General comments:

This paper provides an analysis of snow cover regional variability and trends over the European Alps based on a new in situ daily snow depth dataset developed through the collaboration of more than 20 institutions from six countries. The dataset covers the entire European Alps with more than 2000 surface snow depth observations, and represents an important contribution for research and development. The authors are to be congratulated for their efforts to develop this dataset and in particular, to make it available to the research community. The creation of a pan-Italian snow depth dataset from various agencies is a noteworthy achievement.

The paper presents the results of a PC and cluster analysis to characterize the regional snow climate, along with trend analysis to document trends by climate region and elevation over almost 50 years (1971-2019). The paper is in general well-written and clearly explained, and is close to publishable quality once some issues with overly-long sentence construction are rectified. I have three main comments concerning the methodology. First (comment #6 below), I question the need for the moving window analysis for trend variability, and recommend it be removed from the paper. Second (comment #10), the PC results reflect an uneven spatial distribution of stations with oversampling of elevations below 1000 m and undersampling of elevations above 2000 m. It is unclear to what extent this distorts the analysis results compared to those obtained based on a gridded representation of the station data that evenly samples the full spatial and elevation domain. Third, the paper provides no insights into interannual variability of snow cover and its relative magnitude compared to the long-term trend. The authors may feel this is beyond the scope of the current paper, but presenting trends without discussing the interannual and multi-decadal variability is a major oversight in my opinion.

I look forward to seeing the revised paper and congratulate the authors again for their significant contribution. Ross D. Brown, Canada (ross.brown@canada.ca)

We thank Ross D. Brown for the in-depth review of our manuscript, his positive appreciation of our work as well as the detailed and constructive suggestions for improvements, which have been very helpful in revising the manuscript. In order to address the 3 major comments, in the revised manuscript we plan to introduce the following modifications:

1) The moving window analysis will be removed.
2) The uneven distribution of stations across elevation is an issue. However, we think that interpolating the stations to a grid could introduce more uncertainty, given the complex topography in the European Alps. While it is certainly a good exercise, it goes beyond the scope of this paper. See also the detailed comments below for more on this issue
3) We shall add multiple analyses to show the interannual variability and how this is related to the trends. These include time series figures, ratios of the trend versus variability, and we also modified our statistical model to deal with the changes in variability across time. See the detailed answer below for more information.

Furthermore, we will shorten the sentences, which was suggested by another reviewer, too.

Detailed answers to the other comments can be found below.

Detailed comments:

1. Lines 74-75: Suggest rewording as "The main limitation … that their number decreases sharply with elevation, with few stations available above 3000 m in the European Alps."

Thank you. Done.

2. Lines 90-126: There is a lot of useful material presented here on published snow cover trends in the various countries, but it is difficult to read with very long sentences joined with rather unwieldy constructions like "which, however". I recommend you organize this material in a summary table, and provide a few lines that capture the common elements. This would lead very nicely into the paragraph starting on line 128.

Thanks for this very useful suggestion. We now provide this information in a summary table that will be included in the Appendix, given the length of the table itself (2-3 pages). In the introduction we will summarize the main elements, as suggested.

3. Lines 155-160: It would be instructive to show the main climatic divides on Figure 1.

Done.

4. Lines 204-206: consider rewording as "Many stations contain an important data gap for the 1981–1997 period that rendered a large fraction of the stations unusable for this study."

Done.

5. Line 279: Suggest deleting the following "The predicted variable was the mean monthly HS and the only predictor the year (shifted to 0)" and replacing the previous sentence with "Linear trends in monthly mean HS were computed separately for each month from November to May for stations with complete data in the period."

Done.

6. Line 280: "The second approach was a moving window approach that aims at identifying the short-term changes in trends." I think it would be clearer to say "A second moving window approach was used to examine the variability in 30-year trends over the period from 1961". I would consider removing this analysis for the following reasons: (1) the lack of a clear rationale for the analysis, (2) the inconsistency introduced by the different start period (1961 vs 1971), (3) the fact that overlapping windows are not independent, (4) the use of what is essentially an arbitrary 30-year period for the trend, and (5) the fact that the network density changes over time. I think it would be more instructive to look at the signal-to-noise properties of the 1971-2019 trend, by breaking it up into the amount of variance explained by the trend vs the amount of variance explained by interannual variability. Mapping the two quantities would highlight areas where trend was stronger relative to natural variability and vice versa.

Thanks a lot, we clearly see your point and agree to a large extent. We therefore removed the moving window approach from the manuscript. Instead we introduced the analysis of interannual variability (see also your comment no. 11 below). This analysis presents the time series plots of the mean monthly HS (averaged over 500 m elevation bands because of the number of stations) and a short discussion of the fraction of variance explained by the trend.

Looking at the time series figures, we noticed some changes in the interannual variability across time. The most prominent is the decline in variability at the end of the season associated with the decrease in mean HS, which approaches zero. We decided to account for these changes in the variability in our linear model by including a time coefficient for the error variance (=interannual variability). This results in replacing the standard OLS model with a GLS (generalized least squares) model. For our regression formula $y_t = \beta_0 + \beta_1 t + \epsilon_t$, OLS has a constant error variance $Var(\epsilon_t) = \sigma^2$, while with a GLS we can allow the error variance to depend on time: $Var(\epsilon_t) = \sigma^2 * exp(2 * \gamma * t)$, where $\gamma$ is another estimated coefficient that indicates the change in variance associated to $t$.

The trends in mean HS are not affected, but, with the new model structure, we were able to account for changes in the variance. More details on the model specifics can be found in the revised method section. We shall also discuss the results in the new section on interannual variability.

7. Line 297: Not clear what you mean here … the homogeneity of the data used in a gridded dataset is the key issue. Several reanalyses have well documented discontinuities related to changes in data input streams.

We completely agree. Actually we meant the spatial homogeneity, but this should be captured by the previous part of the sentence. So we removed this part of the sentence that evidently created a misinterpretation.

8. Line 317: There is no season dedicated to the snow-cover onset period (Nov-Dec?), but there is one (March-May) for the spring season. Any reason for this? In my work documenting snow cover variability over Bulgaria (Brown and Petkova, 2007, Int. J. Climatol. 27, 1215–1229) and Quebec (Brown, 2010, Hydrol. Process., 24, 1929–1954) we found different trends in the fall and spring periods as well as different modes of atmospheric variability influencing snow cover variability in each season.

Our idea was to use the single months as an alternative to seasonal aggregates. However, in the conclusion we aggregated by season (cf. your comment 11.), so this was not very consistent.

Furthermore, we decided to include analyses of mean seasonal values of HS, maximum HS, and SCD in the manuscript. However, we will place most of these in the appendix, because the paper is already quite long. The most important results were retained for the conclusion table.

9. Line 400: Can you please include the variable(s) the trend is computed for to remind the reader what the results refer to.

Good idea. We modified the sentence accordingly.

10. Lines 473-474: "In relative terms, the elevations of the stations used in this study oversample the elevations up to 1000 m, are similar from 1000 to 2000 m, significantly underrepresent 2000 to 3000 m, and do not cover elevations above 3000 m". This begs the question of why you did not attempt to transform the observations to an equal area grid (e.g. by kriging, or pseudo obs from modelling) to force the spatial coverage to be representative prior to the PC analysis?

This is a good point. But not a trivial issue. Because of the complex topography and strong elevational gradients in the Alps it is difficult to choose an appropriate resolution for the interpolation onto a grid. This would require a balanced number of stations both horizontally and vertically - a condition which is not met across the whole domain with our station set. While the transformation of the observations into an equal area grid would be interesting, we think it goes beyond the scope of the manuscript.

The main intent of this section is to give an overview of the confidence that can be expected from our assessments with respect to spatial coverage and elevation. We clarified it in the manuscript.

11. Conclusions: This study provided useful new insights into snow-climate regions and trends for the European Alps, but did not look at interannual variability in snow cover e.g. PC analysis of annual time series of snow cover duration and maximum accumulation. Is there a particular reason why you chose to ignore this? Documenting and understanding interannual variability is a key component of interpreting long-term changes (e.g. the signal-to-noise ratio of climate heating induced changes).

We followed your suggestion and included a section on interannual variability, as well as analyses on SCD and maximum HS; see also our replies to comments 6. and 8.