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Interactive comment on "The evaluation of the potential of global data products for snow hydrological modelling in ungauged high alpine catchments" *by* Michael Weber et al.

Anonymous Referee #1

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This is a review of "The evaluation of the potential of global data products for snow hydrological modelling in ungauged high alpine catchments" by Weber et al. The paper investigates the impacts of using a series of climate data products to force a hydrological model and it's advanced snow modules and compare the behavior in terms of snow process representation and runoff. The authors also compare the impact of modifying the DEM resolution to investigate the impact of using coarser (but free) DEMS compared to more refined (but expensive) ones such as LiDAR. The study takes place on a small, 12km2 catchment in Bavaria near the summit of Zugspitze. The authors find that the choice of DEM is not as critical as first thought, and that the use of global climate products can yield reasonable results in hydrological modelling but that there is

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Discussion paper

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still improvements to be made. I have read the paper and found it very interesting and complete. The text flows generally well, although some expressions and sentences don't "sound" right and should be corrected by a native speaker. Scientifically, I have some issues with a few aspects of the work and I also have some suggestions to improve the work and make it more useful to the community. I will start by mentioning the more general points and end with smaller, more technical points.

General comments:

1- The authors use a variety of climate data products to drive the hydrological model to simulate the snow accumulation and melt processes. There are two station datasets (local and from a somewhat distant but similar catchment), satellite products and the ERA20C product. I have a problem with the latter. It can be argued that the 0.05°, and to some extent the 0.2° / 0.25° products can be "reasonable" in terms of spatial resolution to represent the 12km2 catchment. However, the 125km resolution ERA-20C has a resolution of 125x125 = 15625km2, or more than three orders of magnitude difference. The catchment represents less than 0.08% of the tile size. It seems unreasonable to me to include it in the analysis. I think no researcher would use this product for such a small catchment in real world applications. The authors talk about using ERA-20C because of the correction of Gao et al. 2012, but I am positive that using a product such as ERA5-Land (With a 0.1° resolution) would be a better proposition.

2- The GTOPO30 product seems to give reasonable results and the authors state this in several places in the paper. However, it seems that it performs well because it is biased and it is "counteracting" the bias of the meteorological products. Therefore it is better, but for the wrong reasons. I think it would be warranted to add a section (or sentence) in the discussion to clarify this to prevent readers from getting the wrong impression of the quality of GTOPO30. Again, I think users working on very small catchments with high gradients would never use such a coarse product, it was not designed for this.

3- I think the authors should have compared the snow modelling results they obtain with those from a reanalysis directly, such as ERA5. This could be a much simpler way than using reanalysis meteorological data to drive a hydrological model. It seems to me that this would be a much simpler alternative than using these convoluted methods? I think it could be appropriate to at least mention the possibility here as it fits the bill perfectly: using publicly available global datasets to model snow hydrological modelling in alpine catchments. Furthermore, it could be used to force the initial states of the hydrological model to simulate runoff.

4- I notice that there is no section on model calibration, as this model does not require calibration but is instead "parameterized" to the environment. I suggest adding this information as it is atypical for a model to not require calibration.

Specific comments:

Lines 155-160: Do I understand correctly that all precipitations were multiplied by 1.5? The SWE technically also includes the effects of ablation/sublimation/transport, so I think it is dangerous to correct precipitation in this manner as the actual real factor is probably different. Perhaps add some limitations in the text here.

Lines 296-301: This section is a bit confusing. Also, there are 2 peaks of runoff caused by snow accumulation periods? There are two in the year?

Line 322: missing year for reference "Danielson and Gesch".

Line 336: "Adjusted it to the ALOS…" : Should this be altitude-corrected? Please clarify

Line 378: "shorter" should be "less"

Line 394: "snow towers" needs to be defined better.

Lines 413-422: This section is not clear upon reading. I needed to read more of the paper before coming back and understanding this section. Please simplfy and/or clarify.

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