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Discussions

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.

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Response to Anonymous Referee #1

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

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Discussion paper

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 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.

Author's answer: We would like to thank the reviewer for carefully reading the manuscript and providing us with a very constructive and overall positive feedback. We have thoroughly considered all of his/her comments and address them point by point in the following.

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.

Author's answer: This is a very important comment and we are also convinced that showing results of the quite new ERA5-Land product will improve our work. Thereafter, we absolutely agree with your suggestion, and already performed model runs with ERA5-Land (approx. 9 km) data for the updated version. With ERA5-Land we now were already able to achieve considerably better model results (R^2: 0.49 (LWD), 0.86 (DWD); NSE: 0.19 (LWD), 0.54 (DWD)) compared to all presented setups so far driven with global meteorological data. We will present and discuss these results in the revised version of the manuscript. Moreover, as we are also interested in the impact of different products and scales of global meteorological inputs, we will include ERA5 (approx. 31 km) as well. The ERA5 product of the ERA family bridges the gap in terms of spatial scales to the very coarse product ERA-20C. We also already conducted model runs with ERA5 and could achieve even better results than with ERA5-Land (R^2: 0.53 (LWD), 0.77 (DWD); NSE: 0.52 (LWD), 0.75 (DWD)). We will also discuss shortly the fact that although ERA5 is coarser than ERA5-Land, it can lead to better results. Moreover, we absolutely agree with you that the ERA-20C data set is actually too coarse for the application in such a small high alpine catchment. However, we still would like to include this product as it was widely used before ERA-5 came up and we also want to show the results in comparison to products with finer spatial resolution. In the text we will add that nowadays in real world applications (and especially for such a small high alpine catchment) this product will not be chosen (similar to the GTOPO30), but that we added it in terms of showing a broad variety of different products, including also 'older' and coarser global products. In addition, we would like to keep ERA-20C as we test with this product also the performance of the 'Gao et al. 2012' - correction method for ERA-20C temperature data, which was developed with data from RCZ. As already presented, these results showed a better performance compared to the uncorrected data. However, the new ERA5 and ERA5-Land setup performed considerably better.

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.

Author's answer: We agree with you that the GTOPO30 product was not designed for applications in small and highly heterogeneous catchments. We will point this out in the discussion section. Nonetheless, such a product might be useful in large mountain domains and if computational power is limited. Moreover, we will clarify the reason why the GTOPO30 product performed so well at a first glance. The reasonable results derived with the GTOPO30 DEM are, however, just specious due to the coarse resolution, where small scale topographic effects are averaged out. This leads to the fact that the meteorological products are interpolated in a too coarse topographic description of the catchment, which might level out the meteorological data. Therefore, for some parts of the catchment the values are too high, while they are too low in other parts. Nevertheless, it might well capture the mean topographic/meteorological situation of the catchment as it is the case in our study.

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. Author's answer: Indeed, the quite new ERA5-Land reanalysis product includes many very interesting variables such as snow height and SWE as an average over the entire pixel size of the product. We also agree with your statement that integrating ERA5-Land snow would be an interesting addition. In this regard, we will focus on the ERA5-Land SWE product and will compare

it to the specific snow hydrologic variables MSWE, DMSWE, snow cover duration, ablation shown in Table 6 for the entire RZC. In our opinion, it does not make sense to compare this ERA5-Land snow product to single HRU outputs or to compare it to the measurements at the DWD and LWD stations due to the very different spatial scales. In addition, we like your idea that such products could also be used to force initial states of the hydrological models in ungauged basins at certain time steps. However, this is out of the scope for this manuscript, but we will definitely mention that this would be very interesting to integrate in further studies.

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.

Author's answer: We will follow your recommendation and will add the following sentences: "Unlike most hydrological models, the physically based CRHM does not require calibration. The only parameters that we adjusted for modelling are the previously named catchment/HRU specific physiographic parameters."

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.

Author's answer: We only corrected snow precipitation with this factor and agree with you that the real factor might be different for the named effects. However, the factor was determined in a way that minimizes these effects. To minimize the influence of sublimation, data with a 10 minute temporal resolution were used for the comparison of SWE and precipitation. Before the comparison, the effect of wind on the measured snow water equivalent was investigated. Therefore, time periods without snow fall, temperatures below freezing and strong wind were checked. This revealed no wind induced snow redistribution that could be captured by the snow gauge. Nevertheless, there are effects, which could not be taken into account such as sublimation from the snowflakes during the snow fall event due to strong winds. As you suggested, we will add some limitations and clarifications to the text.

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?

Author's answer: We agree with you that this section is a bit confusing and thus will be rewritten. We just wanted to hint at the fact, that there are datasets (the two with in situ measured data, reference and Mt. Wendelstein) which have their precipitation maximum during the snow accumulation period, while the other data sets have their maximum in summer. There are not 2 peaks of runoff within a year on a monthly basis in each setup. The reference data shows a maximum in precipitation in March and November, which is during the snow accumulation season in the RCZ. The regime of Mt. Wendelstein is similar having also a maximum in March, however, without the second maximum in November. All other meteorological datasets indicate their maximum precipitation in summer although they show a local maximum in March. In this comparison, the CHIRPS precipitation data set is in-between. Its precipitation regime is similar to the reference; however, with a more pronounced maximum in August.

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

Author's answer: We will add the year.

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

Author's answer: We will clarify the sentence: ". . .adjusted it to the ALOS, SRTM and GTOPO30 specific HRU altitude, slope and aspect (Table 2, for the methods refer to Section 4.1). . ."

Line 378: "shorter" should be "less"

Author's answer: Will be corrected.

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

Author's answer: We will add the following definition: "Snow towers are an effect in snow hydrological modelling that occurs mainly at higher altitudes and describes the unrealistically high accumulation of snow over several years. Reasons can be the insufficient description of redistribution processes in the model or unrealistic meteorological driver data (Freudiger et al., 2017)."

Line 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.

Author's answer: We will make some changes to the text to clarify this section: "Regarding the model setups with different topographic parameterizations on basis of the three different globally and publicly available DEMs ALOS, SRTM and GTOPO30, the snow cover development at both measurement was simulated realistically (Fig. 6g, h, Table 5). This results in R^2 - values above 0.8, and NSE values above 0.77, for all three setups at the LWD site. The results are very similar to the reference at the measurement stations (Table 2). The two 30 m DEMs SRTM and ALOS, show slight differences in the NSE values at the measurement stations, which can be attributed to the differences in their topography despite having the same resolution. Such an effect was also mentioned for Qsi in Section 4.2."

References:

Freudiger, D., Kohn, I., Seibert, J., Stahl, K., and Weiler, M.: Snow redistribution for the hydrological modeling of alpine catchments, WIREs Water, 4, e1232, doi:10.1002/wat2.1232, 2017.

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