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Hydrology and Earth System Sciences Discussions

Interactive comment on "A bare ground evaporation revision in the ECMWF land-surface scheme: evaluation of its impact using ground soil moisture and satellite microwave data" *by* C. Albergel et al.

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The authors thank the anonymous Reviewer #1 for his/her review of the manuscript and for her/his fruitful comments. For an easier comprehension, general comments of the Reviewer are also reported (1.XX).

1.1 [My only major criticism is that I found the science content of the manuscript to be a little modest. My impression (which the authors might want to correct) is that

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Interactive Discussion

Discussion Paper

the major conclusion of the paper is that increasing soil evaporation leads to lower surface soil moisture and higher brightness temperatures which – in turn – partially resolves known bias issues in ECMWF's land model. This is (arguably) not a particularly surprising and/or interesting conclusion. One area of potential contribution could be a better description of why the high-bias in TESSEL soil moisture predictions was attributed specifically to a soil evaporation problem. There are a lot of processes that impact soil moisture – why did ECMWF narrow in on modifying their soil evaporation parameterization? One easy way to (potentially) address this issue is to look how errors in "old" version of TESSEL vary as a function of bare soil fraction: : :if larger errors (i.e. larger wet biases) are found at sites with larger bare soil fraction then that a strong piece of circumstantial evidence that they have correctly attributed the problem (to a process whose magnitude depends on the amount of bare soil present: : :as opposed to a process like gravity drainage which does not). I'm not sure if this type of analysis has been presented previously in the literature (ine.g. one of the earlier Balsamo et al. papers?). But, even if it has, it would be usefulto repeat it here using the author's new off-line set-up and SCAN data sets. Including this type of attribution analysis would make the manuscript of much greater interest for the general land surface modeling community.]

Response 1.1

Reviewer #1 is right at this point. The use of in situ observations from various networks across the world in different biomes and climates (Albergel et al., 2010, 2012 a & b of the reference list) highlights a general overestimation of ECMWF soil moisture products compared to in situ observations, particularly on dry/bare ground areas. Although not shown in this paper, effectively areas with a higher fraction of bare ground present larger negatives biases (in situ minus model). Using the NCRS SCAN network, considering all the stations, bias is on average -0.079 m3m-3. If we consider now stations with a bare ground fraction equal or higher than 0.4, it is -0.094 m3m-3 and -0.100 m3m-3 for stations with a fraction of bare ground equal or higher than 0.6. Larger negative

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biases are found in areas with high fraction of bare ground. As mentioned by Reviewer #1 (please see 1.7), both Table 3 and Figure 2 are redundant. Following the reviewer recommendation, Table 3 has been replaced by a description of the bias as a function of the fraction of bare ground.

P.6729, L.4, the following paragraph (and associated Table) is added in the revised version of the manuscript: "Table 3 presents the bias between stations of the NCRS-SCAN network and BEVAP_OLD. When biases are computed for stations with a fraction of bare soil greater than 0.6, bias is on average -0.100 m3m-3, it is -0.079 m3m-3 when computed for stations with a fraction of bare soil greater than 0.1. These results indicate that ECMWF soil moisture product negatives biases are more pronounced in areas with a high fraction of bare soil and support the modification of the wilting point in Eq.3 to a weighted average of the wilting point in Eq.5, taking into account the vegetation cover fraction (Eq.6)."

P.6729, L.11: "Results are presented in Table 3 and the RMSD difference between BEVAP OLD and BEVAP NEW as a function of the fraction of bare ground is displayed on Fig. 2." Is now: "The RMSD difference between BEVAP OLD, BEVAP NEW and stations of the NCRS-SCAN network as a function of the fraction of bare ground is displayed on Fig. 3."

1.2 [Line 11-12, Page 6718: ": : :;NWP analyses hardly have their control experiments." I think I understand the point here, but it's phrased awkwardly. Consider re-phrasing.]

Response to 1.2

For a better understanding and following the reviewer recommendation, the last part of the sentence ('NWP analyses hardly have their control experiments') has been removed in the revised version of the manuscript:

P.6718, L.8-12: "Because the improved bare ground evaporation was implemented in 2010 along with others modifications 10 affecting soil moisture (e.g. an Extended

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Kalman Filter for soil moisture analysis, de Rosnay et al., 2011, 2012) it is difficult to isolate the impact of the new evaporation scheme."

1.3 [Line 10, Page 6720: add "model" to the end of "an improved soil hydrology"]

Response to 1.3

It is now added to the revised version of the manuscript. P. 6720, L.10: "[. . .] an improved soil hydrology model"

1.4 [Page 6729 and Table 2: It took be a awhile to realize that the "Fraction of Bare Soil" is a minimum threshold in Table 2 (i.e., it's not that 122 stations have zero bare soil, it's that 122 stations have a bare soil fraction GREATER THAN zero: : :correct?). This point should be clarified. Same issue with the x-axis label of Figure 2.]

Response to 1.4

Yes it is correct, (Reviewer #1 is probably referring to Table 3 instead of Table 2), it is now replaced by 'Bare soil fraction threshold' in (i) Figure 2 (x-axis) and (ii) P.6729 of the revised version of the manuscript. As mentioned by Reviewer #1 (please see 1.7), both Table 3 and Figure 2 are redundant. Then Table 3 is removed and replaced by a description of the biases function of the fraction of bare ground (please see 1.1).

1.5 [Line 22, Page 6734 – Say "increased" instead of "enhanced": : :enhanced implies "improved" and that hasn't been shown directly here.]

Response to 1.5

P. 6734, L.22: "increased" replaced "enhanced" in the revised version of the manuscript.

1.6 [Figure 5/Figure 6 – Add TB look angle to captions.]

Response to 1.6

TB look angle (40°) is added to captions of both figure 5/6 of the revised version of the

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manuscript.

1.7 [Are both Table 3 and Figure 2 necessary? They seem somewhat redundant.]

Response 1.7

We agree that both Table 3 and Figure 2 are providing similar information. Table 3 is removed and according to Reviewer #2, Figure 2 is modified for a better understanding in the revised version of the manuscript. Table 3 is replaced by a description of the biases function of the fraction of bare ground (please see 1.1).

1.8 [Table 4 – Here and throughout the manuscript, clarify what type of correlation is being referred to here (anomaly or raw..via eq. 7 or eq.9).]

Response to 1.8

This information is now added throughout the revised version of the manuscript. Also in Tables 2 and 4 correlations of anomaly time-series are added.

1.9 [As noted in the text, very large biases remain relative to SMOS Tb observations (20 K in TBH!) – even after the implementation of the soil evaporation modification. Given that TESSEL surface soil moisture estimates have been effectively de-biased, the issue must be the parameterization of CMEM...correct? This is arguably outside the scope of this paper but the author's might want to give more information on this parameterization and provide some indication on the direction ECMWF intends to go in the future to address this residual bias issue.]

Response to 1.9

Reviewer #1 is right at this point. Indeed, soil moisture is only a component (although an important one) of the mean bias obtained between SMOS observations and model equivalents. This is why in the main text we added the sentence " The latter are closer to SMOS observations but with large global mean differences and standard deviation (about 10K and 20 K, respectively)", to acknowledge that it is not only an improvement

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in soil moisture which is going to remove the bias.

In order to make this point more clear we have added the following sentence in the next version of the manuscript:

P6733-L13: "These residual biases are also related to other factors such as the SMOS instrument or input parameters of the radiative transfer model, which are not straightforward to assess. Radio frequency interferences affecting the SMOS measurements could also be responsible of the bias ".

And in P6733-L12, we have substituted: "..to SMOS observations but with large global mean differences.." by "..to SMOS observations but with still large global mean differences.."

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6715, 2012.