

Interactive comment on “Operational assimilation of ASCAT surface soil wetness at the Met Office” by I. Dharssi et al.

Anonymous Referee #3

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This paper describes experiments testing the assimilation of ASCAT-derived near-surface soil moisture observations into the Met-Office’s NWP model, using a simple nudging scheme. The system described is the first to use remotely sensed near-surface soil moisture observations in an operational NWP model. While several other NWP centres are actively working towards introducing similar (although typically more complicated) schemes, the experiments presented are one of only a few examples of large-scale comparisons of screen-level observation (T and RH) and soil moisture assimilation, and quite possibly the most carefully constructed example (in terms of QC / processing / quality of the soil moisture data). This is a significant step forward in the use of remotely sensed land surface data in NWP, and so the topic and novel approach used to assimilate the observations at the Met Office will be of great interest to many

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readers. However I feel that in its current form the potential (high) impact of the paper has not been realised. In particular the results of the presented experiments deserve a much more detailed/careful analysis. I recommend that this paper be published only after the following comments have been addressed:

Major Comments:

1. The paper reads like a technical report, both in terms of content and format. -> Please split into more traditional sections (Methods, Results, etc). The introduction also requires a clear statement of what the paper is actually about / the experiments presented. More could be said in the conclusions about the broader implications of this work (rather than the implications specific to the Met Office).

-> There is a very strong emphasis on the development of the assimilation system / data processing, at the expense of presenting an in-depth analysis of the results. The Met Office has used a very simplistic approach to assimilate the ASCAT data, in order to generate results quickly. The interesting output from this work are the results, not the simplistic assimilation method, and to realise its impact the paper should better reflect this. Please place a much greater emphasis on analysing the results of the experiments.

For example, there are many unanswered questions that could be addressed, such as: How does analysing only the surface layer impact the root-zone soil moisture? Does this justify updating only the near-surface layer? In light of the answer to the first question, how does this then affect low-level atmospheric forecasts? Do these results vary by region / climate / season? At individual locations / regions, what is the relative impact on soil moisture and screen-level forecasts skill? What is causing the diurnal cycle in forecast skill in Figs 8-9? How does assimilating the ASCAT data affect this?

A discussion of the possible impacts of using the simplified nudging scheme, compared to the Kalman Filter based approaches being pursued elsewhere, would be very interesting - even if this were limited to a theoretical discussion, rather than experimental

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results (although the authors note that they are developing an EKF as well).

2. The analysis against the in situ data in Section 9 requires significant revision.

The first paragraph in this section notes that there is a large error associated with using in situ observations to represent the area average, and yet the remainder of the section attributes all of the difference between the in situ observations and the model to errors in the model (even though the estimated differences between the model and the in situ observations are generally smaller than the 0.07 vol/vol mean (square?) error estimated for using the in situ obs). Please rephrase this entire section to highlight that better statistics indicates a better match to the in situ data, rather than a better match to the (unknown) true soil moisture. For example, replace "reduced the random error in the UM ..." with "reduces the random differences between the UM soil moisture and the observations".

Please also includes estimates of the significance in your results. Specifically, the right most columns in Table 4 should refer to significant changes in the soil moisture forecast skill.

The evaluation against the in situ data are presented for trial one only. This is a very short time period (2 months). Please repeat the evaluation using all of the trials that you have conducted.

Do you have the data to assess the low-level atmospheric forecasts at these same sites? It would be extremely interesting to see an evaluation of the impact on soil moisture and the screen-level observations at the same locations.

Since you are assimilating observations that have been scaled to match the model biases, I do not expect to see a large impact on the soil moisture bias / RMSD. For this reason, correlations and anomaly correlations are the usual metric for analysing forecast skill. Please include the anomaly correlations.

Please also assess the impact of the assimilation on the root-zone soil moisture skill.

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This is of far greater interest to most readers than the impact on the surface soil moisture.

3. In general, the captions do not contain enough information to fully explain the tables and figures. Please check each caption carefully.

Specific Comments:

Abstract P4314, L11: -change "this note" to "this article"

Introduction: It is not very clear what the purpose of the first paragraph is. I think the authors are trying to highlight that it is impractical to observe soil moisture at large scales, however this information does not come across clearly. For example, what is the relevance of the statistic quoted on P4315, L9 to this work? Please place the discussion of the horizontal variation of soil moisture into the context of this paper.

P4314, L23-24 change "humidity, low clouds" to "humidity and low clouds"

P4315, L3: "This is part of the reason that..." I don't think that high spatial variability is the reason that there is no extensive network, however the high degree of spatial variability limits the utility of the existing network.

P4315, bottom of page: "At microwave frequencies..." I would skip the discussion of the dielectric constant, since the ASCAT soil moisture retrieval algorithm does make use of a radiative transfer model.

P4316, L2: If you reference C- and X- band here, then you should have already stated which remote sensors use which.

P4316, P11: Place Reichle et al (2004) in braces. There are several examples of missing braces, please check all references carefully.

P4316, dot point beginning L20: This is an important point to make, but it is not accurately explained. It is not just the case that the model is "biased", but also that it defines soil moisture in a unique way according to its physics (see Koster et al (2009) [On the

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Nature of Soil Moisture in Land Surface Models, J. Clim.] for an excellent discussion of this). Hence, the difference between models and observations will not be addressed simply by improving the model, as is suggested here. If a model can generate correct flux forecasts from a "biased" (compared to what? we don't know the truth, and in situ / remotely sensed observations have their own bias) soil moisture, then one could argue that its soil moisture is correct.

P4317, L6: Same point as above. The main reason to scale the observations to the climatology of the model is to generate "observations" consistent with the soil moisture climatology defined by the model physics (reducing representivity errors in those observations).

P4318, L8: Delete Table 1.

P4318, L26: Are there any other controls besides timing to prevent soil moisture from being updated in response to other errors? Even restricting the updates to only occur during active evaporation will still result in soil moisture being corrected for unrelated errors.

Section 3: Some of the extraneous information on ASCAT could be removed.

P4319: delete the footnote.

P4320 Since you don't use the surface wetness index generated from the exponential filter, I would delete all reference to it - or reduce it to a single statement noting that it exists, but you expect the UM will more accurately propagate the surface information vertically (as an aside, does it?) .

P4320, L19: replace "ERS/ASCAT" with "ERS or ASCAT"

P4321, L4-L10: "However, they do find..." It is obvious that a 1 cm observation will have different behaviour to a 5 cm observation. This point requires only a sentence (and possibly a comment on how this affects your use of 1 cm observations, given the surface layer in the UM is 10 cm deep).

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P4321, P17: You may need to rephrase to avoid the use of "He" (depending on the HESS style guide conventions)

P4321, start of section 4: Delete reference to the Level 3 product.

P4322, L1-2: "must be converted to a volumetric soil moisture" It is more than this. You are also converting it to be consistent with the model soil moisture climatology.

P4322, L8: "long time period" give the years.

P4322, L10: delete reference to the BUFR code.

P4315, L18: "the driving data covers 1986 to 1995" This is not the same period for which you have ASCAT data. I would expect there to be differences in the climatology in many regions between the two periods. You will need to justify using the time period. The use of observation forcing, rather than the standard model output (which is usually used) is also an interesting choice, which should be discussed.

P4323, and Figure 1: Derivation of b. There is not enough information presented to justify the selection of b. How is each m_s in Figure 1 determined (is it a spatial or temporal mean?)? Please include $\theta_s - \theta_w$ on the plot so that the reader can judge the similarity. Also there are not many points on the plots. What is the error margin in the estimated b? What is the error then introduced by assuming equation 7, rather than the line of best fit?

P4323, L9: Explain why have you presented the results as a fraction of saturation. This is a bit confusing to those not familiar with the UM.

P4323, L13: I don't think the histograms are similar. They have similar behaviour about the mode, but the model has a second mode not present in the obs.

P4323, last paragraph before section 5: It might be better to include the information in this paragraph earlier. Did you use the van Genuchten update when you ran the model with the GSWP data? Also, while your scaled soil moisture might be consistent with

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the soil moisture physics, it will have biases relative to the model due to the differences in forcing (obs. v. NWP - also see earlier comment regarding the time periods).

P4324, QC dot points: This is more information than is necessary here. Keep only the first sentence of each dot point - you can reference the actual technical report for the excess detail.

P4324, L20: Is 7% in the original ASCAT units? If so, how does the translate into your derived soil volumetric soil moisture?

Section 6: I would remove this section, and replace it with one sentence describing the re-gridding in the appropriate place.

P4326, L4: "such a scheme has been developed quickly" Is this what you intended to say?

P4326, eq 10: It would be more straight forward to state that only the surface layer is analysed from the ASCAT observations. In fact, this is an important aspect of your scheme that is not sufficiently discussed. Within the hydrological community it is more common to attempt to update the root-zone soil moisture from near-surface observations. I agree with your approach, but it is not standard and so you will need to clearly justify it, and outline the consequences.

P4326, L19: Delete "and free-wheels".

Section 8: It is not clear from the section what the experiments that you have conducted are. Please state this explicitly. Giving the experiments names that relate to the assimilated observations would help too. Is it possible to include an open-loop? Also, the discussion of Scipal et al does not really fit here. It would have been better in the introduction.

Section 8.1 Include a brief description of the typical length of the experiments in the text. Also, explain why they had to be conducted like this, rather than in a single long trial.

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P4327, L11: Include more details, and a reference, for the global UM NWP index. This seems like an index measuring large-scale circulations, and as you state it is unlikely to be sensitive to the ASCAT assim. Is it necessary to present it in its own section (or at all)? Also, why is it separate from the other validation stats?

Section 8.3. I would move the info in this section to the earlier section on QC. Also, recalculate Table 3 to include only land points - it is obvious that sea points are removed, and the large fraction of sea removed makes the other statistics difficult to interpret. Does each column present the number removed that day? Why not just give summary stats covering the whole period / a single example (since they are all similar)? Please also include some guide to the absolute number of observations (or fraction of land-surface) that remains after the QC.

P4328, L8: I was surprised that the ASCAT assim makes a similar volume of update, given that it applies to 1/10 of the depth. This means that it must be making much larger changes to the vol/vol soil moisture. Please discuss why this is, and whether you think it is accurate / desirable.

P4328, L9-17: I would delete from "At first site...." to the end of the paragraph.

Section 8.4, 8.5 and 9 need much more information than is currently presented.

P4328, L20: What are the consequences of having conflicting directions of updates in the surface and underlying layers? I imagine this will cause some problems in the vertical profile / flow. Do the updates in one layer ultimately dominate the other layer?

P4328, L25: Can you provide a reference for the bug?

P4329, L4-L11: This section needs far more detail. It is hard to get much information from the global maps (can you make them bigger?). Does assimilating ASCAT affect the underlying soil moisture?

Section 8.5 This seems like a very restricted set of results. Please provide summary stats for all trials. Also, can you combine the different trials to give an overall evaluation

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of performance?

P4329, L26: Replace "zig-zag" with "diurnal cycle" (unless I am misinterpreting what this is?) Also, this diurnal cycle is interesting / alarming. What is causing it? Does the assimilation help?

P4330, L19: Expand on what you mean by SD. I assume this is the standard deviation of the difference between the model and the obs?

P4331, L11: I don't think the appendix is necessary. These are all very standard statistics. If you do keep it, then move the discussion of the confidence intervals into it.

Figures: The text on all figures is difficult to read. Please increase font size. The plots themselves could also be larger, particularly the global maps.

Figure 5 Over what domain are these stats calculated?

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