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Interactive comment on "Role of forcing uncertainty and model error background characterization in snow data assimilation" *by* Sujay V. Kumar et al.

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Title: Role of forcing uncertainty and model error background characterization in snow data assimilation

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General comments: This study examines the impact of forcing uncertainty/errors on model simulations and the subsequent model error covariances and analysis increments in ensemble snow water equivalent (SWE) data assimilation in an idealized and real data case. They find that accounting for input forcing uncertainty improves both simulations. This is because without forcing uncertainty, the imposed model state per-

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turbations are not large enough to create a realistic background error covariance matrix, and thus the model states receive too much weight relative to the observations, or result in a Kalman gain matrix of zero, so that the analysis increments are essentially zero at some update times.

Overall, the study is easy to read, follow, and the figures and analysis support the conclusions. I think that acknowledging input forcing uncertainty in land-surface and hydrological model data assimilation (DA) in a more realistic way is a key step to developing useful and robust automatic DA systems. I recommend acceptance after the authors address my comments.

Major comments:

1) It would be nice to see some type of plot of analysis increments for the various experiments in Figures 2 and 3. It is clear that changing the input forcing to Noah increases the magnitude of the background error as shown in Figure 3b, thus increasing the analysis increments so that those experiments better match the observations. Analysis increments are another useful way to diagnose what is happening in the system at each DA time, and would be a useful complement to Figure 3b, especially since analysis increments are not shown, yet discussed in many places.

This could be particularly informative for the spatial runs in Figures 5 and 7, where the model performance has some spatial variability.

It may also be interesting to examine the spatial changes in the background error at key points during the accumulation and melt season.

2) Are the observations aggregated up to the model resolution for Figures 5 and 7? I believe this is a key point that needs to be clarified. The authors should describe the aggregation method, or redo the analysis if direct comparisons to the observation points were made.

Minor comments:

1) Model error background seems to be a non-standard phrasing of the background forecast error covariance matrix (e.g. Hamil et al. 2001, Descombes et al. 2015). I suggest re-phrasing it background model error, or background error.

2) It is interesting to me that the article operates with snow depth rather than snow water equivalent (SWE). Could the authors expand on this choice at all? Noah seems to have SWE as a state variable and AMSR2 does have a SWE retrieval as well, so it would be possible to operate using SWE as well, which seems like a more natural state variable to work with.

3) Line 216: Why is the AMSR2 standard error assumed to be 50 mm when Kachi et al. (2014) cite the standard error as 20 cm (200 mm)? Is the Kachi et al. (2013) citation in the manuscript giving a different standard error than the update?

4) Line 218: Was the model resolution of 25km selected to match the approximately 30-km footprint of AMSR2? If so, it would be good to state that.

5) Line 280, change stronger to larger. The authors may want to check the entire paper for instances of this.

6) Lines 306-308. The two sentences starting with “Though underestimated” and ending with “data assimilation updates” are confusing to me. What are authors trying to describe here?

7) Figures 5 and 7: The authors may want to consider having a gray color that spans zero as small error differences are likely not significant. The figure is nice as it is with the lighter shades near zero; this is merely a suggestion to look into.

8) The authors may be interested in the article Huang et al. (2016) that is in press in HESS. This article uses an ensemble of forcing data to generate and ensemble of internally consistent (with the forcing traces) initial model states uncertainty for EnKF SWE assimilation. They examine the impact of the relative weighting of the model and observational error covariance matrices. They also find similar results to those stated

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on lines 365-368 as well, if the open loop simulation has high quality forcing, DA is less beneficial. I am not suggesting the authors need to cite this paper, as I am a co-author on it; it just seems to be very relevant to the study reviewed here and some of their discussion points.

References:

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