Geoscientific Instrumentation Methods and Data Systems Discussions

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Comment on gi-2021-16

Anonymous Referee #1

Referee comment on "Evaluating methods for reconstructing large gaps in historic snow depth time series" by Johannes Aschauer and Christoph Marty, Geosci. Instrum. Method. Data Syst. Discuss., https://doi.org/10.5194/gi-2021-16-RC1, 2021

The manuscript "Evaluating methods for reconstructing large gaps in historic snow depth time series" by Aschauer and Marty is an interesting technical study focusing on interpolation of snow depth time series. The topic is relevant for the community focusing on the snow modeling and time series analysis. The paper summarizes in an effective way concepts which are known by the community, by applying six different interpolation methods to a snow dataset from Switzerland. I think the manuscript represents a good contribution to the current scientific discussion on the topic. I have a few suggestions that the authors could consider in the revised version of the manuscript:

- Lines 43-45: I only partially agree with this statement. The fact that temperature index models require only precipitation and temperature data is the only reason why someone should apply them. In fact, they provide only a very rough physical representation of snow processes. Moreover, it seems that the authors do not consider in the modeling approach processes such as snow redistribution driven by wind or gravitation. These processes are generally not dominant if the choice of the location of the monitoring station is performed properly, but the authors may comment on it.

- Line 70: I think time series available for this dataset are generally longer than the period selected by the authors (1999-2020). Could they provide shortly a justification for this choice?

- Line 87: should “station or both” be “station of both”?

- Interpolation methods: I fully understand that it is not possible to test all available interpolation methods, but I was surprised that a standard interpolation method like Kriging with external drift or let’s say Kriging based methods were not considered. Could the author please motivate this choice?

- Line 178: Is a threshold of 1 cm really relevant for tourism?

- The Matiu/WNR method should be consistently named in the manuscript

- Line 230 should “derived form” be “derived from”?

- Line 237 I think that beside the value of HSmax it would be interesting to compare when HSmax occurs using the different methods.

- Figure 4: it is very hard to read the values of r2, RMS and BIAS, please move them to a Table

- Although I acknowledge the difficulty of summarizing the results of this work in a graphical form, I find the quality of Figures 4, 5 and A1 low. The black lines in the blue box plots are hardly readable and in general the size of the subplots it is too small allow the reader a quantitative interpretation of the results.

- Line 298: It would be nice to have a ranking of the methods to be applied in these circumstance.

- The authors correctly point out that data interpolation is an important step to have longer time series for climatological analysis. However, after data interpolation the next step would be to homogenize stations. Homogenization of snow depth time series is an actual research topic (e.g., Marcolini et al., 2019) and it would be nice to see in the discussion part some comments given by the authors about the effect of the different interpolation method on the quality of the resulting time series. I do not expect a quantitative assessment, since it would probably result in another paper due to the required amount of work, however some qualitative insights would be interesting to stimulate further research in this direction.

Marcolini, G., Koch, R., Chimani, B., Schöner, W., Bellin, A., Disse, M., & Chiogna, G. (2019). Evaluation of homogenization methods for seasonal snow depth data in the Austrian Alps, 1930–2010. *International Journal of Climatology*, *39*(11), 4514-4530.