Answer to the Community Comment #1 – Manuscript tc-2022-146

Congratulations on this great work, I believe it is an important contribution to the field of SWE modeling. I find your solutions very original and at the same time intuitive and straightforward. The manuscript is very readable and detailed, most of the questions that came up while reading were answered later on. Having said that, I do have a number of comments, which I have gathered in the attached document. I hope they are clear and that they will be useful to you, and my apologies if I interpreted the manuscript incorrectly.

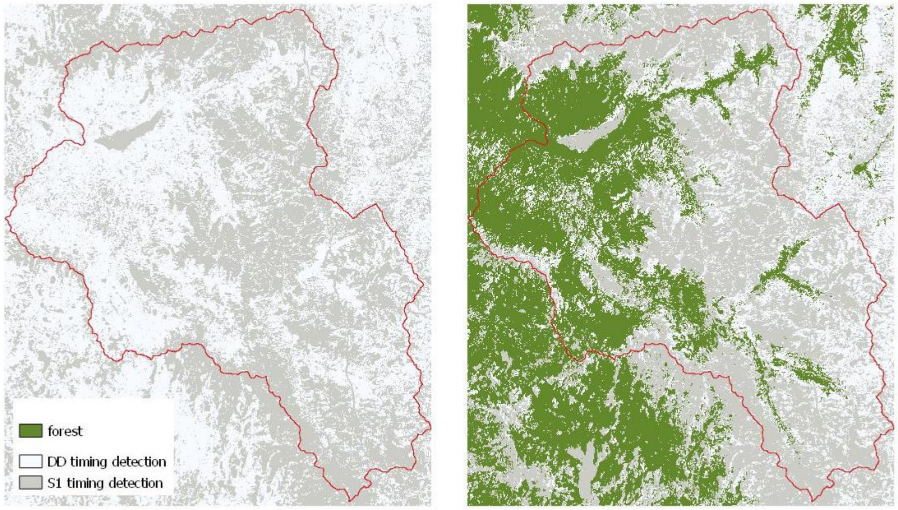
We thank Pau Wiersma for his constructive and deep feedbacks and comments on our manuscript. We went through each point and took advantage of this community comment to improve the quality of the manuscript. Our answers are reported in blue.

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.

  Thank you for pointing this out. The percentage of forested area is around 25% for the Schnals catchment and 32% for the South Fork catchment (data source Masanobu et al., 2014). This information will be included in the description of the study areas in the manuscript. Consequently, masking out these areas would have resulted in ignoring a relatively large portion of the catchment, as for most of the catchments located in our Alps.
  Indeed, the mechanisms that drive the SAR backscattering in forested areas covered by snow are complex. The radar signal interacts with both the wet snow, which covers the ground and/or the top of the canopy, and the various parts of the trees. The different forest density, the type of the trees and the distribution of the snow above or below the canopy may generate a temporal signature that is different from the one described in Marin et al., 2020, or also very similar as shown recently by Darychuk et al., (2022) (in discussion). Without trying to solve all the possible complex backscattering interactions explicitly, in this paper we propose to apply the method only for those pixels in the scene where the characteristic U-shape signature appears. For doing this, we set up the rule described at L341 of the manuscript, i.e. "If at least one track shows a drop of at least 2 dB in the signal (Nagler and Rott, 2000) w.r.t. a moving average of the 12 previous days, that day is considered to be in ablation." We have observed from our experimental results that this simple rule applied to the daily interpolated backscattering time series is effective in detecting if the multitemporal backscattering signal of a given pixel present the characteristic temporal signature or not. Following the proposed approach, we then consider in ablation all the days from this moment until the snow disappearance (this will be better clarified in the next version of the manuscript). For the pixels that do not present the characteristic U-shape, we rely on the degree day model for the identification of the ablation. As an example, we add here a figure showing the forest mask and the pixels where S-1 information has been used to detect the ablation phase. The plot refers to the South Fork catchment for the season 2020/21. For brevity, we omit the other catchment and other seasons that show comparable results. It is possible to see that our algorithm does not identify the temporal signature of the backscattering over forested areas (in white) while it detects the U-shape elsewhere.



However, we should be aware that forested areas are challenging not only for what concerns S1 backscattering, but also for the degree day model as well as the snow cover detection. For example, the interception by canopy influences the melting phase, due to the impact of trees on the heat conduction and radiation balance that are not considered in the degree day model. Furthermore, snow under canopy is difficult to detect by optical satellites. Notwithstanding, we decided not to mask out forests given their quite significant coverage in the analyzed catchments and the effectiveness of the obtained results, which do not show a significant bias in correspondence of forest areas. In any case, we also believe that this represents a hot topic that needs further research.

- The temporal and spatial resolution of the SAR imagery is never explicitly mentioned.

The spatial resolution is 25 m, the same as the final time-series. We will add this information to the manuscript. The temporal resolution is 6 days considering the same geometry of acquisition, however all the available S1 acquisition tracks over the catchment have been considered. For example, in Schnals catchment up to 3 tracks are acquired in the timeframe of 6 days (i.e., t_0, t_{0+1}, t_{0+5}). It is worth stressing the fact that we applied a linear interpolation in time to obtain daily data as stated in L341.

- 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.

Thanks for rising this point. We consider this information very relevant also for this paper and we will recall it in the revised version of the manuscript. Beside the uncertainty introduced by S1, we observed and discussed in the paper that the possible errors of the proposed method are mitigated by the fact that the potential melting calculated via the degree day model is low in the early melting i.e., in the moistening and ripening phases due to the low air temperatures (see answer below for further reasoning on this important part of the proposed approach).
Finally, it is worth mentioning the fact that the uncertainty provided in Marin et al., (2020) has been calculated w.r.t. the output of a snow model, which is still prone to error (even if particular attention has been dedicated to the calibration and to the selection of the test site, a residual error may be still present).

- 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.

Thanks for pointing out this issue. We identify the ablation as the period from the day when a – 2dB drop of S1 backscattering is detected until the snow identified by optical-derived time series disappears. This will be better clarified in the final version of the manuscript.

- Sentinel-1 is only mentioned as S1 and never fully spelled out.

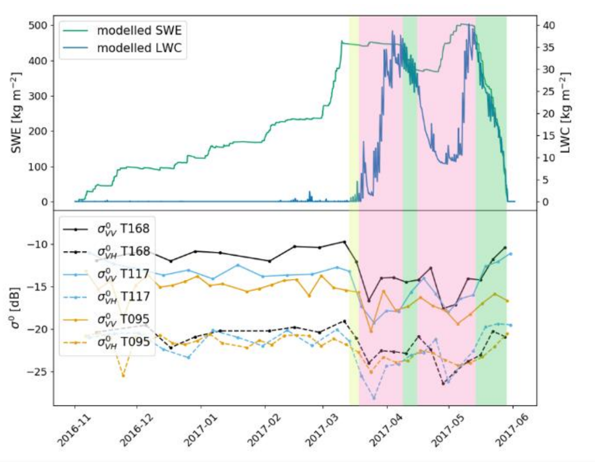
  Thank you for this observation. We will add this in the manuscript (L169).

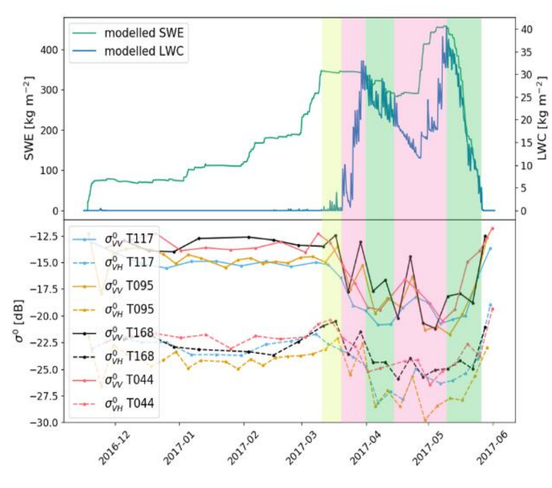
SWE loss during moistening and ripening phase

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.

We appreciate your comment that gives us the opportunity to better clarify this crucial point. We agree that considering the SWE loss happening during the runoff phase, which coincides with the minimum of the backscattering, is physically more accurate than considering the moistening, i.e., the day when a significant drop in the backscattering (at least of 2 dB) is observed. However, as described previously in the third answer, the uncertainty in the phase detection through S1 also plays a key role. For example, as illustrated in Marin et al., (2020) in Table 5 the difference in terms of days regarding the runoff onset had reached +19 days in one unlucky case.

This delay can introduce considerable errors since this phase is characterized by relative hot temperatures and consequently high melting rates. On the other hand, anticipating this phase does not lead to large errors due to lower temperatures and lower melting rates. Given our experimental results, we think that it is more conservative to make a mistake in the anticipate the detection of the runoff onset. Moreover, the backscattering signal may be difficult to interpret especially in case of alternated moistening and refreezing events or multiple runoff events. These situations lead to multiple local minima in the signal, as also shown in Marin et al. (2020) in figure 4c or 4g that we report also here, making the definition of a detection rule involved.





Nevertheless, it is important to stress the fact that the information provided by S1 provides more detailed information about the spatial distribution of the melting process than the DD derived by the spatialized temperature. In this work a first exploitation of this information has been proposed but several future developments are needed for a complete exploitation of the S1 time series as discussed in the paper. This should start from a normalization of the backscattering acquired for each of the available tracks for the effects of the topography. This may help in the identification of the complex variations described in the example before (e.g., Small, D. (2011)) via a definition of more accurate set of rules.

Figure C3 and C2 are more informative than figure 9

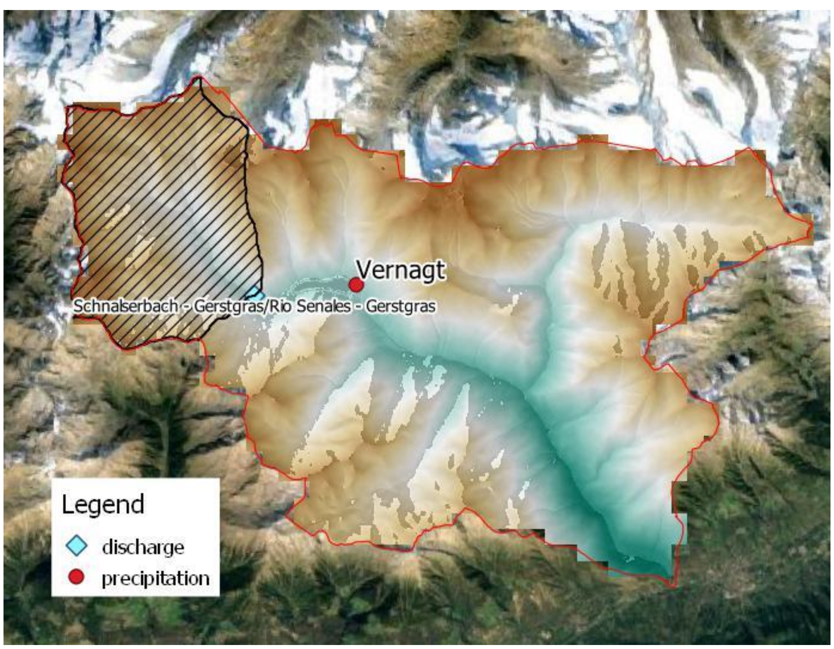
Showing how the modeled SWE behaves as a function of the elevation, slope and aspect is indeed very insightful, but only when compared to the behavior of the observed SWE. Therefore, I find figure C2 more informative than figure 9, even if ideally the maximum SWE would indeed be the best moment for comparison. 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.

We agree that figures C2 and C3 contain many subfigures, but we believe that it is important to show in detail how the proposed approach performs w.r.t. a reference product and to differentiate among the different periods of the year when the ASO product is available. So, we would keep both figures in the Appendix, as already done. On the other hand, Figures 9 and 12 show the temporal stability of the method. They also represent an easy and direct comparison with, for example, the output of a third-party hydrological modeling run on the same basin. A compressed version of C3, as suggested, would show how the errors are distributed over the different topographic parameters but provides less details, which may be useful from a hydrological point of view. Even if we are in favor of adding this plot to the paper, we will wait for other reviewers' opinion on this matter if any.

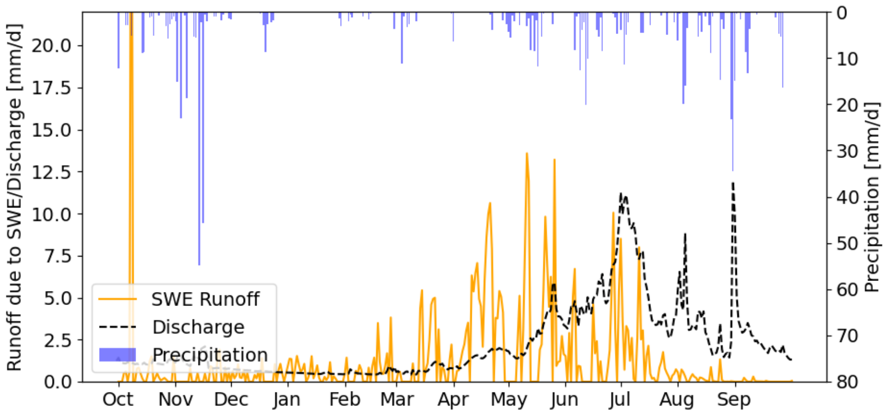
Figure 13

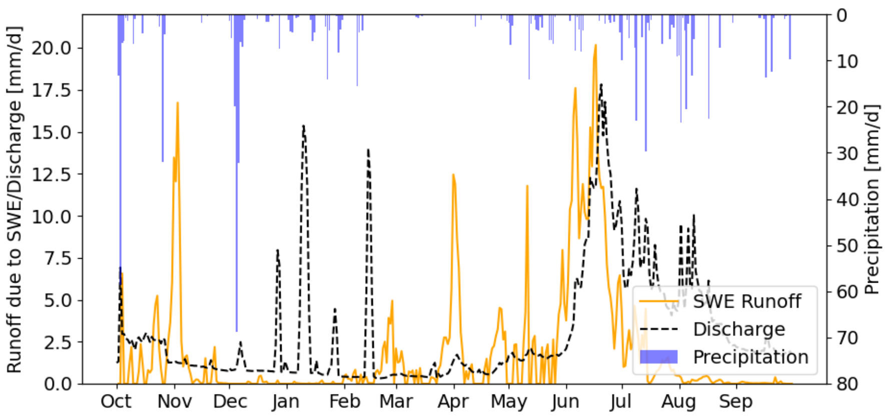
The comparison between the modeled SWE and the discharge is very insightful, but the accompanying explanation is lacking in depth. Indeed the peak SWE correctly matches the peak discharge among the two years, but the timing in both years shows very different behavior. In 2020- 2021 the response is much 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).

We thank you for this meaningful comment that stimulated our investigation even though this is going beyond the scope of the paper. We have reported in the manuscript only the comparison between the total SWE calculated for the entire catchment area and the discharge measured at Schnalserbach – Gerstgras, since this qualitative analysis gives the flavour of how the output of the proposed approach can be exploit from an hydrological point of view.. To answer this question and further investigate our results, we decided to deepen the analysis and consider the SWE related to the subcatchment closed at the outlet point Schnalserbach – Gerstgras, as shown in the figure below. This makes the two variables more comparable, despite the analysis remains qualitative.



We present here these two plots for the two seasons, where we report the SWE variations that are associated with a runoff (i.e., only when they are associated with a decrease of SWE hence generating a runoff) and the discharge expressed in m3/day and with the same scale as suggested, and on the other axis the daily precipitation expressed in mm.





While a proper answer to your question would require a complete hydrological analysis (that is out from the purpose of this paper) and a hydraulic characterization of the watershed properties, we can observe that there is a good agreement in terms of both timing and quantity among snow generated runoff and discharge. The riverine discharge starts increasing in correspondence with the snowmelt and it starts decreasing when also the snowmelt is reduced for both periods. However, it is true that during the first year there is a delay while the response is more direct for the second season. More than differences in terms of precipitation that do not show any evidence, we ascribe this situation to a different snowmelt rate. Indeed, the season 2019-20 shows a earlier, weaker, and longer distributed snowmelt period, interrupted by periods with lover SWE output (as end of March-beginning of April, beginning of May or middle of June). This situation may favor ground infiltration with a predominance of subsurface runoff w.r.t. surface runoff, that contributes slowly to the discharge. On the other hand, the season 2020-21 shows a long and high intensity SWE release (end of May-end of June) that may cause a sudden saturation of the soil, with predominant surface runoff that contributes more directly to the discharge. This hypothesis is also confirmed by recent literature, which shows that when snowmelt is earlier, it is also more intense and runoff response could be reduced, with strong implications for future climate change impacts (Musselman et al., 2017). However, other contributions should be considered for a proper analysis, as for example the storage of water in the two snow reservoirs that are present in the territory. We can add this discussion to the manuscript with the hope to stimulate works that will exploit data derived from the proposed approach.

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.
  We agree with this observation. Unfortunately, due to the large size of the manuscript, we compressed it without taking care of the result. But we will absolutely consider this problem in the next version.

- The legend nor the caption in figure 4 explain what the red points stand for (which I assume to be temperature stations).
  Thank you for noticing this. The red points represent the temperature stations. We will add this information to the legend.

- 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,… ").
  We would like to explain the sentence "the number of days in accumulation varies for each pixel" more clearly. It is true that the state is assumed to be homogeneous for all the pixels of the catchment, but only for those pixels that are snow covered. The necessary condition to have an accumulation, is that the pixel must be snow covered. This information is provided by remote sensing observation. So, even though the state is assumed to be homogeneous, the snow duration varies for each pixel resulting in a different number of days in accumulation for each pixel. Consequently, the coefficient k that is the subject of L289, is a function of time and space. We propose to rephrase the second sentence with "According to the state, that is assumed to be homogeneous for all the snow-covered pixels of the catchment,…" to state this more clearly.

- Even if it's clear from the text, the caption of table 1 should perhaps mention of which catchment these results are.
  Thanks for the comment. We will add this in the caption.

- 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.
  Yes, we meant the first dates acquired before the full melting period in March-May. We propose to rephrase the sentence with "The highest bias and RMSE values are generally encountered for the first available ASO acquisitions during the period March-May."

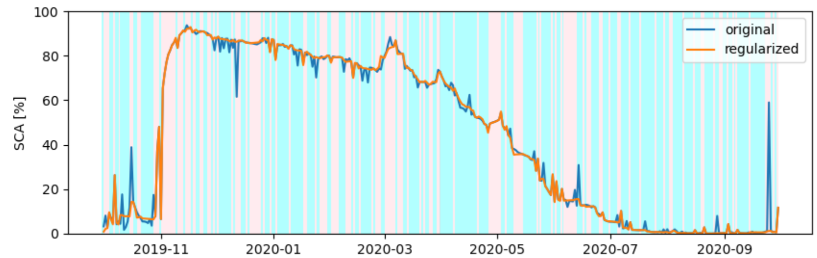
- In the author contributions, MC should be CM.
  Thanks for the comment. We will correct this.

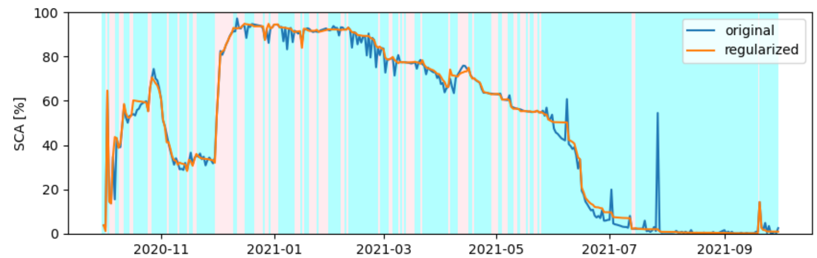
- L433 I'm guessing that you're talking about MODIS, but I believe it would be good for clarity if you mentioned this.
  We are talking about remote sensing products in general. The two examples we reported in the paper (L435) are: i) the SCA variations, which can be derived by optical data as MODIS, Sentinel-2 or Landsat and can be used to identify an accumulation or melting event, and ii) the snow depth, which can be derived from SAR sensor (Lievens et al., 2022 Tsang et al., 2021) (L441), can be used to identify the accumulation events.

- L464 "replacement with snow", I assume this should be "replacement with snow-free".
  Yes, you are right. Thank you for noticing this mistake.

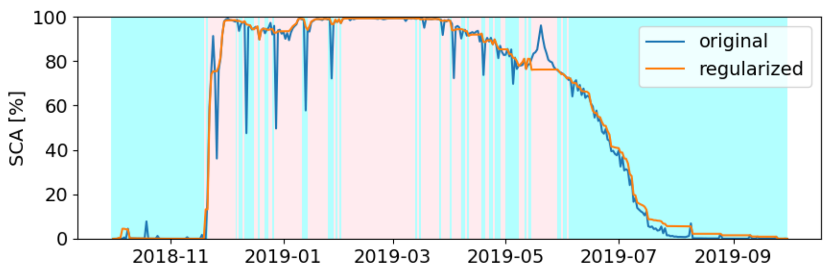
- Figure 14 and 15 are rather small, and I would perhaps have appreciated to see the accumulation and ablation phases reflected in the background of the plots.
  Thank you for this useful suggestion. Here we add the updated plots with a cyan background for days identified to be in ablation and pink background for day identified to be in accumulation.

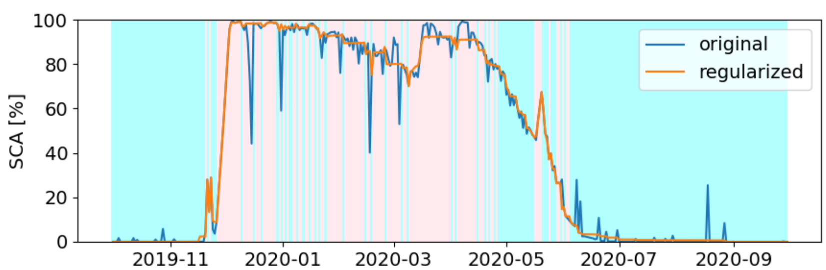
The following figures refer to the Schnals catchment.

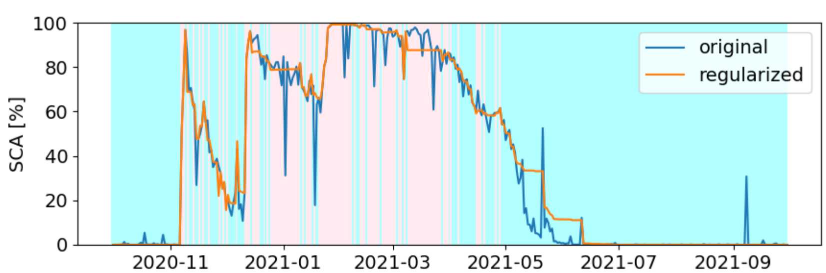




The following figures refer to the South Fork catchment.







References

Darychuk, S. E., Shea, J. M., Menounos, B., Chesnokova, A., Jost, G., & Weber, F. (2022). Snowmelt Characterization from Optical and Synthetic Aperture Radar Observations in the Lajoie Basin, British Columbia. The Cryosphere Discussions, 1-27.

Lievens, H., Brangers, I., Marshall, H.-P., Jonas, T., Olefs, M., and De Lannoy, G.: Sentinel-1 snow depth retrieval at sub-kilometer resolution over the European Alps, The Cryosphere, 16, 159–177, 2022

Masanobu Shimada, Takuya Itoh, Takeshi Motooka, Manabu Watanabe, Shiraishi Tomohiro, Rajesh Thapa, and Richard Lucas, "New Global Forest/Non-forest Maps from ALOS PALSAR Data (2007-2010)", Remote Sensing of Environment, 155, pp. 13-31, December 2014. doi:10.1016/j.rse.2014.04.014.

Musselman, K. N., Clark, M. P., Liu, C., Ikeda, K., & Rasmussen, R. (2017). Slower snowmelt in a warmer world. Nature Climate Change, 7(3), 214-219.

Small, David. "Flattening gamma: Radiometric terrain correction for SAR imagery." IEEE Transactions on Geoscience and Remote Sensing 49.8 (2011): 3081-3093.

Tsang, L., Durand, M., Derksen, C., Barros, A. P., Kang, D. H., Lievens, H., ... & Xu, X. (2022). Global monitoring of snow water equivalent using high-frequency radar remote sensing. The Cryosphere, 16(9), 3531-3573.