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Hydrology and Earth System Sciences Discussions

Interactive comment on "Improving the characterization of initial condition for ensemble streamflow prediction using data assimilation" *by* C. M. DeChant and H. Moradkhani

Anonymous Referee #2

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General Comments:

Ensemble Streamflow Prediction main hypothesis for snowmelt controlled basins is that the snowpack initial conditions drive the snowmelt and summer flow. The authors try to quantify the potential improvement in accuracy and uncertainty quantification in seasonal streamflow prediction if the errors in the initial conditions, in particular snowpack, were minimized and quantified via data assimilation. The results show that the current uncertainty in the data assimilation are too large in order to see a consistent improvement either in accuracy or in the quantification of the uncertainties of the seasonal flow

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forecasts.

The paper complements previous analyses on how improving SWE via data assimilation could improve flow forecasts. The paper would benefit from adding more details on the DA (see specific comments) , and an extended literature review and related discussion in order to further define the scientific contribution with respect to those analyses: SNOTEL vs MODIS, seasonal scale vs short range prediction, different data assimilation approaches - what worked and what did not. Also, only a couple of them assessed the improvement in the probabilistic forecast sense. This is where this paper's contribution is the most significant in my opinion. It means that the paper would also benefit from improving the organization on the prediction verification section, i.e. not only assess the accuracy and the range of uncertainty but further assess the information in the uncertainty (reliability).

Suggested references:

Andreadis K. M., and D. P. Lettenmaier, 2006: Assimilating remotely-sensed snow observations into a macroscale hydrology model. Adv. Water Resour.,29, 872–886.

Wood, A.W. and D.P. Lettenmaier, 2006: A testbed for new seasonal hydrologic forecasting approaches in the western U.S., Bulletin of the American Meteorological Society, 87(12), 1699-1712, doi:10.1175/BAMS-87-12-1699.

Mcguire M., Wood A.W., Hamlet A.F., Lettenmaier D.P., 2006: Use of satellite data for streamflow and reservoir storage forecasts in the Snake River Basin, ID, J. Water Res. Planning and Mgt 132, 97-110.

Clark, and A. G. Slater, 2006: Probabilistic quantitative precipitation estimation in complex terrain. J. Hydrometeor., 7, 3–22.

Slater A. G., and M. P. Clark, 2006: Snow data assimilation via an ensemble Kalman filter. J Hydrometeor.,7, 478–493.

Clark, A. G. Slater, A. P. Barrett, L. E. Hay, G. J. McCabe, B. Rajagopalan, and G. H.

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Leavesley, 2006: Assimilation of snow covered area information into hydrologic and land-surface models. Adv. Water Resour.,29, 1209–1221.

Tang, Q., and D.P. Lettenmaier, 2010. Use of satellite snow-cover data for stream-flow prediction in the Feather River Basin, California. International Journal of Remote Sensing, 31(14), 3745-3762. doi:10.1080/01431161.2010.483493.

Specific Comments:

- describe the DA in more details: âĂć Is the DA assimilating SWE only or also other snow information like the snowpack content (depending on the model structure). âĂć Is the DA for one elevation band and one basin taking into account all the nearby SNOTEL stations in this elevation band used or which one are used and on which criteria. âĂć Is the period for training the DA approach different than the period of evaluation? I.e are those results the maximum or expected improvements from the DA?

- describe the goals of the performance metrics; the volume for accuracy, the ensemble range for the uncertainty. The RPSS is presented as a performance metrics but is not used in the results section. Also the rank histogram (should also refer to Talagrand and Vautard 1997) is used at the end and is more meaningful than the ensemble range in order to quantify that reliability in the ensemble spread. Please clarify what the QQ plot measure – is it equivalent to a reliability diagram? In the paper, the uncertainties are defined as the range of the ensemble. The larger ensemble does not imply a better representation of the uncertainties as long as this is not compared with the observed variability (the rank histogram used at the end).

- the data assimilation approach used does not specify if both SWE and the snow-pack cold content are assimilated. The authors remind the reader that the DA is not flawless, but they then assume that uncertainties in the DA approach are smaller than the uncertainties in the initial conditions. This point alone would clarify some points of the conclusions. In addition, even though the results show otherwise, the conclusions remain optimistic that an improved DA should improve seasonal flow prediction, but

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still without assessing why it did not work. The presented explanation is the inadequate SNOTEL spatial distribution and representation of the elevation band according to Figure 2. Figure 2 would be more representative if instead of showing the average absolute different it was showing the scatter plot of the SNOTEL elevations and the corresponding elevation bands for the different basins so that the diversity does not get averaged. What was the improvement in a basin where the elevation bands were the best represented? and in the worse represented?

- clarify the description on how the DA ensemble members are merged with the ESP ensemble members. (for example, just specify that from each DA ensemble members/ state – and say how many you used- and ESP is performed, driving to a seasonal flow forecast prediction of X times Y members. The figure is fine. Intuitively, it would be expected that the DA_ESP ensemble would be more reliable for shorter time steps (lead one month) when usually the ensemble is overconfident (too narrow)– but apparently it is not obvious that the ensemble reliability increases afterwards, in fact it doesn't. This is not discussed in the paper presently, although this is illustrated in Figures 5-6.

- the technique applied here is a direct insertion – i.e. the simulated SWE is 100% replaced if I understand it right. In order to better explain what is going on, I would recommend showing the current calibration performance of the model. The DA might as well be correcting for a systematic bias and improve some of the accuracy metrics – but might not be appropriate for the calibration parameters used. One of the suggested references addresses it.

- RPSS score presented in the performance metrics but not used in the results section analysis

Technical edits:

- specify Seasonal streamflow prediction in the title - p7209 – add the studies (McGuire et al. ***, Clark and Hay 2004, etc) - p7210- describe acronym NWSRFS -p7219 line 15– specify the low TWS bias. - P7215, line 17-18: please clarify the period used for

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the sequential state estimation in lieu of the spinup. Is the ensemble of state values for a similar date then? _ Figure 3: the bounds defined in a) do not correspond to the traces in b) - Figure 2, see comment earlier - Figure 1: need names or number in order to be associated with the table 1 - reference to Rank histogram also called Talagrand diagram:

Hamill, T., and S. J. Colucci, 1997: Verification of Eta–RSM shortrange ensemble forecasts. Mon. Wea. Rev., 125, 1312–1327. Talagrand, O., and R. Vautard, 1997: Evaluation of probabilistic prediction systems. Proc. ECMWF Workshop on Predictability, Reading, United Kingdom, ECMWF, 1–25.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7207, 2011.