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Hydrology and Earth System Sciences Discussions

Interactive comment on "Improving simulated soil moisture fields through assimilation of AMSR-E soil moisture retrievals with an ensemble Kalman filter and a mass conservation constraint" *by* B. Li et al.

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In this study authors have introduced a new methodology that preserves the mass balance during the assimilation of observations. Specifically the first two layers of the model is updated via assimilation of observations whereas the added/subtracted water to/from the surface is taken/given from/to the root-zone. This is a new methodology and a timely study that addresses a problem that has not attracted much attention. It is a valid approach, however there are some concerns that authors may clarify.

1) The climatology of models may not be right, and it is a very well known case (Koster et al. 2009, J. Climate, 22, 4322–4335). As a part of GSWP2, Koster et al. studied 7 to 15 participating models and concluded that “model-simulated soil moisture variables differ from each other and that these differences extend beyond those associated with model-specific layer thicknesses or soil texture”. They also add “LSM derived ‘soil moisture’ is not (as its name implies) a physical quantity that can be directly validated with field measurements”. Here the model based soil moisture values are only an index of wetness: when it rains it gets wetter and when it does not it gets drier. As Koster et al. tells “true information contentâĂŤand thus valueâĂŤof a model soil moisture product lies not in its absolute magnitudes but in its time variations”. Therefore, we do not trust the model climatology to start with. Briefly, could authors explain why correct the climatology of a model that we do not trust?

2) One way of correcting the model climatology could be done via scaling the soil moisture using the first moments as Koster et al. (2009) suggests. Why use complicated data assimilation methodologies to correct the climatology of a model that can be corrected via simple regressions? Is it any better? As we can see in the figures of this current study, the soil moisture values at 100cm depth of Control, DA, and DA MassCon never come close anywhere near SCAN datasets (Fig. 4). Could regression based correction do a better job?

3) Here the implemented EnKF methodology is not consistent with its theory. Hence EnKF performance comparisons in its current form may not reflect the results that could be obtained using consistent methodology.

The theoretical background of land data assimilation comes from Kalman Filter, which solely is based on the goal of reducing the random error component of the model using observations (=the goal is not correcting the climatology). This theory explicitly requires the innovations to be white and non-biased. On the other hand, in this study authors have not performed a bias correction, because they claimed the mean soil moisture may have information that can be used. However, the presence of biased innovation

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clearly does not fit to the Kalman Filter theory.

I understand authors point that the model climatology can be wrong (and in fact it is in this study) and matching observations to a model with a wrong climatology may not be intuitive. However, this can be fixed by matching observations to model and then the assimilated soil moisture values can be climatologically corrected against the in-situ data once the assimilation of observations are completed.

If EnKF is not done properly, then not surprisingly any climatology correction methodology can beat EnKF, although correcting the climatology is not the real goal of EnKF. Here the question is: can this new methodology produce smaller random errors than the standard EnKF that is climatologically corrected via a post-processing?

4) Updating the first two and the last two layers with an opposite sign creates an artificial vertical gradient between the 2nd and the 3rd layers. Any comments on the effect of this artificial vertical gradient?

These adjustments with opposite signs could be the cause of the high baseflow values we see in Fig 8. Since AMSRE is drier than the model, assimilation of AMSRE using DA MassCon persistently subtracts soil moisture from the top layers and this subtracted water will be persistently added to the lower layers. As a result of the added root-zone soil moisture, the baseflow of DA MassCon becomes higher than both the Control and the DA experiments. Accordingly, the baseflow increase in Fig. 8 perhaps is not related with the rainfall as authors claimed (-same rainfall is used in all experiments-), but it is related with the artificially added soil moisture.

5) Pan and Wood (2006) introduced a methodology that completely preserves the mass balance. Is there any reason why authors have not followed this solution? Is there a problem with it? Given DA MassCon has the artificial gradient (discussed above), the solution of Pan and Wood could be desirable as it redistributes the added soil moisture to all water balance elements rather than a single one. What additional benefits do we get by using DA MassCon when compared to the solution of Pan and Wood (2006)?

Just to note, all necessary inputs that are required for the solution of Pan and Wood (2006) is available in this study too.

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