Answer to the Reviewer #3 – Manuscript tc-2022-146

This paper presents a reconstruction method to produce time series of high-resolution (HR) snow water equivalent (SWE) maps from different sources of remotely sensed data, with specific focus on the applicability on mountain areas. The method's description and results in two selected catchments are discussed, and the potential sources of errors are addressed in the context of general applicability and future improvements.

The topic is relevant for the scientific community in snow-dominated areas due to the lack of HR mapping on a time scale that allows monitoring of quick changes in the snowpack extension and mass change, and the scarcity of direct SWE measurements and methods to provide a dense network of monitoring stations for spatial interpolation approaches. The innovation and relevance of these objectives are sufficiently addressed in the manuscript. However, in its current version, some issues are found that require further assessment before considering further review and potential publication in this journal. Please, find below these major items. I hope that these comments are useful to improve the work and help to further comprehend its context and applicability further than the present results.

The authors thank the Reviewer for his/her constructive feedbacks and comments on the manuscript. We went through each point and took advantage of the comments to improve the quality of the manuscript. Our answers are reported in blue.

- The Introduction section contains good points but requires some structure to get more focused on the specific goals' context. I would also recommend to present this earlier in the narrative. Lines 100-108 can be easily moved/merged to/with section 2 for the sake of clarity.

  Thank you for your suggestion. By taking into account also the comments provided in the open discussion, we structure the introduction as follows: first, we talk about the importance of snow for hydrological applications and why it is important to get SWE maps. Then we introduce the available methods to monitor snow and, among them, we present remote sensing as an alternative that provide natively spatialized observations. In detail, we explain that it is not possible to derive SWE estimates regularly from the current satellite missions. However, important information about the melting phases can be extracted by multitemporal SAR images and snow cover depletion curves from optical sensors. Hence, we propose to exploit this information sources to retrieve SWE.

  We propose to modify the introduction accordingly in the revised version of the manuscript.

- The general objective should be better elaborated in line 99, i.e. not only state what but also what for and some specific scope. For example, the target type of catchment is relevant but it is not declared until lines 130-131 that size is limiting the potential further applicability of the method. Moreover, the order of magnitude of "a not too vast catchment" must be assessed.

  We thank the reviewer for pointing out this issue. We would like to explain better that the approach is not necessarily designed for a catchment. This will be better stated in the new version of the manuscript, where we introduce the state concept at a pixel level and not at a catchment level (see next point). However, we make the necessary assumption to consider the accumulation happening for the entire area of interest. In fact, in our study cases, we lack information to consider better detail. In this sense, considering a "too vast" catchment would make this already strong assumption much less reliable.

  In fact, this assumption works (as also shown by our obtained results) when the catchment is subjected to similar meteorological forcings. 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). This suggestion has been reported in the manuscript.

  In this way we propose to rephase L99 as follows: "This work proposes a new multi-source approach to reconstruct HR daily SWE time-series at the pixel scale. We explore the applicability of the method to two mountainous catchments: i) the South Fork catchment of the San Joaquin river, located in the Sierra Nevada - California (USA), and ii) the Schnals catchment, located in the Alps - South Tyrol (Italy). The main sources of error are discussed to provide insights about the main advantages and disadvantages of the approach, which may be of great interest for several hydrological and ecological applications."

- Section 2.1 is determinant in the methodological approach. In the explanation, it is not clear whether the catchment state is identified for each pixel or for the whole catchment area; this needs a revision to be clear throughout the text. Moreover, the spatial definition of the "total delta-SWE" is missing, which is required, and additionally the use of this variable should be uniform for the three states (i.e. is also total in line 149?). In line 149, I am not sure about the meaning of "no changes WITHIN the catchment", do you mean really that or rather no change when considered as a whole?

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).

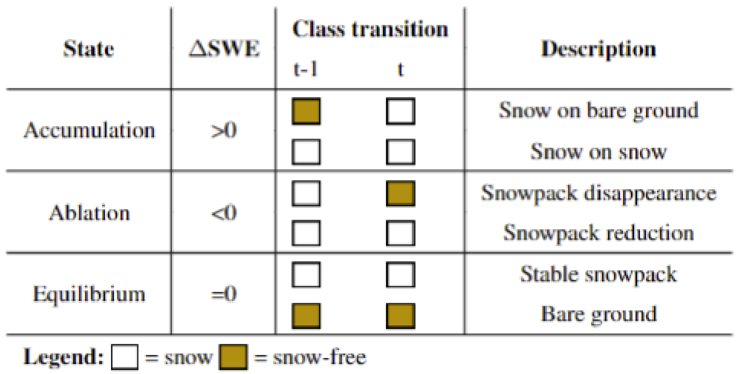

Figure 1 Definition of the three possible states: accumulation, ablation and equilibrium. The possible class transitions at pixel level associated with the state are described.

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.

- I have doubts on the simplification done on the potential combinations of positive/zero/negative values of delta-SWE and delta-SCA in this section. First, it seems that both variables have different spatial definitions, since pixel changes in terms of SCA are assessed. Additionally, some situations are discarded, for example, accumulation is not allowed to happen with negative delta-SCA values, but this is not infrequent in mountain areas in some regions in the world. Other situations are not included in the three potential states. In general, the assumptions are difficult to be validated in semiarid regions with snow relevance or during patchy snow periods in steep slopes, especially if the catchment state is defined uniformly in space. These issues should have been assessed and their discarding justified or at least clarified in terms of the applicability of the method.

  As stated in the previous answer, we hope that our revised version will solve these doubts. Notwithstanding, we agree with the Reviewers observation that the assumptions made cannot work for each situation. A possible situation is represented by a snowfall happening at high elevations while lower elevations experience melting due to rain on snow or higher temperature. If we are aware that this represents a common situation in our study area, we suggest either dividing the catchment into different elevation belts if more AWS are present or implementing a method to detect the snowfall limit (e.g., based on temperature). This limitation has been better highlighted in the discussion section.

- In section 2.3, two issues require further assessment. First, the use of day-degree modelling for melting rates' estimation is not the best choice if accurate HR maps are the goal, in my opinion. At least, some justification of this apparent lack of coherence should be included, together with the comparison of the error of SWE estimation associated to the use of such methods and the error from low resolution satellite products. Secondly, the adoption of the temperature threshold is one of the major sources of error in the SWE estimation in mountain areas, as many works have already shown; so, the selected value needs some justification. Thirdly, and more relevant, lines 280-283 involve that melting is the only process in the ablation of the snowpack, which means that sublimation is neglected (but nothing is said on this); this may result in non-negligible loss of mass in the closure of the balance equation, and it is a constraint for the applicability of the method in some regions or during some periods/under some atmospheric conditions. This must be addressed in the description of the methodological assumptions and their validity. Finally, some comments on the scale effects from the subdaily evolution, not operating in the method, should be included.

  We thank the Reviewer for giving us the opportunity to better explain these relevant concepts and better constrain the applicability conditions of the method in real situations.

  Regarding the use of a degree-day model, we agree with the referee that there is an apparent lack of coherence in targeting HR SWE maps and using a DD model. We are also aware that the temperature threshold is a critical parameter that can introduce errors in the method. This has been clearly stated in the new version of manuscript citing some literature works that can stimulate further research (e.g., Tobin et al., 2013). We agree that a full energy budget model is required to model in an appropriate way the snowpack. However, we decided to use a simple DD model because our aim is to propose an approach as simple and independent form detailed meteorological observations as possible, which maximizes the information content of the remote sensing data. Moreover, since our target is SWE reconstruction, we need a model which can be easily inverted in time, unlike complex energy-based models that involve the solution of differential equations. However, the same approach that we used to define the pixel state could be used also with more sophisticated snow models.

Despite these assumptions, we were also surprised by the goodness of obtained results using this simple model. However, the HR observations of daily SCA and the timing of melting derived by SAR data provide a good modulation of the potential melting that allows us to estimate SWE with high geometrical detail. Please note that in the new version of the paper we also added the results expressed in mm to better analyze the behavior with respect to the topography (see Figures 2-13). In the figures below, it is possible to note 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.

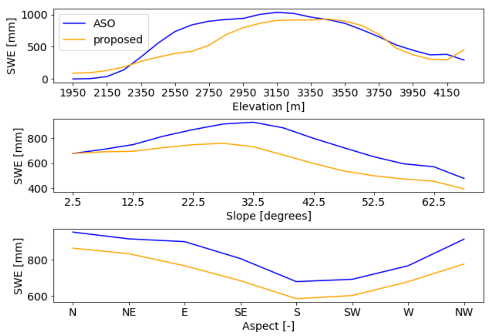

Figure 2 17/03/2019.

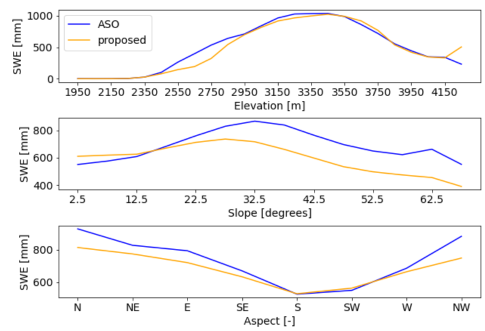

Figure 3 02/05/2019.

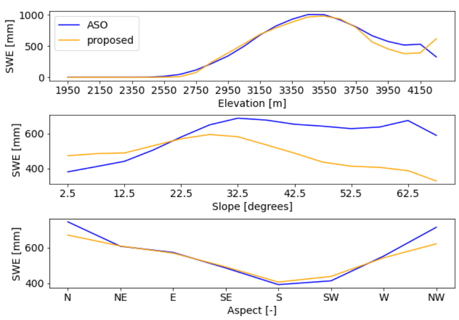

Figure 4 09/06/2019.

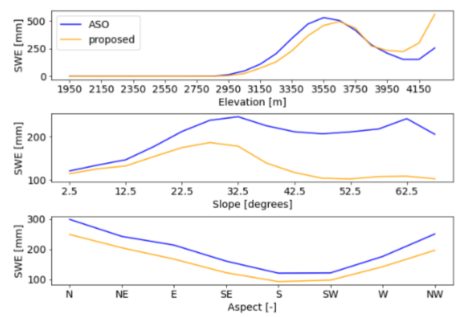

Figure 5 04/07/2019.

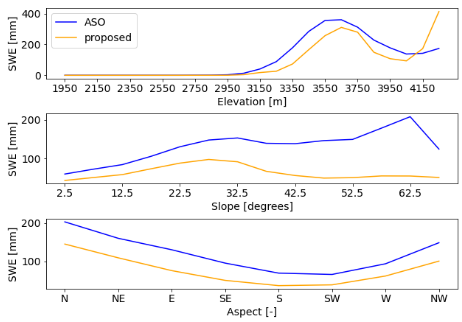

Figure 6 14/07/2019.

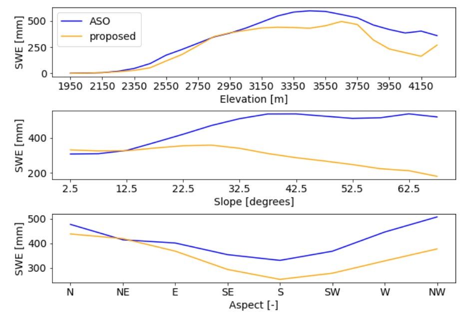

Figure 7 15/04/2020.

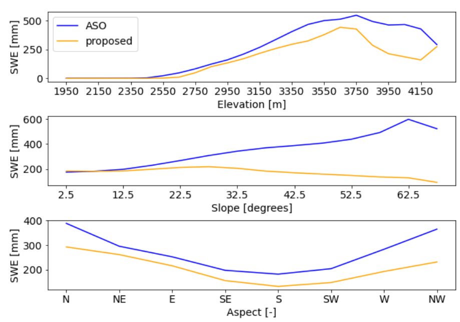

Figure 8 05/05/2020.

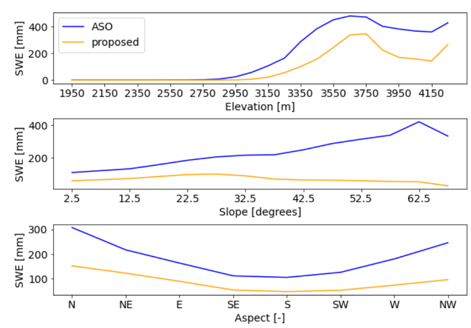

Figure 9 23/05/2020.

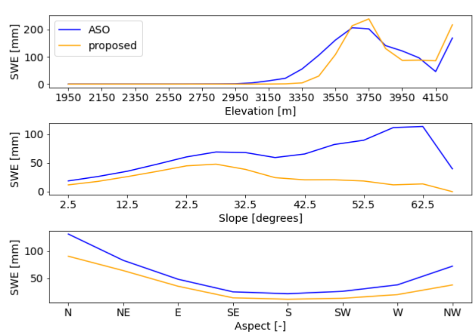

Figure 10 08/06/2020.

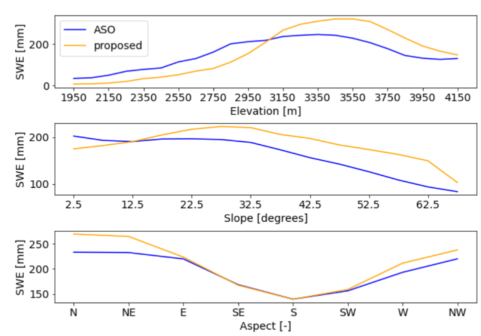

Figure 11 26/02/2021.

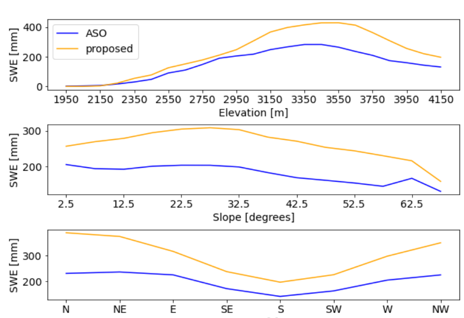

Figure 12 31/03/2021.

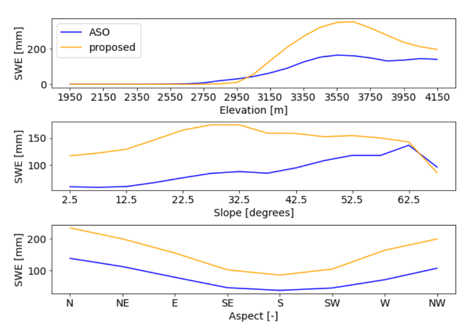

Figure 13 03/05/2021.

We also performed a sensitivity analysis, as suggested also by Reviewer#1 to assess the impact of the DD model to the final results. 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 a, 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.

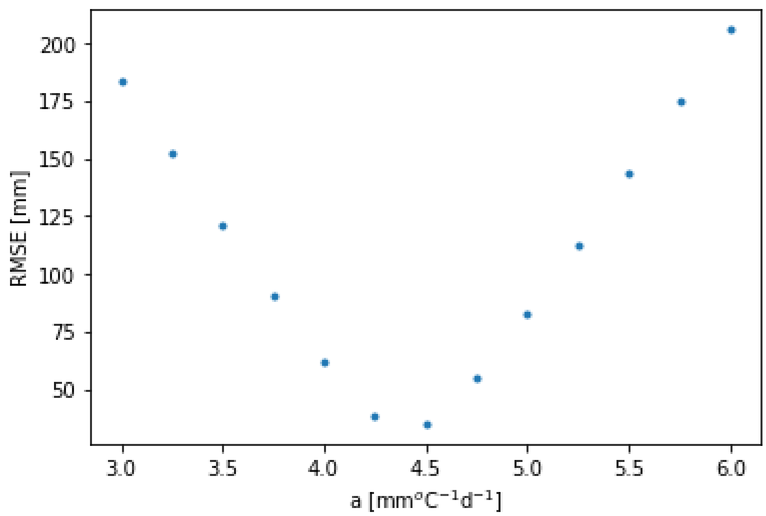

Figure 14 tSD = 27/06/2019, tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), SWE threshold=2mm, a varies from 3 to 6 mm/(°Cd) by steps of 0.2.

It is possible to notice in Figure 14 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).

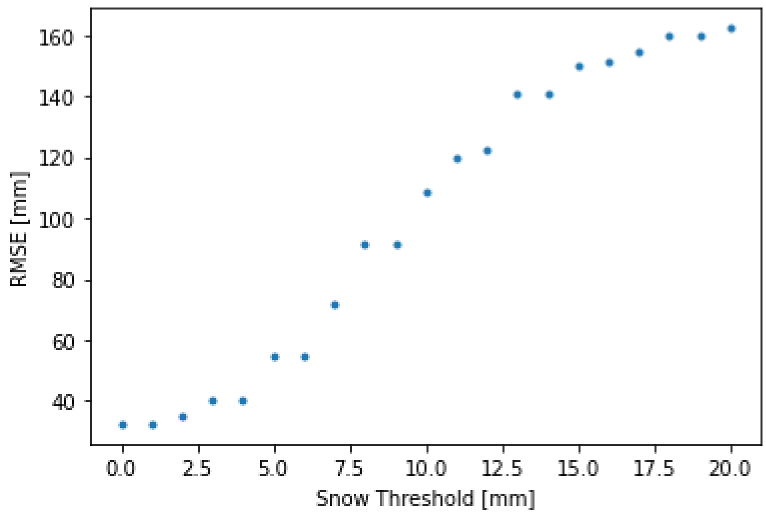

Figure 15 tSD = 27/06/2019, tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), a=4.8 mm/(°Cd), SWE threshold varies from 0 to 20 mm by steps of 1 mm.

It is possible to see in Figure 15 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.

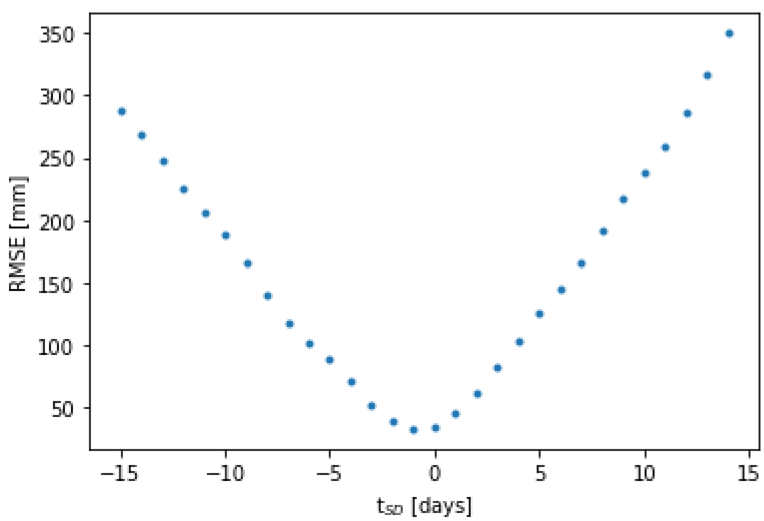

Figure 16 tSA=22/11/2018, tS1=22/04/2019 (as detected by the station), a=4.8 mm/(°Cd), SWE threshold 2 mm, tSD 27/06/2019 +- 15 days.

It is possible to notice in Figure 16 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.

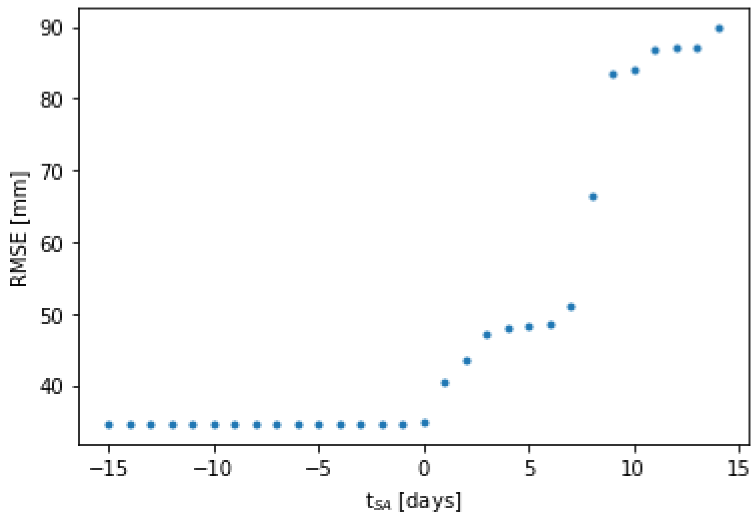

Figure 17 tSD 27/06/2019, tS1=22/04/2019 (as detected by the station), a=4.8 mm/(°Cd), SWE threshold 2 mm, tSA=22/11/2018 +- 15 days.

It is possible to see in Figure 17 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.

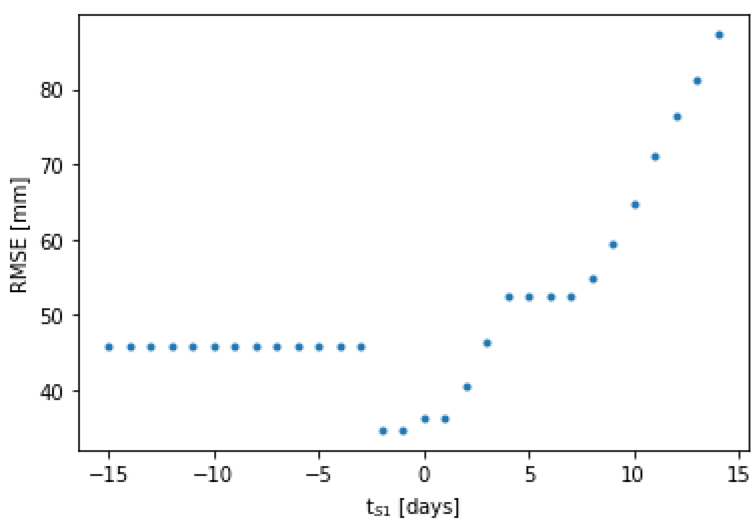

Figure 18 tSD = 27/06/2019, tSA=22/11/2018, a=4.8 mm/(°Cd), SWE threshold 2 mm, tS1=22/04/2019 +- 15 days.

In this case, it is also possible to see in Figure 18 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. However, also possible errors introduce in the potential melting calculated through the degree day play an important role. An error of 150 mm is introduced when using a coefficient of 1 mm/°Cday higher than the optimum. A similar error is obtained when considering a tSD of +- 5 days (that is the acquisition frequency of Sentinel-2 images).

However, in our opinion, the most interesting way to improve the current results is to move from modeling to actual observations that are intrinsically spatialized. We are currently working toward a solution that may consider remote sensing information only (as mentioned also in the old version of the manuscript at L498-500), for example by considering surface temperature measurements from satellites, meteorological radar, or SAR-derived snow depth/SWE information. However, this needs further research and a community effort, and it is left in this work as future development.

Regarding sublimation, we do not implicitly consider it in the method as well as wind redistribution or gravitational transport. In detail, the SWE reconstructed by the proposed method represents the amount of snowmelt water, net of sublimation. This, together with the connected limitations in the applicability of the method, has been better stated in the Method Section. However, while sublimation, redistribution and gravitational transport cannot be explicitly considered in the method, we can still observe their effects on a different snow persistence on the ground, which is detectable in the HR SCA time-series. We believe that this represents one of the advantages of using a daily HR SCA time-series as input to the method.

As it was stated in L190 we do not consider subdaily variations: "However in this work, we do not consider subdaily variations but only changes that are sampled in the temporal resolution of the exploited HR SCA time-series, i.e., one day." We agree that accumulation and ablation happen within an hourly scale. However, our method does not aim to achieve such a detail. Given the general objective of the proposed method, the overpassing time of Sentinel, the many assumptions made, e.g., the use of a simple degree day model, the lack of a method to evaluate the snowfall limit, the scarcity of temperature measurements that would not allow an accurate interpolation with a subdaily detail. But this is an interesting direction for future works.

- In section 4, the results are shown as selected points/transect/ periods in the study catchments, and detailed datasets are included as appendixes. The selection must be justified in all cases. The associated figures and tables' captions must include the catchment name in all cases (see figures 5 to 7, and table 1). Some sentences lack a proper justification, for example, lines 389 and 399 contain comments that can't be rigorously concluded in general from what has been shown. Or line 408, regarding Fig. 13, has a mass balance closure test been done? Figure 9 caption, are these "trends"?

Thank you for these suggestions. For sake of brevity, we reported only some of the results in the main draft. While we think that it might be interesting to show them in the Appendix, putting all of them in the main manuscript would be too much. Additionally, the metrics reported in L396 are already calculated for all the references and give an idea of the accuracy of the method. However, for completeness, we also added Figure 19 where we show the scatterplot between the measured and proposed SWE for the complete dataset as required by Reviewer#2.

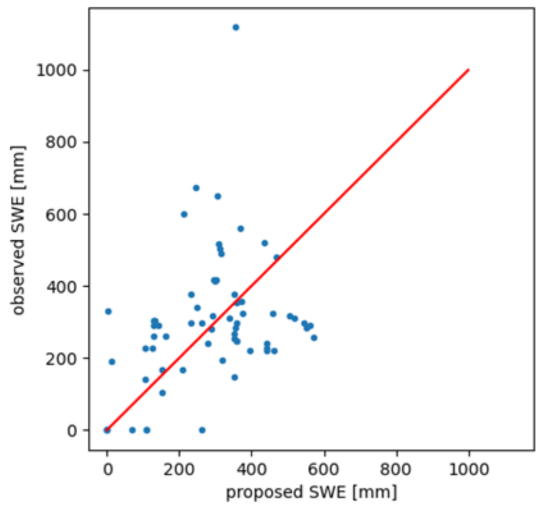

Figure 19 Observed and proposed SWE in the Schnals catchment for the h.y. 2020/21.

We included the catchment name in the Table/Figures.

We agree that the statement in L389 is not scientifically rigorous, hence we removed it. With L399 we 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. This discussion has been added to the manuscript.

According to the other reviewers' comment, we removed Fig. 13 and 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 20. This makes the two variables more comparable, although the analysis remains qualitative and a mass balance closure test has not been performed.

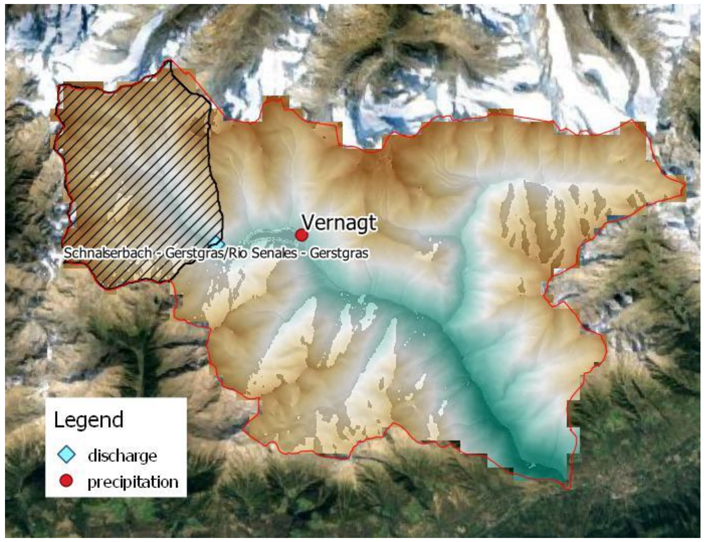

Figure 20 Overview of the subcatchment whose outlet point corresponds to the location of the discharge measurement.

We presented two new plots (Figures 21 and 22) 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.

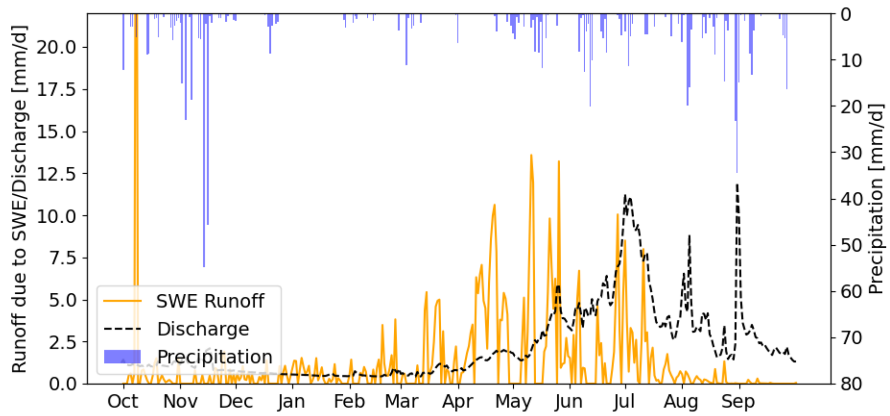

Figure 21 Snow generated runoff, discharge and precipitations for a subcatchment in the Schnals valley for the season 2019/20.

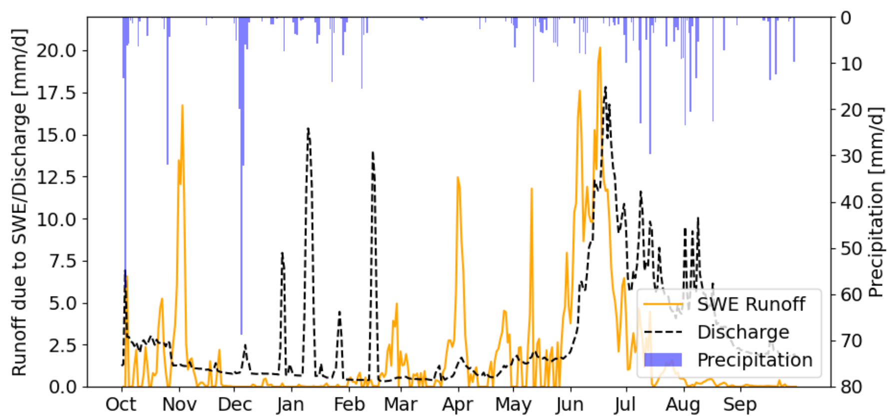

Figure 22 Snow generated runoff, discharge and precipitations for a subcatchment in the Schnals valley for the season 2020/21.

- The discussion in section 5 repeats many facts or comments that have been previously presented or commented. Moreover, the discussion is focused on the sources of error at each step of the proposed method. I miss the discussion on the goodness of the results when compared to other products/methods/data sources that provide less resolution, or other standard or alternative existing methods. This is important as HR SWE mapping is the target goal.

The discussion on the goodness of the results has been expanded. We added the scatterplots as rquired by the Reviewer and commented on that (see next answer). Furthermore, beside comparing the obtained results with ASO data, we propose a comparison with a product at 500 m (Fang et al., 2022), as reported in the answer to Reviewer#2. However, we believe that the ASO dataset remains the key reference having that product a comparable spatial resolution. However, it is worth discussing why we think that it is difficult to estimate in a quantitative manner the improvement due to the use of HR data. 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 said 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 absolute truth, being aware that it presents an uncertainty (see (Premier et al., 2021), for more details). In this sense, the LR SCF is also corrected during the process. Hence, when using this corrected LR dataset to reconstruct the SWE we obtain results in line with the ones aggregated at 500m and first derived at HR by the approach presented in this paper. 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 an incontrovertible geometric detail, which might be of paramount importance for some applications (e.g., hydrology, avalanche forecasting, ecological studies, etc). 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 instead of the completed ones can introduce artificial errors that do not allow one to properly quantify the advantage of using LR or HR data.

We take the opportunity raised by this comment to evaluate the results obtained by the proposed approach against a further 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

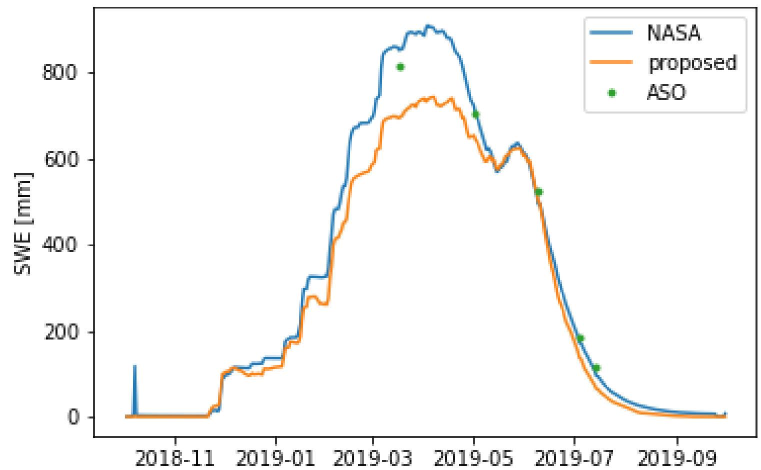

Figure 23 Results for the South Fork catchment of the San Joaquin river for the h.y. 2018/19.

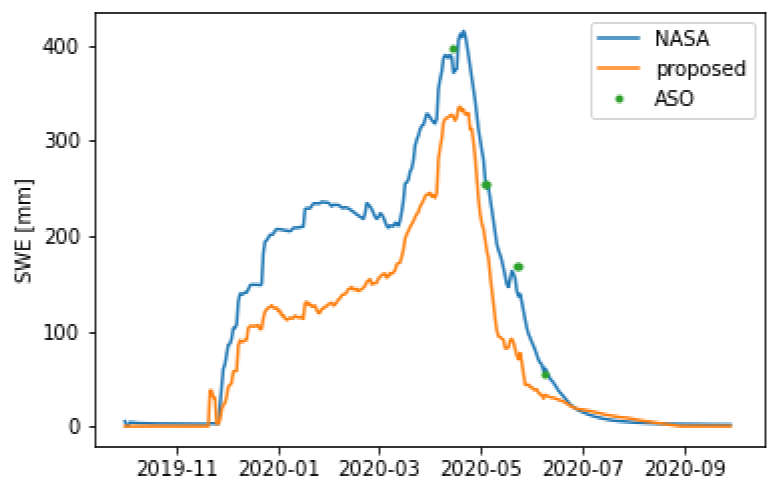

Figure 24 Results for the South Fork catchment of the San Joaquin river for the h.y. 2019/20.

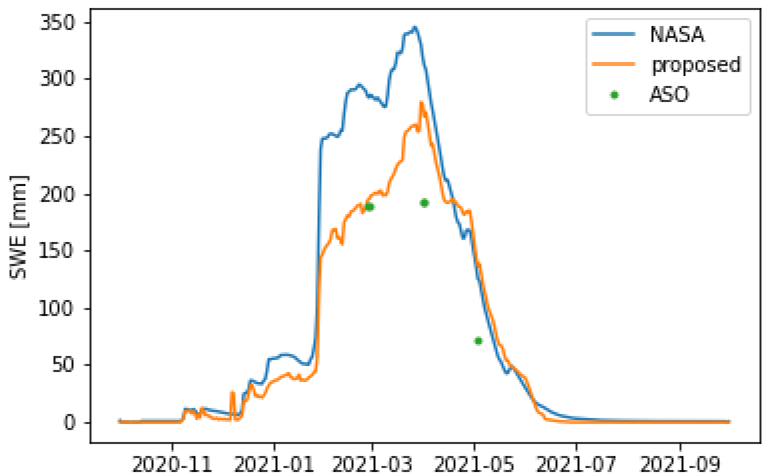

Figure 25 Results for the South Fork catchment of the San Joaquin river for the h.y. 2020/21.

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 product.

- The error indicators in results cannot be properly valued since little information is included from the study catchment in terms of SWE regime, in section 3.

Thank you for this interesting comment. Given the availability of SWE records for the South Fork catchment starting from 2000 in the Volcanic Knob station, we performed an analysis of the SWE records. In the following plots, you can appreciate the average SWE for all the recorded years, the range of variation and the current SWE (referred to the seasons 2018/19, 2019/20 and 2020/21). It is possible to see that the first season presents high SWE, while the other two seasons are under the average regime. Unfortunately, SWE records are not available for the Schnals catchment. Additionally, we do not have long records of snow depth observations that allow us to perform a similar analysis. However, the two seasons are in average with the typical snow conditions in Schnal.

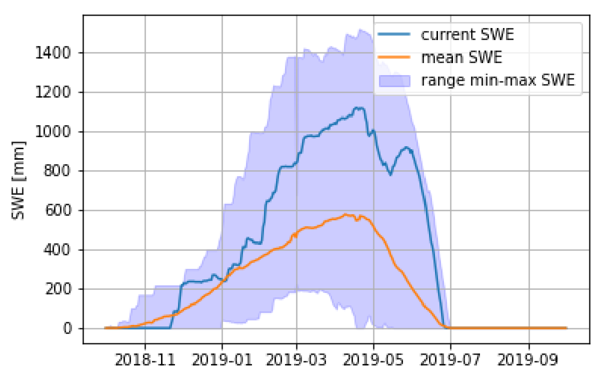

Figure 26 SWE trend for the h.y. 2018/19.

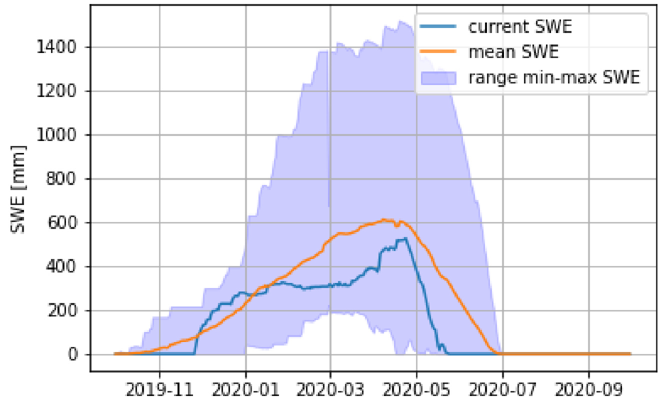

Figure 27 SWE trend for the h.y. 2019/20.

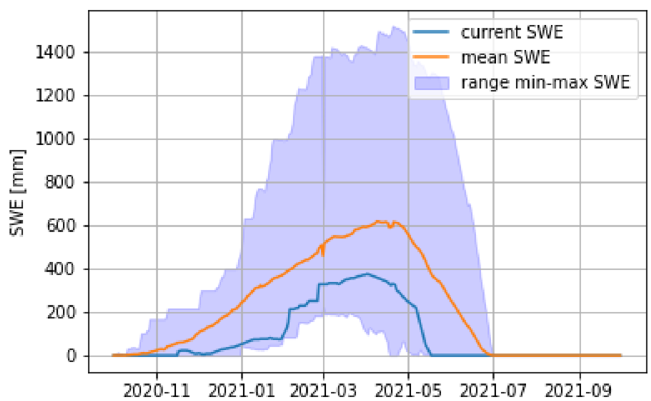

Figure 28 SWE trend for the h.y. 2020/21.

- The discussion/conclusions should also include more reference to what processes can be tracked from the time series obtained of these SWE maps, and what cannot due to the assumptions, etcetera in the approach. This is very relevant to address the further applicability of the method.

Thank you for pointing out this important aspect. We have expanded the discussion on possible applications of our time-series. In detail, we have seen that the time-series provides accurate results, especially when considering the catchment scale and the late melting phase. For this reason, it can be exploited in hydrological applications such as streamflow forecasting, especially in regions where complex topography generates complex snow accumulation patterns and differential melting times (Tappeiner et al., 2001), which controls, besides hydrological processes, also key ecological processes in mountain regions (Park et al., 2021; Seeber et al 2021). Moreover, our approach could be particularly suitable for remote regions where poor data on precipitation amount are available, since it is independent from this information. Also, we recommend applying this approach in environments that are not characterized by strong sublimation processes as well as catchments that present, as discussed earlier, frequent mixed states as snowfalls at high elevations while rain-on-snow at lower elevations. Furthermore, the method can be exploited to provide also historical SWE reanalysis time-series (at least a LR sensor should be present for the SCA reconstruction) that may allow understanding the effects of climate changes.

Some additional comments:

In general, the English usage and edition is good, but some revision is recommended.

Thank you for the suggestion. 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

Please, review the use of some wording. For example, line 381, “while the others (seasons) are drier” really means snow-scarce, which can also be due to high temperature; or the use of “bias” in the work to define “difference” or absolute error.

Thank you for the suggestion. We considered the use of new wording.

When some references are included in a list, please, use a constant criteria to order (increasing or decreasing date)

Thank you for the suggestion. We ordered the references by considering an increasing date.

Reference in line 35 looks not recent enough to be a updated review for remote sensing products, at least, some others could have been included.

Thank you for the suggestion. We added the work presented by Dong (2018).

Line 65, please provide some reference, there are works on that (i.e. Pimentel et al., 2015;2017; or others).

Thank you for the suggestion. We added the following references:

Pimentel, R., Herrero, J., Zeng, Y., Su, Z., & Polo, M. J. (2015). Study of snow dynamics at subgrid scale in semiarid environments combining terrestrial photography and data assimilation techniques. Journal of hydrometeorology, 16(2), 563-578.

Pimentel, R., Herrero, J., & Polo, M. J. (2017). Subgrid parameterization of snow distribution at a Mediterranean site using terrestrial photography. Hydrology and Earth System Sciences, 21(2), 805-820.

Please, assess the error associated to the ASO product, taken as ground-truth to test the results.

As reported in the paper presented by Painter et al., 2016, the SWE uncertainty is dependent on both the uncertainty associated with the snow depth retrieval and the uncertainty associated with the snow density modeling. The authors report “Our best understanding thus far is that snow depth uncertainty at the 3 m resolution is unbiased with RMSE of 0.08 m, resulting in depth uncertainty of < 0.02 m at 50 m resolution. With the snow density uncertainties of 13–30 kg m− 3 described above, we can estimate scenarios of SWE uncertainty. For a snowpack of 0.5 m depth and 100 kg m− 3, the SWE uncertainty is about 1 cm relative to the 5 cm actual. For a snowpack of 4.5 m depth and 450 kg m− 3, the SWE uncertainty is 10 cm relative to the 203 cm actual.” This information has been added to the manuscript.

Beyond the comparison of results and ASO in the appendixes, dispersion graphs are needed to further assess the performance of the method, and some selected cases should be included in the results’ section.

Thank you for your suggestion. We revised the results section. Here we include the dispersion graphs. It is possible to notice a generally good correlation between the proposed and ASO SWE. However, there is a tendency of observing a high dispersion that is also confirmed by the computed RMSE value. However, most of the points that are distant from the diagonal are outliers (low density). The year 2019 does not show a specific trend and the shape of the” cloud” is generally more symmetric. In the year 2020, it is possible to see that our product tends to underestimate SWE, except for very high SWE values (mainly outliers). On the other hand, our product tends to overestimate SWE in the year 2021. The worse correlation in the season 2020 was encountered also from the average value reported in the manuscript. However, given the high number of compared pixels, it is difficult to understand if there is a systematic error that generates the outliers. This analysis has been added to the revised version of the paper.

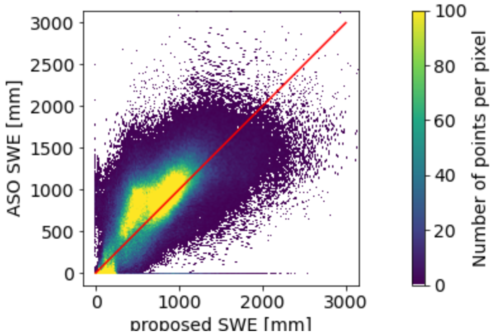

Figure 29 17/03/2019

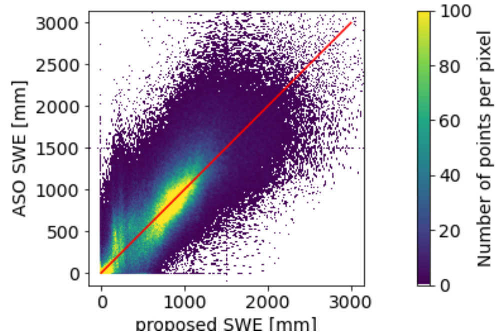

Figure 30 02/05/2019

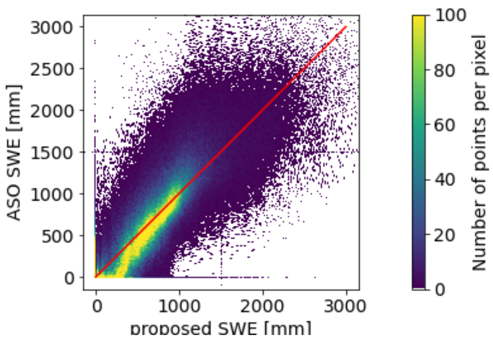

Figure 31 09/06/2019

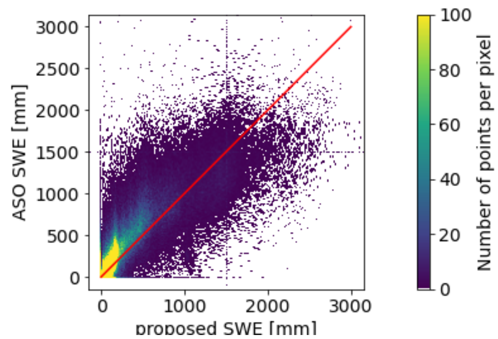

Figure 32 04/07/2019

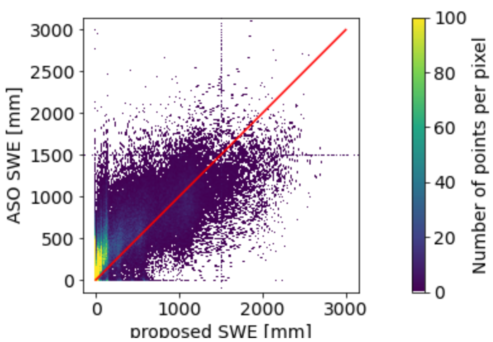

Figure 33 14/07/2019

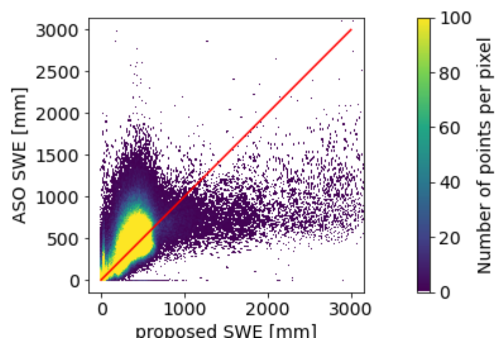

Figure 34 15/04/2020

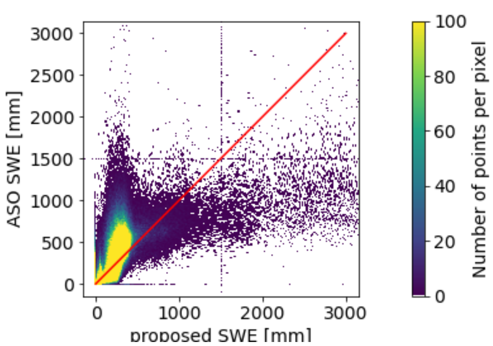

Figure 35 05/05/2020

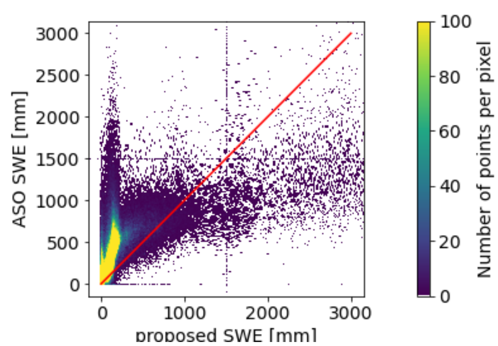

Figure 36 23/05/2020

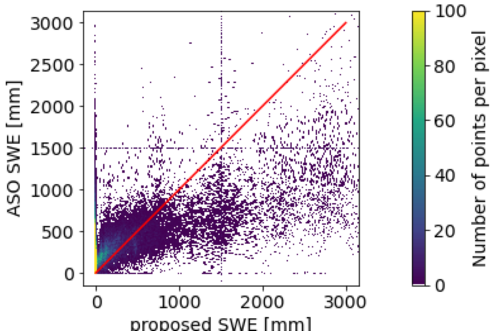

Figure 37 08/06/2020

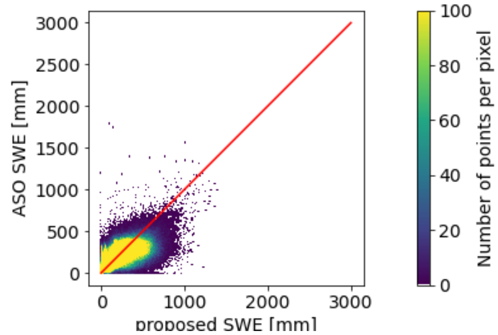

Figure 38 26/02/2021

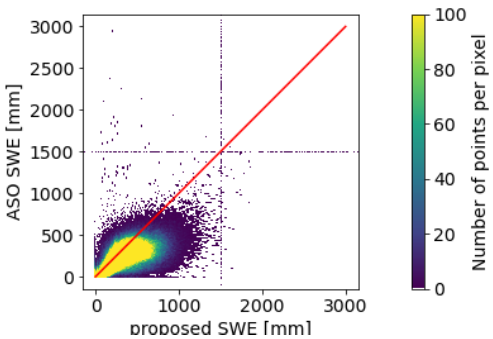

Figure 39 31/03/2021

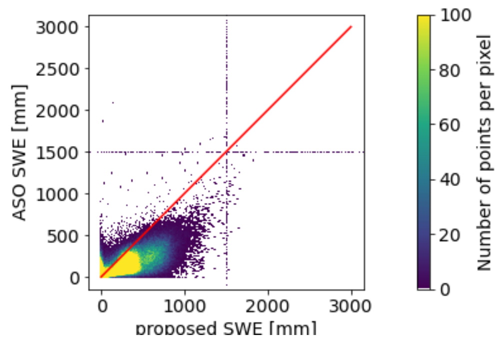

Figure 40 03/05/2021

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