Comment on tc-2022-146
Anonymous Referee #1

I would like to congratulate the authors to this very interesting 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.

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 corresponding to the moistening phase (i.e. a mere drop in backscattering), this decision should be also
explained in more detail.
- 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.
- 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 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 $t_{SD}$ and $t_{SA}$ (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.

Additional Comments

Abstract/1 Introduction:

- 1 reconsider recasting the first sentence.
- 3 how about ablation processes, or are you specifically talking about peak SWE?
- 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.
- 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.
- 21 not only on local hydrology, as many regions of the world rely e.g. on the spring freshet hundreds of km downstream.
- 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.
- 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.
- 65 “accumulation and melt = „accumulation and ablation“ (i.e. in this sense ablation
includes erosion and evaporation etc.)

- 66 consider topography vs geomorphology (throughout the text)
- 80 they range from DD to complete energy balance models (as used in Bair et al 2016), and these do not require calibration
- 114 (Italy) like (USA)

2 Proposed approach to HR SWE reconstruction

- 120 first sentence is obsolete iMo.
- 132 please specify „too vast“.
- 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.
- 141 „extension“ = extent
- 143 „...dSWE < 0 due to melt water drainage“.
- 145 snow depth, not height. Anyway, better to talk here in terms of mass/SWE. e.g. „...if the snowpack is melting only partially“.
- 167-168 maybe better placed in the discussion section
- 169 potentiality = potential to detect the presence of a melting snowpack...
- 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.
- 181 replace “quotes” with “elevations“, “elevation bands“ or similar (throughout the manuscript)
- 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!
- 196 contamination = obstruction?
- 276 which variable is used as external drift, elevation?
- 296 contemporary = simultaneously ?
- Figure 3, I don’t see this figure referenced anywhere in the text,

3 Study Areas and Dataset Description

- 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.
- 321 Are manual SWE observation only available for one of the two seasons?
- 336 S1 is not introduced in the text (unlike sentinel-2)
- Figure 4: resolution should be improved; it is very hard to read.
- 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.
4 Results

- Figure 6: image resolution should be increased. “Trend” could be recast as “time-series”
- Figure 7: rather small, also image resolution could be increased, caption does not mention ASO
- 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.
- 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.

- 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?
- 401 tends to increase
- Figure 12, see fig 9. Since no spatial observations are available in Schnals, I don’t consider this figure very helpful.
- What about an evaluation against the automated snow depth sensor in Schnals?

5 Discussion

- 413 maybe better “...quantitative and qualitative evaluation of the proposed SWE reconstruction over two study areas”
- 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)
- Section 5.1 only focusses on errors in predicting the accumulation state. Please expound upon the uncertainties associated with predicting the ablation/equilibrium 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.”

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