



EGUsphere, referee comment RC1
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Comment on egusphere-2022-385

Anonymous Referee #1

Referee comment on "Snow data assimilation for seasonal streamflow supply prediction in mountainous basins" by Sammy Metref et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-385-RC1>, 2022

The paper aimed at using real data assimilation to assess how much the streamflow supply prediction can be improved by assimilating additional snowpack information. The performance of directly assimilating streamflow (Q), fractional snow cover (FSC) and cosmic ray snow sensor data (CRS) and their combination are assessed in three basins with first Sobol sensitivity analysis and then real streamflow data. The authors found that Q assimilation notably improves streamflow estimations during both reanalysis and forecast period, while additional combination of CRS and FSC data to the assimilation further ameliorates the A48 prediction in two of the three basins. Overall, the topic is of interests to operational streamflow forecasting for snow-dominated areas. The data and methods are reliable, and the results are supporting what's concluded. However, I have some major frustrations with the writing, the main figures, and some moderate concerns with the methodology, as shown below. This should warrant at least major revision.

Major:

- The Introduction will benefit from a re-structuring. For example, Line 28-30 and Line 63-65 present research questions and objectives of this study, which can be combined. Also, the authors seem to have mixed their results with the Introduction (see Line 35 and Line 75 for examples of unnecessary results), which are usually presented in the conclusion or discussion part. I suggest authors to overhaul their Introduction to give clearer outlines.
- I think there are major issues with many figures. Fig. 1: Resolution is too low, and subscripts are not recognizable. Using figures from past studies is okay, but needs better caption to describe each individual term appearing in the figure (e.g., what is AEP, what is PET? Seems Potential ET is used as a forcing but not described in your

model description part). Fig. 2 lacks description about the shading. Please add a legend. Figs. 5-7 lack the description about the variables plotted (there are need to describe them both in the caption and the main texts). This type of figure presentation is difficult to be accepted by the academic community. Suggest authors to re-draw many figures.

- Lack of variable descriptions in Figs. 5-7 is preventing readers from clearly getting your methodology: which ones are the most sensitive? (see comments above)
- I think in the Method section, it is lacking the spatial plots for the FSC and CRS measurements locations. These are key information (how much? Where are they located?)
- About Methods: the DA framework/perturbation and the model are relatively better presented. But how about the measurements? How are the FSC and CRS obtained? What about their uncertainty? How about their available number and spatial distribute (this is asked above)? I see some information is presented in Intro, but measurements uncertainty is the most important, and should receive a much more balanced writing and description in a specific Method section.
- Line 181: not sure how is the 900-member ensemble determined? Usually, we use much less ensemble members than this in DA studies. I understand the computation demand may be low for your hydrologic model, but scientifically why is this large number needed? Any justification and supporting evidence on how this satisfies your research goal? If there's a need for inflating the uncertainty, this should be clearly clarified. It may be tested results to maximize performance in Figs. 9-10, but I think understanding the uncertainties (as denoted by the spread of your ensemble) is more crucial to DA rather than to maximize performance.

Moderate concerns:

- For the Sobol indices equation, it would be better if the authors can provide detailed explanations of the variables in the cases of temperature and precipitation forcing. Also, the equation takes the presumption that the variables are independent and has known probability distributions. However, in geographical analysis it is often hard to determine whether a variable is completely independent. It would be more convincing if the authors can provide some assumptions and preconditions.
- What is the resolution of the reanalysis data used in the paper? When assimilating observation data, will the resolution differences cause uncertainty and what is the solution used by the authors?

Minor ones:

- Line 6. 'Lead to' mis-spelled as 'leed to'.
- Line 10. 'A series of' not 'a serie of'
- Line 35: 'play a role in' not 'on'
- The subtitles like "Hydrological system" do not exactly match the content.
- Line 311. Is the "prediction time" the same as the forecast period mentioned in line 72? How could the prediction time be lengthened?
- 9 and 10: to improve readability for the readers, please add the legend directly to the plots.
- What exactly does A48 represent? In line 2 it seems to mean "between April and August", while in line 20 it seems to stand for seasonal streamflow supply. This type of writing will confuse readers.