We appreciate your interest in our work and thank you for your comments which touch on some very fundamental issues regarding soil moisture and its role for land surface modeling. The followings are your questions/comments and our responses.

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 content and thus value of 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?

The notion that "LSM derived 'soil moisture' is not (as its name implies) a physical quantity that can be directly validated with field measurements" may be true for some land surface models but not for all models. For example, the prognostic variables in the NASA Catchment model (Koster et al. 2000) represent water excess and deficit relative to the hydrostatic condition and therefore are not directly comparable with real-world states; as a result, soil moisture diagnosed from these variables may not be comparable with field measurements, except its temporal variability. But for models that are based on the Richards equation such as Noah, CLM and VIC, the state variable, soil moisture, and parameters such as hydraulic conductivity, are physical quantities that can be measured in fields or laboratories. There is no reason that simulated soil moisture from these models (Noah, CLM and VIC) cannot be compared to field or retrieved soil moisture values and, in fact, many studies compared these model outputs with in situ measurements (e.g., Mitchell et al., 2004; De Lannoy et al., 2007). As illustrated in this study, model estimates may not match observations exactly which is due to uncertainty in input forcing, ET/runoff, and hydraulic parameters as well as deficiencies in model physics, not because the physical meaning of Noah soil moisture is inconsistent with that of field measurements.

The goal of our AMSR-E data assimilation was to nudge estimated soil moisture in its full magnitude (versus anomaly) towards the true soil moisture represented by AMSR-E retrievals. Improving the full magnitude of soil moisture should lead to improved model climatology (or mean), but its impact goes beyond climatology as the temporal variability of soil moisture is also affected by data assimilation. The anomaly correlation was not evaluated in the study because the simulation period is too short to generate meaningful climatology.

The need to correct the mean of estimated soil moisture comes from the fact that flux estimates depend on the actual value of soil moisture as shown in Figure 7. In addition, due to the non-linear relationship between soil moisture and other physical processes, there is no guarantee that inaccurate mean soil moisture will not impact the anomaly calculation (Mo, 2008).

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?

As we indicated in the above, our data assimilation affects the full magnitude of soil moisture estimates, not just the climatology.

Koster et al. (2009) showed that soil moisture estimates from one model can be very similar to those by other models, after scaling which includes de-trending, mean-removal and normalization (i.e. their equation (1)). Scaling does not actually correct climatology; in fact, in order to restore the full magnitude of soil moisture, the climatology of the target model has to be used along with the scaled anomalies (their equation (2)). We disagree with their view that the true value of model estimated soil moisture only lies in the temporal variability, not in the full magnitude of soil moisture. One obvious reason is that flux estimates are based on the full magnitude of soil moisture not on anomalous soil moisture values; consequently, improving the full magnitude of soil moisture should lead to improved flux estimates. Using their Figure 3 as an example, had they used observed soil moisture (in its full magnitude) for their ecological model, they could have significantly improved the estimation of vegetation respiration rate.

The issue with the Noah estimates at 100 cm is due to the boundary condition, the free drainage condition, whose performance varies from region to region depending on the climate condition and how deep the groundwater table is. If there were soil moisture observations at the 200 cm depth (Noah's lower boundary) around the globe, the problem could be fixed by using a prescribed boundary condition, without the need for regression.

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 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?

You are correct that the use of EnKF in our study does not satisfy the fundamental assumption associated with EnKF (as we indicated in the Discussion session): the model and observation need to be unbiased. As pointed out by Kalnay (2008) and illustrated in this study, this assumption is hardly satisfied in reality. But, as we stated in the Discussion, without the unbiased assumption estimates may not be optimal, i.e., the estimation error is not minimized, but it is still a valid interpolation between observations and model forecasts. The issue with applying EnKF in a biased model is the loss of water budget (as specified by the precipitation) which needs to be taken care of.

Zero-mean innovations are guaranteed when models and observations are unbiased (relative to the truth). But having zero-mean innovations does not mean an EnKF is optimal or satisfies the unbiased assumption of EnKF. This is why matching the climatology of observations to that of the model, which produces zero-mean innovations, does not lead to an optimal EnKF, unless the model is unbiased (relative to the truth, field measurements). When a model is biased, assimilating climatology-scaled observations is equally inconsistent with the underlying assumption of EnKF as that without scaling.

Data assimilation studies using scaled AMSR-E retrievals (Reichle et al., 2007; Draper et al., 2009; Drape et al. 2011) have not shown any reduction in estimation errors. So it is questionable if we can compare our approach with that of the scaling approach in terms of reducing random errors.

Your suggestion of matching the climatology of assimilated soil moisture fields to that of in situ measurements may not be practical because there are simply not enough in situ soil moisture measurements for large-scale modeling.

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.

We do not consider the reversed vertical gradient artificial because it is a profile consistent with the observed soil moisture. The change in profile may slow down the movement of soil moisture from the top layers to the lower layers, but does not necessarily induce upward soil moisture flux as moisture fluxes are determined by the combined impact of capillary force and gravity. Regardless, its impact may be short lived depending on soil wetness since model physics (the free drainage condition) can smooth out the upward gradient easily, more quickly when it rains.

Yes, the increased base flow in DA MassCon was due to moving water down from the surface layer which we explained in line 27 of Page 8147. We did not say it's related to rainfall.

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)?

Pan and Wood's approach relies on an error covariance matrix to distribute mass imbalances to states, fluxes and precipitation. ET and runoff observations are not commonly assimilated in land surface models due to lack of gridded data sets. In addition, their model was calibrated against in situ measurements, so it is unknown how well the approach would work when models are biased. Redistributing mass imbalances (which are no longer anomalies for a biased model) directly to ET requires careful considerations in the assimilation algorithm to ensure that the estimated ET remains consistent with the seasonal cycle defined by the climatology of vegetation on which most models rely to obtain ET estimates. In comparison, our approach directly distributes the mass imbalances to the lower layers with some of the water partitioned to base flow through model physics. It is much simple to implement for large scale modeling. More importantly, the mass conservation scheme avoids applying the conventional EnKF on the lower layers, which can be adversely impacted by deficiencies in model physics. The mass imbalance from the mass conservation scheme is also considerably smaller because the AMSR-E related updates are limited to the top two thin layers.

We will add some of these comments to our revised manuscript.

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