Answer to Anonymous Referee #1 – Manuscript tc-2022-146

I would like to congratulate the authors to this very interesing work, as they have presented a novel methodology and useful contribution to snow science. The presented approach might open new doors for reconstructing snow water resources also on larger scales, by employing multiple data sources. Especially, the usage of SAR backscattering signals to both, detect snow ablation, and indirectly correct daily high resolution SCA maps is an original methodology. The simple, yet efficient basic concept of the reconstruction approach, together with the parsimonious use of in-situ data is highly appealing. In my opinion, the organization of the work is good and the presentation of the results is rather clear. However, many sentences could be restructured to make for an easier read. I have made various comments on the manuscript that I hope will help to improve the paper.

The authors thank the anonymous referee for the positive comments. In the following, we address each point raised by the referee and take advantage of them to improve the quality of the manuscript. Our answers are shown in blue.

There are still some important details that remain unclear to me, especially concerning the determination of the catchment state:

- It would be helpful to see a more detailed description about how the ablation state is derived from Sentinel 1 data and how this would translate to the three snowpack phases described in Marin et al. (2020) (i.e. moistening, ripening, runoff). In line 185 only a "relevant drop" in backscattering is mentioned. Does this mean the catchment state ablation already starts when any liquid water is present in the snowpack? Since it seems possible (at least with S1A and S1B) to identify the snowpack runoff phase and a separation to previous moistening and ripening phases, the use of a DD-style melt model would be much more justified - as this is practically eliminating the need to track energy states (cold content) in the snowpack modelling. Please clarify if ablation is based on a drop in backscattering (melting phase) like line 185 suggests, or on the minimum of backscattering (runoff phase), as implied line 174. If the latter is the case, there is more physical grounds to employ a DD-style melt model and this should be brought forward in the text. However, if the ablation state is corresbonding to the moistening phase (i.e. a mere drop in backscattering), this decission should be also explained in more detail.

  We thank the reviewer for this comment. The same point was noted in the Community Comment #1, for which we provided an answer. As stated by the Reviewer, the choice to consider the runoff onset as the beginning of the ablation phase is more adequate from a physical point of view. In the current version of the manuscript, we identified the ablation as the period beginning from the day when a – 2dB drop in S1 backscattering is detected until the total disappearance of the snow, as identified by optical-derived time series. During this period, snowmelt is possible only if the stations do not identify any accumulation event and if the DD method provides an effective potential melting. In other words, we assumed that the ablation onset corresponds to the moistening onset. We decided to consider the moistening onset instead of the runoff because Sentinel-1 presents an uncertainty that is about 7 days for the runoff phase as quantified by Marin et al. (2020). Following the suggestion of the Reviewer to perform a sensitivity analysis (see the answer below for all the details), we show that the error introduced in the final SWE reconstruction is higher when introducing a delay in detecting the runoff onset rather than anticipating it. In fact, temperatures are lower and consequently, the potential melting calculated through the DD method is lower in the early moistening/runoff phase. On the other hand, a delay may cause a significant loss in terms of SWE, being the temperatures higher, and consequently also the potential melting, as the melting season advances. For these reasons, we decided to implement a conservative approach based on the moistening onset i.e., anticipating the runoff onset to be sure to not delay it and thus introduce significant errors.

  However, we agree that from a physical point of view, the start of runoff should be considered as the initial day of ablation, especially if we consider that the revisit time of SAR satellite missions may improve greatly in the future. To this purpose, and to provide a more quantitative analysis, we recalculated the metrics with both approaches. The new results differ from those reported in the initial manuscript since during the time of the revision we improved the accuracy of the snow cover maps. For the catchment in the Sierra Nevada by considering the three hydrological seasons (2018-2021), we obtained a bias of –22 mm, an RMSE of 212 mm, and a correlation of 0.74 when considering the runoff, and a bias of –20 mm, an RMSE of 216 mm, and a correlation of 0.75 when considering the moistening. The results refer to the evaluation of our product against the ASO product. For the Schnals catchment, the evaluation against the manual measurements for the season 2020/21 showed a bias of -5 mm, an RMSE of 191 mm, and a correlation of 0.35 when considering the runoff phase while a bias of 19 mm, an RMSE of 218 mm, and a correlation of 0.36 when considering the moistening phase. It is worth noticing that this is a limited amount of point observations taken using an SWE coring tube that may not be fully representative of a 25x25 m pixel. Given the negligible difference in this specific case, we report in the final version of the manuscript only the version with the runoff onset identification. We introduced both a dedicated subsection (2.1.2) where we better explained how the ablation state is identified, and a dedicated subsection in the Discussion where the sensitivity analysis is reported

- If I understood correctly, the catchment states ablation and equilibrium can exist both at the same time-step (different pixels have different states), but in accumulation all pixels have this state. Line 104 and 189 contradict themselves in this regard. Please clarify.

Thank you for pointing out this aspect that may generate misunderstandings. By considering the constructive comments received by all the reviewers, we introduced important changes in the method section. We explained that ideally the state should be identified for each pixel (and not at the catchment level as done in the previous version of the manuscript). The possible states are (see Figure 1): i) accumulation that represents an SWE increase (ΔSWE>0), ii) ablation that represents an SWE reduction (ΔSWE<0), and iii) equilibrium that represents a stable SWE (ΔSWE=0).

State	ΔSWE	Class transition t-1	Class transition t	Description
Accumulation	>0	■	□	Snow on bare ground
		□	□	Snow on snow
Ablation	<0	□	■	Snowpack disappearance
		□	□	Snowpack reduction
Equilibrium	=0	□	□	Stable snowpack
		■	■	Bare ground

Legend: □ = snow ■ = snow-free

Figure 1 Definition of the three possible states: accumulation, ablation and equilibrium. The possible class transitions at pixel level associated with the state are described.

The phenomena that cause a SWE variation are several, as snowfall, melting, sublimation, human activities or redistribution due to wind or gravitational transport, e.g., avalanches. However, we refer mainly to snowfalls if accumulation and to melting if ablation. In fact, we propose to estimate the SWE to be added considering a quantity proportional to the snow depth/SWE variations, thus, in an ideal case, including only fresh snow as the main driver. Similarly, the amount of SWE to be subtracted is calculated using a DD model and, therefore, it only represents melted snow. Trivially, SWE remains constant when equilibrium.

The state varies pixel-wise due to the topography and meteorology of the study area. However, it is difficult to extrapolate this information with the necessary spatial detail. For the accumulation identification, the sources of information that can be exploited are few. In the paper, we propose that the accumulation can be retrieved by a network of automatic weather stations (AWS) that measure snow depth/SWE. It is important to note that a station is representative of a limited area whose extension is highly variable depending on the complexity of the terrain. However, by considering only the snowfall events, one can think that from a network of stations distributed with elevation, it is possible to divide the catchment into different elevation belts that can be considered homogenous. However, in many basins, this is quite far from reality. As a common configuration for snow monitoring, we have a single station located at a high point of the catchment, that is informative enough to identify the accumulation events but not their extent. In such a situation, as described in the paper, we considered that the snowfalls occur throughout the snow-covered area of the catchment. We are aware that this assumption may be erroneous, especially in the case of mixed conditions. For example, snowfall may be observed at high elevations, together with rain-on-snow at low elevations that causes snowmelt. However, it has been shown in the literature that the estimation of the snowfall limit may be very challenging (see, e.g., Fehlmann et al., 2018). For this reason, we believe that introducing an approach based on temperature thresholds to define the snowfall limit may still represent a strong simplification that does not necessarily add value to our approach.

On the other hand, the ablation state can be retrieved by using i) temperature index models (it is generally easier to spatialize temperature data w.r.t. snow depth observations), but also ii) multi-temporal SAR observations derived by Sentinel-1. Marin et al. (2020) investigated the relationship between the SAR backscattering and the three melting phases, i.e., the moistening, ripening, and runoff phase. In detail, they showed that if the SAR backscattering is interested in a decrease of at least 2 dB, the snowpack is assumed to get moistened (Nagler and Rott, 2000). First, this decrease affects only the afternoon signal (beginning of the moistening phase). When it also affects the morning signal, the ripening phase starts. Finally, the backscattering increases as soon as the SWE starts to decrease, which corresponds to the beginning of the runoff phase. This moment represents the first contribution of the snowpack to the release of water. The multi-temporal analysis of the SAR backscattering represents a novel way to identify the ongoing melting in a spatialized manner. By integrating this information into a degree-day model, it is possible to exclude false early melting dates.

We included all these explanations in the manuscript. Currently, we are working toward a solution that exploit remote sensing information only i.e., natively spatialized information (as already mentioned in the manuscript, for example by considering surface temperature measurements from satellites, meteorological radar or SAR derived snow depth/SWE information). However, this needs further research and it left as a future development in the current manuscript.

- Although bringing the reconstructed SWE time-series in context to the catchment discharge might provide some insights into the estimated snow cover dynamics, however, the way this information is presented in Figure 13 and interpreted in the text (line 409 -411) is not suitable for this purpose. There are basically two statements emerging from this analysis: i) there was more snow in one year than the other, ii) SWE decreases and subsequently discharge increases at some point. As it can be seen in Figure 13 there are very different discharge responses in the spring freshet between the two years. I do not see this analysis to be much helpful in the current state and would advise either to remove it from the manuscript, or expand the analysis (giving more information about precipitation, hydrological characteristics of the catchment, and changing the units in the figure (e.g. to mm)).

The authors thank the reviewer for this comment. This is also in line with a comment provided by the community (see CC#1). As suggested by Pau Wiersma (CC#1), we deepened the analysis on the relationship between the SWE runoff and the measured discharge. We considered the SWE related to the subcatchment closed at the outlet point Schnalserbach – Gerstgras, as shown in Figure 2. This makes the two variables more comparable, although the analysis remains qualitative.

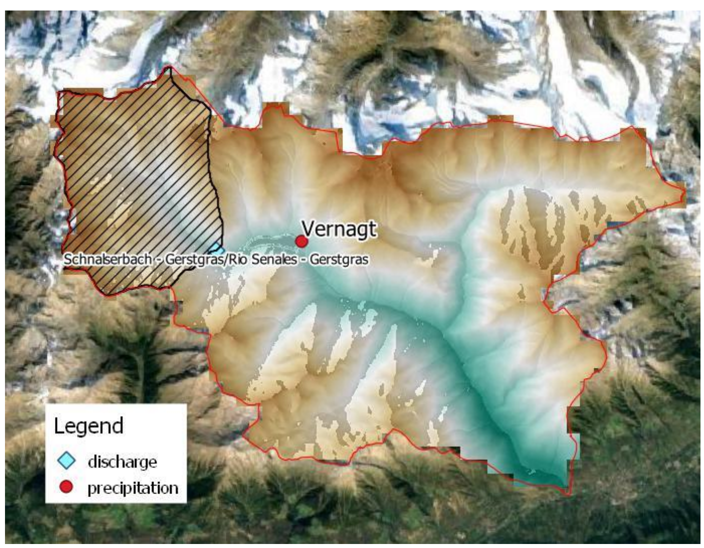

Figure 2 Overview of the subcatchment whose outlet point corresponds to the location of the discharge measurement.

We presented two new plots (Figures 3 and 4) for the two seasons that may replace Figure 13 in the manuscript. The analyzed variables are: i) the SWE variations that are associated with a runoff (i.e., only when they are associated with a decrease of SWE), ii) the discharge; and iii) the precipitation measured at Vernagt expressed in mm/day. We better analyze what we can observe from these plots. In detail, we can observe that there is a good agreement in terms of both timing and quantity among snow-generated runoff and discharge confirming that the catchment is snowmelt dominated. The discharge starts increasing in correspondence with the snowmelt and it starts decreasing when also the snowmelt is reduced for both periods. We can observe that the first year shows a delay while the response is more direct for the second season. More than differences in terms of precipitation, we ascribe this situation to a different snowmelt rate. Indeed, the season 2019-20 shows an earlier, weaker, and longer distributed snowmelt period, interrupted by periods with low SWE output (such as the 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 2020-21 season 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 may also be confirmed by recent literature, showing that when snowmelt is earlier, it is also less intense and the runoff response could be reduced, with strong implications for future climate change impacts (Musselman et al., 2017). However, other contributions should be also considered, as for example, the storage of water in the two snow reservoirs that are present in the territory. While a proper analysis requires a complete hydrological study and a hydraulic characterization of the watershed properties, we believe that this simplified analysis shows the potentiality of the presented results in a real application. For this, we think that adding the information provided here to the Reviewer also to the manuscript may be interesting to the reader and stimulate works that will exploit data derived from the proposed approach.

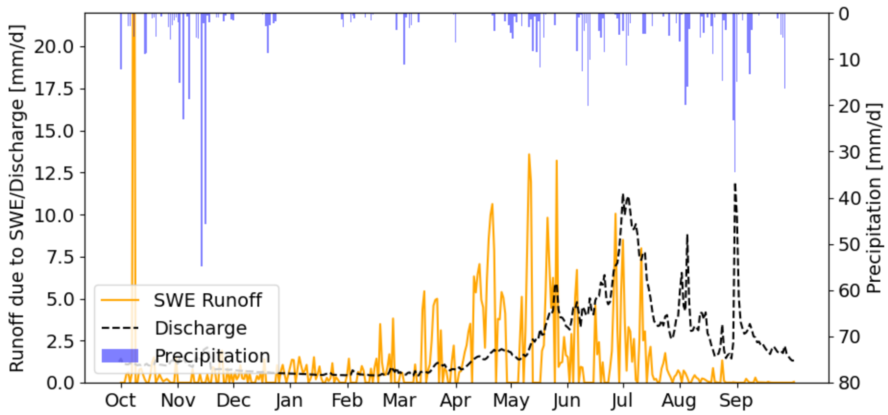

Figure 3 Snow generated runoff, discharge and precipitations for a subcatchment in the Schnals valley for the season 2019/20.

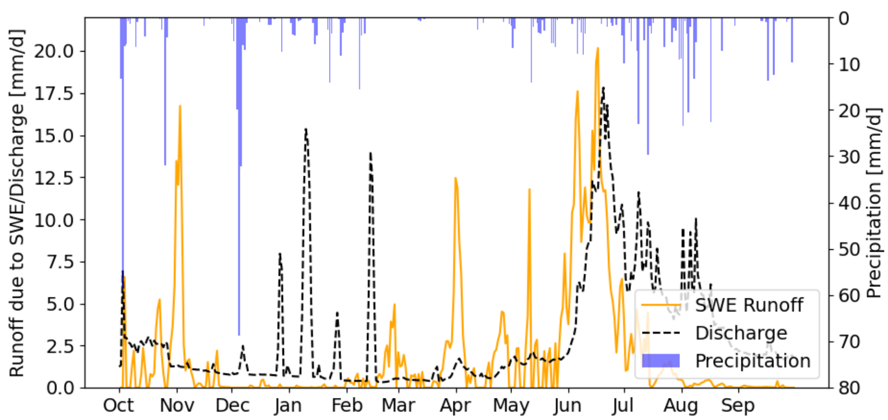

Figure 4 Snow generated runoff, discharge and precipitations for a subcatchment in the Schnals valley for the season 2020/21.

- The authors discuss various sources of uncertainty in the methodology and state that due to a number of preprocessing steps a formal sensitivity analysis is difficult to perform. However, it might be still very valuable for the reader to get a feeling about how possible errors might translate to the SWE reconstruction. Please consider the possibility to provide a simplified version of part of the problem, by e.g. perturbing the values of tSD and tSA (and perhaps keeping the states constant during this time) and showing the consequences in terms of peak SWE. This could also help to underline the statements in line 480f.

We thank the Reviewer for this suggestion. As mentioned, it might be difficult to really estimate all the uncertainty sources and provide a proper sensitivity analysis on the parameters that play a role in the proposed approach. However, we follow the suggestion of the Reviewer and we carried out a simplified sensitivity analysis. For the sake of clarity, we propose to investigate how the parameters affect the final SWE reconstruction by considering the pixel where the station Volcanic Knob provides continuous SWE measurements in the Sierra Nevada catchment. The parameters that we believe play an important role in the methodology are i) the degree day factor, ii) the SWE threshold used to identify the states, iii) the time of snow disappearance (tSD), iv) the time of snow appearance (tSA), and v) the time of first ablation detected by S1 (we call it here tS1). We vary each of these parameters separately keeping the others constant and equal to the optimal case (i.e., the one with the lowest RMSE). The test is carried out for one season (2018/19). Although we are aware that this analysis is not exhaustive, it can give an overview of the most important sources of error.

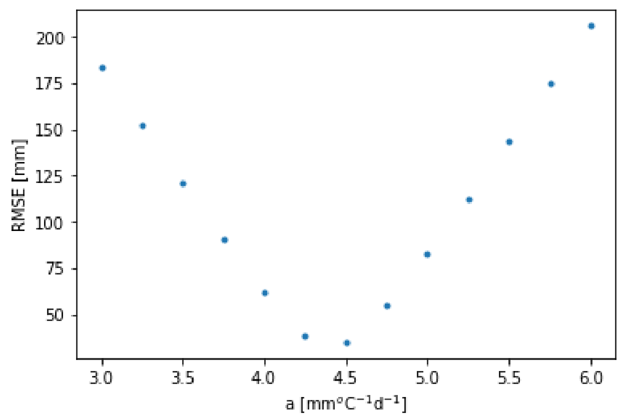

Figure 5 tSD = 27/06/2019, tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), SWE threshold=2mm, a varies from 3 to 6 mm/(°Cd) by steps of 0.2.

It is possible to notice in Figure 5 that the error increases linearly when a moves away from the optimal value, that is a=4.5 (as it was set in the manuscript).

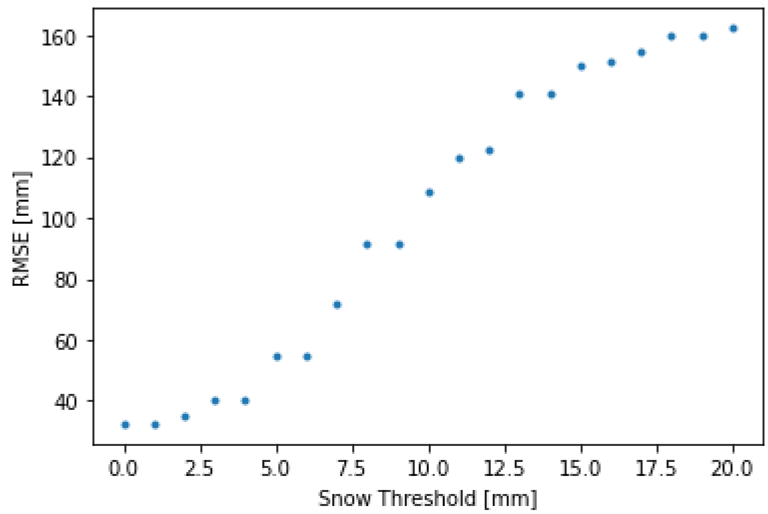

Figure 6 tSD = 27/06/2019, tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), a=4.8 mm/(°Cd), SWE threshold varies from 0 to 20 mm by steps of 1 mm.

It is possible to see in Figure 6 that, as expected, the higher the threshold, the greater the error. In fact, for too large thresholds, the method fails to detect the accumulation states. A snow threshold of 2 mm, as set in the manuscript, is acceptable.

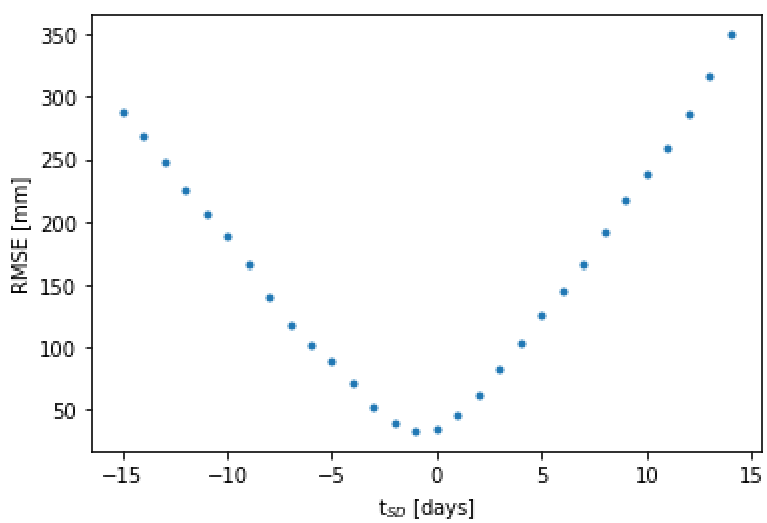

Figure 7 tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), a=4.8 mm/(°Cd), SWE threshold 2 mm, tSD 27/06/2019 +- 15 days.

It is possible to notice in Figure 7 that both underestimating and overestimating tSD introduce important errors in the reconstruction. In fact, at the end of the melting season the temperature is high and consequently the potential melting. A difference of +-5 days (which corresponds to the S2 repetition time) already introduces around 50 mm of RMSE.

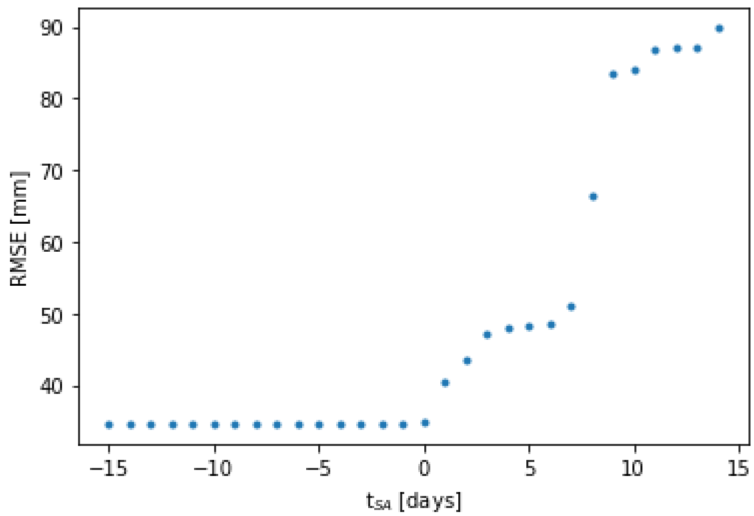

Figure 8 tSD 27/06/2019, tS1=22/04/2019 (as detected by the station), a=4.8 mm/(°Cd), SWE threshold 2 mm, tSA=22/11/2018 +- 15 days.

It is possible to see in Figure 8 that the shift of tSA does not strongly affect the RMSE as tSD does. For negative shifts, the accuracy RMSE is constant since no SWE is added to the reconstruction. In fact, for those days, we find that the coefficient k (see Eq. 4) is 0 since it is calculated from the AWS. In other words, it means that the accumulation is not really happening before at least one station detects an increase in SWE.

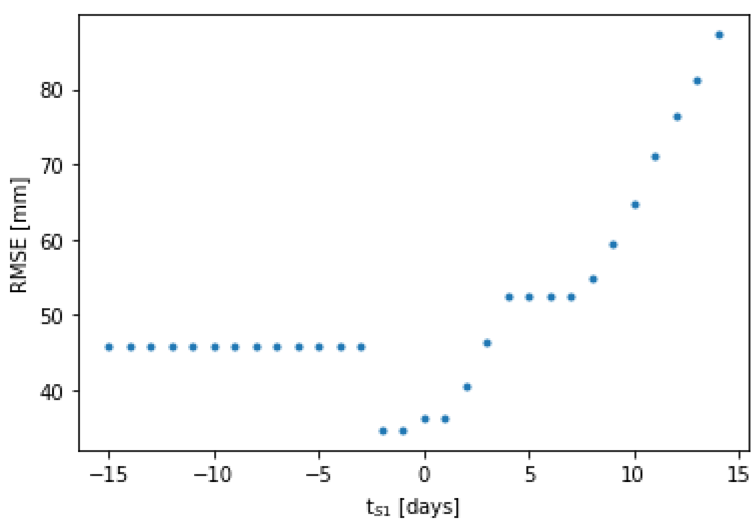

Figure 9 tSD = 27/06/2019, tSA=22/11/2018, a=4.8 mm/(°Cd), SWE threshold 2 mm, tS1=22/04/2019 +- 15 days.

In this case, it is also possible to see in Figure 9 that the shift of tS1 does not strongly affect the RMSE as does tSD. The RMSE for negative shifts remains constant after a certain point, since for those days the DD model returns 0 potential melting, so there are no differences. This means that it is in general better to make an error anticipating the melting phase than postponing it.

Even though we are aware that this represents a very simplified analysis and might be not exhaustive, we can summarize that we expect that the error that most strongly affects the results is a shift in the date of snow disappearance. For this reason, we believe that the SWE reconstruction can fully benefit from the introduction of an accurate daily HR time series.

Additional Comments

Abstract/1 Introduction:

- 1 reconsider recasting the first sentence.

  We propose to change the sentence with: "The hydrological cycle is strongly influenced by seasonal snow accumulation and release. For this reason, mountains are often claimed as the "water towers" of the world."

- 3 how about ablation processes, or are you specifically talking about peak SWE?

  We meant here the peak of SWE, but for completeness and clarity it is better to include all the ablation processes. We change with "..the complex snow accumulation, redistribution, and ablation processes."

- 11 At this point, the reader might not follow what you mean with "time-series regularization from impossible transitions". Mentioned again at 105, 111, and 127 before finally explained in section 2.2 at 194.

  In the abstract we specify "… from impossible transitions, i.e. the erroneous change of the pixel class from snow to snow-free when it is expected to be in accumulation or, vice versa, from snow-free to snow when ablation." We also report the definition in L105 and 127.

- 17 I'm not particularly fond of using present perfect (throughout the manuscript). But that might be personal preference. Please consider using present tense (or simple past) consistently.

  Thank you. We also prefer the present tense.

- 21 not only on local hydrology, as many regions of the world rely e.g. on the spring freshet hundreds of km downstream

  Thank you for noticing this. We change with "…local and global hydrology"

- 23 „from at least 50%"please check again with Vivironi et al (2003). Although snowmelt is a major contributor of mountain water resources, as far as I know, the numbers given by Vivironi and others include mountain waters in general.

  Thank you for pointing out this. You are right. We changed the sentence with "Contribution of snow-dominated catchments to streamflow ranges from 40% of the total flow to sometimes more than 95%, depending on the region (Viviroli et al., 2003)"

- 27 precipitation variability is affected by orography, interpolation is affects by sampling density among other factors, and observations can be erroneous due to e.g. undercatch

  Thank you for this clarification. We propose to rephrase the sentences in this way: "In fact, precipitation data used as input for physically-based models are often affected by uncertainties, thus limiting the spatial accuracy of snow accumulation and melt models (Engel et al., 2017; Günther et al., 2019). The main challenges to obtain an accurate precipitation field arise from the strong spatial variability of the variable related to the orography, a generally scarce sampling density of the phenomenon that strongly influences the interpolation results, and possible inaccuracies in the measurements caused, for example, by undercatching (e.g., Prein and Gobiet, 2017)."

- 65 "accumulation and melt = „accumulation and ablation" (i.e. in this sense ablation includes erosion and evaporation etc.)

  Thank you. We accept your suggestion.

- 66 consider topography vs geomorphology (throughout the text)

  Thank you. We accept your suggestion.

- 80 they range from DD to complete energy balance models (as used in Bair et al 2016), and these do not require calibration

  We agree with the Reviewer, also according to the comment of Reviewer#2, that this sentence is not correct. With "calibration", we erroneously meant the tuning of parameters such as the degree day (or empirical melt factor) that can affect the final results but cannot be considered as a proper calibration parameter. We removed this sentence.

- 114 (Italy) like (USA)

  Thank you. It has been replaced.

2 Proposed approach to HR SWE reconstruction

- 120 first sentence is obsolete iMo.

  Thank you. We accept your suggestion and deleted it. Also, we introduced changes in the Methodology section according to the comments of the other reviewers.

- 132 please specify „too vast".

  We thank the reviewer for pointing out this issue. We noticed that this is also generating confusion based on the comments of the other reviewers. In the new manuscript, we propose to define the state at the pixel level. We explained that the method does not necessarily require working at the catchment level. However, the limitation is represented by how we define the accumulation. Given the scarcity of AWS within our study areas, we preferred to consider the accumulation state to occur for the entire snow-covered area of the considered catchment. For this reason, in the previous version we talked about a not "too vast" area. In this sense, we mean that the catchment is subject to similar meteorological forces, so the assumptions made for the accumulation state identification hold. In other words, this implies that the basin should be subjected to similar snowfall events. Fortunately, this is the case in many situations, except the catchment is not "too vast". Hence, rather than suggesting a perfect size we suggest analyzing the climatic and hydrologic characteristics of each subcatchment when dealing with a large area, i.e. to check if a correlation exists among the subcatchments in terms of climatic variables (temperature, precipitation, discharge).

- 136 Maybe this would be clearer: „in detail, the catchment state is characterized by the change in SWE, but is also associated with possible changes in SCA". Or similar.

  Thank you. We revised the whole section according to the other comments, so please refer to the new version where we talk about a pixel state and associated pixel class transition.

- 141 „extension" = extent

  Thank you. It has been replaced.

- 143 „…dSWE < 0 due to melt water drainage".

  We removed this part from the new version of the manuscript.

- 145 snow depth, not height. Anyway, better to talk here in terms of mass/SWE. e.g. „..if the snowpack is melting only partially".

Thank you. It has been replaced.

- 167-168 maybe better placed in the discussion section

  Thank you for your suggestion. We removed the sentence from Sec. 2.1 and we moved it in Sec. 5.1.

- 169 potentiality = potential to detect the presence of a melting snowpack…

  Thank you. It has been replaced.

- 174 most important in terms of what? Certainly not in terms of melt water production as melt rates increase towards later season. Peak SWE is not necessarily the peak of melt water runoff. Please clarify.

  Thank you for pointing this out. Yes, we agree that the SWE peak does not correspond to the melt water runoff peak. We meant here that this is the moment when water starts to be released from the snowpack. We propose to change the sentence in this way: "This moment represents the first contribution to the water release…"

- 181 replace "quotes" with "elevations", "elevation bands" or similar (throughout the manuscript)

  Thank you. All this part has been rephrased, so please refer to the new manuscript version.

- 189 ablation and equilibrium classes can exist at the same time, but 104 states that the state is assumed homogeneous for all pixels of the catchment. Please clarify!

  Thank you for pointing out this aspect that may generate misunderstandings. As explained in a previous answer, in the new version of the manuscript, we propose to introduce the state concept not at the catchment level, but at the pixel level. Ideally, each pixel may have a different state. Since the accumulation cannot be identified pixel-wise, we consider all the pixels to have the same state when the AWS detects a snowfall. On the other hand, ablation can be identified pixel-wise thanks to the use of S-1 and a spatialized degree day model. Hence, ablation and equilibrium can coexist.

- 196 contamination = obstruction?
  Thank you. It has been replaced.

- 276 which variable is used as external drift, elevation?

  Exactly. It has been added to the new version of the manuscript.

- 296 contemporary = simultaneously ?

  Thank you for correcting it. It has been replaced.

- Figure 3, I don't see this figure referenced anywhere in the text,

  Thank you for noticing this. We added a reference at the beginning of this paragraph.

<u>3 Study Areas and Dataset Description</u>

- You acknowledge forest canopy as an important source of uncertainty but do not give any information if, and how much of the area is forested.

  Thank you for pointing this out. The percentage of forest is around 25% for the Schnals catchment and 32% for the South Fork catchment (data source Shimada et al., 2014). We added to the description of the study areas (Section 3).

- 321 Are manual SWE observation only available for one of the two seasons?

  Yes, they are available only for the season 2020/21. We added this information to the text. The reason is that it was not possible to collect measurements during the previous season due to Covid restrictions.

- 336 S1 is not introduced in the text (unlike sentinel-2)

  Thank you for noticing this. We added this to the text.

- Figure 4: resolution should be improved; it is very hard to read.

  Thank you for noticing this. We improved the quality of the image.

- Figure 4b: Why show all SWE observations if you only use a few of them (e.g. in Fig 10)? Or is the mean performance metrics based on all of them? Please clarify.

  Thanks for pointing this out. The metrics are evaluated on the whole dataset, while the plot in Fig. 10 reports only a few examples. We clarified this in the text in this way "The results show a bias of -5 mm, an RMSE of 191 mm, and a correlation of 0.35, indicating a generally good agreement. The overall performances are calculated w.r.t. the complete dataset, for sake of brevity we report in Fig. 10 the reconstructed SWE evaluated against the manual measurements only for a subset of the collected points. For more details on the measurement location of the selected subset, please refer to Appendix C, (see Fig. C1)." We also specified this in the Appendix. Moreover, for completeness, we also added Figure 10 (below) where we show the scatterplot between the measured and proposed SWE for the complete dataset as required by Reviewer#2. However, we think that it is interesting to keep Fig. 10 of the manuscript since it gives an overview of the SWE trend for different pixels selected from different locations, showing a variability that seems to be properly caught by the proposed method.

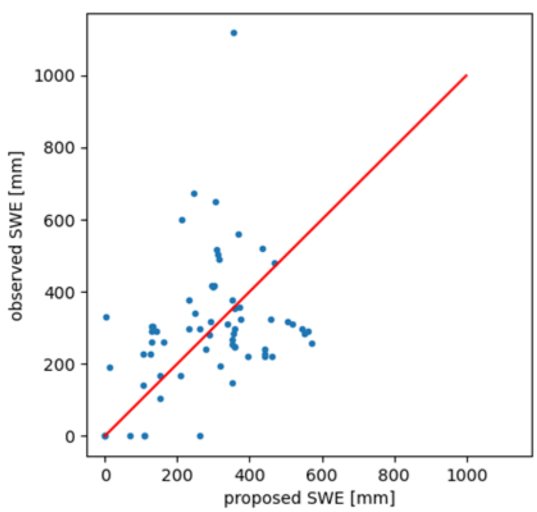

Figure 10 Observed and proposed SWE in the Schnals catchment for the h.y. 2020/21.

4 Results

- Figure 6: image resolution should be increased. “Trend” could be recast as “time-series”

  Thank you for noticing this. We check the quality of all the figures for the next manuscript version. We also replaced “trend” with “time-series” throughout the manuscript.

- Figure 7: rather small, also image resolution could be increased, caption does not mention ASO.

  We included ASO in the caption.

- Figure 8 / Table 1. Although it can make sense to specify total catchment wide SWE in Gt, I would much rather prefer it to be given in mm as well.

  We calculated the total SWE in mm and replaced Fig. 8 and Table 1.

- Figure 9 might be more helpful when ASO observations are included. I also think the plot does not support your interpretation that the drop in total SWE in very high elevations is due to gravitational redistribution (L 387), nor that steeper slopes present less SWE, nor that there is more snow on north facing slopes. Total SWE amounts are presented (in Gt), so this strongly depends on the area covered by this class (i.e. elevation bands). Same is true for slope and aspect bins. You need to scale with the area of a class to allow for these relative comparisons (express SWE in mm). Otherwise, you carrying the information about the catchment topography (e.g. hypsometry). x-axis: Aspect is not in degrees but in cardinal and ordinal directions

  We thank the reviewer for this suggestion. However, we cannot add the ASO observations since we are plotting the SWE maximum against elevation, slope, and aspect and the maximum of the season is not available for ASO. We propose to remove Fig. 9 from the main text. We added to the Appendix the figures below, where SWE is reported in mm for the different elevation, slope and aspect band (similar to Fig. C2) for all the dates when ASO was available. However, we believe that also Fig. C2 expressed in Gt is still informative. In fact, the two plots represent something different. We originally expressed the SWE in Gt because we wanted to show how the total cumulated SWE is behaving w.r.t. the reference. For example, high slope areas show to agree when expressing SWE in Gt since these areas are very small and do not count that much when considering an overall balance. On the other hand, the analysis expressed in mm highlights relevant differences linked to the biases we encounter when analyzing the spatial maps of Fig. C1. In the figure below, it is possible to notice differences, especially for steep slopes, where the proposed method underestimates SWE w.r.t. ASO. However, we would expect less SWE for these steeper slopes that promote gravitational transport. This point can be better investigated and discussed in the revised version of the manuscript.

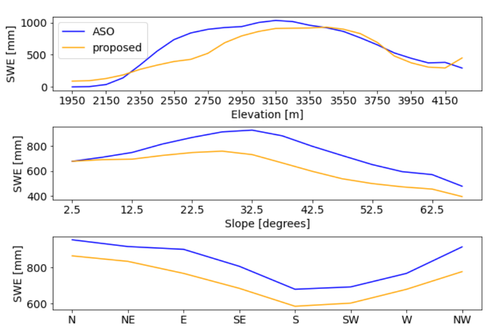

Figure 11 17/03/2019.

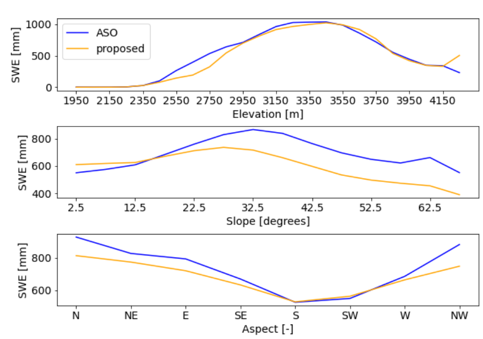

Figure 12 02/05/2019.

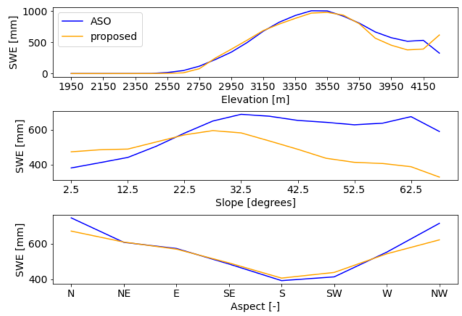

Figure 13 09/06/2019.

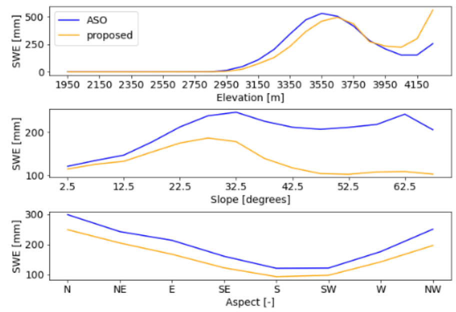

Figure 14 04/07/2019.

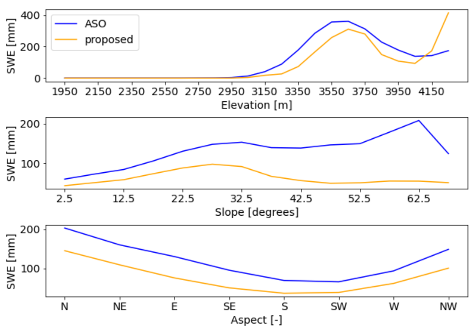

Figure 15 14/07/2019.

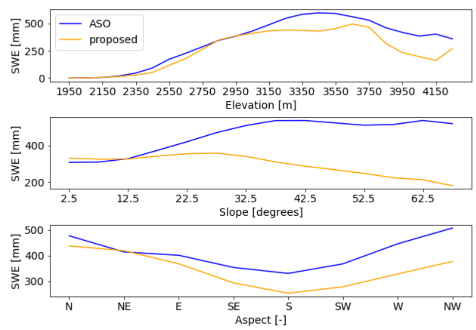

Figure 16 15/04/2020.

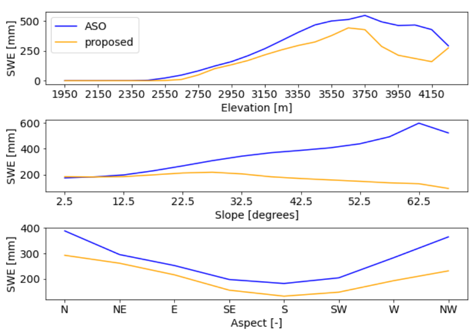

Figure 17 05/05/2020.

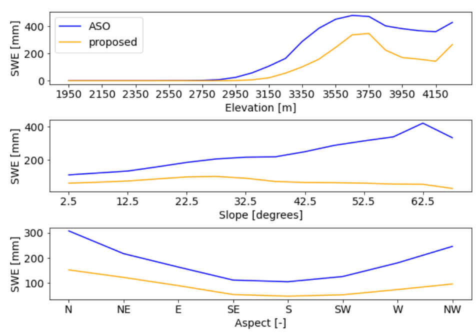

Figure 18 23/05/2020.

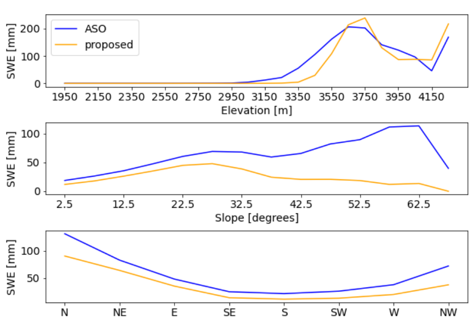

Figure 19 08/06/2020.

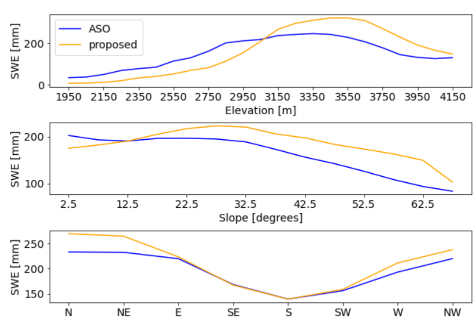

Figure 20 26/02/2021.

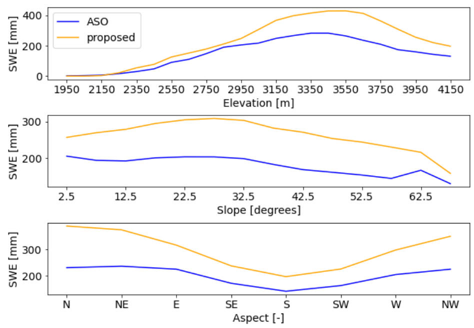

Figure 21 31/03/2021.

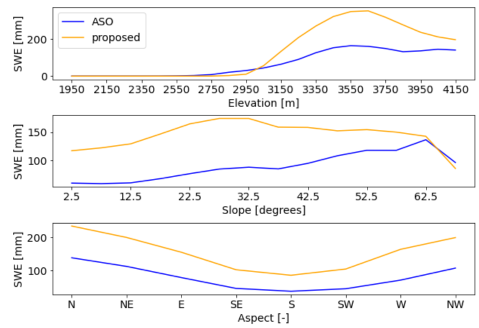

Figure 22 03/05/2021.

Finally, we propose to include these figures in the Appendix, keep Fig. C2 and include one of the dates as an example to show also in the main text.

- 397 what is meant by "only few examples"? Are there manual SWE observations for other years as well, or is this just a subset of the locations shown in Fig 4b?

  Thank you for noticing this. As specified in the previous comment, we clarified this in the text.

- 401 tends to increase

  Thank you for noticing this. It has been corrected.

- Figure 12, see fig 9. Since no spatial observations are available in Schnals, I don't consider this figure very helpful.

  According to the previous comment and the CC1 comment, we removed both Fig. 9 and 12.

- What about an evaluation against the automated snow depth sensor in Schnals?

  Thank you for raising this point. However, since we do not produce snow depth maps with our method and since we do not estimate the snow density, this comparison cannot be done. We could simulate SWE in the stations through a snow model (e.g., SNOWPACK). However, the station in Schnals is disturbed by wind erosion/accumulation (wind speed is not measured at the station) so we believe that this might affect the accuracy of the final simulation making the comparison difficult.

5 Discussion

- 413 maybe better "…quantitative and qualitative evaluation of the proposed SWE reconstruction over two study areas"

  Thank you for your suggestion. We modified the text accordingly.

- Figure 16: caption: use the same naming (i.e. "original snow cover map") as in the legends and captions of the previous Figures (14, 15)

  Thank you for your suggestion. We modified the caption accordingly.

- Section 5.1 only focusses on errors in predicting the accumulation state. Please expound upon the uncertainties associated with predicting the ablation/equilibrium state.

We apologize for this lack. We believe that we can integrate this part also by adding the previous discussion about the uncertainty and how the errors propagate. In this sense, we discuss the importance of predicting an accurate time of snow disappearance. On the other hand, errors in predicting the start of the ablation through Sentinel-1 seem not to strongly affect the performances. However, we would like to stress the fact that the value of the max SWE for a given pixel is driven by the number of days in ablation. Therefore, we expect that especially in the last melting phase it is very important to accurately identify the ablation state.

- 480-485 Please clarify/recast: “M” and “potential melting” are used in the same sentence and might confuse readers. Suggestion: “Since potential melting values at the end of the ablation period are high, an erroneous estimation of tSD strongly affects the reconstruction of peak SWE.”

  Thank you for your suggestion. We modified the text coherently.

Appendix:

- Figure A1: caption does not state that this is Schnals

  Thank you for your suggestion. We modified the caption coherently.

- Figure C2: x-axis: Aspect is not in degrees but in cardinal and ordinal directions; As a line plot it would be easier to compare observation and the proposed approach.

  You are right. Apologies for this oversight. We also replaced the bars with a line plot.

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