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Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #1

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Overview

The study investigates the assimilation of the ASCAT soil moisture product into the Unified Model (UM) of the UK Met Office, that is employed for both numerical weather prediction and climate research. Specifically, a simplified nudging scheme is adopted for the assimilation of the ASCAT surface soil moisture product (level 2), along with the assimilation of screen temperature and humidity observations. The added value derived by the assimilation of the ASCAT soil moisture product is demonstrated in terms of air temperature and humidity forecasting for the tropics, North America and Australia. Moreover, the assimilation also improves the agreement between the modelled soil moisture values and the in-situ observations obtained by the USDA SCAN network in

US.

General Comments

The paper is fairly well written and structured and the topic is very relevant. In fact, given the recent availability of satellite soil moisture products with improved accuracy and spatial-temporal resolution, a number of studies testing the assimilation of these soil moisture products for improving numerical weather prediction have been carried out (see also the recent studies by *Albergel et al., 2010; de Rosnay et al., 2010; Draper et al., 2011*). However, in my opinion, several aspects should be enhanced before its publication, mainly in relation to the performed analysis and to the presentation of the results.

Some of the analyses conducted in the paper are only briefly described and, hence, it is not easy for the reader to understand the results. Specifically, I firstly refer to the evaluation of the assimilation experiment in terms of the global UM NWP index. Likely, for the Met Office employees this index is obviously known but I do not know to what it refers and, hence, I do not understand this section (section 8.1). Moreover, its impact is not statistically significant. I suggest reducing this section to only one/two sentences or removing it. Secondly, the results reported in Figure 6 are affected by the bug in the UM model, as it is underlined by the authors at lines 23-25 of page 4328. I do not understand this analysis and, mainly, how the bug of the model affects the overall results of the paper. The authors say that, by correcting the bug, the results are more reliable as it can be seen in Figure 7. On this basis, I suggest re-performing the analysis related to Figure 6 (bug-corrected); otherwise this part should be removed. Thirdly, it is not clear why five different trials are conducted in the paper (reported in Table 2). The differences in the results for some of them are trivial (see page 4330, lines 1-2). Why are these particular trials selected? Moreover, for trial 1 the assimilation is done only at 12Z, thus producing the unusual ziz-zag pattern in the air temperature and humidity forecasts. On the other hand, trial 1 is the one characterized by the longer time period, and hence it is the more robust. In my opinion, it is more desirable to have only one/two

trials characterized by a time span as long as possible and, for instance, two values for the K parameter of the nudging scheme (see *Brocca et al., 2010b*).

In addition, the presentation of the results should be enhanced. The labels of the figures are too small and the captions only briefly describe the content of the Figures. By way of example, in Figures 8, 9, 10 and 13 a number of information given in the figure is not reported in the caption.

Another important aspect is related to the soil moisture product selected for the assimilation. In fact, the ASCAT Surface Soil Moisture, SSM, product used in the study, that should be the operational soil moisture product disseminated by EUMETSAT, does not represent the latest version of this data set. As reported in very recent studies (*Brocca et al., 2010a; 2010b*), the new version of the ASCAT SSM product provides more consistent soil moisture estimates with a reduced noise level, especially for dry conditions. I suggest mentioning this aspect in the paper (I expect it is not feasible to quickly perform the analyses with the new version of the soil moisture product) and to discuss, in section 3.2, the results of the above mentioned studies which proved the good behaviour of the new ASCAT SSM product against in-situ and modeled soil moisture data for different sites across Europe (see also *Brocca et al., 2010d*).

Moreover, the satellite surface soil moisture product, representative of a layer depth of 2-5 cm, is used to adjust the UM modelled soil moisture of the first layer that considers a depth of 10 cm. This difference in the layer depth should have a significant impact on the results and, hence, it should be deeply discussed. I agree with the authors that it could be more robust to use the land surface model MOSES2 to propagate the satellite data to the deeper soil levels (see page 4321, lines 24-25), but the problem is that the surface layer of the land surface model is too deep.

The authors introduced, without using it, the level 3 soil moisture product, i.e. the Soil Water Index (SWI), obtained through the application of the exponential filter. The SWI can be considered representative of deeper layers (and it is also characterized by

reduced noise) and can be tuned analyzing its variability with the characteristic time length parameter, T. I suggest trying the use of the SWI in one of the trials (for instance assuming a constant T-value equal to 4/5 days). I expect that the results, at least in terms of agreement with USDA SCAN soil moisture observations, will significantly improve. On the other hand, if this analysis cannot be performed, I suggest removing the description of the SWI index and including this possibility as one of the future investigations.

Finally, the purposes and the (control and test) experiments carried out in the paper should be clearly stated at the beginning of the paper. The control experiment (CTRL) should consider the assimilation of only screen level temperature and humidity and it is quite different from the previous studies for which the CTRL experiment considers the unconstrained soil moisture evolution. Therefore, it should be specified. The test experiment should consider the assimilation of both screen level observation and soil moisture, not only soil moisture as in *Scipal et al. (2008)*. I suggest addressing these aspects at the end of the Introduction section.

In the specific comments, I report a number of further changes and clarifications that are required.

On these bases, in my opinion, I find that the paper may become worthy of publication on HESS after a moderate revision.

Specific Comments/ Technical Corrections (P: page, L: line or lines)

P4314, L11: Change "note" with "study".

P4315, L1: I agree with the authors that, usually, soil moisture measurements made within a small area are characterized by high spatial variability. However, the temporal stability of the soil moisture spatial patterns allows estimating the average temporal pattern for a given area with a very limited number of point measurements (see e.g. *Vachaud et al., 1985; Famiglietti et al., 2008; Wagner et al., 2008; Brocca et al., 2010*).

Therefore, the comparison of in-situ observations and coarse resolution satellite data frequently provides good performance (depending on the satellite data accuracy) and the same could occur with the output of meteorological models (25 km resolution). Therefore, also the representativeness error of point measurement,reported at P4330, L10-12 and P4332, L3-5, seems to be too high (contrast with *Miralles et al., 2010; Loew and Schlenz, 2011*). Please revise these parts.

P4315, L4-6: The International Soil Moisture Network (http://www.ipf.tuwien.ac.at/insitu/) initiative is trying to establish and maintain a global in-situ soil moisture database (*Dorigo et al., 2011*). Please cite it here. Moreover, other regional networks are already present (REMEDHUS, SMOSMANIA, Illinois, Oklahoma, ...).

P4315, L19-21: Cite also the AMSR-E (*Njoku et al., 2003; Owe et al., 2008*) and WINDSAT (*Li et al., 2010*) sensors that are currently used for soil moisture estimation.

P4316, L6-7: Cite also *Albergel et al., 2010; de Rosnay et al., 2010; Draper et al., 2011* as recent studies assimilating satellite soil moisture observations into meteorological models.

P4316, L18-19: Basically, both *Draper et al., 2009b* and *Rudger et al., 2009*, among many others, obtained that the AMSR-E soil moisture product derived by the Land Parameter Retrieval Model, LPRM *(Owe et al., 2008)*, provides the better agreement with in-situ observations.

P4316, L23-24: Throughout the paper it seems that the air temperature (usually measured 2m above the ground) is referred with different terms (surface temperature, screen temperature, ...). Please be consistent to avoid confusion.

P4316, L28: Please specify the acronym T/q. Also UM at P 4317, L2, Met at P4317, L4, ... and others (MOSES, BUFR, RMS, ...).

P4316, L4: Please add a reference for the Meteorological centres that uses bias cor-

rected satellite soil moisture data. Operationally or for research purposes?

P4317, L12-13: Why is the bias corrected satellite soil moisture more likely to improve model surface fluxes? Please specify better.

P4320, L11-15: Equation (2) has general validity. It was developed for Ukraine but also applied in other countries (e.g. *Ceballos et al., 2005*). However, likely it is better to remove it because the SWI is not used in the paper (see General Comments).

P4321, L11: Please specify that the results of *Naeimi et al. (2009)* refers to ERS-scatterometer soil moisture data and also the obtained performance (e.g. in terms of correlation coefficient).

P4321, L21: Please specify here the ASCAT soil moisture product that is used in the study (operational or reprocessed?).

P4322, L5: What does "climatology" mean? Please specify.

P4322, L10: Please remove the reference to bufr code. Also below in the paper.

P4322, L24: How is it possible to compute different values for $m_s(t)$ and $\theta_{UM}(t)$. Are they computed for a given area? Please specify.

P4323, L7: How is the fraction of vegetation cover computed? Which is the reference data set used for that?

P4325, L12: I do not understand why is the reference to Fig.4 reported here?

P4326, L4: What is the ASCAT super-ob value θ_{scat}? Is it θ_{scat} after the quality control?

P4326, L2-15: Please mention here *Brocca et al. (2010b)* because they recently applied the same nudging scheme used in this paper. Moreover, I think that a K-value equal to 0.5 is quite high. In fact, the assumption that the model and observation error is usually not correct (model error is usually lower than observation error).

P4331, L15: "For operational use K =0.2 in Eq. (10)". The sentence should be revised.

Additional References

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