Hydrol. Earth Syst. Sci. Discuss., 8, C5093–C5095, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C5093/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Spatial horizontal correlation characteristics in the land data assimilation of soil moisture and surface temperature" *by* X. Han et al.

Anonymous Referee #2

Received and published: 25 November 2011

The paper presents an interesting topic, but has several critical technical deficiencies. Major revisions are required before it can be accepted for publication.

MAJOR COMMENTS:

1. The experiment setup is extremely selective in that the assimilation is done only for a single day, at a single time instance. Though the results indicate that there is value from incorporating spatial correlation information, it is not clear how these improvements translate (and grow) with time. In order for this study to be relevant, the period of assimilation should be extended to include several days (I would consider

at least several months during a summer season), so that the dynamic variabilities in soil moisture (and temperature) are incorporated in the assimilation setup. Are the improvements more significant during dry/wet/drydown periods?

2. It is not clear why a single day in september is used. Isn't it better to choose a day in summer where any potential effects of winter season can be ruled out? This domain seems to be at a high latitude and the temperature fields indicate that the ground is frozen at several locations of the domain. In this case, how was soil moisture (and temperature) fields updated? From my understanding, the state variables in CLM account for both liquid and frozen part of the soil moisture storages. Which of them were updated? The article provides no mention of the details of the state vector that is updated in the assimilation scheme.

3. The experiment uses an identical twin setup in that the same model is used in assimilation and for generating synthetic observations. However, the observations are generated from a different year (2007), which is likely to introduce systematic biases. The assimilation algorithm is designed to only account for random errors. I suggest that the authors include some description of potential biases in the experiments. I would also include bias errors along with RMSE and NSE.

4. Here the masking of the observations is done based on cloud masks, based on MODIS - a visible sensor. The soil moisture observations typically are measured from microwave sensors, which do not have issues observing through clouds. They, however, have difficulties with dense vegetation and such. The temperature sensors on the other hand are from visible sensors and they have difficulties with clouds. These issues are ignored in the experimental setup. To be clean and to mimic a real environment, it is better to apply different masking schemes for both types of observations and assimilate them separately.

5. Soil moisture and surface temperature have very different temporal behavior in terms of their memory. Soil moisture has more memory, where as the skin temperature

HESSD

8, C5093–C5095, 2011

in models have no memory because the model assumes a low (close to zero) heat capacity associated with the surface layer. When assimilating skin temperature obs, the increments do not persist in the system as a result of this low memory. Unless more advanced diurnal updating schemes are used (Reichle et al. 2010), the assimilation do not yield any improvement except at the time of the assimilation. These issues require that the period of assimilation be extended temporally. Otherwise, it is not clear if the improvements seen here will translate in time.

MINOR COMMENTS: 1. Abstract: I'd modify to state that LETKF is used as the assimilation scheme and to say that the upper limit of 9 observations is in the 'spatial' sense.

2. Section 3.1: Better notation is to say "2008, 06Z" instead of "2008, 06"

3. The identical twin experiment setup typically overestimates the model performance – It is in general easier for one model to simulate its own simulations (please see Kumar et al. 2009, "role of subsurface physics in the assimilation of surface soil moisture observations", J. Hydromet. So the results in this paper may be an overestimation of the potential improvements you could get. A acknowledgement of this issue would be helpful.

4. A mention of the following reference may be appropriate for the background section: De Lannoy, G.J.M., Houser, P.R., Verhoest, N.E.C., Pauwels, V.R.N. (2009). Adaptive soil moisture profile filtering for horizontal information propagation in the independent column-based CLM2.0, Journal of Hydrometeorology, 10(3), 766-779.

5. It would be instructive to provide measures of statistical significance to the values in Tables 2 and 3. Are the improvements on the order of 6-9 percent statistically significant?

6. Labels in Figure 2, 6, and 7 are hard to read.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 8831, 2011.