Answer to the Referee #2 – Manuscript tc-2022-146

My overall impression of the work is that it potentially represents a novel contribution and one that takes the SWE reconstruction approach into a very new direction with potential value. I am excited by this potential and think the authors should be applauded for taking on this work. However, I found some aspects of the paper extremely difficult to follow (identification of catchment state) and in general think the quality of the writing and use of english to be quite problematic. I think the paper is valuable but I would strongly suggest more editorial consideration in the context of sentence structure and grammar as nearly every-other sentence suffers from some type of grammatical error. Most of these errors were quite small and did not interfere with my understanding of the points being made but some errors were more considerable. These errors were far too numerous for me to spend the time to point them all out or to correct them all. My comments are included in the comments margin of the attached PDF. Most of these are broader-context comments that align with my perspective that the paper needs very major revisions. Thank you, Noah Molotch

The authors thank the Reviewer Noah Molotch for his meaningful comments and suggestions. We agree that some parts of the manuscript need to be revised and restructured following the constructive comments from the referees. In particular, the identification of the catchment state has been rewritten to make this part easier for readers. In addition, we revised the document and tried to correct all grammar mistakes. We also believe that the English-language copy-editing that the journal offers in case of acceptance will further help improving the quality of the writing. We went through each comment reported in the PDF. Our answers are reported in blue.

L81 I don't believe this statement is true. I am unaware of any "calibration" in the application of SWE reconstruction models.

We agree with the Reviewer 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. Since there are no calibration parameters in the SWE reconstruction, we removed this sentence from the revised version of the manuscript.

L83 This is not true. Presumably 30-m resolution would not be considered low resolution in this context and there are many papers in the literature that have applied SWE reconstruction using Landsat data - examples that I am aware of are below but there may now be additional papers.

Molotch, N.P., T.H. Painter, R.C. Bales, and J. Dozier, Incorporating remotely sensed snow albedo into a spatially distributed snowmelt model, Geophysical Research Letters, VOL. 31, doi:10.1029/2003GL019063, 2004.

Molotch, N.P., and R.C. Bales, Scaling snow observations from the point to the grid-element: implications for observation network design, Water Resources Research, VOL. 41, doi: 10.1029/2005WR004229, 2005.

Molotch, N.P., and R.C. Bales, Comparison of ground-based and airborne snow-surface albedo parameterizations in an alpine watershed: impact on snowpack mass balance, Water Resources Research, VOL. 42, doi:10.1029/2005WR004522, 2006.

Molotch, N.P., and S.A. Margulis, Estimating the distribution of snow water equivalent using remotely sensed snow cover data and a spatially distributed snowmelt model: a multi-resolution, multi-sensor comparison, Advances in Water Resources, 31, 2008.

We agree with the Reviewer that there are several works that exploit Landsat data, which are considered high-resolution data in this context. However, we meant with this sentence that there are no daily high-resolution acquisitions. This is the reason why we consider the work presented by Premier et al., 2021 and the proposed SCA correction based on the state determination as the foundations of the proposed SWE reconstruction. We believe that the use of such daily time-series, which is regularized coherently with the catchment state, represents also an important novelty for the SWE reconstruction. However, we agree with the Reviewer that the section needs to be better rephrased. We propose the following changes:

"To estimate SCA, many works presented in the literature exploit low-resolution (LR) images, since the large swath allows a high repetition time, i.e., with daily or sub-daily acquisitions. This allows the mitigation of the cloud obstruction and proper sampling of the SCA. However, the LR images do not provide sufficient spatial detail on the variability of the snow cover evolution in the mountains, which is on the order of a few dozen meters. Moreover, the use of LR sensors results in a non-linear combination of the different land cover type present within the pixel, and this should be properly considered by the snow classification approaches to avoid large errors, especially in complex terrains. On the other hand, the use of HR snow maps introduces important benefits both in determining SWE as well as in streamflow forecasting (Molotch and Margulis, 2008b; Li et al., 2019). Landsat products were exploited to retrieve SCA in many works in the literature (e.g., Molotch et al., 2004; Molotch and Bales, 2005, 2006). However, they acquire an image every 16 days (at the equator). With the introduction of the Copernicus Sentinel-2 (S2) mission, HR images are made available with an improved temporal resolution of 5 days (at the equator). This opens up new opportunities to monitor the heterogeneous snow conditions in the mountains. However, due to cloud coverage, useful acquisitions may be reduced by up to 50% in the Alps (Parajka and Blöschl, 2006). Therefore, even if the Landsat images are exploited together with the S2 images, only a few acquisitions are available per month. Recently, we proposed an approach to the reconstruction of daily HR snow cover maps. […]"

L85 what is this? "Shannon"?

The Nyquist-Shannon theorem establishes the sample rate that allows us to capture information from a continuous signal. In detail, the sample rate must be twice the highest frequency contained in the signal. In this case, we meant that the variation in space of the snow cover (i.e., the continuous signal) must be sampled at least every few meters, especially in complex alpine terrain. To avoid loss of information when using MODIS data, with a resolution of 500 m, the SCF must be considered (i.e., a quantization) although: i) the actual location of the snow/snow free cannot be reconstructed inside the pixel; and ii) the SCF error can be large under certain conditions (e.g., see Aalstad et al., 2020 for quantitative comparison). Vice-versa if the phenomenon is sampled with the correct sample rate (both in time and space), aggregation at a lower resolution is then always possible without losing information.

Even though all modern communication systems are founded on the Nyquist-Shannon theorem, this is a theorem specific to the signal processing field, and therefore to avoid misunderstandings we removed the sentence from the paragraph (see comment above for the revised version of the paragraph).

L104 This assumption needs to be discussed at length in the paper as there are many instances – for example in the Sierra Nevada and other maritime mountain ranges – when the higher elevations are still in the accumulation state whereas the lower elevations are in the ablation state.

We agree with the Reviewer that this represents a strong assumption. 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 anymore). 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.

L157 this should be discussed in that reliance on in situ data has issues in terms of transferability to areas without ground measurements – this is often a motivator behind classic SWE reconstruction approaches (as cited herein) which are designed to be widely transferable and independent of in situ observations.

Thank you for this comment. This part has been better discussed in the paper. In fact, we believe that working on a robust method for the determination of the accumulation state is one of the most attractive future developments of the proposed approach. Notwithstanding the approach depends on two information acquired by the in-situ stations: i) temperature, to determine the potential melting; and ii) snow depth/SWE variations, to identify the accumulation state. However, we are not considering a spatialized snow depth/precipitation input as commonly done for physical snow models, but only an indication of the snow depth variation, thus representing an advantage. As discussed in the previous answer, the accumulation state could be derived by exploiting remote sensing data, but this requires further research. Similar to classic SWE reconstruction, our proposed approach is dependent on temperature data that need to be spatialized. Therefore, this represents not only a limit for transferability but also a possible source of error. As discussed already in the current version of the paper (L497-500), S1 represents a spatialized way to identify the melting, but it presents as a major disadvantage a poor temporal resolution, i.e., a few days. As a future development, we aim at exploiting S1 and the new HR land surface temperature acquired by the next Sentinel generation as a proxy of the potential melting estimation, thus enlarging the applicability of the proposed method in remote areas.

L171 It has been shown that when the backscattering is interested by a decrease of at least 2 dB, the snowpack is assumed to get moisted

Thank you for highlighting this grammar mistake.

L181 quotes

Thank you for highlighting this mistake. We meant elevations. This mistake was corrected throughout the manuscript.

L211 so does this mean there is no "viewable gap fraction" correction? If so, that is problematic.

Yes, there is no "viewable gap fraction" correction since the classification algorithm that we are using proposed by Barella et al., 2022 is meant to detect viewable snow. We know this is problematic, and this is the reason why we encountered the problem mentioned in the old version of the manuscript in L 207-209. In detail, we refer to an underestimation of snow presence in forested areas when the snow falls below the canopy and is no longer visible from the satellite point of view, i.e., snow on ground. However, we have proposed an SCA regularization that has the potential to detect snow under canopy (see Section 2.2 and in detail, Algorithm 1 which is discussed in the text L224-259). In fact, while an HR pixel may not show snow (on the ground) due to the dense canopy presence, it is likely that an LR pixel intercepts some open areas surrounded by forest that are covered by snow. In this sense, we proposed to infer the snow presence under canopy not through a correction (as it is traditionally done through a VGF correction that however still represents a hot research topic) but by considering what is happening in the surrounding area (from the LR acquisition) and by analyzing the multi-temporal snow presence (SCA regularization). In this way, the presence of snow on ground is detected in a relatively robust manner as demonstrated by the obtained results.

L305 The South Fork of what river, the San Joaquin? Many of us know our Geography of this region quite well but this nomenclature doesn't seem correct.

Thank you for letting us know the correct nomenclature. In fact, we mean the South Fork catchment of the San Joaquin river. We apologize for the confusion in the nomenclature, but we do not know the region well. We select this region for the availability of the high-quality spatialized reference data provided by ASO, which we believe will make this area famous in the snow hydrology community in the future.

L317 why use the German name when the catchment is in Italy and the journal language is English.

We agree that this may sound weird, but the province where Senales/Schnals is located is a bilingual province. Formally, we can use either German or Italian toponymy, (which was introduced after the first World War). It should be mostly complete, and politically correct, to add both names, but for brevity, we chose the German one since it is the most spread around Europe.

L332 how accurate is this algorithm for SCA detection and how do / will errors in the algorithm propagate in the model to cause errors in SWE estimation?

The algorithm for SCA detection, which is described in detail in Barella et al., 2022, presents an RMSE of 22.82 and an MBE of 6.95 when compared with a set of three reference snow maps derived at 1 m resolution over the European Alps during winter 2021 (covering approximately 250km2).

To evaluate the propagation of errors related to SCA detection, we conducted a simplified analysis as suggested by Reviewer#1. For the sake of clarity, we considered only the pixel where the station Volcanic Knob provides continuous SWE measurements in the Sierra Nevada catchment. The test is carried out for one season only (2018/19). While we are aware that this analysis is not exhaustive, it can provide an overview of the error propagation.

In detail, errors in the SCA detection influence the time of snow disappearance (tSD) and the time of snow appearance (tSA). To see the effects on the two parameters separately, one is kept constant, and the other is varied.

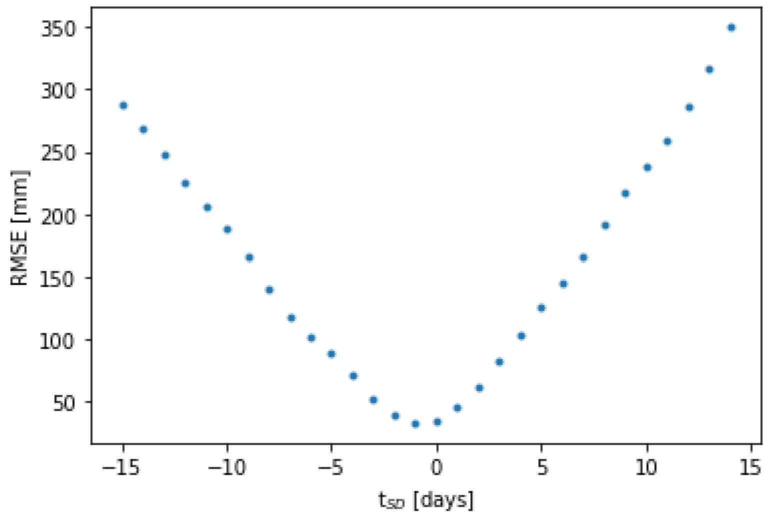


Figure 2 tSA=22/11/2018, tSD 27/06/2019 +- 15 days.

It is possible to notice in Figure 2 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.

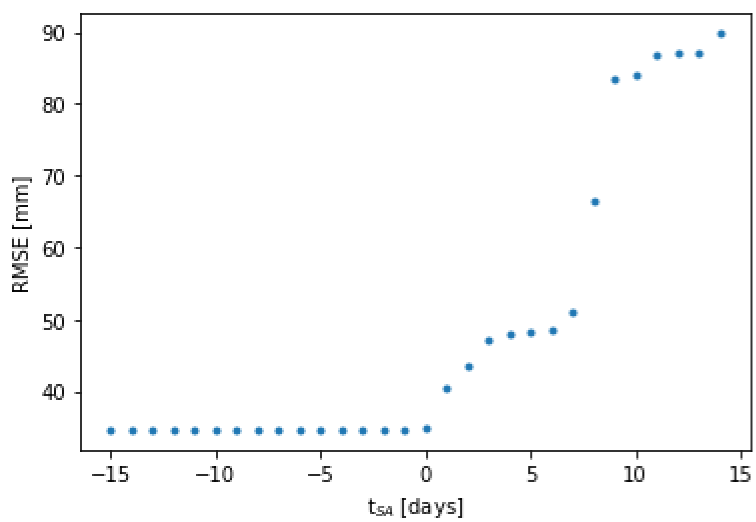


Figure 3 tSD 27/06/2019, tSA=22/11/2018 +- 15 days.

It is possible to see in Figure 3 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.

In addition, to complete the sensitivity analysis we also tested the sensitivity of the method to the degree day factor a, the SWE threshold used to identify the states and the time of first ablation detected by S1 (we call it here tS1). These parameters were kept constant in the previous cases (a=4.8 mm/(°Cd), SWE threshold 2 mm and tS1=22/04/2019).

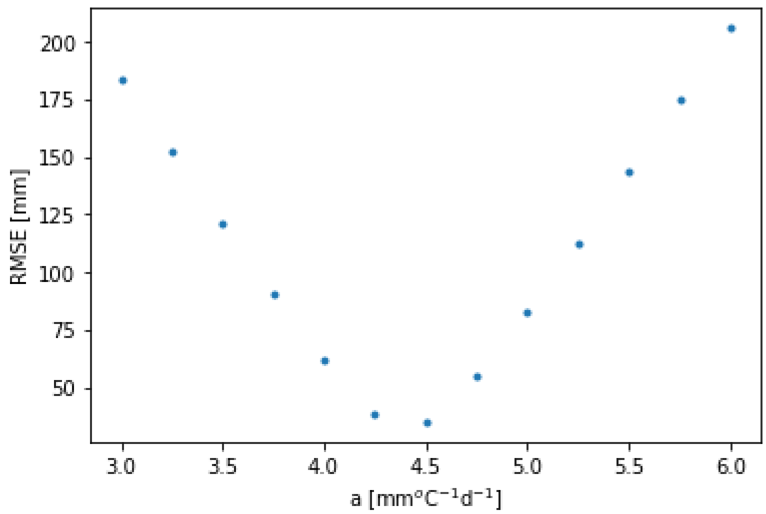


Figure 4 tSD = 27/06/2019, tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), SWE threshold=2mm, a varies from 3 to 6 mm/(°Cd) by steps of 0.2.

It is possible to notice in Figure 4 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).

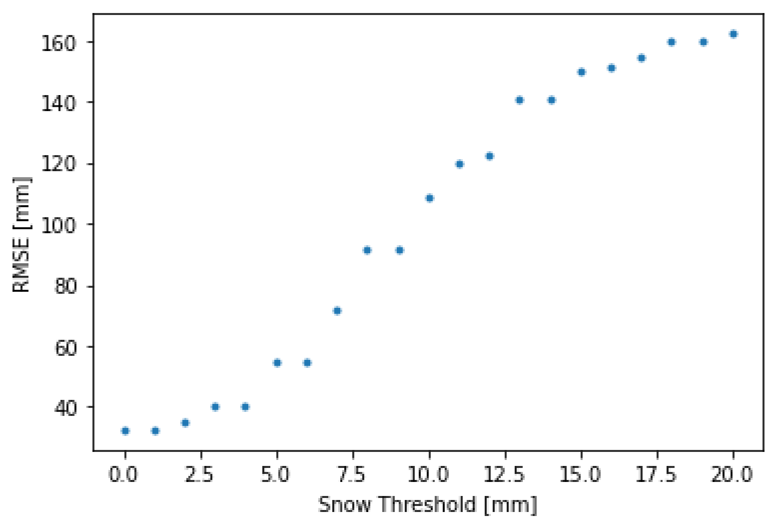


Figure 5 tSD = 27/06/2019, tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), a=4.8 mm/(°Cd), SWE threshold varies from 0 to 20 mm by steps of 1 mm.

It is possible to see in Figure 5 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.

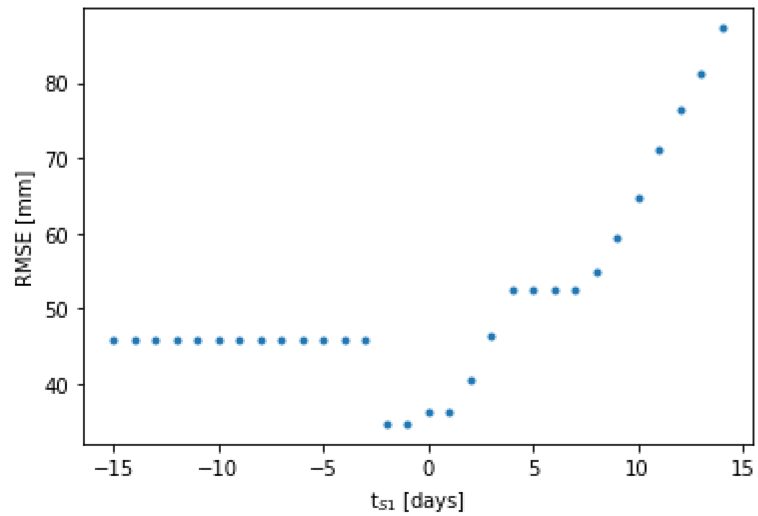


Figure 6 tSD = 27/06/2019, tSA=22/11/2018, a=4.8 mm/(°Cd), SWE threshold 2 mm, tS1=22/04/2019 +- 15 days.

In this case, it is also possible to see in Figure 6 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.

L354 I agree that this may be quite important but to really show this you would want to show a result with course resolution data and then another result when adding in the high resolution data to show the relative improvement associated with the addition of the high resolution data.

We thank the Reviewer for pointing out this important aspect of the proposed approach. The sentence at L354 points out the good agreement in the geometrical details between the SWE maps derived by the proposed approach and the reference ASO products, which have a comparable spatial resolution. Having such a reference dataset allows us to evaluate how the algorithm performs at that specific spatial detail. We agree with the reviewer that this is not enough to show the relative improvement associated with the use of HR data. However, quantify the improvement in a rigorous manner may be difficult since different source of errors are difficult to be isolated when using LR or HR images separately. First, a proper comparison should preferably refer to the obtained SCA time-series rather than the SWE time-series in order to isolate the errors coming from the reconstruction method. Having saying that, several works have already shown the benefits introduced using HR data (e.g., Aalstad et al., 2020 for citing the most recent). In fact, SCF retrieved by LR sensors presents some intrinsic limitations, e.g., variable viewing angle that changes the spatial resolution inside the image, high heterogeneity of land cover, illumination and atmospheric conditions inside the resolution cell especially over mountainous areas (see e.g., Rittger et al., 2016). The development of a robust and accurate algorithm to SCF retrieval able to address all the aforementioned problems has been the main research topic of the last 20 years. On the other hand, as showed in our recent work (Premier et al., 2021), it is possible to reconstruct a daily HR SCA time-series that presents high accuracy by learning from the historical snow patterns that repeat inter-annually. In this work, we used the information provided by LR sensors more as an indication than as an absolute truth, being aware that it presents an uncertainty i.e., we added an uncertainty value to the retrieved LR SCF (see (Premier et al., 2021), for more details). In this sense, the LR SCF is also corrected during the process. Hence, when using the corrected daily LR dataset to reconstruct the SWE we obtain results comparable with the ones derived by the high resolution approach presented in this paper and aggregated at 500m. In other words, if the LR time series is accurate and filtered out from the above-mentioned problem, the real benefits provided by the time series at HR is only an incontrovertible geometric detail, which might be of paramount importance for some applications (e.g., hydrology, avalanche forecasting, ecological studies, etc) but does not change the total amount of reconstructed SWE. This is also in line with the findings of Bair et al., 2022. It is finally worth stressing the fact that also the different temporal resolution plays an important role in the final evaluation of the SWE reconstruction. Therefore, considering partial time series (e.g., only HR images) instead of the completed ones (e.g., HR and LR images) can introduce artificial errors that do not allow one to properly quantify the advantage of using LR or HR data in the SWE reconstruction.

However, we take the opportunity raised by this comment to evaluate the results obtained by the proposed approach against another dataset that is available at 500 m resolution (Fang et al., 2022), but that is derived by assimilating Landsat data. The metrics obtained for the two methods are shown in the Table. Also, the figures report the results for the three hydrological seasons.

Date	BIAS [mm]		RMSE [mm]		Correlation [-]	
	proposed	NASA	proposed	NASA	proposed	NASA
17/03/2019	-121	36	242	292	0.80	0.66
02/05/2019	-61	-4	208	307	0.90	0.77
09/06/2019	-25	-32	182	302	0.93	0. 79
04/07/2019	-49	-14	129	201	0.90	0.71
14/07/2019	-51	-20	125	163	0.84	0.65
15/04/2020	-73	-26	159	169	0.80	0.78
05/05/2020	-59	5	151	154	0.82	0.80
23/05/2020	-95	-27	179	150	0.79	0.78
08/06/2020	-25	3	96	100	0.72	0.70
26/02/2021	5	96	92	157	0.75	0.65
31/03/2021	74	119	124	202	0.85	0.72
03/05/2021	65	54	121	121	0.89	0.66

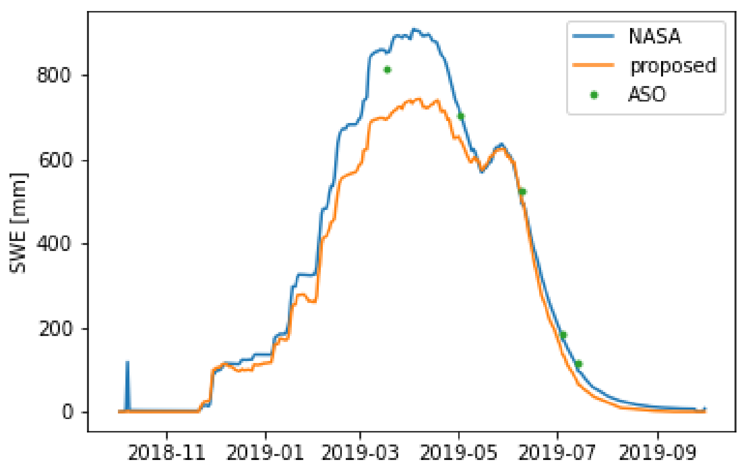


Figure 7 Results for the South Fork catchment of the San Joaquin river for the h.y. 2018/19.

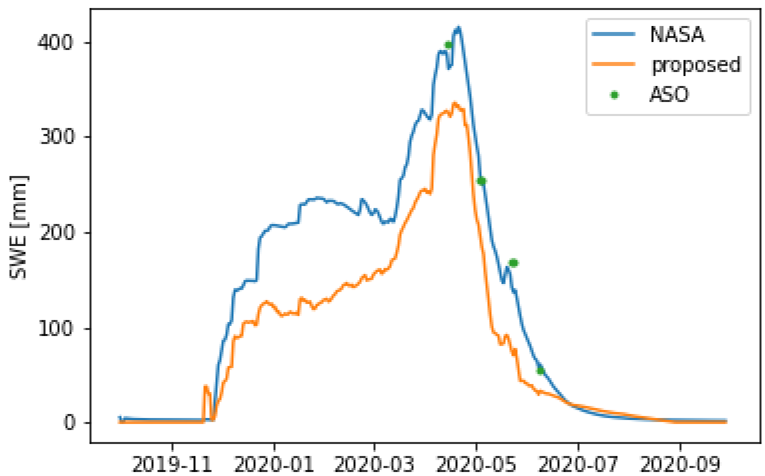


Figure 8 Results for the South Fork catchment of the San Joaquin river for the h.y. 2019/20.

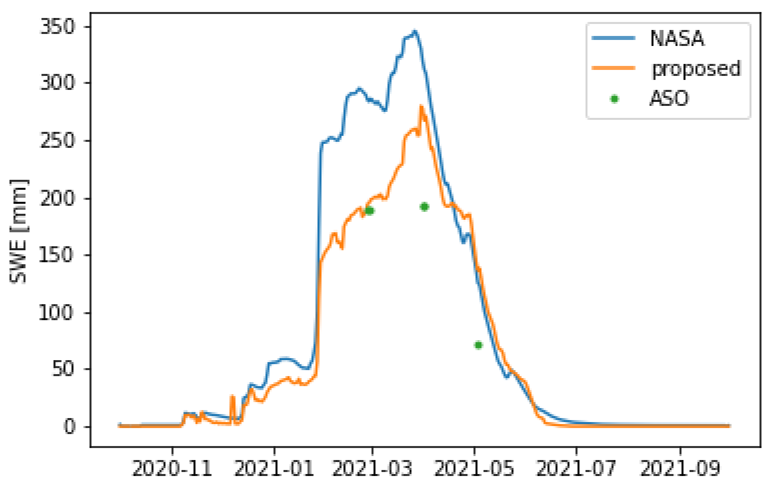


Figure 9 Results for the South Fork catchment of the San Joaquin river for the h.y. 2020/21.

It is possible to see that the two time-series show a similar trend. Generally, we encounter lower BIAS for the NASA product while lower RMSE and higher correlation are observed for our proposed product when compared with ASO data.

Table 1 percent bias would also be helpful

Thank you for this suggestion. We added this information in the revised version of the manuscript. The new table is also reported here. The metrics slightly differ from the previous version, since during the time of the revision we improved the accuracy of the snow cover maps for this new version. Furthermore, according to the feedback provided by Reviewer#1, we decided to consider the runoff onset instead of the moistening onset ad date of first possible ablation.

Date	BIAS [mm]	PBIAS [%]	RMSE [mm]	Correlation [-]	SWE tot ASO [mm]	SWE tot proposed [mm]
17/03/2019	-82	-11	315	0.73	778	699
02/05/2019	-33	-5	299	0.84	679	647
09/06/2019	-7	-1	268	0.88	511	504
04/07/2019	-37	-24	185	0.86	177	143
14/07/2019	-41	-58	164	0.80	106	67
15/04/2020	-71	-21	242	0.63	389	321
05/05/2020	-53	-26	224	0.67	250	199
23/05/2020	-91	-112	235	0.65	166	78
08/06/2020	-18	-58	161	0.66	48	30
26/02/2021	12	6	125	0.67	186	198
31/03/2021	81	29	168	0.73	191	271
03/05/2021	74	50	160	0.80	72	145

400 what is meant by this sentence? I think there are many arguments to avoid a degree day model so I am curious as to the point being made here but I do not find the statement to be decipherable.

We meant with this sentence that we have two factors that affect the SWE determination, i.e., the degree day that we use to calculate the potential melting and also the snow duration on the ground. If for example, two close pixels have a very similar temperature but differ in aspect, the resulting potential melting does not differ as it is calculated based on temperature only. However, the different snow persistence on the ground will result in a different amount of SWE for those pixels.

Figure 9 two points: first: what are the units "Gt" on the vertical axis. Second: I do not think it is appropriate to use the wrt abbreviation.

Gt is gigatons. However, according to Reviewer#1 and Community Comment#1, we replaced the unit with mm (see also Table above). We also removed the abbreviation.

Figure 10 Respectfully, I think it is problematic to only show "some locations". There should be more transparency around the model performance for ALL available evaluation data.

We agree with what the Reviewer says. In fact, the metrics were reported for all the locations (L396). The purpose of selecting a few points in Fig. 10 was more for visualization reasons and to show that different point correctly present different trends. However, we added a scatterplot (Figure 10) with all the observations for completeness.

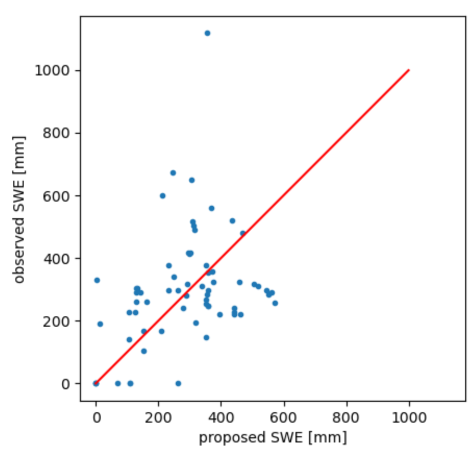


Figure 10 Observed and proposed SWE in the Schnals catchment for the h.y. 2020/21.

L418 I think the very lengthy section (early in the paper) on defining the accumulation, equilibrium, or ablation state would be greatly improved if more context were given to the reader about the relevance/importance of that procedure. For example, it is a big difference from previous reconstruction works, and thus it requires a delicate and well-organized presentation - and some emphasis placed on why it is so important in light of the differences of your modeling approach relative to other SWE reconstruction approaches.

Thank you for this suggestion. According to the comment of Reviewer#3, we stressed the importance of the state already in the introduction: “The concept of snow state of a pixel is also introduced in this work, representing the direction of the SWE change for that pixel, i.e., accumulation, ablation or equilibrium. This allows SWE reconstruction without the need for spatialized precipitation data. In fact, the method redistributes the amount of melting by exploiting the information about the state rather than quantifying the precipitation input. Moreover, the state allows us to set up the novel regularization of the SCA time-series. “

Furthermore, we revised Section 2.1 in the new version of the manuscript as described in the previous answer by defining the state at a pixel level and not at a catchment level. We believe that this makes the concept more understandable.

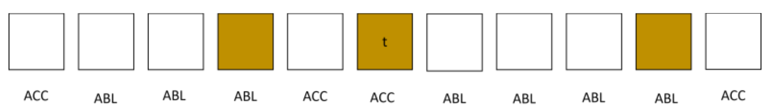
L421 interested

Thank you for highlighting this grammar mistake.

L454 i am not sure that the methods section is clear enough about the regularization (for example the "majority rule") such that this discussion is meaningful. I think the methods section needs work to make this clearer. Perhaps by providing some clear examples.

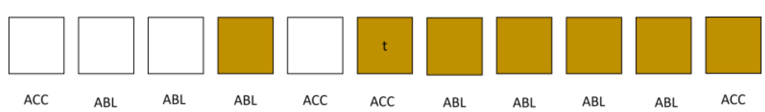
Thank you for this suggestion. We have revised the method section, as explained in the previous answer. We hope that the state definition at pixel level will make the method clearer. However, for sake of clarity, we would like to report here some examples, that are related to the most frequent errors that we observed from our analysis of the obtained results: i) an underestimation of snow presence in forested areas when the snow falls below the canopy and is no longer visible from the satellite point of view, i.e., snow on ground, and ii) the missed identification of snow-patches at the end of the season. Please, note that in these examples the equilibrium is omitted but, in that case, both transitions are not possible.

Let’s consider the first case (i.e., transition snow to snow-free is not allowed). A pixel at time *t* has been erroneously classified as snow free when its state is accumulation.

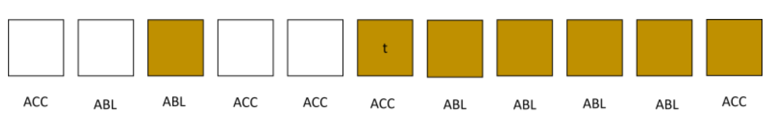


In this example, the last previous accumulation happened at t-1, so we are in the case of a “recent snowfall”. According to the proposed method we check the ±5 adjacent dates and correct the pixel by counting for the most frequent label, which is snow. So we corrected the classification to snow. The possible reasons of this error can be due to a misclassification of snow under canopy or snow in shadow, or an error introduced during the SCA reconstruction.

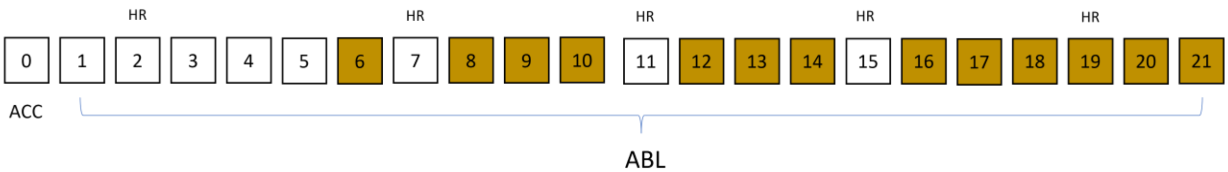
In the second example (i.e., transition snow to snow-free is not allowed see figure below), the most frequent label is snow-free. In this case, we will replace *t-1* with snow-free.



In the third example (see figure below), we need to replace not only t-1 as snow-free, but also t-2. In fact, according to the proposed correction algorithm, to ensure coherence the replacement has to be done until the date of the last previous ablation.



Let’s look now at a more complex example, to understand what may happen during the main ablation phase. You can notice that the time-series presents some snow dates in correspondence of HR acquisition (t=7, t=11, t=15) while the other dates are snow-free. This is a typical error since in the SCA reconstruction, the LR may fail to detect the snow patches i.e., low SCF. By going through the example, let's start from t=0, when the last accumulation happens and then only ablation follows for all the remaining dates. At t=5, it looks like the snow is disappearing. Since we are in ablation, this is coherent with the state. However, at t=7 we have a transition snow-free to snow that is not allowed. At t=7 we are still in the case of a recent snowfall, so by applying the rule that we set up, we obtain that the majority of the labels is snow. Hence, t=6 will be set as snow. The next incorrect transition is t=11. Now we are in the case of an old snowfall. If we consider the time window ±5 days, we would observe a majority of snow-free pixels. But in this case, we would make a mistake, since the LR acquisition is not able to correctly see the snowpatches as said before. Hence, we proposed in our correction algorithm, to consider only the HR acquisitions available from the last accumulation (t=0 in this case), up to a maximum number of 5 pixels, and count the occurrences of snow labels. In this case, this results in a majority of snow pixels, so we replace t=8, t=9, t=10 with snow. The same procedure is repeated when we encounter t=15. We proposed not to consider the HR acquisitions after the time to be corrected in the majority rule since the snow patches may disappear quickly.



These examples are not exhaustive but are provided to better understand the proposed approach.

It is finally stressing the fact that these empirical rules have been introduced to cope with the lack of optical observations acquired with appropriate spatial and temporal resolution to sample the snow cover. However, it is interesting to note how simple rules together with snow state knowledge can be used to try to correct a SCA time series.

L486 i think it would be appropriate to discuss the ways in which a DD model might insufficiently characterize potential melt. E.g. a uniformly applied degree day model (and associated melt coefficient) would underestimate melt for south facing slopes and would over-estimate melt for north facing slopes and this would result in corresponding biases in reconstructed SWE. The topic of discussion would need to consider the issue of how well the melt coefficient mathematically transposes degree-days into melt flux relative to all the terms of a physically based melt calculation (i.e. all radiative and turbulent fluxes). Hence, spatial variability in solar radiation, albedo, wind speed, vapor pressure, temperature, atmospheric and surface emissivity - these would all be relevant to this discussion and have been covered / discussed in many SWE reconstruction papers (notably those authored or co-authored by Bair, Dozier, Rittger, Molotch, etc).

We thank the reviewer for this suggestion. We will deepen the discussion on this topic. However, we did not notice from our analysis patterns in the bias maps that can be attributed to this issue. This is also the reason why we did not investigate the use of other modified temperature index models. Please, refer to Figures 11-22 that we produced as suggested by Reviewer#1. The figures are similar to Figure C2 but with SWE expressed in mm. It is possible to notice that we usually can observe an underestimation for North facing slope when comparing our product with ASO. This is not in line with the correct observation of the reviewer that we would expect an overestimation for North exposed area since we are using a constant degree day model. Hence, we cannot directly ascribe the errors that we observe to the use of a degree day model.

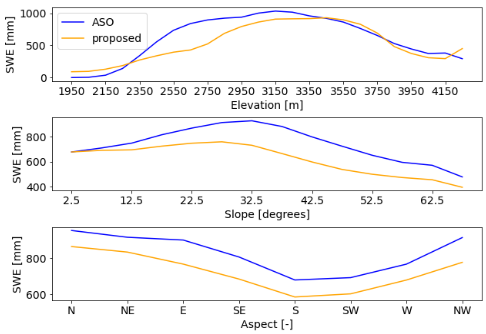


Figure 11 17/03/2019.

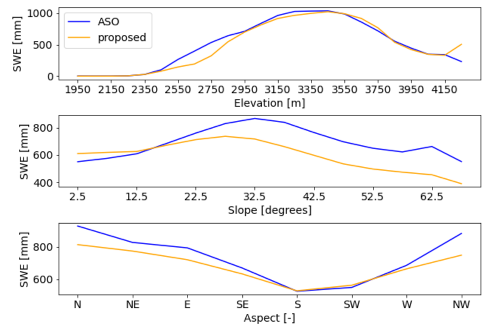


Figure 12 02/05/2019.

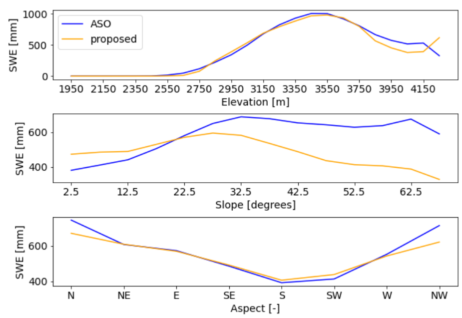


Figure 13 09/06/2019.

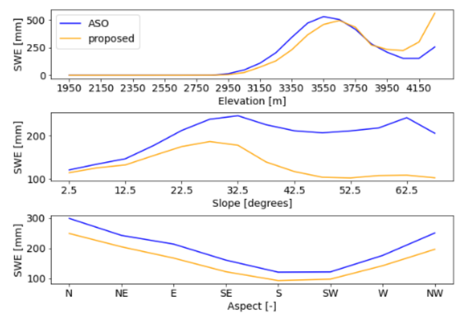


Figure 14 04/07/2019.

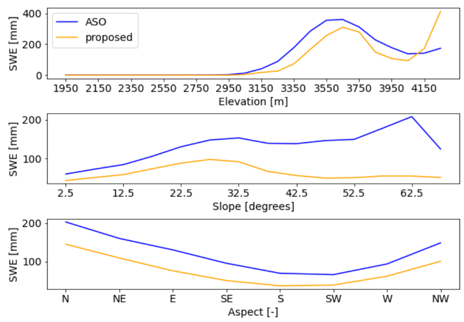


Figure 15 14/07/2019.

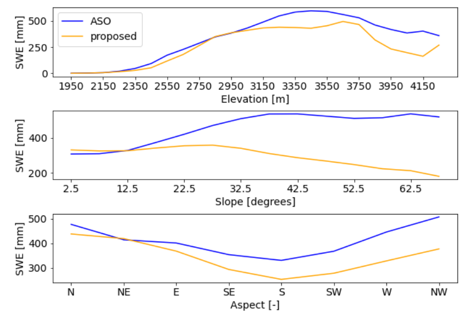


Figure 16 15/04/2020.

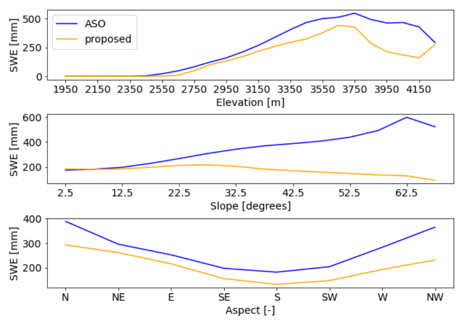


Figure 17 05/05/2020.

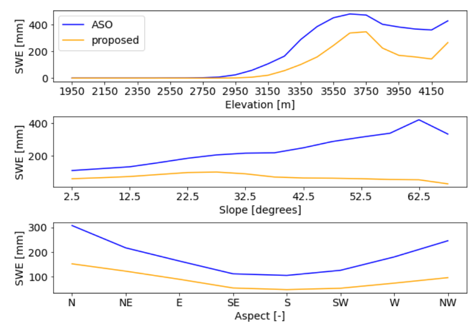


Figure 18 23/05/2020.

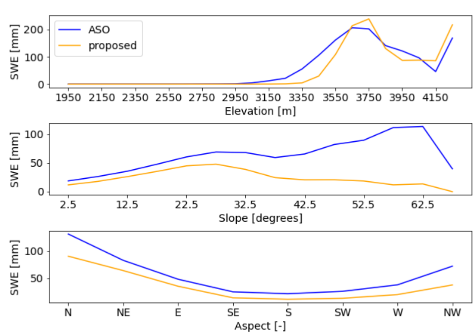


Figure 19 08/06/2020.

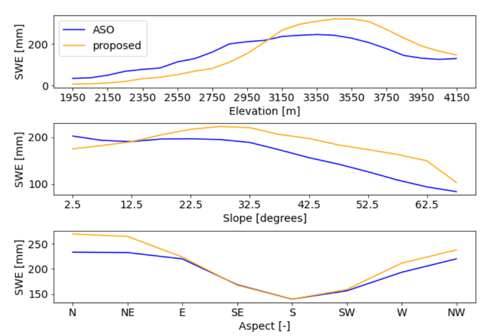


Figure 20 26/02/2021.

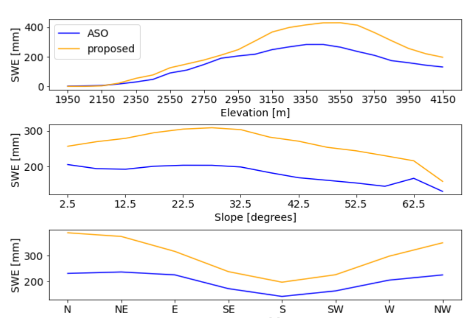


Figure 21 31/03/2021.

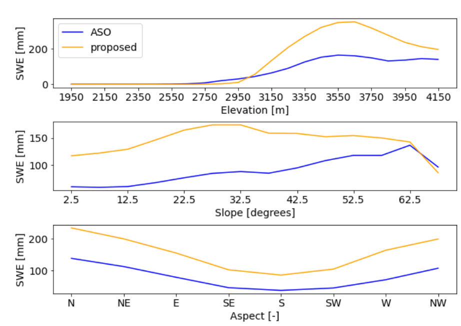


Figure 22 03/05/2021.

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