Response to CC#1

We thank the independent reviewer for the comments. We found them extremely useful for improving the manuscript. Here, our point-by-point response:

DISCLAIMER: This review was prepared as part of graduate program course work at Wageningen University, and has been produced under supervision of Ryan Teuling. The review has been posted because of its good quality, and likely usefulness to the authors and editor. This review was not solicited by the journal.

The paper considers a large dataset of Snow Water Equivalent (SWE) for the titled time period. The stated objective of the paper is to understand snow climatology and characteristics better, verify previous studies with less dense datasets and create a simple polynomial model that can predict average snow depth and SWE for different elevation levels during the year. This is done by using an Italian dataset for which snow samples were taken the first of every month from February until June and additionally on the 15th of April. The research considers 6 major basins aggregated for their own elevation and climatological characteristics. It becomes apparent that elevation and resulting average temperatures influence the snow cover most, with regard to the different basins, that there is a negative trend over the years and that there is a change point around 1988.

The study uses the data for a Moving Average and Running Trend Analysis, which presents the data in a clear and fitting way. Additionally, they plot the data to see temporal changes from two time periods ('67-'93 and '94-'20). For the basins with the most datapoints, Piave-Brenta and Oglio-Chiese-Sarca, there are significant changes in average SWE, on all altitude levels, especially in the early melt season (April 15th & May 1st). From this, and plotting the data with linear regression, they infer that snow depth decreases with -0.12 m per decade and SWE decreases with -37mm per decade for the Oglio-Chiese-Sarca basin. Their results are summarized in the creation of the polynomial model, which can be used in a climatological way to simulate snow depth and SWE for elevation level and the day of the year. The declining trend confirms previous studies and is similar to other mountain ranges. Next to that, other studies often had no or limited access to bulk density measurements, which are needed to calculate SWE. The availability of these data makes this study novel as well. To my judgement, the study can be accepted with major improvements in the framing and delivery of the text. The statistical analysis is sound, but the framing of the text is too generic, which makes the objectives unclear to the reader. Also, some disclaimers should be added regarding data aggregation and the discussion of the results. The main points of my criticism will be elaborated in the next section of this review.

Issue 1: Data Aggregation

My first issue with the research is the data aggregation done for the different basins. It is logical that for this research, a spatial average is needed for the different basins, so I agree with the idea of aggregation. If one consults figure 1 however, it becomes clear that for some basins, the gauging stations were really clustered in a certain area of the basin. Especially basin 3 (Adda) and 5 (Adige) on the map in figure 1 have this issue, as Adda has all the measurements done central north and Adige mainly on 1 mountain ridge. These measurements may cover only a fraction of the entire macro basin that is considered, whereas for basin 4 and 6 the spread of the gauging stations is much better. This makes the reader to question the representativeness of the data for a whole macro basin. Namely, research on precipitation uncertainty in Austria by O. & Foelsche (2019) found that dense and regular distribution of gauges is necessary to obtain accurate rainfall estimates, which will also be comparable to snow estimates. Similarly, Volkmann et al (2010) found that precision and accuracy strongly with network density for catchment hydrology purposes, which could be compared to this study's basin aggregation. To add to this, many of the concluding remarks are based on the well-measured Oglio-Chiese-Sarca basin, which is the only basin that is significantly affected by a Glacier. This glacier might have an influence on the snow dynamics in the surrounding region. This issue concerns section 2.1 of the methods and section 3.1 & 3.3 of the results & discussion.

To solve this issue, the text has to specifically state that the measurement spread and density differs per basin and that the researchers choose Oglio-Chiese-Sarca (and maybe Piave-Brenta) for the most representative results, as these have good measurement density and spread. Then add that the results of the other basins are more uncertain due to the lower spread of the data. This is also seen in the limited correlation for some of the mentioned basins in table 3, so it would be only fair to address this in the text. Moreover, add that Oglio-Chiese-Sarca is affected by glacier dynamics, so that it is uncertain if the results are representative for other basins or the larger Italian Alps. Also, the abstract states that -0.12 per decade is found on average, but this is again for the specific Oglio-Chiese-Sarca basin. That must be mentioned in the abstract, because otherwise it seems like the whole study finds this decrease, unless that is the case, but that is not explicitly mentioned in the results. In general, be more transparent on the data aggregation and admit that there is data shortage in some basin areas or revise the basins to be a smaller area of the landscape, so that the data is more representative. With that addition the data aggregation procedure is clearer for the reader and the researchers are more transparent.

R: Thank you for this comment. To solve this issue you pointed out we added a paragraph in section 2.2 describing the spread of the numerosity of the data among the elevation classes in the different basins. Accordingly, we enriched the discussion of the results obtained in section 3.1 for the results. We believe that this is a useful observation, in particular to describe the different results obtained for the elevation dependency analysis of snow depth, affected by the number of observations at different elevations. We specified that in the abstract the trends presented are referred to the Oglio-Chiese-Sarca, thanks for noticing.

Issue 2: unclear aims and objectives

Secondly, the introduction is not clear on the objectives and the aim of this research. The most concrete statement made by the researchers is that the dataset is important to understand snow variability and climate change impacts. This, however, is a rather vague and broad objective and it is unclear to the reader what the specific knowledge gap is and how this study will try to find novel knowledge or what the different hypotheses and questions will be evaluated. This undermines the rest of the paper, as a proper introduction would be greatly beneficial to introduce the objectives, aims and point out the novelty and relevance of the later results that are actually well-presented in the different figures and results section. This issue concerns paragraph 1 and 2 on page 3, the end of the introduction section, which should be expanded to be more concrete and research specific.

To improve on this the research objectives and aims should be clearly stated in the introduction. These are partially to verify earlier research that was done with modelling or with less available data, such as from Colombo et al. (2022) and Steirou et al. (2017). That objective would be to use the dataset to show and verify temporal trends in snow depth and SWE decline. Another objective would be to make a general polynomial model for climatological estimation of SWE and snow depth for different days of the year and elevation levels. The third objective could then be to state that

NAO and WeMO indices will be correlated with the snow data, as other studies found a relationship and that should be verified by checking if this dataset shows a similar correlation. The aim of the research is next, as it should state what will be the contribution of these conclusions. The conclusions could for example help to better understand climate change effects on snow cover and are essential for future Ski tourism policy and for hydraulic power companies that are affected by changed snowmelt. With additions such as these, the paper has more societal significance and for the reader it will be clear what different subjects will be addressed in the paper and why.

R: Thank you for this comment. It has been pointed out also by other reviewers. Accordingly, 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."

Issue 3: vague section on climatological oscillations

Thirdly, the part on the correlation between snow depth and SWE and the two meteorological oscillation indices could be better. The overview of the Pearson's correlation coefficients in figure 12 should visualize every coefficient, instead of just the significant ones. I would suggest the researchers highlight the significant values, but show the other values as well, for transparency. Mainly for the WeMO correlation, it is now unclear if there might have been negative values for the other basins and elevation classes. If the values are transparently shown, this will benefit the transparency and conclusions on a positive correlation with WeMO and a negative one with NAO. This issue concerns figure 12, section 2.4 of the methods, the last paragraph of section 3.4 and possibly the conclusion.

Next to that, the description in the manuscript on these correlations is somewhat blunt. The correlation is described and links are made with other literature that prove these correlations, but there is no description of the effect or substantiality that this correlation has. There is no impact of this result described, only that the correlation is present. I would suggest you add a concise description of what this experimental correlation would do for i.e., SWE prediction, SWE temporal variability or analysis of older records. The conclusion adds that further research is needed into this, but some general hypothesis on the effects of these climatological indices could definitely be mentioned, which could be a short adaptation of the study by Osborn (2011) and an additional source on precipitation. Otherwise, this part of the research feels like a detached addition to the whole of the paper and which could be remedied by integrating it into a more comprehensive section of the aims and objectives, which was already mentioned in issue 2.

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.

> Minor arguments
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> The title could reflect more in depth what the research is actually related to. Climatology is now an overly broad term. Other ideas could be long-term snow cover trends or snow variability in a warming climate or anything else that is more specific for the research.

R: We believe that the term climatology reflects this study, as it investigates averages and variability over a sufficiently long time span of a variable that affects and is affected by local climate conditions. We are open, of course, for a discussion also with the editor about possible changes in the title.

> Write in the abstract, line 13-15, that these results were found for basin Oglio-Chiese-Sarca, not for most of the investigated areas. Or write that a similar decline was found for most areas, now it seems like all areas have this average, which is not the case.

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."

> In section 3.1 the text first handles different results on decline per decade and later discusses decline between the first and second half of the measurement period. This could maybe be emphasized a bit more, as it can be confusing that the decline is suddenly much bigger, because it concerns several decades.

R: Yes, we divided the two results by starting a new line when describing the differences between the two periods, making the paragraph begin as "Subsequently, we evaluated the difference in average snow depth for each macro-basin, elevation class and date of measurement." We changed similarly also the SWE results subsection.

> In section 3.2, line 311-313, I would add a reference about snow bulk density increase over the season, as it might not be common knowledge for most readers.

R: Thanks, we added the reference to Strum et al. (2010) and Pistocchi (2016).

> In section 3.5, line 399-401, the sentence suddenly stops, as if there was more that the writer wanted to say, but he forgot to write it down. Make sure this is fixed.

R: Thank you for this comment, probably a copy and paste error lost in the formatting the manuscript. The completed phrase is "This model must be intended in a climatological way, as it has been conceived from average values over a time period of 27 years, and it can provide a simple yet useful estimate of the expected snow water equivalent at a given day of the year and at the specific altitude of interest, albeit constrained by challenges related to knowledge of actual snowpack conditions at elevations exceeding 2500 m."

> Figure 8, and section 3.2 are used to conclude that bulk density does not vary significantly with elevation, which confirms earlier studies. This is, however, only shown for the basin Piave-Brenta. It

might be considered to state that the other basins showed comparable results, but Piave-Brente was visualized because of the data abundance.

R: We changed this figure and the comments including all the basins as requested by another referee.

Minor issues

Line 20, p1, Mediterranean is spelled wrong (like Mediterrean)

R: Thanks. Corrected.

Line 175, p6, same here, Mediterranean is spelled wrong again.

R: Thanks. Corrected.

Line 183-184, p6, the reference of Rosso and Kottegoda is not included in the bibliography.

R: Included, thanks for noticing!

Line 205, p7, maybe add a reference for the Pearson's coefficient, as you did add a reference for the other statistical tests (t-test, MK-test, Pettitt)

R: Yes, sure. We included Karl Pearson's first paper mentioning the coefficient to our knowledge "Pearson, K.: On lines and planes of closest fit to systems of points in space The London, Edinburgh, and Dublin philosophical magazine and journal of science, 2(11), 559-572."

Figure 3, p22, consider making the limits of the Y-axis of the plots narrower so that the individual lines can be viewed better. Now it is hard for the reader to see all the differences.

R: According to the comments of other reviewers we decided to take this figure out of the manuscript.

Figure 4, p23, maybe only add the slope of the trendlines, as the intercept value has no physical meaning here.

R: Same as Figure 3, this figure has been removed for the revised manuscript according to the suggestions of another reviewer.

Figure 13, p32, consider adding lines for the confidence interval and prediction interval of the polynomial fit, to show the (un)certainty of the fit.

Figure 14, p33, you could consider using colours to represent these plots, which would add to the visibility and clarity of the figure.

R: Figure 13 and 14 have been merged in a single figure. We prefer to keep the contour plot for Figure 14, in our opinion sufficiently clear. To these figures we added two additional panels. The first containing an error analysis, aimed to discuss the uncertainty issue. The second containing a comparison between the first and second sub periods of observation.

Table 1 to 5, p34-p38, the captions of the tables should be above the tables, not below the tables.

R: Thank you for this comment, of course. The caption of the tables will be properly placed in the final version of the paper.