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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Combined assimilation of streamflow and snow water equivalent for mid-term ensemble streamflow forecasts in snow-dominated regions" *by* J. M. Bergeron et al.

Anonymous Referee #1

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In this study, the authors explore the assimilation of discharge, SWE and SCA in a hydrologic model for the potential to improve streamflow forecasting in a mountainous basin in western Canada. Synthetic data sets are developed and used. The authors first determine which state variables are adequately predicted by the three data types that are candidate for assimilation. SCA was found to not be a good predictor. Then, the impact of assimilating SWE and discharge on hydrologic forecasts was tested.

Overall, this is an interesting study with good results. Forecasts were improved with SWE and Q assimilation both when assimilated individually and simultaneously. It is demonstrated that the data were useful for adjusting several model states (VOL, GWL, and SWE) in the CEQUEAU model. The methods in this paper show promise

for applications in forecasting provided the results remain consistent for non-synthetic studies.

General comments:

1) There needs to be more detail provided in the methods section. As is, it appears as though the methods are valid, but I could not replicate this study with the information provided. I find myself having to assume I know what the authors did during some steps. Therefore, specific comments about where to add necessary detail are provided below.

2) In the results section, the authors should make a stronger effort to link their findings to other studies. There are several papers referenced that explore assimilation of SWE and/or discharge in snow-dominated areas. There are also likely studies that have examined this type of data in other modeling and forecasting contexts. While there are a few comments about results from other studies, the authors should try to add more to the discussion.

Specific comments: Page 2, line 30. The last sentence might be better as a statement rather than a question. It seems out of character with the rest of the introduction.

Page 3, line 9: "such that the difference in elevation reaches about 1700m" is oddly stated. The difference in elevation between what? If 1700m is the total relief of the mountain, simply state it that way.

Page 4, line 8: US Army Corps of Engineers should be capitalized. Also, is the inclusion of "1956" intended to be a citation? There is nothing listed in the references regarding this.

Page 4, line 17: SI is not in equation 1. How is it relevant to this discussion?

Page 4, Line 18-19: As written it is implied that Hall et al. (2002) calibrated the three parameters in Equation 1. I do not think that is the case. Additional explanation of this calibration is needed. Who conducted the calibration? Was it conducted for this

region? If not, is it considered to be universally applicable?

Page 4, line 20: Tampered not tempered.

Page 6 , line 6-8: this statement is becoming repetitive. It was mentioned several times in this section that it has been shown useful in hydrologic studies. I recommend removing earlier statements like this, or combine them into one or two sentences.

Page 6, lines 14-24: It isn't clear what variables are referred to when using the term "observations". This may be stated earlier, but it would help the reader if they were explicitly stated here. In general, this section lacks detail. In what way and by how much were the data perturbed? How do you get synthetic observations by perturbing "true states"? More specific terminology and combining or pulling in information from Section 3.2 would be helpful in understanding the procedure of creating synthetic data.

The methods section includes very little description of how ESP forecasts are generated. A more thorough explanation should be provided for readers unfamiliar with the process. Please clarify whether only 20 years of meteorological data (1990-2000) were used to generate the ESP ensembles. Also, was only the mean value of the state variable predicted by the EnKF used to generate each ensemble in the ESP forecast, or were multiple state values from the state variable ensemble used?

Page 10, line 24 onward: What is the timestep of the data evaluated? Hourly, daily, etc? I cannot find where this is explicitly stated, but it is important to understanding the results of the study. If the streamflow is evaluated at a daily timestep, it makes sense that the SWE is not beneficial for predicting streamflow; however, results might be quite different if output is evaluated at a monthly or seasonal timestep. In addition, I could not find the interval between assimilation, is it done at each model timestep, daily, weekly?

Page 14, lines 1-6 and Figure 8: It is not quite clear what VOL is in the model. As presented on page 4, it appears to be water that is being routed to the outlet (i.e.

runoff). If that is the case, the quick decline in adjusting the VOL state on the CRPSS makes sense not because of a linear relationship with discharge, but because of the likely short residence time of the water represented by VOL within the watershed. The authors discuss the residence time issue in the next paragraph with respect to GWL and SML, I would like to see similar insight regarding the VOL as the linear relationship explanation is not obvious.

Page 16, lines 14-16 and Figure 10: It would be helpful to add a sentence putting results from Figure 10 in context of results from assimilating only Q or only SWE.

Page 17, lines 28-29: The SCA was not tested on the forecasts due to the lack of improvements in state variables (page 13, lines 19-20). The authors should not make any conclusions regarding the impact on forecast skill from this study, and restate this with respect to state variable improvements.

Page 18, line 4: The statements made in this paragraph are not hypotheses, they are assumptions and limitations.

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