**RC1**: 'Comment on essd-2021-249', Anonymous Referee #1, 11 Oct 2021

Review of:

**The S2M meteorological and snow cover reanalysis over the French mountainous areas, description and evaluation (1958 - 2020)**
By Matthieu Vernay et al.,

The authors would like to thank the reviewer for his/her remarks and suggestions to improve the quality of this manuscript.

Major comments:

This is a paper about a very useful, unique and high-quality snow cover data set in mountain regions in Europe, which is relevant for researchers as well as various applications. The uniqueness of the data set lies in the full physically based information on snow cover properties. It is clearly worth publishing in ESSD. In fact, it contain even more than data on snow cover properties in a consistent manner (as snow cover is derived from a set of atmospheric driving variables), which are also part of the publication. Data are generally well described and presented. My comments are mainly related to the data evaluation/homogeneity and the description of the data set.

Data are derived from re-analysis simulations by well described unique models (SAFRAN for atmosphere and CROCUS and MEPRA for snow cover) originally forced by ERA-40 and ARPEGE. All models used are well described by peer-reviewed publications and are thus perfectly suited for the purpose of the application. Although the used approach of doing simulations at a spatial scale of mountain massifs results in some loss of spatial information, this is a suitable approach.

Data evaluation and data homogeneity:

Driving data of re-analysis are both ERA-40 and ARPEGE. Additionally, e.g. precipitation data are used as guess for data assimilation which are based on AURELHY interpolation for period 1958-2017 and on ARPEGE thereafter (as ARPEGE is only available from 2017 onwards). All these changes cause (or at least could cause) inhomogeneities in the data series. Even if this inhomogeneities cannot be removed in the reanalysis, their effect should be discussed and if possible quantified (e.g. showing differences for precipitation between AURELHY and ARPEGE).

First please note that the available data do not allow a complete quantification of the impact of the guess transition from ERA40 to ARPEGE.
The impact of the temporal heterogeneity introduced by the different precipitation guess has been evaluated over one single season (2017-2018) by comparing a simulation made with precipitation guess based on the AURELHY analysis method to the reference one (with precipitation guess from ARPEGE). The comparison of the simulated snow depth mean deviation and root mean square deviations (RMSD) for these 2 configurations didn't show any significant impact on the performance of the system (see Figure 1 bellow).
Furthermore, the simulated annual precipitation amount with the precipitation guess based on the AURELHY analysis method seems to be slightly higher (about 3% on average) than those simulated with precipitation guess from ARPEGE: for the season 2017-2018 the average total precipitation over the 665 stations of the simulation with the guess based on the AURELHY analysis method and from ARPEGE respectively is 1507 mm (resp. 1464 mm) with accumulation ranging from 728 mm (resp. 675 mm) up to 3125 mm (resp. 3242 mm).
This impact is now mentioned in the revised manuscript in the new Section 4.1.

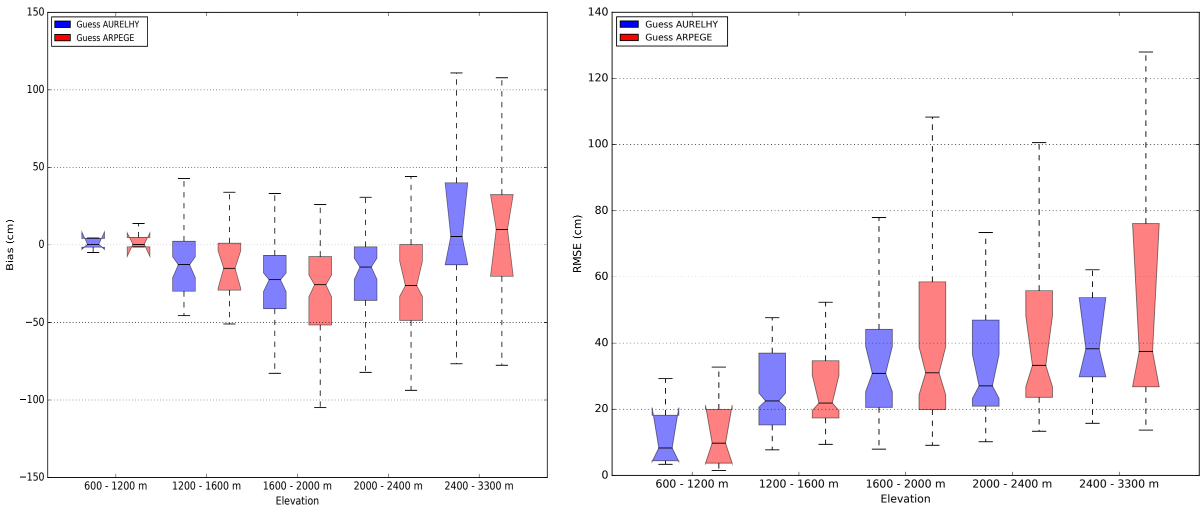

*Figure 1: Mean deviation (left) and RMSD (right) between the simulated and observed snow depth values on the 665 validation sites grouped by elevation range for two configurations of the system: with precipitation guess coming from the AURELHY analysis method (blue) and from ARPEGE (red)*

Interestingly, the temperature trend of the S2M reanalysis seems to be rather week over the period 1958-2020. This seems to be significantly weaker if compared to other data sets (as also mentioned in the paper). Are the trends described significant? It could be useful for the reader to see these differences in the trend curves in the figure (e.g. Fig. 5).

Details on simulated temperature trends and their significance have been added to the text related to Figure 6 (Line 308):
"The mean temperature trends simulated by the S2M reanalysis are +0.10°C per decade at 2700 m, +0.26°C per decade at 1800 m and +0.18°C per decade at 900 m. These trends are significant (all p-values of the trend slope significance are lower than 0.014)."

Data presentation:

The description of the S2M data set should generally be somewhat more detailed (see also the examples under "Minor Comments"). It should also be considered that non-meteorologists potentially want to use the data set and therefore the description of the metadata of the variables should be as detailed as possible.

More details have been added to the description of the data set. In particular a « metadata » section (section 3.1.1) describes the practical way to access specific simulation points and the new Table 2 summarizes the metadata which were previously not described. In addition, section 7 (data access) has been extended to guide the data downloading, and a new Table 8 in Appendix A has been added to summarize massifs information and their associated departments.

Minor Comments:

Overall, be more specific with describing surface variables. E.g. is surface temperature 2m-temperature (which is used as term several times but not in all cases, is this something different then)?

The description of all surface variables (2m-temperature, 2m-humidity, 10m-wind) has been systematically specified, we thank the reviewer for pointing out this inaccuracy.

Figures 3 and 4 as well as in the related text: Even though explained in the text, the terms “available” and “used” number of observations are misleading (as the number of used is higher compared to available). Suggest using other terms here.

The terms “available” and “used” have been changed to “Available in massifs” and “Overall used” respectively in Figures 3 and 4 (now Figures 4 and 5) as well as in the related text.

Table at page 6 (which has no number, but should have one): Which variable is used for 300hPa? Which variable is measured at 10m? Which variable is measured at 1500m (again rel. humidity)? Be more specific as in table 2.

We thank the reviewer for pointing out the missing table number. This table has been numbered (Table 1) and entirely rebuilt to be more specific as suggested.

Figure 6: Why having a scale between 0 and 240 when values in the figure are much smaller?

Figure 6 (now Figure 7) has been modified with a more consistent scale up to 120 cm, we thank again the reviewer for highlighting this issue.

4.2.3 is on trends of temperature and precipitation. However, related Figure 11 does not show trends but differences. Additionally, difference of total precipitation is in kg/m2 which is not a unity of precipitation used frequently (suggest to change).

To make the text easier to read, the term “trend” is used to describe the differences between two climatological periods. To clarify that use, the following sentence has been added at the beginning of Section 4.2.3 (now Section 4.3.3):
“Here, climatological trends are defined by the difference between the mean of a variable over two 30-year long periods (e.g. 1990-2020 and 1960-1990).”
The legend of Figure 11 (now Figure 12) has been modified anyway, as well as the difference of total precipitation unit.

3.1.2 introduces the snow cover and soil variables. I could imagine that information on snow temperature could be useful as well. Please include the time reference (e.g. UTC) of variables.

The snow surface temperature is effectively in the data set, Table 4 has been modified to be more specific. The time reference of the variables has been included.

Figure 8: Why has the trend of fraction of solid precipitation such strong increase for SON (and only for SON)? Additionally, be consistent between the figure and the figure caption by either showing trends or differences. Given that there is also an increase of precipitation and decrease in air temperature (for all elevations) for SON over 1960-90 to 1990-2020, I would expect also stronger increase for snow depth.

We thank the reviewer for this interesting remark concerning the interpretation of Figure 8 (now Figure 9).
The strong increase of fraction of solid precipitation in autumn only and the comparatively low amplitude of the increase of the simulated snow depth may be explained by an averaging effect and the very short lifespan of snow on the ground during this season. We added this assumption to the text related to Figure 9 (Line 347). The caption of Figure 9 has also been modified to be consistent with the Figure showing differences.

Figure 11: The S2M assimilation shows a clear elevation dependency of max. temperature change for 1960-90 vs. 1990-2020 for JJA, however not visible in station observations. Could be worth mentioning this EDW effect in the text.

We added a mention of this divergence in Figure 11 (now Figure 12) between the simulation and the observations in term of elevation dependency of max. temperature change between 1960-1990 and 1990-2020 in summer in the text at line 439:
"Figure 12 (b) shows that the simulated trends of maximum air temperature at 2 m in summer significantly increases with elevation up to about 1800 m a.s.l. However this elevation dependency of the simulated trend of maximum 2 m-temperature is not visible in station observations."

4.3 evaluated snow depth observations: Given its relevance, it would be good to see how trends of snow depth are captured by the S2M reanalysis (compared to the independent station data).

We thank the reviewer for this interesting suggestion. The evaluation of the simulated snow depth trends by the S2M reanalysis against the trends observed by independent station data is challenging due to the lack of long-enough observation series of snow depth. Most snow depth observations start in the 1990s and thus do not cover the 63 years of the S2M reanalysis. This temporal extent of available observations is a generalized issue over all European states (cf. Matiu et al, 2021).
However Verfaillie et al.,2018 carried out a rough evaluation of the simulated snow depth at the long-term and independent observation site of Col de Porte which is one of the few stations with available observation data going back to 1960. Figure 2 of Verfaillie et al.,2018 indicates that the trend of simulated snow depth seems to match the observed one quite well at this specific point. It is especially worth noting that the simulation matches the observed decrease of snow depth since the 1990s.

Matiu, M., Crespi, A., Bertoldi, G., Carmagnola, C. M., Marty, C., Morin, S., Schöner, W., Cat Berro, D., Chiogna, G., De Gregorio, L., Kotlarski, S., Majone, B., Resch, G., Terzago, S., Valt, M., Beozzo, W., Cianfarra, P., Gouttevin, I., Marcolini, G., Notarnicola, C., Petitta, M., Scherrer, S. C., Strasser, U., Winkler, M., Zebisch, M., Cicogna, A., Cremonini, R., 620Debernardi, A., Faletto, M., Gaddo, M., Giovannini, L., Mercalli, L., Soubeyroux, J.-M., Sušnik, A., Trenti, A., Urbani, S., and Weilguni, V.: Observed snow depth trends in the European Alps: 1971 to 2019, 15, 1343–1382, https://doi.org/10.5194/tc-15-1343-2021, 2021.

Verfaillie, D., Lafaysse, M., Déqué, M., Eckert, N., Lejeune, Y., and Morin, S.: Multi-component ensembles of future meteorological and natural snow conditions for 1500 m altitude in the Chartreuse mountain range, Northern French Alps, The Cryosphere, 12, 1249–1271, https://doi.org/10.5194/tc-12-1249-2018, 2018.