Reviewer 1

We would like to thank Reviewer 1 for the constructive comments. Please find below a point-by-point response to the review. Most of the minor comments have been addressed directly in the manuscript (highlighted in red). In the following, we address (in italic font) the major and moderate comments of the Reviewer (in bold font).

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 interest 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:

1. **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.**

The introduction has been restructured. In particular, the results paragraph has been removed, the research questions and objectives have been gathered in a single paragraph and a paragraph on the role of uncertainties in our study has been added. The introduction is indeed clearer and more impactful now.

2. **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).**

Figure 1 has been removed because it was not essential. We have decided to provide a more detailed model description in Section 2.1 and to refer the reader to the Garavaglia et al. (2017) paper for further details. The Potential ET (EvapoTranspiration) can be expressed as a forcing but is often parametrized as a function of temperature instead of an independent external forcing. Garavaglia et al. (2017) describe this parametrization.

Fig. 2 lacks description about the shading. Please add a legend.

A legend has been added to Figure 2 (now Figure 1). The shading is in fact multiple gray curves, for the 50 ensemble members, on top of each other.

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.

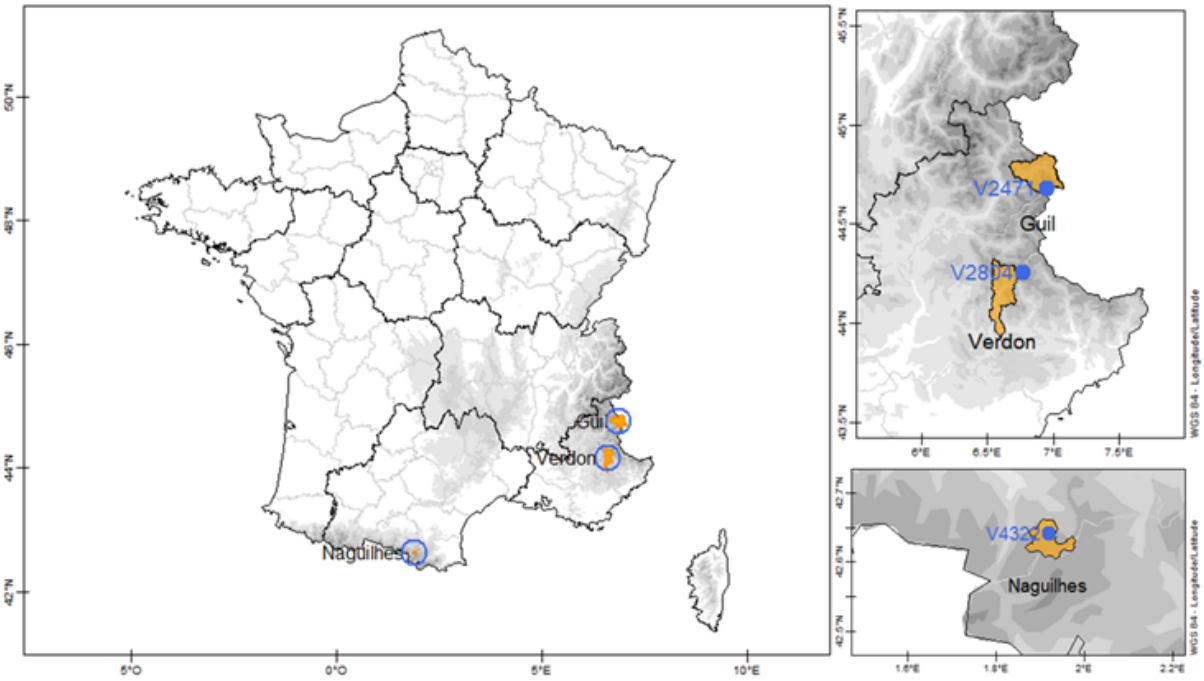
A description of Figure 5-7 has been added to both the caption and the main text. The caption now describes the figure and provides a better understanding of the results.

3. **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)**

Same answer as previously. The caption and the main text now contain indications on how to understand and interpret the results of this sensitivity experiment. The uncertainty on the snow stocks at level 4 to 7, in the Verdon and the Guil, and at levels 2 to 4, in the Naghuiles, generate the highest Sobol indices. Meaning, they are the variables that generate the most uncertainty on the seasonal streamflow supply.

4. **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?)**

Figure 3 (now Figure 2) has been replaced with a new figure that includes a zoom on each basin and displays the location of in-situ CRS observations.



5. **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.**

We acknowledge the lack of information on observations and the uncertainty related to their location and measurements. Although, the stance taken in this paper is to regard the observations as they are made available to operational centers, some more information on observation uncertainties has been added to Section 2.2. However, the main source of uncertainty is most likely due to the spatial representativity of the observations with regards to the semi-distributed model representation than from measurement errors.

6. **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.**

We use a large ensemble size to ensure experimental reproducibility, which was not ensured with smaller ensemble sizes due to high nonlinearities and thresholding steps in the model. The focus of the paper is on the information content of snow observations, not the assimilation performance, so we did not consider the ensemble size issue as a point to investigate in detail.
As this point is very pertinent for many users and was not clear in the first version of the manuscript, a small paragraph has been added to discuss it (lines 233-239).

Moderate concerns:

1. **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.**

The Sobol equations indeed make the assumption of independence. However, the Sobol indices are not used here as an attribution diagnostic, where we would need to disentangle the variable dependencies to attribute the contribution of each variable, but as a controllability diagnostic. In this sense, the Sobol indices can help us conclude that if we manage to reduce the uncertainty on the snow stocks (with the use of observations) we will be able to reduce the uncertainty on the seasonal streamflow supply regardless of the original cause of these uncertainties. A paragraph has been added to the manuscript to clarify the Sobol equations' assumptions and to explain the intent with which we use these equations (lines 173-177).

Also, a more detailed explanation of the variables behavior in response to the forcings has been added in the manuscript (lines 181-187).

2. **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?**

The model used here is semi-distributed hence only one temperature and one precipitation is prescribed on the overall region. This simplification is the source of strong uncertainties. The temporal resolution of the forcings is daily (l. 103-105). For MORDOR-SD model, the required input data are a representative estimate of areal precipitation and air temperature. Two orographic gradients are then used for describing the meteorological spatial variability at the elevation zones scale. The precipitation and temperature reanalysis are available at 1-km/1-day resolution, and are therefore averaged at the catchment's scale. The impact of the forcings resolution has not been investigated but it is likely that the main source of uncertainty on the meteorological forcings remain spatial due to the semi-distributed nature

of the model. As stated in the paper, observations are in-situ and assimilated in a semi-distributed model, which leads to inevitable representativity errors.

Minor ones:

1. **Line 6. 'Lead to' mis-spelled as 'leed to'.**

This mistake has now been corrected in the article.

2. **Line 10. 'A series of' not 'a serie of'**

This mistake has now been corrected in the article.

3. **Line 35: 'play a role in' not 'on'**

This mistake has now been corrected in the article.

4. **The subtitles like "Hydrological system" do not exactly match the content.**

The subtitle is now "Model and observations".

5. **Line 311. Is the "prediction time" the same as the forecast period mentioned in line 72? How could the prediction time be lengthened?**

The sentence at line 311 was poorly formulated. The sentence should not have said that the prediction time is lengthened but rather that the seasonal streamflow supply prediction can be improved. A new formulation is now proposed at lines 348-349.

6. **9 and 10: to improve readability for the readers, please add the legend directly to the plots.**

Legends have now been added to the plots.

7. **What exactly does A48 represent? In line 2 it seems to men "between April and August", while in line 20 it seems to stand for seasonal streamflow supply. This type of writing will confuse readers.**

The paper now defines the acronym SSS as the seasonal streamflow supply that is computed as the accumulated streamflow between April and August. Hopefully, the confusion is now lifted.