



EGUsphere, referee comment RC2  
<https://doi.org/10.5194/egusphere-2022-715-RC2>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## **Comment on egusphere-2022-715**

Anonymous Referee #2

---

Referee comment on "The benefits of homogenising snow depth series – Impacts on decadal trends and extremes for Switzerland" by Moritz Buchmann et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-715-RC2>, 2022

---

The paper by Buchmann et al. studies the impact of homogenization on snow series in Switzerland. It is a continuation of previous work on break detections. The application and testing of different homogenization approach for in-situ snow cover series is important for many climatological applications, but yet little researched. The authors have made a considerable contribution in this area previously and also with this paper.

The manuscript is well organized and, generally, well written. Figures focus strongly on displaying trend magnitudes as colours and not as x- or y-axes, which sometimes only allows a rough interpretation. Finally, there are two major concerns that would need be addressed before publication (see below). One is related to the methodology, which is not well described. Also it is difficult to understand how the authors arrived at some of the conclusion (e.g., in the abstract). The other concerns the non-random changes in trends.

**# Major comments**

- The description of the different homogenization approaches is very explanatory and vague. I find it hard to distinguish between what the methods do and need in general, and how the authors applied them. Maybe a table would help? I think it is important to understand if the approach works daily or monthly, what thresholds on distance (h and v) were used – btw, are they the same for all methods, it is unclear to me – what thresholds on correlations, etc. Are the adjustment factors applied additive or multiplicative? I think the authors conclusion in the abstract are based on their understanding, but for readers it is impossible to follow.

- Throughout the manuscript I was surprised to see that homogenization drove trends to be more negative. What are the reasons for this? In theory, I would expect non-climatic breaks to be random, especially since I think manual observing procedures have remained the same for snow depth for the past, or? So there should be no instrument bias. Inhomogeneities are then mostly related to observer changes or relocations, which should not be biased towards less snow consistently. But please correct me, if I'm wrong here. The authors also confirm this supposition in L194, where the percentages they give show more or less similar strengthening and weakening of trends. I think this issues deserves some more consideration. It might be partly related to how strength and direction of trends interact, especially when close to zero, where small changes in trends can lead to a change in sign. Could the authors provide a figure where they show explicitly how trends have change in a numerical fashion? Currently, all plots show trend magnitude with colours, where it is hard to judge values from. Also, since the authors have a larger dataset in the background (the homogeneous series they did not use here, but in previous works), they could compare inhomogenous vs homogeneous series in their fraction of positive/negative trends to check whether inhomogeneities were really biased to more snow.

# Minor comments

L16: A station cannot be significant, but its trend. Please adjust.

L19: Do you have a reference for that 50%? Does it refer to all of NH?

L41: Would be good to include Marcolini here, too.

L53: If I remember correctly, Marcolini did that.

L106: IQR is a widely used term in statistics for inter-quartile-range (note R not N in quantile/quartile), please use another abbreviation for clearness. And I think you mean that you split into different bins based on quantiles, and not split into inter quantile ranges, which are only numbers (differences between the quantiles).

L118: "Since it was decided..." : I do not understand what you mean, or who decided.

L127: The authors should explain why they choose exactly this specific approach to define "better" performance for R50HSmax, since this is not a trivial task.

L146: For what exactly did you use the KS-test?

L174(paragraph): I find the reasoning irritating in this paragraph. Are the trends in snow driven by absolute temperatures? Which the authors imply in L177. And then the statement in L179 is based on what source? Is it stating theory or factual evidence?

Figure 1: Could you please add an elevation distribution of the stations? And highlight in the distribution homogeneous and not.

Table1: Would be good to know how many stations are below 1500m and above 1500m. Also is the number constant across dHS5, 30 and 50? Also the numbers seem to not add up: Shouldn't positive plus negative always be 100% for each column? It is for some columns, but not for all. And these should be consistent to the rows "pos to neg" and "neg to pos", no?

