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Comment on gi-2021-16

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Community 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-CC1>, 2021

I've been following this work since over 1 year and am happy it's being published. Congratulation to the authors for this evaluation, which is immensely helpful for future climatological assessments of station observations of snow depth. Also, they are to be thanked for making the code publicly accessible.

I suggest using consistent naming of the Matiu/WNR method in figures, tables, and text. Personally, I prefer WNR, since it's not "my" method...

The comparison of more traditional approaches to ML tools is very useful.

The only thing that I found odd is the large bias and errors in dHS1. A higher one for HSmax is to be expected, because one value (maximum) can behave very sensitive. But if daily HS is reconstructed well, as well as HSavg, why not dHS1? However, I reproduced the results of dHS1 based on Matiu et al. 2021 and found basically the same order of bias in dHS1 for the WNR method. Even though, it appears to be highest in middle elevations (1000-1500m) and lower otherwise, and the bias decreases with a higher SCD threshold, e.g it's halved for 2cm, and becomes negligible (almost 1/6) for 5cm. There seems to be a minimal positive bias for low HS in daily reconstructions, negligible for HSavg, but enough to introduce errors in dHS1.

Maybe the authors could provide a table in the applicability and limitations section to summarize the reconstruction methods evaluated? Showing e.g. "Best/Good", "OKish", and "Not recommended" methods, depending on parameter (daily, seasonal, ...) and network density? I know, this might involve some arbitrary choices, but could be useful nonetheless. And, this could highlight the dHS issue and warn against using IDW for snow - in addition to what the authors already write in the text...