

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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General comments

The paper presents some modifications to the evaporation scheme of the ECMWF land surface model, and shows that with the new scheme the soil moisture (SM) from pixels with a relatively large fraction of bare ground agree better with in situ SM observations (directly) and with SMOS observations (indirectly, through mapping of SM

C3299

into microwave brightness temperatures). Given that the main objective seems to be to improve the evaporation, and that the authors seem happy in evaluating their estimates with in situ observations, I am wondering if anything in terms of using in situ evaporation observations has been considered during this work, or if there are plans for this and they could be discussed in the paper. Even without any comparison, some simple figures, as basic statistics reflecting the changes in evaporation (differences between old and new runs) will help putting in perspective the this new formulation in terms of surface fluxes. It may also be worth discussing the choice of one specific soil moisture network for this work. Given the very recent years where the study is conducted, one may expect that more in situ SM observations are available and may allow extending the study to other regions. In our experience, land surface models tend to work better over regions where more ancillary data to parameterize the model exist (e.g. soil texture, porosity, etc), precisely the regions that are later on used to evaluate the model (and US may be one of those regions). The comparison in SMOS brightness temperature space is certainly very preliminary, but useful to indicate the difficulties of assimilating observations related to a quite uncertain (for all land surface models, not just for the ECMWF land scheme) model state parameter.

The paper is well written, and the subject is of interest for HESSD readers. Their contents are well presented in general, but I have some doubts about the need of the discussion section. As it is, it seems to me that there is anything new there that is not already commented somewhere else in the text (apart from perhaps the discussion on assimilation of precipitation, which could be moved to the conclusions). For instance, the main numbers about the study are given again here, and repeated again in the conclusions. Figures may need some work and/or additions to make them more readable/interesting.

Specific comments

P6727-L8. I understand the message the authors are trying to pass here, but to say that in situ data contains “representativeness” errors may not be the best way of presenting

C3300

this. As it is expressed here, all the “blame” seems to go to the in situ observation, but what about the 6400 km² spatial representativity of the off-line model runs? If I get this right, the station may be located and/or correspond to one specific biome within the ECMWF pixel, as the ECMWF uses a tile scheme, while the compared SM from the model is the integrated value for the whole pixel. For the same reason than a tile scheme is justified in a model, it is easy to also imagine some combinations of surface characteristics, climate, and hydrological conditions where a in situ station placed in a reasonable location cannot capture the integrated response from the 6400 km² around the station (without being anything wrong with the station measurements or location strategy).

P6728-L25. I see the point of filtering the in situ dataset so only stations that reasonably correlate with the model values are retained for the analysis. But perhaps the name of “quality control” is not the most appropriate. I can imagine again places where there is nothing wrong with the station sensors, but geographical particularities or even wrong modeling resulting in low correlations.

P6729-L3. What is it meant by “does not alter the conclusions of the paper”?

P6729-L14. Selecting stations for a given bare ground fraction can also selects different climate/hydrological conditions, which can be affecting the comparisons. For instance, more bare ground and less vegetation could be a relative indication of a drier climate, less precipitation, stronger seasonal cycles, etc. This can result in higher absolute correlations and smaller RMSD. This may be worthy mentioning in the context of comparing statistics before and after the threshold is selected.

P6729-L28. True, but a very small increase. The decrease in RMSD seems more significant.

P6731-L1. What angles are used for the simulations? I’m assuming a choice has been made.

C3301

P6731-L10. Monthly mean “difference” mean “biases”? 4.7 should be 4.72? 14.85K for an annual value, is it bias (too high?) or SD (more likely)? 4.12 to 4.14K? 3.7K is now bias and no SD?

P6731-L14. It would help to evaluate the TB differences to also see a map of the soil moisture differences.

P6731-L26. Choice of angle based only on data availability? Is there an optimal angle (or angle range) for SMOS observation where the soil moisture may be better mapped?

P6733-L9. What is it meant here by “sensitivity” and what does the figures given mean? Changes in TB with respect to changes in soil moisture? This may require to be elaborated a bit more.

P6733-L13. It will be helpful to point out that the bias between SMOS observations and ECMWF simulated Tbs is not just related to the SM, but that other factors are also important (e.g. SMOS internal calibration, assumed model soil roughness for the RT simulations, other input parameters affecting the radiative transfer, or the radiative transfer itself). I guess that the authors have some idea about the expected sensitivity of TB to changes in soil moisture, so you are in the position to say whether the biases observed at the moment could be “fixed” by realistic changes in soil moisture, or other elements in the RT model or the input parameters also need to be investigated (I guess so, as a CMEM platform calibration is suggested).

Figures

Fig 1. The huge title in that figure seems awkward. The chose of colors in the scale does not help to interpret the figure, a larger number of color will help interpreting the map.

Fig 2. Both RMSD and number of stations are discrete quantities, plotting one as a solid line, the other in dot form is confusing, both solid (with markers) or both in dot form (different shapes)?

C3302

Fig 3. ut_XXXX meaning? Explain in the figure caption, or remove? Year 2010 added twice? Adding the location of the station (lat-lon, station name, type of biome) may be valuable information. The lack of in situ SM for J-F-M is because the ground is snow-covered?

Fig 4. Same comment than in Fig 3 about ut_XXXX. More uniformity in Fig 3 and 4 may be an interesting addition to the paper. For instance, just one example of the off-line run but 3 for the IFS? It would be very interesting to see the same 3 examples of Fig 4 also used for Fig 3, so we could also see the effects of different spatial integration and/or other changes in the ECMWF scheme by comparing the ECMWF SM for the off-line and IFS runs for 2010.

Fig 5. As most of the differences are positive, the map will be better read if a larger part of the color scale is used for the positive differences. As I said above, it would be nice to see also a map of the soil moisture differences.

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C3303