Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-414-RC1, 2016

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Assimilation of SMOS Brightness Temperatures or Soil Moisture Retrievals into a Land Surface Model" *by* Gabriëlle J. M. De Lannoy and Rolf H. Reichle

Anonymous Referee #1

Received and published: 19 September 2016

General Comments:

This work is trying to assimilate SMOS brightness temperature, soil moisture product and TB difference between SMAP and SMOS into GEOS-5. The outputs are compared with each other, as well as against the SCAN and USCRN in-site soil moisture networks. By using strict mathematical test, inputs quality control and assimilation scheme, the result is reliable and reasonable. The method established in this paper imitates the SMAP Level 4 product but expands its technique. I think it is very useful for the future application of SMOS/SMAP product and further soil moisture-weather/climate model researches. This paper is recommended for publication.

However, I have some doubts, not necessarily comments to the authors:

Line 6, section 3.1, Is the spatially distributed the same concept as three-dimensional? If each grid was updated separately as said in Line 6-7, then which three dimensions are they?

The variables listed in P4, Line 26-30 will be updated after assimilation loop as described in P7, Line 11-15. The variables includes part soil moisture/temperature defined in the land surface model but not all of them. In this case, the soil moisture/temperature will partly altered by the assimilation, is that right? How do the authors select which layer should be assimilated into? Will this selection affect the weather forecasting?

Figure 7 indicates the changes due to assimilation. Large marks are assigned for statistically significant sites but it is really hard to distinguish them from the rest. Maybe the authors could use other symbols or give some explanations on how many sites are improved actually. Besides, what is the evaluation of simulation result without data assimilation while only changes are illustrated in Figure 7? I see the magnitude in changes is quite small, about 0.01, which is lower than the accuracy of SMOS/SMAP mission. If the model simulation doesn't match well (for instance, difference larger than 0.1), how important the improvements brought in by assimilation should be reconsidered. I know the comparison between model and in-site observation is a very complicated issue and may be too much if it is discussed in this paper. What I recommended is to add some simple figures which could give an estimation of model v.s. observation difference, without assimilation.

For Figure 8, the problem still exists. Figure 8 adopts a similar method used in another paper in De Lannoy and Reichle, Journal of Hydrometeorology, 2016. It is for sure that the correlation increase indicates the effect of assimilation but the correlation increase doesn't mean the absolute soil moisture value is improved. Usually in the current forecast model, soil moisture is a diagnostic variable which does not interact with the atmosphere directly. As mentioned in Line 21, if the soil moisture will be used to improve weather forecast, its absolute value is more important for evaporation/Bowen ratio calculation. By enlarging or restricting soil moisture, the land surface model in climate forecast could also collapse while correlation coefficient increases. Without seeing the soil moisture time series for particular site, or at least any time–series which reflect the variation, the conclusion that assimilation improved soil moisture simulation should be made with caution.

Minors:

P4, Line 3, "][" should be replaced by "[]" P4, Line 10, What is the SM uncertainty? Is it one of the products from SMOS?

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