model was forced with precipitation at a given grid point on days in which precipitation was actually observed? This would seem to be an important consideration especially in the case of comparing the ability of ORCHIDEE and LPRM to react to precipitation events.

This could potentially be done with a dataset such as TRMM or CMORPH, using a satellite precipitation dataset to temporally disaggregate the CRU precipitation forcing.

7) In section 3.2.1, the manuscript states that TOT_SM and ROOT_SM show the best correlation with LPRM (or AMSR-E), and in large areas of Europe, east Europe, North America, and South America, the correlation between LPRM and ROOT_SM is close to one.

Can the authors provide a potential reasoning for such a high correlation between LPRM (0 – 1 or 0 – 2 cm soil moisture retrieval, which is very sensitive to precipitation events) and ORCHIDEE TOT_SM and ROOT_SM (essentially 0 – 2000 cm, forced with a monthly, low resolution precipitation dataset)?

Was any attempt made to remove the seasonal cycle of SM which is potentially dominating the correlation signal, while not providing much information about the interannual skill of LPRM or ORCHIDEE? For example, when the seasonal cycle of SM is not removed, the analysis is potentially only showing that LPRM has very high skill (r near 1) of denoting wet vs. dry seasons, not denoting daily changes in SM (from precipitation events). Furthermore, in this section is it shown that while LPRM and ORCHIDEE have very high correlation in TOT_SM and ROOT_SM, the correlation between LPRM and the CRU precipitation forcing is not very high? As a reader, I had trouble rectifying how LPRM and ORCHIDEE TOT_SM/ROOT_SM can have a correlation near 1, while LPRM and the CRU precipitation forcing shows very low correlation. The SM evolution of ORCHIDEE is dominantly driven by the precipitation forcing, so I would expect poor precipitation forcing would lead to poor soil moisture predictions (i.e. better agreement between the correlation of LPRM and ORCHIDEE SM and the correlation of LPRM and

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CRU precipitation).

8) What does DVGM stand for? Please specify at its first reference in the manuscript.

pp. 4283, line 17 – should not be considered a direct observation, soil moisture is retrieved through application a radiative transfer model, radiance (or brightness temperature) is directly observed.

pp. 4283, line 21 – should provided a quantitative measure of "significant" vegetation

pp. 4285, line 14 – please make it clear that microwave satellite soil moisture covers on the first few centimeters, indirect soil moisture estimation using thermal wavelengths have been shown to potentially provide a root-zone soil moisture signal over moderate to dense vegetation.

pp. 4285, line 27 – provide the reader with the reasoning that descending retrievals are more reliable.

pp. 4291, line 7 – it is a bit confusing what correlation you are referring to in the heading of 3.2.1, please specify

pp. 4297, line 8 – should be more like 1 to 2 cm when retrieval is based on C- or X-band, L-band sensors are more on the order of 3 to 5 cm

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4281, 2011.