Answers to Reviewer #1

We thank the reviewer for taking his highly valued time to reassess our manuscript.

Major Comments:

1. I appreciate the authors' efforts to better define the variables from SNOWPACK (Table 1) but I unfortunately remain confused about exactly what parameters are being used in the study. For instance, snow height has both a measured and modeled variable in Table 1 and the caption for Figure 2D states "measured snow height (HS) at the station as extracted from SNOWPACK data." As such, is this measured or modeled?
   Thanks for pointing this out, we understand that while we show it in the table, the difference between modeled and measured is not as clear in the text and we will make it more clear. Instead of using this phrasing we will simply state: "measured snow height (HS)". The measured data at the station is forced into the model at the measurement station and we use this measured value. However, in the new version we plot the modelled SWE rather than the measured HS to address the reviewer's later point. We have improved the figure, adjusted our wording throughout the manuscript to remove the ambiguity.

   How do the modeled and observed snow depth variables differ? (For example, is the modeled snow depth based on precipitation forcings and model physics alone? And if so, how does this compare to the measured snow depth time series?).
   Solid precipitation measurements in mountain areas are difficult and prone to large uncertainties: the sensors need to be heated and therefore can not be installed off-grid, the sensors need to be shielded from wind, ideally with a Double Fence Intercomparison Reference (DFIR) which is expensive and requires lots of flat space (simpler wind shield still suffer from significant undercatch). Therefore the Swiss network of automated snow and weather stations for avalanche forecasting (IMIS) is only equipped with very simple, unheated rain gauges to detect rainfall together with snow height sensors. The Snowpack model then assimilates the positive differences between the measured snow height and the modeled snow height as precipitation input, therefore accounting for new snow fall and snow deposition by the wind (Bavay, 2024) without interfering with the modeling of snow sublimation and melt.

   Also, as noted below in minor comments, would modeled SWE provide a better parameter for helping disentangle densification vs mass loss from the snowpack?
   Done, thanks.

2. The S1 wet snow retrievals performed considerably worse in comparison to station/SNOWPACK obs at the two other sites (DAV5, SLF2), which was attributed to local conditions (aspect, presence of avalanche infrastructure and construction/security features associated with World Economic Forum). While these factors likely play a role, there is much more to investigate and report here that is relevant to the current study.
   A couple questions that come to mind include:

how does the snow depth and melt history factor into the backscatter time series (these two sites have much shallower snowpacks that appear to experience intermittent melt)?
Thanks, we address this question by expanding the discussion section with ongoing works on the topic.

Is the median approach for establishing the background backscatter value the most robust for these shallow snowpacks or would a different approach work better?
To assess the robustness of the all-year median as a reference for background backscatter, we additionally tested alternative configurations using both the median and mean over a late-summer period (July to September) and a winter period (December to February). The effects on the output were negligible. Given the pronounced elevation gradients and the climatological variability of the study area, any month-based selection of a reference period is inherently subject to similar degrees of arbitrariness. We argue the all-year median represents the least assumption-dependent choice for our application.

What are the implications of this poor performance relative to the wet snow avalanche comparison (assuming start zones are typically high perhaps this isn't a huge issue)?
As the reviewer notes, avalanche release zones are typically located at higher elevations, where such conditions are less prevalent. We therefore argue that the poor performance in these areas has limited implications for the overall applicability of the method to wet snow avalanche precondition detection, and that this aspect would be better addressed in a more physically focused follow-up study. We make this more clear in the discussion section.

I think at least one of the Appendix figures should be included in the main text and further described as it is highly relevant to understand where it worked well and where and why the approach did not work well.
See comment above.

3. The paper is partly framed around the comparison between the LRW approach and the Copernicus SWS product. However, the findings on this topic are qualitative in nature and at times, vaguely described/supported (e.g., Ln 10: "we found clear benefit in terms of spatial…"). I strongly suggest developing additional approaches for directly comparing the wet snow detections between these two approaches beyond the visual comparison offered in Figure 2 (Figure 5 is nice, but layers on the uncertainty from the avalanche catalogue) and the general description provided in Ln 237-241.

   Thanks, we introduced the following statement in the results and add corresponding parts in methodology and discussion:
   "Comparing our detection of wet snow to the SWS product is not trivial due to the differences in spatial and temporal resolution and the lack of a spatially continuous reference data. However, the overall MCC value of 0.90 between SWS and LRW at WFJ2 station suggested a general agreement on the main melting phases (Fig. 2.C

for SWS and shadings in .D and .E for detected from LRW product), with yearly MCC values varying between 0.87 (2017/18 and 2019/20) and 0.94 (2018/19)).

Overall, the Copernicus SWS layer provides spatial coverage of 30–60\% of the study area (median: 48.6\%), whereas the multi-track LRW approach achieves 94–100\% coverage (median: 99.9\%). The reduced coverage of the SWS product is primarily attributable to masking strategies applied to minimise artifacts arising from radar geometry effects, from land cover types such as forest and water bodies (visual in appendix Fig. A10).

At the WFJ2 station, the Copernicus SWS product resulted in 17 false positive wet snow detections across the time series, instances where wet snow was indicated in the absence of snow cover, whereas the dual-polarised multi-track LRW approach produced none.“

On a related note, the Figure A7 caption suggests a comparison in the wet snow ratio threshold from the two approaches (LRW and SWS) but it unclear exactly what approach is being plotted here.

Thanks, good catch! It shows not a comparison to SWS but four versions of the LRW assessment, when parsed by aspect ranges. We corrected the figure caption accordingly.

4. As described below with a few minor comments, the manuscript text would benefit from a very careful review and revision to improve clarity of the findings and general readability.

   We went over the entire manuscript again and worked towards a general higher readability.

Minor Comments:

Title: I suggest retaining “Wet snow” somewhere in the title given the focus of the paper
Title: What is meant by preconditions? From the paper, I know this is referring to the snow conditions prior to the avalanche but I don’t think this is clear from the title. Further, given that the LRW products contain observations from multiple days, I don’t think it is reasonable to describe these as conditions prior to the avalanche but rather as conditions around the avalanche.

Thanks, for the comment. We would argue “preconditions” is the most accurate for the temporal binning and assignment of avalanches to the LRW composites that we performed in this work. As described in line 198: Avalanches were assigned to the latest available LRW composite preceding their occurrence (and illustrated in Fig. A1) . So we believe wet snow avalanche “preconditions” is more representative for our assessment than “around”, which would indicate a matching to the avalanches happening during the day of acquisitions.

We adjusted the title as follows:
“Wet snow avalanche preconditions from Sentinel-1 Multi-Track Composites”.

4: define ESA
Thanks, done.

4: consider alternative word choice to propose: evaluate perhaps?
Thanks, implemented.

6: why is snow-pack hyphenated here?
Thanks, corrected.

7: explicitly state that this is modeled liquid water content
Thanks, done also for runoff.

11: include more specific finding/statement regarding “clear benefits in terms of spatial performance.” See major comment.
Thanks, added.

16: revise for more technical language
Done, thanks.

19: no further reference to increase in wet snow avalanches in the text; suggest developing this point further if included in the abstract
Adjusted main text to: “Wet snow avalanches, with their highly destructive potential, have been found to increase in frequency relative to dry snow avalanches (Eckert et al., 2024), yet they are difficult to predict (Hendrick et al., 2023; Mitterer and Schweizer, 2013; Baggi and Schweizer, 2009).”

30: replace on with over
Done, thanks.

45: There have been a number of recent studies conducted in the western US that that have compared Sentinel 1 wet snow onset to SNOTEL parameters. I suggest providing a brief summary of these findings at this point given the direct relevance to this study’s objectives.
Thanks, we did not include a presentation of the entire project, however implemented a reference to the most relevant work.

56: clarify meaning of backslopes; why do these have high ground resolution compared to other locations?
Backslopes, in radar geometry, are slopes oriented away from the sensor such that its terrain is sampled more often than in flat or foresloped terrain. The number of samples is higher, giving higher local spatial resolution and improved radiometric stability.
We make this more clear in the manuscript. by changing to:
“The approach of generating local resolution weighted (LRW) composites makes use of multiple viewpoints on mountainous terrain to improve local resolution and reduce missing data (shadow) from an individual orbit \citep{Small2022}. This method exploits the enhanced ground resolution available on slopes oriented along the radar line of sight (backslopes). This increases resolution arises from an increased number of resolution cells per standardised

ground range interval, resulting in higher spatial sampling density and, consequently, greater radiometric stability \citep{Small2022}."

70: add commas to elevations

The Cryosphere's submission guidelines do not require commas as thousands separators. After the first revision we have moved away from our previous formatting (using an apostrophe) to a space separator and since commas can be confusing too, we would prefer to retain this convention.

70: clarify wording of "The here assessed area"

We clarify to "The study area…"

71: pronounced winter season is rather vague; suggest adding more details on snow-covered season, depth of snowpack, etc. to support this statement

We rephrase to:
"...and a pronounced winter season featuring a mean annual snow height maximum at WFJ station of 2.84 m within the time period 2018-2021 and an average of 257 days of snow coverage."

77: add commas to elevations

as above.

77: add data citation reference to in situ station data

Thanks, done.

81: should this be measurements to reflect the multiple stations?

Thanks, done.

83: SNOWPACK is more than a snow cover model; suggest describing this in more detail

SNOWPACK is a snow cover model, it is a physically-based, energy and mass balance model with an arbitrary number of layers (Morin et al., 2020).

Table 1: suggest revising table to include the headers used in the caption to differentiate the different classes

Thanks, done.

Section 3.3: How was vegetation accounted for (masked) in the Sentinel-1 data? Include details here.

Based on the results in previous versions of this manuscript and the new, adjusted storyline, we did not anymore mask out forest areas (this means the full area is covered as displayed in Fig. A10).

112: clarify wording/meaning of "perform an aspect dependency analysis…."

Thanks, we rephrased to: "A digital terrain model (DTM; swissALTI3D by the Swiss Federal Office of Topography / swisstopo, 2022) with a ground sampling distance 5x5 m was used in SAR data processing and to assess potential impacts of the slope direction on the provided results. For this data was split into four aspect categories namely, N: 315–45°, E: 45–135°, S: 135–225°, W: 225–315°."

116: what is the WEkEO platform? Define and include URL

We refined to “We downloaded the SWS products (version: 2025, Copernicus land monitoring service (2025)) for the period between 1 January 2018 to 31 July 2021 via the WEkEO platform (https://wekeo.copernicus.eu/, data downloaded on 12.02.2026), the EU Copernicus reference service providing environmental data and virtual processing environments.”

117: include explanatory clause for GRD for those less familiar with radar terminology

We extended the sentence to: “The SWS dataset has a resolution of 60~x~60~m and is based on standard ESA ground range detected (GRD) imagery, *i.e.* already focussed SAR data, which was detected, multi-looked and projected onto an ellipsoidal model WGS84 (S1-documentation, 2026). “

Table 2: suggest replacing season with “year” in title

We adjusted the table to:

Table 2. Count of avalanches from DAvalMap sorted by type and year starting 1. August.

Avalanche count per year	2018–19	2019–20	2020–21
Wet snow	77	723	261
Dry snow	282	380	598
Unknown	144	36	6

137: what is gamma symbol indicating here? define meaning.

We refined to: “To create LRW composites from S1 data, we first performed radiometric terrain correction (RTC; using the terrain-flattened $\gamma 0$-convention (Small, 2011)) on the S1 acquisitions and then combined the data into LRW composites. “

146: revise sentence structure, no commas needed.

We rephrased to:

“Images with less than 75~\% of the acquired scene valid were discarded. This resulted in 444 LRW composites: 186 entailing all tracks, 29 three-track, and 6 two-track composites per polarisation. Two additional dates had only one track available therefore did not improve over RTC level.”

159: revise wording; replace cope with more technical alternative

Thanks, we replaced “cope with” with “account for”.

170: insert “shown” at IMIS stations shown in Figure 1b.

Done, thanks.

173: what is meant by considered?

We rephrased to:

“We calculated Pearson's correlation coefficient and associated RMSE values, along with Spearman's rank coefficient.”

195: Eq. rather than Equ.

Thanks, done.

Figure 2: label A and B as S1 Backscatter; suggest expanding C to make it easier to see; label D as Modeled Runoff;
Done, thanks.

Figure 2D: Is this modeled or observed HS? The caption states “measured snow height (HS) at the station as extracted from SNOWPACK data. Really unclear to me whether this is measured or modeled.
Measured, but we switched to “modelled SWE” to answer the following comment.

Figure 2D: as modeled SWE is available (presumably based on measured HS and modeled density), this might offer a more useful metric for disentangling mass loss vs densification that is inherently included in the HS timeseries
Thanks, we will switch from measured snow height to modelled SWE in the timeseries visualisations in Fig. 2, A1 and A2

Figure 2E: to confirm, this is measured TSS? I suggest cutting this to the snow covered season, as this otherwise represents ground/soil surface temperature at times.
Yes, confirmed. We improved the figure accordingly by masking out when measured snow height at station is zero.

Figure 3: how is SNOWPACK being averaged prior to assessing the correlation S1 backscatter (assuming this is LRW that is averaged over multiple days, but please specify.)
The correlation is calculated on RTC basis (in Figure column name) so no averaging was performed but instead the correlation is calculated between the RTC image and the closest available timestamp of IMIS and SNOWPACK as described in lines 174 – 176. We rephrase this paragraph to make it more clear.

Figure 3: clarify what should be included in parentheses (.) after Spearman’s
Thanks, we replaced the “.” to “I”.

228: check for consistency with abbreviations with months (sometimes Jan. sometimes spelled out fully)
Thanks, changed all to fully written out in the text.

235-236: I don’t follow this finding/statement, as no runoff is shown in Nov 2018 in Figure 2D. Please clarify.
Thanks, this seems an artefact from a previous manuscript version and we deleted the argument.

237: where is this comparison shown quantitatively? This is a clear focus of the study that needs to be better supported.
Thanks, added additional metrics (MCC, false positives, percentage covered area).

249: atypical figure referencing; suggest revising
Thanks, done.

250: delete duplicate “proportion”
Thanks, done.

253: revise “When resolving the data towards the four aspects” with something like “When parsing the data by aspect ranges”
Thanks, done.

Figure 5: wet only sections still show dry snow avalanches (yellow) – I assume this is related to uncertainty in avalanche catalogue?
Thanks, for the question. Since we don’t have additional data of the precise location, we selected the most analogous case available and assessed the snowpack characteristics as modelled by SNOWPACK on the day of the avalanche in question (13 May 2021; Davos: Bärentälli, 2 558 m a.s.l., north- and west-facing aspects, see Fig. 1). The DAvalMap catalogue entry for this event, classified as slab avalanche, carries a temporal uncertainty of ±2 days, leaving the precise release timing unknown. A minor snowfall event had deposited a dry snow layer at the surface during the night preceding the 13th (<25 cm). The underlying snowpack, however, remained in a wet state (liquid water content > 4%) also throughout the night, and the freshly deposited snow subsequently transitioned to melting conditions by late afternoon of the 13th. We therefore argue that dry snow avalanches may occur in the dry new snow while the underlying snowpack remains wet. This is consistent with the observed Sentinel- 1 signal. We address this more explicitly in the discussion section.

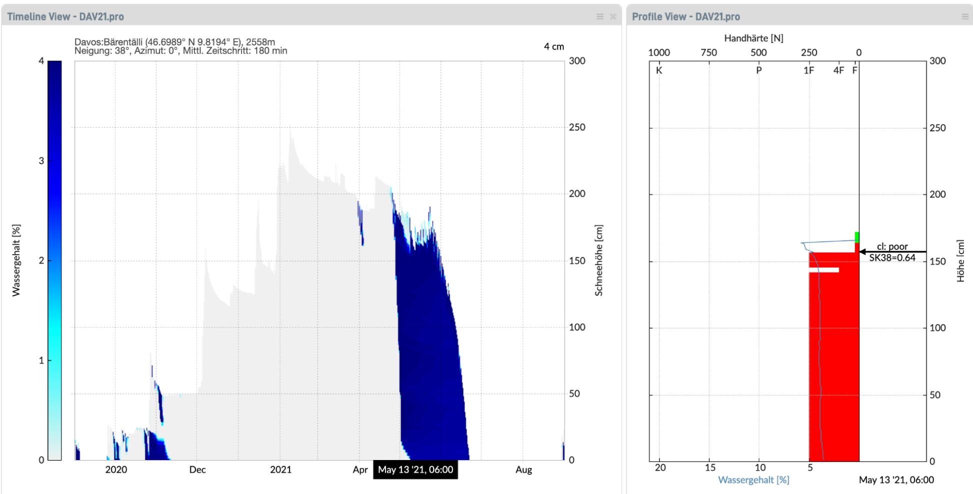

Fig. 1: Modelled snow characteristic on 13.05.2021 at the Bärentälli station at 06:00 am (by SNOWPACK, not included in paper).

Figure 5. Suggest more standard subpanel labeling here: A, B, C, D rather than two As and two Bs
Done, thanks.

269-271: Please clarify/expand the meaning of this statement regarding the use of in situ data as an indicator of snow melt.
Thanks, done.

287-289: is lag a good alternative phrasing for “a time difference”
We understand the term time-lag to indicate a slight delay in time (S1 detection after measured runoff), while time difference can also indicate a time lag in the opposite direction.

In our case this would mean: detection from S1 before measured runoff.
We would therefore keep time lag in this sentence.

293: clarify end of sentence: “governed by surface”
Thanks, done.

388: why are these avalanches the most prone to misclassification?
We made it more clear that these events are particularly susceptible to misclassification because they fit the problematic scenario outlined in the DAvalMap dataset limitations: avalanches that started as dry snow in the release zone but developed wet characteristics during descent. During short-lived wetting phases, observers document wet deposits in accessible sections, while the initiating conditions in the source zone may have remained predominantly dry, creating classification ambiguity with respect to the original release mechanism.

390: clarify sentence structure and use of commas
Thanks, done.

Figure A7: y-axis labels overlap tick labels and need to be adjusted
Thanks, done also for A8.

Figure A8: It would be valuable to use the same color scheme as Figure 4
We specifically refrained from using the same colourscale to make it visually clear that this is not the same source dataset. The colourscale steps (intensity) were kept the same between Fig. 4 and Fig. A8 but the colours themselves change to 1) highlight the difference in data source and 2) since the image is smaller, better highlight the avalanche occurrences. As a result we would argue to keep the current setup.

Figure A9: font size is very small on the figure labels and therefore not legible unless zoomed in substantially on pdf
Thanks, done.

Figure A10: I suggest revising the subplot labels to be more consistent and sequential rather than having replicate labels
Done, thanks.

Reference:

Bavay, M., Wever, N., Fierz, C., & Lehning, M. (2024). Looking back at the last 15 years of operational avalanche warning with the snowpack model in Switzerland. In K. Gisnås, P. Gauer, H. Dahle, M. Eckerstorfer, A. Mannberg, & K. Müller (Eds.), Proceedings of the international Snow Science Workshop 2024 (pp. 82-87). Norwegian Geotechnical Institute.

Detre, A., McGrath, D., Gagliano, E., Bonnell, R., Webb, R., Marshall, H. P., & Shean, D. (2025). Sentinel-1 SAR Estimates of Snowmelt Onset Coincide With SNOTEL Soil Moisture Pulses Across the Western United States. *Hydrological Processes*, *39*(12). https://doi.org/10.1002/HYP.70341