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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.

Anonymous Referee #1

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

This is a well-written and technically sound description of important, on-going activities at ECMWF to improve their land surface modeling capabilities in preparation for the enhanced assimilation of satellite-based land surface products. 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 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 (in e.g. one of the earlier Balsamo et al. papers?). But, even if it has, it would be useful to 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.

Minor points:

1) 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.

2) Line 10, Page 6720: add "model" to the end of "an improved soil hydrology"

3) 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.

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Interactive Comment

4) Line 22, Page 6734 – Say “increased” instead of “enhanced”. . .enhanced implies “improved” and that hasn’t been shown directly here.

4) Figure 5/Figure 6 – Add TB look angle to captions.

5) Are both Table 3 and Figure 2 necessary? They seem somewhat redundant.

5) 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).

6) 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.

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