Answer to referee 1

The manuscript submitted by Buchmann et al. is a nice contribution that complements the analysis performed by the same group of authors in the past years. I consider the manuscript of interest for the community, however I believe the methodology should be significantly better described. In particular, the fundamental step of the calculation of the adjustment factors and their comparison in terms of applied equations is missing. Moreover, the role played by autocorrelation in the analysis of the time series is not discussed. Additional comments are contained in the pdf file.

Thank you for your time to work through the manuscript and for sharing your thoughts to improve it. We really appreciate it! We reworked the methodology-description for making it easier to understand in the new version. Regarding the autocorrelation: We used seasonal derived indicators (e.g. seasonal number of days with snow depth > 5 cm), so autocorrelation does not play a role here as it would of course do to daily snow depth series. Some examples for this are shown in table 1 below.

50: I think the authors here have chosen the wrong publication. It should be Marcolini et al. 2017 (Marcolini, Giorgia, Alberto Bellin, and Gabriele Chiogna. "Performance of the standard normal homogeneity test for the homogenization of mean seasonal snow depth time series." International Journal of Climatology 37 (2017): 1267-1277.)

We added Marcolini et al. 2017 to the list.

52: Probaly the work of Marcolini et al., 2019 is more appropriate here, since it already investigated the impact of different homogenization approaches on trends.

Done. We rewrote the paragraph to give a better overview of recent snow depth homogenisation efforts and changed the citations.

"In spite of recent efforts of studying the homogenization of snow depth (Marcolini et al., 2019; Schöner et al., 2019; Buchmann et al., 2022; Resch et al., 2022), it is still an open question whether the use of one of the methods is more advantageous than the others, and how the methods impact trends and extreme values. So far, Swiss snow depth time series have not been homogenized and an impact assessment has not been carried out neither for Switzerland or any other region."

was changed to

"First steps towards detection and adjustment of breaks were made by Marcolini et al. (2017). Schöner et al. (2019) used homogenised seasonal time series for calculating trends and identifying homogeneous regions of snow depth in Austria and Switzerland. Marcolini et al. (2019) compared the applicability of two homogenisation methods and their effects on trends in seasonal mean snow depth. These analyses showed the need for improved adjustment methods to enable both the application to data with higher temporal resolution, such as daily data, and make the adjustments of large values more robust. For this, an implementation of an adjustment method using quantile matching was proposed by Resch et al. (2022). Buchmann et al. (2022) analysed the

break detection capabilities of three widely used semi-automatic detection methods. The question remained open as to how well other widely used adjustment methods are applicable and how they affect other indicators of interest besides the seasonal mean snow depth, e.g. extreme values. In order to consider the homogenised part of time series as more credible and statistically robust, a more detailed study of the effects of homogenisation methods on snow depth and derived metrics was necessary.

79: Some steps in the methodology are not clear to me:
1. I assume that for some time series all three methods detect a breackpoint (let's call it set A) and for some time series only two methods detect a breakpoint (let's call it set B). I think it should be somehow indicated in the presentation of the results which stations belong to set A and which to set B in order to see if any difference is present.
This is described in Buchmann et al. 2022, Table 3. (https://doi.org/10.5194/tc-16-2147-2022). As a better description of the break detection results was added to the text: "For details, see Buchmann et al. 2022." was changed to "For details, e.g. on the differences between the methods in the detected breaks, see Buchmann et al. 2022."
2. Is the set of stations used to detect the breakpoint different from the set of stations used to adjust the breakpoint and compute the correction factor? I think they should remain the same and if they change, I would expect some more discussion about the implication and the mathematical soundness of this choice.
We used the same set for both detecting and calculating the adjustments, but the chosen stations used for calculating the adjustments is unknown except for interpQM. Given your comment we reworked the methodology section to make this more clear.

81: So, probably autocorrelation is present. The authors should discuss this point both in the analysis of the following results as well as in the methodology.
Thanks for bringing this up as autocorrelation is an important topic in time series analysis. We analyze seasonal values, therefore autocorrelation would only be present in the case that snow did not completely melt between seasons. This is not the case in any of our stations. We performed an autocorrelation-analysis prior our analysis for all indicators for the lags 1 - 10. It showed that the correlations are generally low and therefore a pre-whitening, e.g. from Yue et al. (2002, DOI: 10.1002/hyp.1095) is not necessary. A short summary of the correlations for lag 1 of each indicator analyzed is shown in the table below (n = 42 stations):

Table 1: Summary of autocorrelations for lag (year) 1 for all stations (n = 42)

Indicator	Minimum	Maximum	range 25. - 75. percentile (IQR)	Mean	Median
HSmean	-0.34	0.36	0.22	0.1	0.1
HSmax	-0.25	0.42	0.24	0.03	0
dHS5	-0.1	0.48	0.14	0.18	0.21
dHS30	-0.29	0.36	0.15	0.06	0.06
dHS50	-0.3	0.26	0.16	0.03	0.06

Daily snow depth time series inherit a very strong autocorrelation. In this study, seasonal indicators were analysed, which imply that autocorrelation would be present in cases that the snowcover did not completely melt between seasons. However, this is not the case in any of the selected stations and for any of the seasons analysed. In order to prove our hypothesis, autocorrelation was investigated for the lags 1 - 10 years. A summary for lag 1, where it would be strongest, is presented in table 1. The results showed that autocorrelation is very low and a Trend-Free-prewhitening (e.g. Yue_2002) was not necessary.

83: I found this sentence more confusing than explanator. I suggest to remove it.
Changed from "The analysis described in Buchmann et al. (2022) is re-run with the three additional Austrian data series along the eastern Swiss border to increase the number of available reference stations for Eastern Swiss series, thus increasing the 85 valid break points to 45 (from the original 43 mentioned in Buchmann et al. (2022))." to "To improve the station density near the eastern Swiss border, three Austrian stations were added."

Figure 1: Please add this information in a legend as well.
Done.

95: It is fundamental in my view to add the equations used for the calculation of the adjusment factors either here, in an appendix or in the supporting information.
Done. We decided that it's most useful to add the equations and short explanations directly to the main text and slightly improve the corresponding description-paragraphs.

118: Unclear sentence:
"Since it was decided that interpQM does not change the total number of snow days (HS > 0), the thresholds of 5, 30 and 50 cm (dHS5, dHS30, dHS50) were used and summed up for each hydrological year between November and April."
Changed to: Since interpQM does not change the number of days with snow (HS > 0), no changes at the 1 cm threshold were expected for the homogenized series. Instead, the threshold values 5, 30 and 50 cm were

used and the number of days between November and April were summed up for each hydrological year.

122: Please indicate if autocorrelation is still present (I expect there is still significant autororrelation if seasonal time series are used). Why did not you use a trend test which can handle autocorrelation instead of the standard Mann-Kendall test?
As mentioned (answer to line 81) from the physical standpoint seasonal values of snow are usually not autocorrelated as the snow cover disappears in summer. Nevertheless, we performed an analysis for lags 1 - 10, which showed that autocorrelation in the seasonal time series of the indicators analyzed was very low, so we did not account for it, e.g. with a pre-whitening or a modified MK test. To clarify this in the article, "To avoid the influence of autocorrelation on the seasonal trend analysis, it was examined for all indicators and time series. It was found that the values were low enough (mean 0.03 - 0.18, interquartile range 0.14 - 0.24) that trend-free-pre-whitening or the application of a modified MK-test was not necessary." was added.

128: Please provide a reference for this choice
Done. Buchmann et al. 2021 (https://doi.org/10.5194/tc-15-4625-2021) and Marty and Blanchet 2012 (https://doi.org/10.1007/s10584-011-0159-9) (100 year return period).

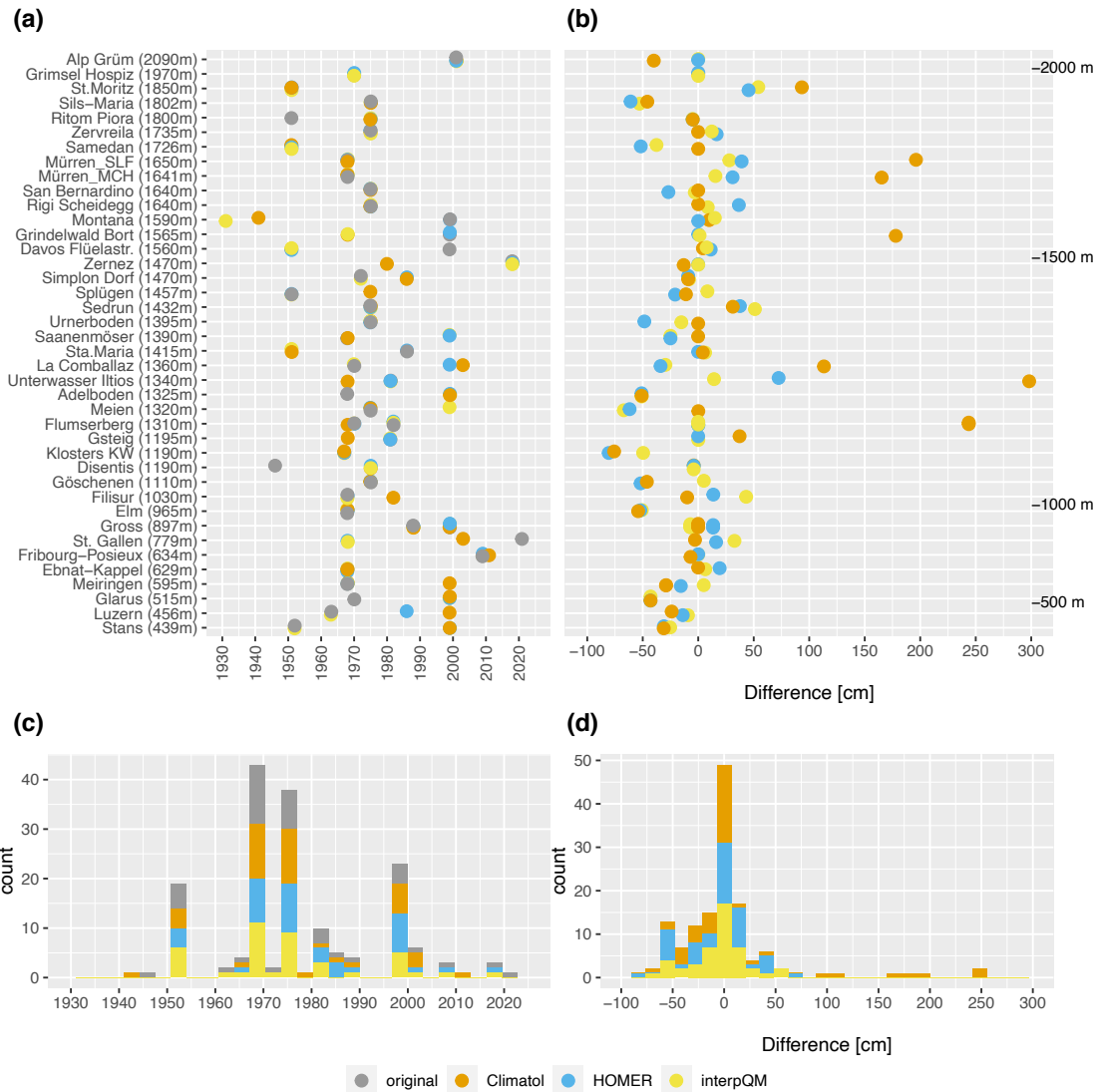
130: Sentence unclear (To assess the significance of the homogenization performed, a Kolmogorov-Smirnov test was conducted with seasonal data.)
Changed the sentence to make it more clear: "In order to determine to what extent homogenised and original time series differ, as well as to assess the differences between the results of the applied adjustment methods, a two-sample Kolmogorov-Smirnov test was conducted with seasonal data."
This can be achieved by different tests, e.g. via a two-sample KS-test or a Wilcoxon rank sum test followed by Dunn's test. Here, we have chosen the two-sample KS-test for this task.

137: why only for these and not also for the aggegated measured time series with HOMER and Climatol?
Because HOMER and Climatol only provide monthly data, but daily data is needed to calculate the number of snow days. We changed the corresponding sentence to make this important detail more clear: "Trends of days per seson with a certain snow depth (snow days) are only compared for the original data and interpQM, as Climatol and HOMER only provide monthly data."

141: could you please be more specific? it is hard to follow.
"Climatol automatically fills in missing values, thus artificially
increasing the length of the series. However, for the trend analysis,
only the time period available in the original series are considered for
the homogenized data in order to make it comparable."
changed to
"As the series need to have the same length for Climatol to work,
missing dates and their values are automatically interpolated, thus
leading to an artificially increased length these series. However, for
our analyses we only considered the original length of the series in
question."

169: not sure it is the right wording in English.
"This may be irritating, but it has several reasons:…"
changed to:
"There are several reasons for this:"

Figure 3: Here I suggest the use of a better colorbar. In fact, low
elevation sites have lower values for the trend and with this colors it
seems there is no trend. Maybe one possibility could be to normalize
the trend values by the long term mean value.
We adjusted the colorbar to make the colors stronger and differences
clearly visible. As we want to highlight the difference between HSavg
and HSmax, identical color code for the trend was our choice.
Different color sets for HSavg and HSmax, would have been a possibility.
But as especially the lower elevation sites (<1000 m) sites only have
small trends for HSavg, varying between -4.3 - 0.5 cm/decade, in our
view, these small trends would not distinguish better from larger
values as with the current color choice. Your suggestion using
normalized values is also a good idea dealing with a large span of
values, but with the reworked colorbar we think that this should not be
necessary.

Figure 4: fonts too small.
Font size increased.

250: Here a reference should be added or some additional analysis should
support this statement
"Moreover, the size of the adjustment factor is not
directly responsible for these large differences, as the impact on the
trends is more sensitive on the length and location of the
adjusted sub-period than the magnitude."
We decided to delete this as we neither found reference nor could do
additional analysis. It is also not relevant for our results.

252: I do not get this sentence. A multiplicative adjustment factor of
0.1 for instance is much more relevant than a multiplicative adjustment
factor of 1.1.
This is of course true. With "smaller than one" we didn't mean much
smaller than 1. The mentioned examples have adjustment factors around
0.6 - 1: Samedan and Sils-Maria (~0.8), Zernez (0.6 - 0.8) and Adelboden
(0.8 - 1). Here, we also decided that this sentence is not relevant for
our results and therefore deleted it.