Community comment on tc-2022-146

Major comments

Missing information on the SAR data

Using the Sentinel-1 SAR imagery to correct the daily snow cover maps is a very creative and effective solution, and probably one of the main contributions of this paper. However, even though most of the methodology is based on the SAR-derived catchment state and it is practically treated as ground truth, there is a lack of background information and uncertainty estimation:

- It is not mentioned how forests are dealt with in the identification of the catchment state. Marin et al. (2020), from which this part of the methodology is adapted, mention that "the response to the wet snow becomes more complex in case of the snowpack in forest", but leave this out of the scope of their study. Therefore, if forests are included in this study (which I assume from the elevation ranges of the catchments), it should be elaborated how effective the methodology is in forested areas. It would also be useful to know what percentage of the catchments is covered by forests.
- The temporal and spatial resolution of the SAR imagery is never explicitly mentioned.
- The uncertainty as quantified by Marin et al. (2020) is not mentioned either. They estimate the RMSE of the start of the moistening phase to be 6 days. An error of 6 days in the ablation identification could lead to large differences in the SWE reconstruction (equation 3), and should therefore be acknowledged or taken into account.
- In L186 and L342 it is mentioned that ablation only occurs with a drop of the SAR backscattering signal, but during the runoff phase the backscattering signal is increasing (Marin et al., 2020). I assume that this increase should also be included in the definition of the ablation days.
- Sentinel-1 is only mentioned as S1 and never fully spelled out.

SWE loss during moistening and ripening phase

In the methodology the assumption is made that during an ablation day with a non-zero DD there will always be a loss of SWE. However, if I understood it correctly these ablation days include all three phases of snow melt (moistening, ripening and runoff), even though during the moistening and ripening phases there is rarely any SWE loss, as also shown by Marin et al. (2020). It would perhaps be more physically accurate to limit the SWE loss to the runoff phase. This would not change the total amount of calculated melt energy and therefore accumulation, but it would possibly change the peak SWE estimate and likely change the timing of the SWE loss. In figure 3 for instance, if the first ablation phase would consist only of moistening or ripening there would be no loss of SWE and the peak SWE would be higher after the second accumulation phase. Moreover, if the runoff is limited to the runoff phase, the SWE loss would be delayed and more concentrated. This would have significant hydrological implications.

Since the classification between the three snow melt phases can easily be determined from the SAR imagery, it would seem that adding this information to the SWE reconstruction would not lead to a large increase in complexity of the methodology. It would, on the other hand, possibly lead to an increased physical basis of the methodology, especially given the known issues around a constant DD factor (Magnusson et al., 2015). I'd be interested to know your thoughts on this.

Figure C3 and C2 are more informative than figure 9

Showing how the modeled SWE behaves as a function of the elevation, slope and aspect is indeed very insightful, but only when compared to the behavior of the observed SWE. Therefore, I find figure C2 more informative than figure 9, even if ideally the maximum SWE would indeed be the best moment for comparison. However, figure C2 does not remove the bias and therefore does not allow for a relative comparison between the model and the observations per terrain parameter. Figure C3 does this, but the high number of figures does not allow for an easy general comparison. A compressed version of figure C3, for example with averaged biases, could potentially be more informative and would be easier to include in the main text as well. Other than that, I believe figure C2 would be clearer with lines instead of bars, and figure 12 could be left out of the manuscript.

Figure 13

The comparison between the modeled SWE and the discharge is very insightful, but the accompanying explanation is lacking in depth. Indeed the peak SWE correctly matches the peak discharge among the two years, but the timing in both years shows very different behavior. In 2020-2021 the response is much more direct, while in 2019-2020 it's more delayed. Is this because the soil in 2020-2021 is already saturated after the rainfall events in Jan-March? And which part of the discharge after July originated from snow melt and which part from rainfall? On a side note, it would perhaps be insightful to show the rainfall rates on the inverted y-axis, and have the SWE loss and discharge on the same y-axis with the same units (e.g. m3/day).

Minor comments

- The resolution of the figures is often not high enough to be able to distinguish important details, especially in the spatial plots. Unless this is a result of the compiling of the preprint, increasing the resolution or saving the figures in vector format (for the graphs) would benefit the manuscript.
- The legend nor the caption in figure 4 explain what the red points stand for (which I assume to be temperature stations).
- Line 289 ("Note that the number of days in accumulation varies for each pixel and consequently the coefficient is function of time and space.") contradicts L104 ("According to the state, that is assumed to be homogeneous for all the pixels of the catchment,… ").
- Even if it's clear from the text, the caption of table 1 should perhaps mention of which catchment these results are
- L372: "The highest bias and RMSE values are generally encountered in the mid-winter acquisitions." As I see it, this is not reflected by the RMSE and bias values we see in table 1, unless mid-winter means march-may.
- In the author contributions, MC should be CM
- L433 I'm guessing that you're talking about MODIS, but I believe it would be good for clarity if you mentioned this
- L464 "replacement with snow", I assume this should be "replacement with snow-free".
- Figure 14 and 15 are rather small, and I would perhaps have appreciated to see the accumulation and ablation phases reflected in the background of the plots

Magnusson, J., Wever, N., Essery, R., Helbig, N., Winstral, A., and Jonas, T.: Evaluating snow models with varying process representations for hydrological applications, Water Resour Res, 51, 2707–2723, https://doi.org/10.1002/2014wr016498, 2015.

Marin, C., Bertoldi, G., Premier, V., Callegari, M., Brida, C., Hürkamp, K., Tschiersch, J., Zebisch, M., and Notarnicola, C.: Use of Sentinel-1 radar observations to evaluate snowmelt dynamics in alpine regions, Cryosphere, 14, 935–956, https://doi.org/10.5194/tc-14-935-2020, 2020.