

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2022-69

Anonymous Referee #1

Referee comment on "Spatio-temporal reconstruction of winter glacier mass balance in the Alps, Scandinavia, Central Asia and western Canada (1981–2019) using climate reanalyses and machine learning" by Matteo Guidicelli et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-69-RC1>, 2022

In "Snow accumulation over the world's glaciers (1981-2021) inferred from climate reanalyses and machine learning", a machine learning model is applied to 95 glaciers on 3 continents to downscale precipitation and other variables from commonly used reanalysis products.

The problems begin with the title, which overstates its importance. Only a tiny fraction, in fewer than half of the continents, of the world's glaciers are examined. The manuscript has too many figures and tables. The manuscript is supposed to be within 12 journal pages for TCD. The tables and figures alone, most of which occupy a full page, would take up this much space. The figures are bloated. For example, there is no need to illustrate "Tree 1" nor "Tree N", both of which are identical in Figure 3. The PCA section (4.1) doesn't tell the reader much more than the fact that elevation is the most important downscaling predictor. The leave one out validation is problematic as there is no independent validation dataset used, meaning that biases in precipitation are unlikely to be identified.

ERA-5 and MERRA-2 reanalyses are used without any mention of their potential large biases in the mountains. For example, Liu and Margulis (2019) report that MERRA-2 underestimates snowfall (which is based on the "PRECTTOLAND" variable used here) by 54% in High Mountain Asia. It's not clear to me that the downscaling techniques presented here will correct that bias, as no independent evaluation of precipitation is presented. Melt and sublimation are ignored in the "winter mass balance," which is then the wrong term.

After carefully searching through the text, I still cannot understand how precipitation phase was treated. It seems to have been ignored as SWE is used interchangeably with the downscaled precipitation on glaciers. But then, in Table B1 and B2 ERA-5/MERRA-2 snowfall variables are listed as predictors?

Because of its excessive length, lack of clarity, and questionable assumptions, I recommend this manuscript be rejected. For a resubmission, I suggest the authors consider an independent evaluation of snow accumulation and at least an explanation of how precipitation phase was treated. The size of the figures and tables needs to be cut approximately in half.

Works cited

Liu, Y., and Margulis, S. A.: Deriving Bias and Uncertainty in MERRA-2 Snowfall Precipitation Over High Mountain Asia, *Frontiers in Earth Science*, 7, 10.3389/feart.2019.00280, 2019.