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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A comparison between remotely-sensed and modelled surface soil moisture (and frozen status) at high latitudes" *by* I. Gouttevin et al.

Anonymous Referee #3

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General Comments

This paper presents a study comparing the freeze and thaw surface status as well as surface soil moisture from ASCAT satellite observations and the ORCHIDEE land surface model over a high latitude study area. The issues addressed in this study are of great importance and very challenging, both from a modelling as well as a remote sensing point of view.

The analyses and results presented in this paper are very interesting. The authors thoroughly identify different areas of agreement and divergence between model and

observations and provide good explanations for the possible causes behind the observed behaviour.

My main issue with this paper concerns the completeness of the analyses presented. For the different model-observation divergences identified, the authors very nicely identify the possible causes for the observed behaviour, but then fail to follow up on pinpointing the exact cause, even though they seem to have the necessary information. For example, it is hypothesized that the differences observed are either due to the forcing data or the model parameterization and it is mentioned that a model run with different forcing data as well as versions of the model with different parameterizations are available. So the reader asks himself why this information is not used to verify or reject the different hypotheses. At least some more convincing argument needs to be provided for not presenting these results here.

Another point of improvement is the presentation of the results. Section 3 contains a lot of very interesting information, but the way it is presented sometimes makes it difficult for the reader to follow. The presentation of the different metrics (correlations, normalized difference) would fit better in the 'Methods' section, the metrics should be defined more clearly and for each of the different plots/metrics presented it should be stated clearly what the purpose and the main finding of the analysis was.

Specific Comments

p.11244 l.20 – p.11245 l.11 The discussion you give here is very thorough and correct, but you should state more clearly that the issues you are discussing apply to *in situ* observations. So instead of using the very general term 'data', it would be better to use '*in situ* data' or '*in situ* observations'

p.11246 l.10 – l.27 This discussion of remotely sensed soil moisture products is a bit short and a more general discussion would be appropriate. I think you could men-

tion the existence (or planning) of dedicated soil moisture missions like SMOS (Kerr et al. 20110) and SMAP (Entekhabi et al. 2010) as well as other soil moisture remote sensing products, like the passive microwave product of the Vrije Universiteit Amsterdam (which does provide the longest record of remotely sensed, global soil moisture; Owe et al. 2001 and 2008) or the merged WACMOS product (Liu et al. 2011). After that or in section 2 you could then argue why you chose the ASCAT product for your study.

Kerr, Y., Waldteufel, P., Wigneron, J.-P., Delwart, S., Cabot, F., Boutin, J., Escorihuela, M.-J., Font, J., Reul, N., Gruhier, C., Juglea, S., Drinkwater, M., Hahne, A., Martin-Neira, M., and Mecklenburg, S. (2010). The SMOS Mission: New Tool for Monitoring Key Elements of the Global Water Cycle. Proceedings of the IEEE, 98(5):666–687.

Entekhabi, D., Njoku, E., O'Neill, P., Kellogg, K., Crow, W., Edelstein, W., Entin, J., Goodman, S., Jackson, T., Johnson, J., Kimball, J., Piepmeier, J., Koster, R., Martin, N., McDonald, K., Moghaddam, M., Moran, S., Reichle, R., Shi, J., Spencer, M., Thurman, S., Tsang, L., and van Zyl, J. (2010). The Soil Moisture Active Passive (SMAP) Mission. Proceedings of the IEEE, 98(5):704 –716

Owe, M., Jeu, R. D., and Holmes, T. (2008). Multi-sensor historical climatology of satellite-derived global land surface moisture. Journal of Geophysical Research, 113.

Owe, M., Jeu, R. D., and Walker, J. (2001). A Methodology for Surface Soil Moisture and Vegetation optical Depth Retrieval Using The Microwave Polarization Difference Index. IEEE Transactions on Geoscience and Remote Sensing , 39:1643–1654.

Liu, Y., Parinussa, R., Dorigo, W., Jeu, R. D., Wagner, W., van Dijk, A., McCabe, M., and Evans, J. (2011). Developing an improved soil moisture dataset by blending passive and active microwave satellite-based retrievals. Hydrology and Earth System Sciences, 15:425–436.

p.11246 l.16 – l.20 The advantages mentioned here apply to other microwave soil moisture retrievals as well. What is the advantage/reason of choosing the ASCAT product over other products such as the SMOS product (dedicated sensor), the VUA product (longer data record) or the WACMOS poduct (longer data record and combination of active and passive microwave)?

p.11249 l.19 – l.20 Passive microwave sensors also allow for observations day and night.

p.11250 l.9 In very arid regions, ASCAT can have a penetration depth of up to 5cm, however, in your study area typical maximum penetration depths are probably more on the order of 2 cm. Please clarify this.

p.11251 l.21 – l.24 You could probably solve the issue of the seasonal extent of water bodies by filtering with a dynamic surface water extent product, such as GIEMS (Prigent et al. 2007, JGR)

Prigent, C., Papa, F., Aires, F., Rossow, W., and Matthews, E. (2007). Global inundation dynamics inferred from multiple satellite observations, 1993-2000. Journal of Geophysical Research, 112(D12107)

p.11254 l.18 – l.25 As before, I think that in your study area, maximum penetration depths of ASCAT are much lower than 5 cm, so when using the first 5 layers of ORCHIDEE you might not be comparing the same thing. I think using the first 4 layers, corresponding to approximately 2 cm, might be a better choice here.

p.11255 Table 1 I am not convinced that these percentages are the best metric to use here. For example, if the model and the observations give a different result for the 'unfrozen status' for only one day, the impact in winter will be huge, because of the few 'unfrozen' days, whereas in summer the impact will be much smaller. So I think you cannot really draw any conclusions on the model skill from Table 1, because the metric is too sensitive to the number of days available.

p.11256 l.2 This information is provided in the ASCAT product available at EUMETSAT, so it might be interesting to check for biases due to the acquisition time in the DUE product. However, it is understandable if this is beyond the scope of this paper.

p.11256 l.9 – l.12 Could this be related to the surface depth issues addressed before? If you compare the ASCAT surface status representative of 1-2 cm with the modelled surface status representing 5 cm, you would expect to see a delay in the model compared to ASCAT or not?

p.11257 l.17 – l.21 This is very interesting! I think you should emphasize these results more. If - as you mention - you do not see an improvement in the SWE estimation when using WATCH forcing data, this indicates that the snow albedo parameterization is the culprit and not the forcing data. Of course it would be ideal to also include a run with the new snow scheme (if it is available for forested areas by now) to verify this.

p.11259 l.28 As mentioned before, effects of seasonal changes in surface water extent could be removed by filtering with a dynamic surface water extent dataset such as the GIEMS dataset of *Prigent et al. 2007*

p.11261 l.8 – l.10 Given this low Nash-Sutcliffe efficiency, could you please comment on the validity or usefulness of the model-data comparison.

p.11261 Figure 6 Could you please indicate how this correlation has been computed? Have the correlations been computed from absolute values or from anomalies from the seasonal cycle? Were correlations computed using the entire data time series in each pixel? Furthermore, you mention before that soil moisture data has been composited to weekly values, could you please explain how you obtain a daily correlation?

p.11261 l.20 and following In North-East Siberia strong anti-correlations can be observed. If the correlations are computed from absolute soil moisture values (as opposed to anomalies from the seasonal cycle) this would mean that model and observations do not even agree on the sign/phase of the seasonal cycle. I am not convinced that the explanations given in the following (biased evaporation, misrepresentation of precipitation events) could explain such a strong difference.

p.11263 l.1 – l.4 It might be interesting to compare the forcing data precipitation and the ASCAT soil moisture over these areas to confirm this hypothesis.

p.11263 l.20 To complete this correlation analysis, it might be interesting to look at the data time series from one point with a positive correlation and one point with an anti-correlation. You could also include a time series of (observed) precipitation for each point to confirm your hypothesis from before.

p.11236 Figure 8 It might be interesting to include and observed precipitation signal in these plots, e.g. from GPCP or CMORPH.

p.11264 l.2 – l.4 Could you please elaborate on this statement.

p.11264 Figure 9 In the lower latitudes around 45°N the model and observation signal seem to be out of phase, i.e. in the ASCAT data soil moisture peaks in the winter months, whereas in the model it is highest in the summer. However, this does not show up as an anti-correlation in Figure 6. Could you please comment on this.

p.11265 l.8 – l.11 Could you please indicate why you decided not to use this improved version of ORCHIDEE in this study?

p.11266 l.2 – l.6 Again, could you indicate why this version of the model was not used in this study? Would it be possible to include a small example with the improved ORCHIDEE versions (maybe only on the basin scale) to give the reader an idea of what the improvement on the performance would be?

p.11267 l.3 – l.6 I completely agree, I just wish you would mention this issue much earlier.

Technical Corrections

p.11243 l.7 replace 'though' by 'nevertheless' or 'nonetheless'

p.11243 l.23 replace 'proceed' with 'originate'

p.11245 l.5 replace 'hazardous' with 'difficult'

p.11246 l.9 replace 'though' by 'nevertheless' or 'nonetheless'

p.11246 l.12 please use 'in essence' or 'by nature'

p.11246 l.14 '... data are acquired at **temporal** frequencies ...'

p.11246 l.16 – l.20 For the ASCAT product please include the references *Wagner et al. 2009, RSE* and *Bartalis et al. 2007, GRL*

p.11247 l.28 'hence an assessed good accuracy for this product...', better 'and showed a good accuracy of this product'

p.11248 l.7 please include reference for ORCHIDEE

p.11248 l.18 please use 'originate' or 'stem' instead of 'proceed'

p.11249 l.9 '... and **were** designed ...'

p.11249 l.20 '**The** MetOp ...'

p.11250 l.16 ERS

p.11250 l.24 replace 'opposite' by 'vice versa' or 'the other way around'

p.11251 l.26 '... **in** the course of ...'

p.11252 l.16 please also include

de Rosnay, P., Polcher, J., Bruen, M., and Laval, K. (2002). Impact of a physically based soil water flow and soil-plant interaction representation for modeling largescale land surface processes. Journal of Geophysical Research, 107(4118).

p.11254 l.9 – l.11 better '...would lead to an overestimation of the occurrence ... would lead to an underestimation.'

p.11254 l.18 please use an original reference for the 11-layer ORCHIDEE version here, e.g. *de Rosnay et al. 2000, GRL*

p.11256 l.19 please change reference to *Brown and Brasnett 2010*; 'et al.' is only for 3 or more authors

p.11258 l.24 indices

p.11259 l.9 indices

p.11259 l.21 results

p.11261 l.10 Sutcliffe; please also correct this in the bibliography

p.11264 l.13 replace 'though' by 'although'

p.11264 l.27 replace 'can be incriminated' by 'are responsible for'

p.11266 l.11 replace 'though' by 'although'

p.11266 l.25 replace 'though' by 'although'

p.11267 l.25 replace 'though' by 'however'