Response to CC#2

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.

Review of Climatology of Snow Depth and Water Equivalent measurements in the Italian Alps (1967 - 2020)

The paper investigates with a large data set the snow dept and SWE between 1967 and 2020 in six subregions and for four elevation classes in the Italian alps. It uses multiple statistical techniques to investigate the data and its changes over time. The data is then also correlated with the NOA and WeMO indexes. The paper then also proceeds to propose a SWE model that depends on elevation and DOY.

The language is clear and of good quality. The work seems to be done in a good and scientific way, with no major flaws or errors. The paper fits in my opinion very well within the scope of HESS. The extensiveness of the data set used in the paper is furthermore unique, but the work done with the data is in my opinion not. The paper also fails to present a clear contribution and lacks novelty. I do think however that after revisions the work could be published.

Major arguments

The first major concern is the lack of novelty, or argumentation for why it is novel. The paper cites a lot of papers that have done similar research for different time periods and/or geographic locations. This is good, since it shows that large parts of the methods, since they are similar, are widely accepted and commonly used. However, Colombo et al. (2022) is also focused on the Italian alps and has a time frame from 1960 to 2020, which is the same except it includes 7 more years. The only research not also done in Colombo et al. (2022) is the proposed SWE model. This model is improved from Guyennon et al. (2019). The paper does however not argue or state why their coefficients are better. This results in a paper that seems to combine and repeat the work of two recent papers.

It can however probably be solved by stating better what the differences are between this paper and the two mentioned before. For Guyennon et al. (2019) it is enough to show why your parameters are better than the ones they presented. I assume it is because you have more data, but it would be nice to get some proof or explanation. Especially, since the south-west of the Italian alps does not seem to be included in the research in the paper, while Guyennon et al. (2019) calibrated with this area included. For Colombo et al. it is probably most important that they only use 19 stations that are spatially distributed, while this paper has 299, which could result in much more significant results. The statistical tests are a difference that also could help stress the novelty of the paper. For the NOA and WeMO indexes, it could be nice to stress that the WeMO has never been correlated with snow dept or SWE.

R: Thank you for pointing this out. We actually wrote in the introduction "The dataset we use here covers almost the same period of previous studies, but it is spatial distributed over the Italian Alpine Region and includes bulk snow density measurements to estimate SWE. Such combination of spatial and temporal coverage makes this dataset an extremely precious support to understand snow variability and climate change impacts in the Italian Alps.". To further stress this we extended the paragraph, highlighting the differences in the dataset used with respect to previous studies: "Differently from previous studies, we make use of not only measurements of snow depth but also extended observations of bulk snow density instead of modelled estimates. This novelty and the strength of this study also relies in the robustness of SWE measurements due to the availability of bulk snow density information.". For what concerns the snow density regression model, as it is the same function used in Guyennon et al. (2019), we do believe that stating "our model is better" would not be correct. We added a sentence saying that the parameters here obtained better represent our dataset and, consequently, is more suitable for the considered basins. About the comment concerning the WeMO index we specified in section 2.5 the novelty related to the use of WeMO index for correlation analysis with snow depth.

The second major argument is the failure to present a clear contribution. In the introduction a few statements are made that could be seen as a contribution, but all of these have flaws. It starts off by stating that it is important to know the snow extent for the 10-billion-dollar ski tourism industry. The paper however only looks at snow dept and SWE and therefore does not contribute to more knowledge about snow extent. The paper then continues to state it is important to know SWE for water managers, agriculture and hydropower plants and this is true and would seem like a good contribution. These parties do however need almost real time value to make their day-to-day decisions on either storing or discharging water. A long-term trend can of course give inside in typical values and a model would be a great contribution. The paper only already proves that the model needs significantly different parameters for different time periods, thereby creating the question if the model would be valid and useful for the upcoming years. The model also takes the average over multiple years and does therefore not take single year variability into account. This is however very important for water managers, agriculture and hydropower plants (HPP), since they need accurate, current values and not averages. It could result in massive over or underestimations of the amount of water stored in the mountains if these parties would use this model and see it as true values. This could then have severe influences resulting in droughts and water shortages or floods. It would be good to make a good and clear statement in the introduction on what the paper will contribute. I think the paper could still be very useful for water management, agriculture and HPP. The WeMo and NOA show some correlation and could therefore be used to estimate the amount of snow, especially when the model would be able to give some uncertainty or spread of possible values. Multiple scenarios for wetter and dryer conditions or warmer and colder conditions could also help. In the conclusion then revere back to the statement in the introduction and argue why the paper succeeded in, partially, solving the issue raised in the introduction.

R: In the introduction we mentioned a list of sectors (e.g. winter tourism, agriculture, hydropower energy production) that rely or are at least affected by the presence or absence of snow. Even if we are not directly discussing snow cover extent, the amount of snow present on ground is related to snow extent. Anyhow, it is of great interest to the sector to know what are tendencies, at least to be able to make informed decisions for the future years considering what happened in the past. For what concerns the climatological model, by definition it is supposed to be representative of a certain climatological condition: 1967-1993 o 1994-2020.

It is not supposed to be used for any kind of real-time prediction. This is clearly stated in the manuscript. Of course, NAO and WeMO correlations could be used for possible predictions. However, this would require a more sophisticated model and this is not an objective of this study.

For what concerns the lack of clear objectives in the introduction, however, we agree. 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."

Proksch et al. (2016) raise the issue that the method used to measure the density was not accurate for ice layers. Ice layers also have a density that is above the 0.75 g cm-3 (Watts et al., 2016) threshold that the paper used as errors in the data. The paper however completely fails to mention these layers and it is therefore not clear what was done with them.

The SWE could be underestimated, if they were ignored and deleted, since their density was too high. The paper either needs to explain how these ice layers were considered or add a brief explanation and discussion on this topic. It would be preferable if an estimation could be given of the potential underestimation if the ice layers were neglected.

R: Thank you for this comment. We are aware about the possible impacts of ice layers on swe estimates. However, even if an ice layer in a snow column could have a density higher than 0.75 g cm-3, it is unlikely that such high density affects the entire snowpack condition. It's effect is more likely going to be smoothed when considering the average density of the entire snowpack. Moreover, before removing the outliers in the dataset, we checked nearby measurements of both snow depth and density to be sure we did not remove a valuable information from the dataset.

Minor arguments

It would be nice to show an overview of how the 299 measurements were distributed over the different basins and elevation classes. It might also be good to show the elevation range of a basin. Both would be nice, since it could help understanding why there is no data in certain basins at certain elevations and/or why data is not significant.

R: We thank you for your suggestion. Accordingly, we prepared a table with area, maximum, minimum and average elevation of the considered basins, together with the number of the measurement points per basin and elevation class. From the original dataset of 299 measurement sites over a wider part of the investigated basins we selected a subset of 240 grouped in the 6 areas we investigated.

Here the table we made:

	Area (km^2)	H min (m)	H max (m)	H med (m)	$N_{1000\text{-}1500}$	$N_{1500\text{-}2000}$	$N_{2000\text{-}2500}$	$N_{2500\text{-}3000}$
Piave+Brenta	4857	106	3342	1315	18	23	7	2
Adige	3815	238	3899	1874	1	8	5	6
Oglio-Chiese-Sarca	3615	136	3556	1428	14	18	25	12
Serio-Brembo	2609	230	3052	1213	1	18	9	1
Adda	1225	213	4050	1844	0	11	18	2
Toce	1533	198	4633	1641	3	12	25	1

For the correlation of NOA, WeMO and snow dept it is explained why winter NOA is used. It is however not explained why this is also done for WeMO. It is also not discussed why April snow dept values are used and why not for example march, may and/or June. It would be good to shortly argue this.

R: We compared winter NAO and WeMO with April snow depth because we want to investicate correlations between the winter precipitation, linked to teleconnections, with the accumulated snow, generally reaching its maximum in April (always for elevations higher than 1500 m). We specified this as "The comparison between winter teleconnections and April snow depth is aimed to investigate the impact of atmospheric circulation patterns on winter precipitation and snow accumulation, generally reaching maximum values in April."

It would be nice to shortly explain why certain order polynomials are used and why they differ for different parameters. This is explained in Guyennon et al. (2019), which the paper references, but a brief explanation in the paper itself would be good.

R: We included references to other authors (other than Guyennon et al.,2019) that proposed different functions to model snow density evolution. We added references (Strum et al, 2010; Pistocchi, 2016) and an explanation about the choice of a second order polynomial in sections 2.6 and 3.2

P1, line 28: "Modifications of the Greater Alpine Region climate have been confirmed by the analysis of the HISTALP dataset, with significant trends in temperature, twice as the global average, precipitation and relative humidity (Auer et al., 2007; Brunetti et al., 2009)." Is not completely true since Brunetti et al. (2009) concludes: "If only the low-level areas are considered, relative humidity also has a clear long-term trend, with a decrease of about 5% per century. Such a decrease is, however, not shown by the record representing the high-level locations." The paper is largely about high elevation locations, so this statement is therefore not completely true and misleading. Please adjust accordingly.

R: Thank you for the accurate comment. We specified in the corrected manuscript as "Modifications of the Greater Alpine Region climate have been confirmed by the analysis of the HISTALP dataset, with significant trends in temperature, twice as the global average, precipitation and, in low elevation areas, relative humidity (Auer et al., 2007; Brunetti et al., 2009)."

> P2, line 33: "Snow cover regulates the surface energy balance, affecting circulation patterns and atmospheric flow regimes (Ge and Gong, 2009)." Ge and Gong (2009) do state in the introduction: "Anomalous snow cover can influence the surface energy balance and temperature over a broad land surface region, and in turn affect atmospheric flow regimes, circulation patterns, and hemispheric climate." They however do not provide any data or sources to prove this statement, other than stating that literature has focused on this. The paper itself is furthermore about snow dept and its influences instead of snow cover and/or snow extent. It is therefore probably better to cite other literature, such as for example: Groisman et al. (1994), Gong et al. (2004) and/or Fletcher et al. (2009).

R: Thanks for the comment. We added the reference to Gong et al. (2004).

> P2, line 58: The sentence should be: "… Lejeune et al. (2019) used a snow dataset of 57 years ...".

R: Corrected.

> P7, line 222: It is not clear which slope is meant with m (slope) in this sentence. Up until that point slope has been used in reference to the soil/ground below the snow. The m here however depends on DOY, which leads me to think it is the slope of the snow in this case. It would be good to make this clearer, by adding a sentence on what slope is exactly meant in this context.

R: The slope here is the elevation dependency, how many meters of snow depth we expect each 1000 m of elevation. We clarified it in the manuscript and in the tables.

> P13, line 401: The sentence ends with "and". Either a sentence is missing here, or the sentence should end with: "… day of the year."

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."

> P20, Fig 1: There is a red line in the figure, which is barely visible especially for colorblind people. It would be good to explain what the lines represent, I assume the division of the basins, and make them better visible, by for example changing the color.

R: Thank you for this comment, it has been reported by other reviewers as well. Accordingly, we made some modifications in Figure 1 in order to be more representative and easier to read. We reported in the Figure the grouped basins only in order to avoid confusion related to an excessively fine basin disaggregation. We aggregated to this figure also the photographs of the probes.

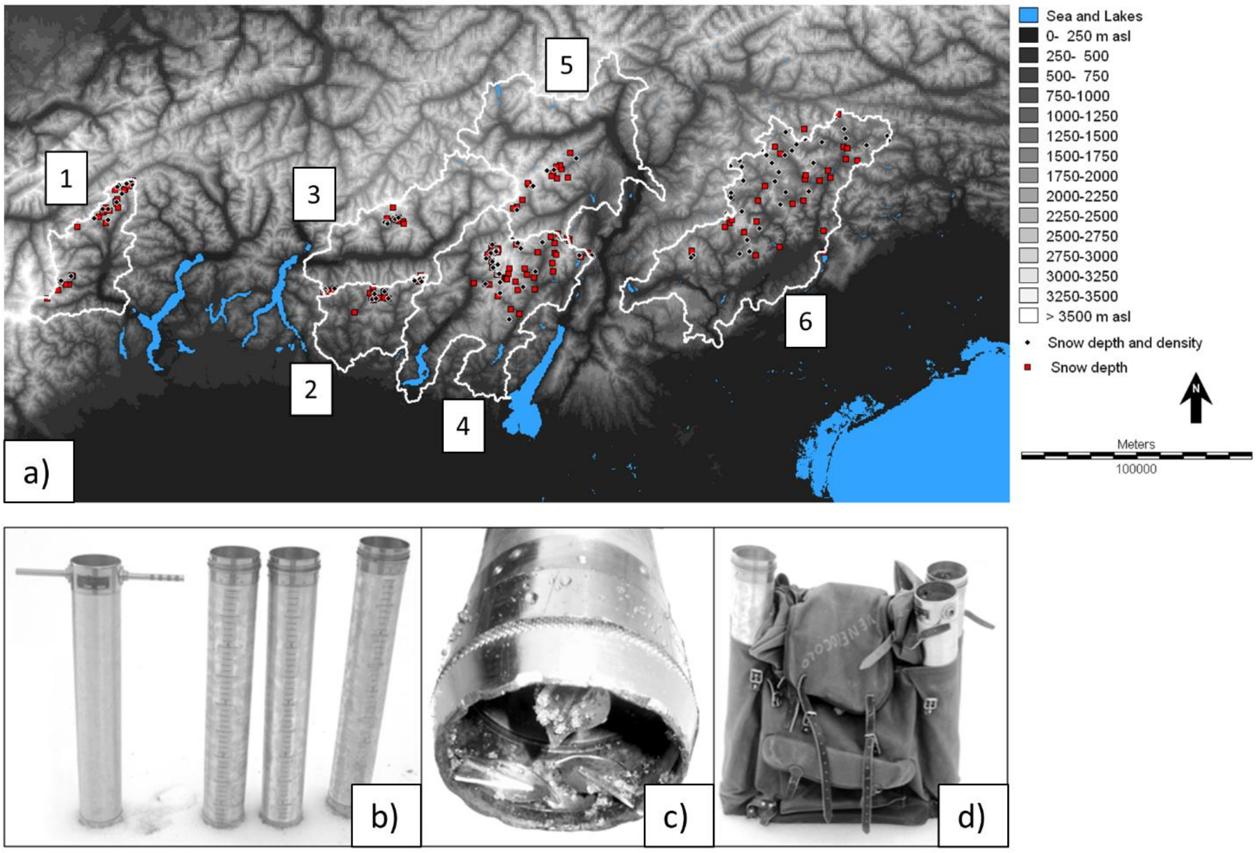

Figure 1: a) Map of the research area. The individual basins are grouped in the six macro basins by number: (1) Toce, (2) Serio-Brembo, (3) Adda, (4) Oglio-Chiese-Sarca, (5) Adige and (6) Piave-Brenta. Locations of snow depth and density (black dots) and snow depth (white squares) are also reported. At the bottom, Photo of (a) CN2 type snow sampler and (b) detail of the cutting knife with the three internal fins and (c) the complete kit in its transporting bag.

P26, Fig 7: I assume “mag” and “giu” in the legend should be “May” and “Jun”.

R:Yes thanks, we corrected it.

P31, Fig 12: In general elevation is presented on the vertical axis instead of the horizontal. I would therefore change the figure to show elevation on the vertical and the basins on the horizontal axis.

R: This figure was converted to a table according to the suggestions of Reviewer 2. This suggestion has been also accepted rotating the orientation such that the lines of the table represent the elevation classes.

P34, Tables 1,2,4 and 5 show a lot of “ND” and “—” values which makes it hard to read the data that is actually in there. It might be good to present this data in another way that focuses more on the values that are obtained. It would also be good to explain the difference between “ND” and “—" a bit better.

R: Thank you for this comment. We specified in the captions the meaning of “-“ symbol as “If the trend is not statistically significant for either of the two tests, the value is not reported (-).”

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