Response for Reviewer#2

First of all, we would like to thank the reviewer for the precious comments provided and for taking the time to read our manuscript.

Here our point-by-point response to the reviewer's comments:

> The Authors present an analysis of variability of snow depth and snow water equivalent from 1967 to 2020 in the Italian alps based on a large dataset of manual measurements from 299 stations collected by the Italian national electric board. The data set present the peculiarity of been conducted at fixed date during the snow season (1 February, 1 March, 1 April, 15 April 1 May and 1 June).
>
> The variability of snow depth and estimated snow water equivalent is presented, for six macro basins aggregation, in term of seasonality, elevation and non-stationarity (trends and changing points). The authors conclude on a significant decrease in both snow depth and snow water equivalent starting from the late 1980, with higher decrease at lower altitude, mainly attributed to increase 2m air temperature has seen by the HISTALP dataset.
>
> Finally, the authors propose a simple mean SWE climatology regression model for one selected macro-basin (Oglio-Chiese-Sarca) as function of seasonality (day of the year) and elevation, for the sub period 1994-2020.
>
> The paper is of interest for the scientific community working on the snow climatology, well written and in line with the HESS aims and scope, using a unique dataset collected by the Italian national electric board.
>
> Nevertheless, the overall structure is confusing in both the objectives and the use of regressive approach vs direct observations, and in my opinion the conclusion on the role of climate driver and teleconnection should be better presented/supported. In general, I think the Authors should better highlight and clarify the main contributions of the proposed contribution, which in my opinion are:
>
> #1-confirmation of a negative trend of observed snow depth in the Italian alps using a large independent dataset of manual observation with identified changing point.
>
> #2- confirmation of a major variability of snowpack bulk density along season rather than elevation using a large independent dataset.
>
> #3- confirmation of a negative trend in snow water equivalent using observed snow depth and observed /estimated(?) bulk density (based on a recalibrated regression on seasonality?), with identified changing point.
>
> #4- Identification of the major role played by temperature over precipitation by comparison of changing point obtained with the HISTALP product, and discussing the major decrease of both snow depth and SWE at lower elevation.
>
> Point #1 to #3 are results, while point #4 may be consider as discussion.

R: Thank you for pointing this out. This lack of clear statements concerning the actual objectives of the research has been highlighted also by other reviewers. We expanded the introduction, including a paragraph clearly stating what are the objectives of the current research.

"In this study, we present a detailed long-term trends and variability analysis of snow depth and SWE measurements in a wide portion of the Italian Alps between 1967 and 2020. The first objective of this research is to quantify how snow depth and SWE has changed, evaluating trends, differences and change-points during the monitoring period using an independent dataset. The second objective is to establish elevation and seasonal dependencies of snow depth and snow density. A large dataset covering a wide area and spread at different elevations like the one presented here is suitable for such considerations and for fitting simple models able to describe those dependencies, with the aim of obtaining a climatological estimate of SWE. The third objective is to understand the links between meteorological variables with snow depth and SWE. In particular, we aim to better understand what are the weights played by temperature, precipitation and teleconnection indexes."

We agree that the introduction was confusing in defining the objectives. We also corrected the abstract, accordingly. We believe that the conclusions section summarizes the main findings obtained and, with the addition made in the introduction, we clarified the main contributions proposed in the paper.

> In the following, some general comments hoping it may help the authors to increase the overall readability:
>
> -They are too many figures (11) in the current manuscript. The Authors should consider to focus on the main results, and move some figures to the supplementary to improve readability. Moreover, some of these figures are presented for a sub selection of the macro basins (figure 4, 5, 7, 13 and 14). In such case, the other macro basins should be reported in supplementary. I would suggest to limit the figures in the main text to figure 1 (maybe including figure 2), figure 6 (supporting point #1 with table 1 and 2), figure 7 (supporting point #2, improved using all macro-basins, and maybe including figure 8), figure 9 (supporting point #3, with table 4 and 5), figure 11 if better adapted (see below) to support the discussion of section 3.4 and point #4 (figure 12 can be a table or moved to supplementary) and a merge of figure 13 and 14 to present the mean snow climatology model.

R: We thank the reviewer for this comment. We agree, and decided to reduce the number of figures, partially moving some of them to the supplementary and partially merging figures together. Specifically, we would merge figures 1 and 2 as suggested by the reviewer, modifying Fig1 according to the comments received by the reviewers and the community. We agree that Figures 3 and 4 are redundant, since the information is already available in other parts of the manuscripts (MARTA triangles of precipitation and temperature and tables of the trends of HS). According to this we decided to remove Figure 3 and 4 (maybe in the supplementary). We think that Figure 5 (together with Figure 10 and 11), although limited to two basins, it still add values to the paper and is a useful graphical representation of the timeseries. We enriched the comments about such figures in the manuscript. About Figure 7 and 8, we merged those figures together, improving Figure 7 including all the basins. This is more representative since the average densities in figure 8 are computed including all basins. For figure 8 we report the standard deviation of the average densities. Figure 12 has been converted to a table, with the elevation classes as rows, according to the suggestions from the community comments.

Here the new figures:

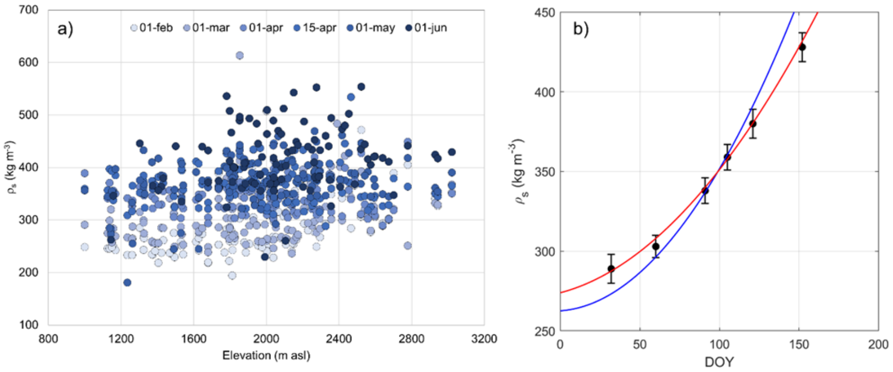


Figure 4: a) Bulk snow density dependence on elevation. Average bulk snow density for each measurement date is represented with different color intensity with changing date of the year. b) Temporal variability of bulk snow density. Average bulk snow densty for each measurement date is represented as a black diamond and the error bars represent the standard deviation. We also report in red the computed polynomial model and in blue the one proposed in Guyennon et al. (2019).

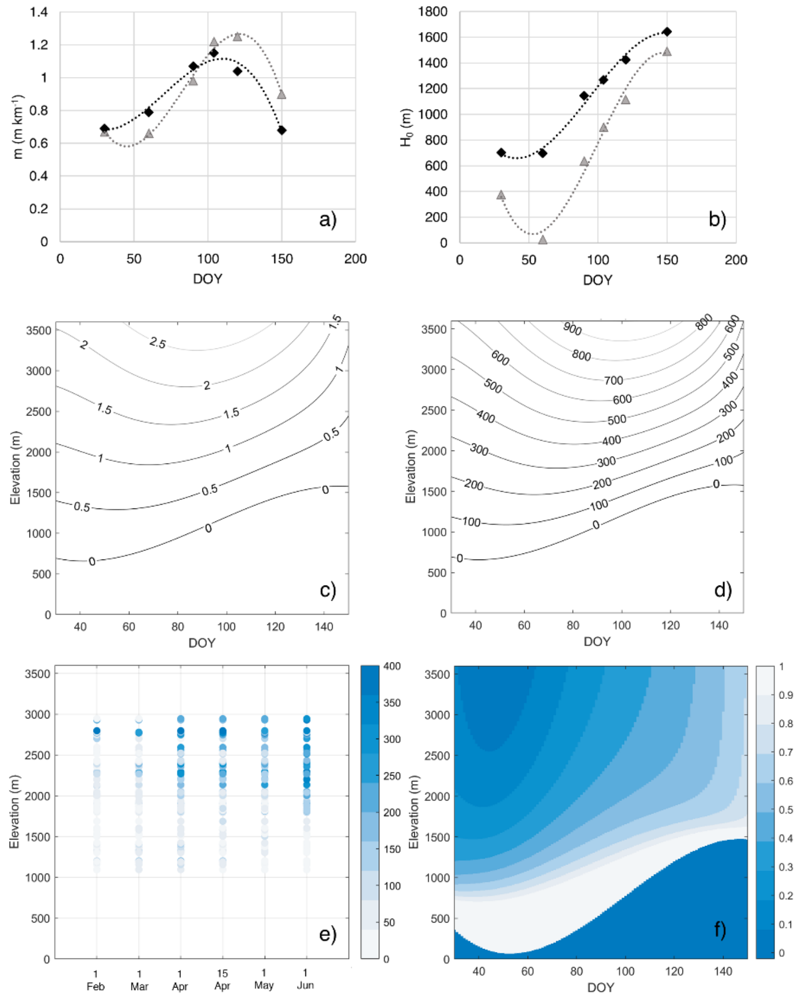


Figure 8: Slope m (a) and null snow depth elevation H0 (b) as function of the day of the year for the sub periods 1967-1993 (grey triangles) and 1994-2020 (black diamonds). Dotted lines represent the third order polynomial fitting curve (Equation 4 and 5). Countor plots of snow depth (c) and snow water equivalent (d) in the time-elevation (DOY-H) space for the 1994-2020 sub period. In panel e) the absolute average errors between the modelled and measured average SWE. Panel d) reports the comparison between the modelled climatological SWE in the first and in the second sub periods, expressed as percentage decrease of SWE.

- The mean snow climatology model could be slightly developed, improving its added value of the paper, by comparing the 1994-2020 period with the previous period. Moreover, here the choice of the sub period is based on the Mann – Whitney U test run over the dependency of snow depth versus elevation (line 288-89), while a huge effort has been done by the Authors to identify the changing point in the non-stationarity of snow depth and SWE. The authors may consider those changing points (e.g. 1988) to separate the two mean snow climatology (Note that in the current figure 13 and 14, the limitation to a single macro basin should be state in the caption)

R: We thank the reviewer for this comment. However, we decided to divide the dataset in two sub periods having the same number of years of observations for two main reason: first, such that the two sub periods could have the maximum possible number of observations to be sufficiently representative of the climatology; second, such that we could compare the results between the two periods more consistently. We developed the analysis by comparing the results for the two periods. According to previous comments, we merged figures 13 and 14 in a unique figure, adding a panel containing the comparison between the two periods.

- In general the MARTA triangles figures are not discussed in the text, excepted for the location of the changing point (also reported in table): Figure 5 is discussed line 255-256; figure 10 is discussed line 355-258 to highlight the lack of linear trend in precipitation; figure 11 is basically not discussed (while the interesting discussion on early/late season temperature trend in not supported by a figure, cf line 359).

R: We thank the reviewer for this comment. We extended the comments concerning Figures 5, 10 and 11 in the revised manuscript, discussing the role of precipitation fluctuations and temperature trends in snow accumulation. We believe that the information contained in such graphical representation and analysis of the timeseries is useful to understand variability and fluctuations along the observation period.

- The conclusions on point #4 (role of temperature and eventually role of teleconnection) need to be better supported. For exemple, the Authors may further discuss the elevation dependence of changing point (supporting the role of temperature).

R: We extended the discussion on the role of temperature and precipitation according to other references and extending the discussion abut the influence of teleconnection indexes.

- Section 2.2 line 147-154 create a lot of confusion (to me). Here the Authors state here that bulk density is reconstructed when missing based on mean values of corresponding macro basins and elevation range, while in section 3.2 a regressive model is proposed (and recalibrated) and in 3.3 the SWE non stationarity is discussed based on a “SWE estimates” (line 325) which is still unclear for me (my guess is observed snow depth and estimated bulk density as presented in section 3.2?). This should be clarified.

R: We thank the reviwer for this comment. The results related to the climatology of SWE arebased only on SWE computed from observed snow depth and observed bulk snow density or, in case of missing observation, estimated from local averaged measurements of bulk snow density. We reported that “In case there are no density measurements in the corresponding geomorphic class, we consider the SWE data for the specific date, macro-basin and elevation class as missing.”. The proposed snow density model has been used for the climatological SWE model and could be useful for readers to have an estimate of snow density for a given day of the year. However, we agree that the term estimates is confusing. We corrected it accordingly.

- The Authors may reconsider their abstract and conclusion on the light of the actual contribution (main results).

R: We completed the abstract to cover the findings related to the main objectives. . As said in previous comments, we believe that the conclusions section summarizes the main findings obtained and, with the addition made in the introduction, we clarified the main contributions proposed in the paper.

In the following some minor comments:

- line 15 please be more precise than "most of the six investigated areas"

R: Corrected as "Significant decreasing trends over the years at fixed dates and elevation classes were identified for both snow depth, equal to -0.12 m decade-1 on average in the Oglio-Chiese-Sarca basin, and snow water equivalent, equal to -37 mm decade-1 on average in the Oglio-Chiese-Sarca basin. Similar declines were found in most of the other investigated areas."

- line 25-31: The highlighted role of glacier in the introduction is somehow misleading as nothing is said about the interaction between snow and glacier in the paper.

R: We thank the reviewer for the comment. We inserted this initial paragraph to introduce evidences of modification of climate and the cryosphere in general in the Alpine Region, including glaciers retreat.

- section 2.1: It is hard for the reader to have an opinion on the choice of the measurement spatial aggregation.

R: we aggregate the data according to proximity and similarity criteria, and according to the numerosity of the data. A coarser aggregation (fewer and larger basins) would have reduced the representativeness of the series and the usefulness of the results for hydrological considerations, while a finer aggregation (more smaller basins) would have produced too localized results and, in some cases, timeseries not supported by sufficiently numerous datasets.

- in section 2.3 The average temperature over each macro-basin did non consider elevation dependence. This should be discussed.

R: Thank you for pointing out this. We included the comment: "These averaged timeseries do not take into account the altitudinal dependence of climatic variables, but they serve as a valuable indicator for assessing the average variability and trends of temperature and precipitation in the considered basins. Additionally, the use of the HISTALP dataset enables a consistent analysis across the six different basins."

- in section 2.4. The Authors cite Ranzi et al.2021 in evaluating the role of teleconnection, but it has also been done by others.

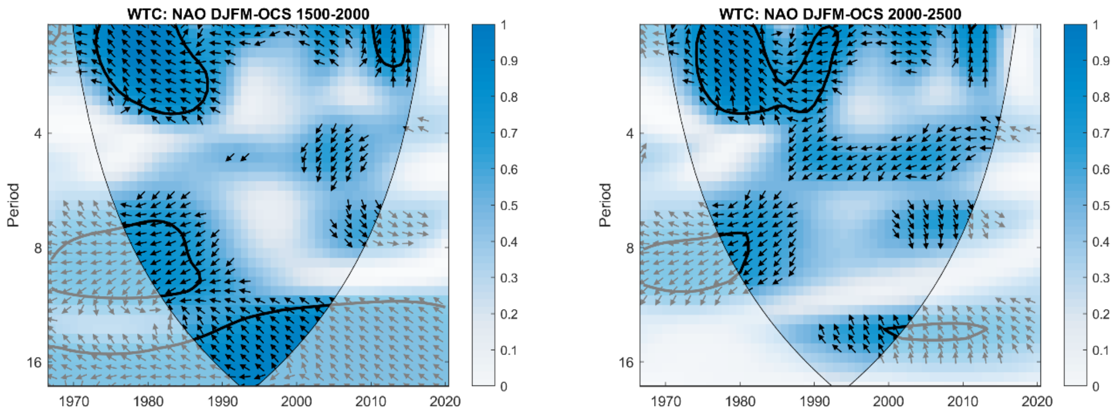
R: Thank you for pointing this out. We expanded the referenced literature as "Following an approach widely adopted (Maragno et al., 2009; Bocchiola and Diolaiuti, 2010; Diolaiuti et al., 2012; Ranzi et al., 2021), we evaluate the link of SWE with large scale circulation variability."

-in section 2.5. The Authors may better choose between parametric and non-parametric approach for linear trend estimation and associated significance. The t-test is fine for the least-square regression, while the Mann Kendall should better be associated to the Sen's slope.

R: We thank the reviewer for this comment. We believe that the proposed combination of statistical methods (least-square regression trend, t-test, Mann-Kendall test for the trends and Mann-Whitney test for the differences, the moving averages and running trends analysis combined with the Pettitt's test) provides a more than sufficiently wide representation of the analyzed series. The combination of a parametric and a non-parametric test was used to have a more robust information about the existence of a trend.

-in section 2.5. The role of teleconnection is investigated only in term of correlation over the whole period (this non considering eventual non stationarity in the response of snow climate to teleconnection indexes). The Authors may consider to use other statistical instruments (e.g. wavelet coherence or others), or to test the correlation on different base line (e.g. using identified changing point).

R: We extended the discussion about NAO and WeMO, highlighting the similar correlation previously found by other authors with precipitation and the correlation found in our results. We actually performed a wavelet coherence analysis but kept out of the manuscript for further analysis in a possible future work. Here two figures related to the wavelet coherence spectra analysis for Oglio-Chiese-Sarca basin in the 1500-2000 and 2000-2500 elevation classes. As pointed out by the reviewer, there are a lot of figures in the manuscript. We put our effort to reduce the number of those figures. For this reason, and because it is a preliminary analysis related to a single Basin, we prefer not to insert this figure into the manuscript. Perhaps, it could be inserted into the supplement. We added some final comments related to this figure in the text, furtherly expanding the discussion.



-section 2.6 (as well as section 3.5) may be renamed to avoid confusion with SWE variability analysis (3.3). "Mean snow climatology model" could be a proposition. Maybe the section could be rephrased to better introduce that such model is used for “mean” consideration.

R: Yes, thank you. To avoid confusion we accept the suggestion and changed the titles of sections 2.6 and 3.5 to “Mean snow climatology model”, better describing the content.

-section 3.1 line 300-303: The Authors state that the (snow depth) model “show less reliable” results for 3/6 macro basins. Based on table 3, I would say that the model is not working at all for Toce and Adda (r2 ranging from 0 to 0.26) while results of Adige seems rather similar to the other (with r2 ranging from 0.63 to 0.85).

R:Yes, you are right. We specified the results in the revised manuscript as “The results obtained for Adige show similar results, although presenting lower values of R2 (between 0.63 and 0.85), while the model is not reliable for the Toce and Adda basins (R2 ranging from 0 to 0.26)”

-section 3.4 The authors should consider the results of Colombo et al 2023 (DOI 10.1088/1748-9326/acdb88) on the role of temperature / precipitation.

R: Thanks, this other reference certainly adds value to the discussion of the presented results. We added the reference and accordingly enriched the discussion.

-Supplementary: The current second table in the supplementary should be better shared as a data source, as it is hardly readable in the present form.

R: Yes, we will prepare for the next phases of the review process a data source easily accessible and readable.