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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 #4

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This manuscript uses products derived from the ASCAT instrument to evaluate the soil moisture and surface status (frozen/unfrozen) simulated by the Orchidee land surface model over Siberia. The analysis focusses on the year 2007 to 2009. The authors identify several factors that might have reduced the ability of the model to reproduce aspects of the satellite-derived product.

General comments

I applaud the general aim of using data, Earth Observation (EO) data in particular, to evaluate land surface models. In my opinion (speaking as a modeller) very often too

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little time is spent on model evaluation. Despite the fact that such comparisons can be time consuming, it is often difficult to pinpoint definite conclusions, particularly if a single product or model is used. Unfortunately I fear that this study, despite involving considerable effort, also struggles to define substantial conclusions and insights.

The Abstract declares a primary aim of the study (to evaluate the model) and a secondary aim (to evaluate the EO product). These two rather different aims meant that it was sometimes difficult to know where the paper was coming from, in that sometimes we are assessing the model (effectively assuming the EO data are good) but then in a nearby section the focus is on potential pitfalls of the EO data. Both sides of the story have to be told, but certainly as presented in this paper it sometime makes for a rather confusing or unsatisfying read - the reader can be left rather uncertain as to what to trust.

Several times we are told that revised parameterisations are available (possibly in another version of the model), or alternative datasets are available, but these are not explored (apart, I think, from one reference to runs using the WATCH Forcing Data). I realise that the model development is an ongoing process and it is necessary to jump in at some point and take a snapshot of its performance, but I think more could have been done in terms of examining the sensitivity of model results to different input datasets (e.g. meteorology, soils). It just felt like there were several tantalizing references to possible improvements, but these were generally slightly out of reach/not used.

The difficulty for reviewers and editors is to decide how to weigh up the difficulties inherent in such studies with the desire to publish sound new results. Frankly I'm not quite sure where this paper sits, but I would say that it represents a sound base that could be improved on with a relatively small amount of extra work. (In particular I would think about possible sensitivity experiments - e.g. soil data, meteorology - and revising the text so that it conveys a more confident/consistent message - e.g. focus more on model evaluation in areas where the EO data are considered more certain - to make it a more satisfying read.)

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Specific comments

The Abstract is often a bit loose, or vague, and qualitative. In my opinion it would be improved by focussing more on results and being more quantitative. Less on hypotheses as to causes of model error (possibly summarise in one sentence).

p11245 Line 28 - Evapotranspiration is not a good example as it also cannot be directly observed at large scales (eddy covariance techniques can be used at “field scale”. On the other hand it might be useful to mention some of the recent studies that use GRACE data to estimate the total water storage (e.g. Alkama et al., 2010, J.Hydromet.).

p11247 Line 25 - This is an example of where the dual aims of the paper are unsettling. In essence the EO product is still very much under evaluation/validation (which by itself is reasonable), but it does leave the reader wondering about how far it can be trusted for the present paper.

p11248 - Line 17 - “Parallel investigations...”, again, we get lots of warnings about the EO product.

p11249 ALANIS-methane. Is there a reference for this project? I found it on the web, but it would be better to be directed towards the right resource.

p11251 Line 15 - Slightly confusing reference to a 2011 paper when describing “the present study” (which I would take to mean the work in the manuscript under discussion).

p11252 Line 17 or thereabouts - Would be good to describe the vertical discretisation of the soil grid (i.e. layer thicknesses). One gets a partial answer on p11254.

p11255 Line 5 - In general I thought this was a good attempt to match the model diagnostic to the EO product, in terms of ensuring the same scaling and averaging, rather than just “eyeballing” vaguely comparable but rather different variables.

p11256 Line 18 - CMC SWE data. The apparent agreement between locations where

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modelled SWE is incorrect (relative to CMC) and areas where soil thaw is not modelled well is a useful observation. But I wonder how much confidence we can have in the CMC estimate in these often remote areas? At least it might be worth a sentence describing the CMC data, so that non-experts have some idea of what it is (and that is inevitably imperfect).

p11259 Line 9 - Spelling mistake, repeated more than once. 'indeces' should be 'indices' or 'indexes'.

p11259 Line 14 and following. Perhaps the satellite observations in these areas should be ignored, given the caveats expressed in this paragraph? In particular, if the aim is to use the EO data to evaluate the model, a conservative approach to EO data quality would be best (i.e. use more demanding criteria for the acceptability of the EO data, so that only the 'best' or 'highest confidence' data re used for model evaluation. Although the colour scale in Fig.A1 makes it slightly tricky to estimate numbers, it does appear that some locations have relatively few data per year and should perhaps be excluded, although I doubt this would change the general conclusions of the study.

p11260 Line 17 - ASCAT data over mountains. Should we trust these? On p11259 we are warned about possible difficulties over thin soils. If we can't trust ASCAT there it becomes rather pointless speculating about possible model deficiencies in mountainous areas (although generally I would imagine that modelling mountain hydrology is indeed a stiff challenge for the model).

p11262 Line 13 - The possibility that precipitation is underestimated in these areas should also be mentioned (particularly as it is mentioned earlier).

p11262 Line 17 and following - Can you really say that these are "improbable" just because the model doesn't have a mountain scheme? Could incorrect parameter values (e.g. soil properties - a three texture class dataset is used, which inevitably restricts the range of values, if I understand correctly) not also have a role?

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p11263 Line 1 - Following from the above, although poor meteorology is a definite possibility, I find it hard to rule out the possibility that the model struggles in these mountainous areas in part because of the simplicity of its parameterisation compared to the complicated hydrology and possible deficiencies in parameter values.

p11265 Last paragraph - An example of where it would be good to see some other results - e.g. use an alternative soil dataset.

p11267 Line 5 - 5cm soil thickness. Given that, as I understand it, Orchidee was run using near-surface soil layers of <5cm thickness, it might be interesting to compare the results when the top 5cm of model soil are analysed with those when depths of say 2 and 4 cm are used. This might also be of interest given that on page 11254 you note that the EO soil moisture represents a value for ∼5cm of dry soil but <5cm for wetter soils (which are common in this area).

Figure 2 (and some others) - Broad patterns can be discerned from this figure, but a map of differences (model-EO) or a scatter plot 9EO v. model) would make comparison much easier.

Figure 3 - Consider showing shorter sections of these time series, if possible, as these figures are likely to appear at relatively small size online or in a printed version. e.g. Discussion at top of page 11257 would benefit from zooming in on the time period in question.

Figure 6 - In passing I wonder if there is any connection between the generally low correlation near 60N and the smaller number of data in that area (Fig.A1)?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11241, 2013.