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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assimilation of ASCAT near-surface soil moisture into the French SIM hydrological model" *by* C. Draper et al.

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This is a very well-written manuscript on a methodologically sound data assimilation analysis. It's examination of the added utility associated with assimilating ASCAT surface soil moisture retrievals into an operational model represents a significant contribution to the hydrological data assimilation literature. However, I do have two (related) major concerns that should be addressed prior to publication.

Major

1) Page 5439 (lines 15-20). The primary focus of the manuscript is on the correction of bias in the SIM_NRT results (relative to the baseline SIM_DEL case). This focus

is surprising given that the assimilated ASCAT have been pre-processed to be non-biased relative to SIM_NRT surface soil moisture predictions. As far as I can see - the only way the assimilation of a non-biased observation can invoke a biased response in analyzed model states is for there to be some type of non-linearity in the forecast model (i.e. some reason why the impact of positive filtering innovations are not simply canceled by negative filtering innovations in the long term...as they would be in any linear model). Consequently, I think the author's need to describe exactly what type of nonlinear mechanism is responsible for the non-mean-zero response they see to the assimilation of a mean-zero observation. Judging from Figure 8c it might have something to do with the nonlinear relationship between evaporation and soil moisture (soil moisture tends to accumulate because positive soil moisture perturbations do not impact ET due to energy-limited conditions but negative perturbations push the model into water-limited ET conditions which reduces ET...thus the net impact over time is to decrease ET which, in turn, produces a net increase in root-zone soil moisture?).

The authors are obviously aware of this issue and address it directly on lines 20-25 of page 5439...but I had trouble following their exact reasoning there (e.g. "signal of the low-biased response to individual precipitation events"...not sure what that means) and it doesn't seem like a complete explanation is possible without invoking some type of model non-linearity.

2) A second (highly related) concern is that, if the non-mean-zero response of the SIM_NRT case is actually due to a model non-linearity, than the known cause of the bias problem appears to be detached from the proposed solution. The bias is explicitly caused by a bias in the NRT forcing data but then the analysis involves a bias solution that requires a specific non-linearity in the model. So it seems like the authors are proposing an ad hoc solution that is detached from the true source of the problem. This raises the possibility of the (generally positive) results presented here being somewhat non-robust. For example, a purely-linear model would experience the same bias due to overly-dry NRT forcing but (after rescaling) ASCAT soil moisture data assimilation

would have no discernible impact on this bias. Or, again for example, if SIM_NRT soil moisture was POSITIVELY biased, than the model non-linearity would actually cause ASCAT soil moisture data assimilation to exacerbate this positive bias. This might be an unfair perception...but the revised manuscript should address this concern.

Minor point/suggestions:

1) p.5434 (lines 22-25). I had problems getting a grasp on the author's definition of the observation operator here. In (3), H is defined as a diagnostic operator (mapping between states and observations at the same time) so why is a dynamic model integration with a 24-hr forecast window required to define it? This makes it sound like the observation operator is mapping between two quantities at different times. . .which is inconsistent with the definition of H in (3). This is probably just my ignorance...but it should be clarified.

2) p.5437 (lines 10-20). Equation 1 has be inverted in order to output VSM and perform the mapping discussed here (ASCAT SDS to VSM in the SIM_NRT range)...right? If so, that should be clarified here.

3) p.5434 (lines 12). In the SEKF, does P evolve during the forecast step? The text here seems to suggest that it does but then doesn't describe how it evolves.

4) p.5437 (lines 20-25). Isn't the fact that the transferred soil moisture are unbiased (despite having the same max/min bounds) just due to non-equal skewness in the two soil moisture distributions? I don't know if you need any exotic explanation for this...maybe just say that it's well-known that modeled and remotely-sensed soil moisture almost never demonstrate the same pdf (i.e. the same 1st, 2nd AND 3rd order statistical moments)?

5) Figure 4 – explicitly define what is meant by "improvement in RMSE" in the caption. At first glance, I wrongly interpreted positive values to indicated degradation.

6) p.5444 – define "discharge ratio"

7) The data assimilation evaluation strategy applied here is very similar to the "data denial" approach applied in Bolten et al. (2010) (i.e. use good retrospective forcing to create a baseline, degrade using realistic real-time data and evaluation data assimilation based on its ability to recover the baseline). A citation using be useful to establish that this is an appropriate and accepted methodology for evaluating a land data assimilation system.

Bolten, J.D., W.T. Crow, T.J. Jackson, X. Zhan and C.A. Reynolds,"Evaluating the utility of remotely-sensed soil moisture retrievals for operational agricultural drought monitoring," IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 3, 57-66, 10.1109/JSTARS.2009.2037163, 2010.

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