Response to Referees

Comparing high spatial and temporal resolution snow depth measurements and modelling results in an avalanche release area

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Dear editor and referees,
Thank you for your constructive and thorough feedback. This helps to improve the manuscript considerably. We copied your comments into the blue boxes and enumerated them, so we can answer each part separately below.
Yours sincerely,
Pia Ruttner & co-authors

Response to Anonymous Referee #1

GENERAL COMMENTS

> This manuscript presents an interesting study on comparing high spatial and temporal resolution snow depth measurements and modelling results in an avalanche release area. The manuscript is generally well written, and the overall structure is clear. I particularly appreciate the authors' efforts in continuous ground/near-surface observations of mountain snow and in linking these observations with a modelling approach for spatio-temporal mapping, which is especially valuable given the significant data gaps and research challenges in complex mountain environments. However, there are some issues that should be addressed before the manuscript can be considered for publication. In particular:

> R1-1: The proposed models heavily rely on the water equivalent of new snow (HNW) derived from ICON. However, the uncertainty of this dataset in alpine environments is not clearly discussed. The reported maximum correlation of 0.57 (corresponding to a coefficient of determination of about 0.3) indicates that only about 30% of the observed snow depth variability is explained by the model. This explanatory power limits their suitability for more general spatial and temporal mapping. Given that the influence of terrain parameters on snow spatial variability is already well established in the literature, the manuscript would benefit from a clearer discussion of the added value provided by the proposed modelling approach.

We acknowledge that uncertainties in ICON and in snow density affect the absolute agreement between modelled and measured snow depth changes. In the revised manuscript, we will discuss how these uncertainties propagate into the results. In our modelling framework, HNW is primarily used as a scaling factor, such that its uncertainty mainly affects the magnitude of snow depth change rather than the spatial distribution patterns.

Regarding model performance, we agree that the reported correlations indicate only a moderate explanation of the observed variability. However, this reflects the inherent complexity of snow distribution processes in alpine terrain, where multiple processes (e.g. redistribution and preferential deposition) act simultaneously and cannot be fully captured by simplified models. We will revise the manuscript to better contextualize these values with respect to previous studies.

While the influence of terrain parameters on spatial snow variability is well established in the literature, the objective of this study is to assess which low-complexity models can reliably complement spatial snow depth observations in an avalanche release area. The unique high temporal resolution of our lidar dataset allows us to evaluate model performance under different dominant conditions, including periods governed by redistribution processes and preferential deposition.

The added value of this work is therefore a well-informed assessment of the applicability and limitations of low-complexity models using high-resolution validation data. This includes (i) evaluating the transferability of models beyond their typical scale and application, (ii) quantifying their ability to reproduce spatial snow distribution patterns across different event types, and (iii) identifying the potential of low-complexity models to complement a low-cost measurement setup.

Ultimately, this provides a basis for spatio-temporally continuous (gap-filled) snow depth information and much-needed short-term predictions, for example during major snowfall events relevant for timely avalanche warning assessments.

R1-2: High-quality TLS measurements are acquired and supported by a well-documented accuracy assessment presented in the authors' previous work. Rather than using the TLS data solely for comparison, it may be more informative to directly incorporate TLS-derived snow depth differences (ΔHS) into the development and calibration of the models—particularly the statistical terrain-indicator-based models—rather than relying on ICON-derived HNW. The authors are encouraged to more clearly justify their current modelling choice and to discuss the potential advantages of using or integrating TLS-derived ΔHS in model development.

We agree that the availability of high temporal and spatial resolution snow depth maps can be exploited more. For comparison, we will integrate the use of TLS-derived ΔHS as model input. With that, we will also add to the discussion of the accuracy of precipitation input, as mentioned in the previous comment.

Originally, we did not use the TLS-derived ΔHS on purpose in this study, to keep the option of potentially using the models in a predictive context, where only model-based meteorological input is available.

SPECIFIC COMMENTS

R1-3: In the abstract, the authors refer to “three terrain-based modelling approaches". However, in the methodology, the models based on TPI and Sx are presented as terrain-based, plus a preferential deposition model from statistical snowfall downscaling PD. Please check and ensure consistency in how the modelling approaches are described and categorized across the manuscript.

We will clarify that all 3 models are terrain-based and adapt the classification and wording throughout the revised manuscript.

R1-4: The Introduction starts with an emphasis on the wind factor and later shifts to a discussion of terrain effects (around Line 45). It would improve clarity to merge these aspects more coherently earlier in the section, before introducing the measurements and modelling framework.

We will revise the structure and contents of introduction as suggested.

R1-5: In Section 2.2, three weather stations are mentioned (line 101), whereas Figure 1 shows only two. Please verify and revise the text or figure accordingly.

We notice that text and figure are misleading. All three stations used for wind estimation (B1, B2, and HFY) are shown in Figure 1, but the symbols and text are not clear. We will revise both.

R1-6: In Section 2.2 and 2.3, it lacks a description of data accuracy for the ICON products and the DSM. As the results strongly depend on the quality of these datasets, additional information on their accuracy and associated uncertainties should be provided.

We will add this and discuss the impact on the results.

R1-7: In Table 1, the upper and lower borders of the table are missing. Please update the table.

We will revise Table 1 accordingly and ensure that the formatting is complete.

R1-8: The methodology section can be challenging to follow due to the large number of equations, variables, and symbols, which also increases the chances of typos. Please carefully review all equations and ensure that all variables are clearly defined and consistently described throughout the text. For example, the summation index (j=0,. . . ,n) for α in Eqs. (4) and (5) appears to be missing. Variables in the definition of μ (Line 241) are not explained. There seem to be typos in $\mathrm{Y}_{\mathrm{dsc,j}}^{\mathrm{aspect}}$ (Line 249), and in the reference to $\mathrm{X}_{\mathrm{dsc,j}}$ which is cited as Eq. 7 (Line 262) but seems to be Eq. 8. Please verify and correct accordingly.

We will carefully revise all equations and check for comprehensiveness and consistency of all used variables.

R1-9: In Section 3.1.3, the parameter selection for TPI and Sx appears to be more closely related to the results rather than the methodology. It may therefore be more appropriate to move this section to the Results.

We will move the according paragraphs to the result section.

R1-10: In Section 3.1.4, There is a lack of explanation regarding why the scaling is required and what added value it provides. Additional explanation is needed to justify the use of scaling, and the corresponding equation for scaling should be explicitly included before Eq. 4 to make it more clear to the audience.

The scaling is required to transform the terrain-based indices Sx and TPI, which are dimensionless or expressed in angular units, into quantities representing snow depth change (ΔHS). We will expand the explanation of this step and clarify corresponding scaling equations (Eq. 4–6) in the manuscript. We prefer to keep the explanation after the introduction of the terrain parameters, as the scaling directly builds on their definition. However, we will improve the structure and clarity of the section to make this step easier to follow.

R1-11: In Section 4.3, it states 'underestimating areas with large, positive ΔHSs, and overestimating areas with small, or negative ΔHSs'. However, this is not straightforward to infer from Figure 8 alone. Adding a scatterplot of the differences between measured and modelled ΔHS as a function of ΔHS would help to better illustrate this.

We will add the requested scatter plot to better illustrate the described behaviour.

R1-12: In Figure 4, The caption does not explain what the upper and lower rows represent, which makes the figure difficult to interpret. In addition, for the TPI panel, part of the margin is cropped due to border artefacts, but there is no explanation of how much margin was removed or the relevant reference. Please check and clarify.

We will revise the figure and its caption accordingly.

R1-13: In Figure 5, please explicitly indicate whether the sub-event plots correspond to E1 or E2 to improve clarity. In addition, the caption states that ΔHS is calculated as the difference between the DSMs before and after the snowfall events and during the sub-events. Please double-check whether this is defined as after minus before or before minus after, and ensure that the description is consistent.

We will revise Figure 5 and its caption accordingly.

R1-14: Please check the usage of ΔHS versus HN throughout the manuscript, especially in the figures. In some cases, HN appears to be used interchangeably with ΔHS, while in others it is not. Please review the manuscript and ensure consistency.

We realize that the terminology adds confusion rather than clarity. We will revise the manuscript to use ΔHS throughout and include an explanation of how this relates to the amount of new snow, where needed.

R1-15: Both the abstract and the discussion mention the potential of machine learning, but the discussion remains rather brief. It would be helpful to further elaborate on how machine learning could realistically contribute to this workflow, rather than referring to it only in general terms.

We will expand the discussion on machine learning by providing concrete examples of how it could improve the current workflow.

Response to Alexander Prokop

General Comments

> R2-1: Indeed a well written, easy to understand and technically correct manuscript about the validation of two simple terrain based modeling approaches and one preferential deposition model from statistical snowfall downscaling utilizing high temporal and high resolution automated low cost LiDAR spatial snow depth data. While the terrain-based modeling approaches have been extensively validated with similar data in lower temporal resolution, the validation of the preferential deposition model is new to my knowledge. Unfortunately the results show what numerous similar studies have found in the past, the models work according to their well-known advances and limitations. Depending how well the underlying process is described by the model, the better the correlation between measured and modeled spatial snow depth data is but never really satisfying as different complex processes usually occur at the same time. Therefore the scientific value of the paper is currently a bit low, but can be improved significantly. I strongly suggest same as reviewer 1 to incorporate TLS derived snow depth differences (ΔHS) into the development and calibration of the models. While the spatial patterns of snow accumulation in mountainous terrain can be described to a certain extend the amount of snow that is accumulated is usually not represented in a satisfying manner. There is great potential in using the measured snow depth data in improving the results of the presented models as it was done in the past e.g. using snow-particle-counter data. In this way the advantages of the automated LiDAR measurements fully apply as the high temporal resolution of spatial snow depth data allows to determine how much snow was actually eroded and accumulated by the different processes e.g. saltation, suspension, preferential deposition. Furthermore the chosen model can be then used for a greater area, not just to fill data gaps, as the results will be much closer to reality than using the water equivalent of new snow (HNW) derived from ICON.

We agree that the current results largely confirm strengths and limitations of some indicators and models tested in previous studies. However, the novelty of this study lies in the use of high temporal resolution lidar observations and the focus on avalanche release areas, including measurements during active snowfall periods, which enables a process-oriented analysis at finer temporal scales. Following this comment (and Reviewer 1) to stress the novelty of our research, we will extend the analysis in the revised manuscript by incorporating TLS-derived snow depth changes (ΔHS) into the modelling framework. This allows us to reduce one source of uncertainty related to snowfall input and to better isolate and analyse the processes leading to snow depth variability. We will revise the manuscript to highlight this aspect more clearly and to discuss how incorporating measured snow depth changes can improve the model evaluation.

Specific Comments

> R2-2: 40 The first that published the use of low-cost LiDARs to measure spatial snow depth was Kapper et al. (2023), please cite accordingly

In the referenced line, we specifically refer to permanently installed low-cost LiDAR systems for snow depth monitoring. The study by Kapper et al. (2023) uses a mobile low-cost lidar system in the context of avalanche detection validation. To address this comment, we will revise the manuscript by adding a separate sentence introducing low-cost lidar applications for snow measurements more broadly, including the suggested reference.

> R2-3: 70 In this paragraph it would be good to lead to incorporating measured snow depth data in the modeling approach as Schön et al. (2018) did using blowing snow fluxes or Prokop and Procter (2016) did using LIDAR derived spatial snow depth data. Please also cite accordingly

We will extend the paragraph to acknowledge more previous work using lidar derived spatial snow depth data for model development, validation and practical applications. In particular, we will include the references to Schön et al. (2018) and Prokop and Procter (2016).

> R2-4: 90-110 It is not clearly indicated what data is used for what model as input. E.g. all studies so far used wind direction data from on site automated weather stations (or very close by stations) for the Sx model, as those studies found much better results than using data from numerical weather prediction models, as wind direction is often not represented well in a 1 km grid. I guess you use such numerical weather prediction model

data as input for the preferential deposition model, as it makes more sense there. Please clarify, discuss and justify why you used which input data for what model.

We will clarify in the revised manuscript that ICON numerical weather prediction data were used consistently as input for all models. This choice was made intentionally to ensure comparability between modelling approaches. We further include a comparison of model results using different wind inputs, which allows us to assess the sensitivity of the models to wind input data. We will revise the manuscript to more clearly emphasize this analysis.

R2-5: 117 2 times „the“, reduce to 1

We will correct the typo.

R2-6: 165 and so on: As reviewer 1 already indicated it is not clear why those model approaches are selected. While TPI and SX are somewhat similar and described as terrain based modeling approaches the PD from statistical snowfall downscaling intends to model a different process (preferential deposition) and is intended and made for much lower resolution grids. It’s nice to see that a model for preferential deposition also works best for a preferential deposition event (E3) and e.g. Sx describes better a snow redistribution event (E2), but that should have been clear to begin with and is found in literature. Please clarify your choice and discuss in detail what the benefit from this choice/study is.

Here it would be also good to let the reader know, what search distances you used calculating Sx, usually small search distances are able to represent snow redistribution in particular around small terrain features, while longer search distances are usually better suited to model preferential deposition or blowing snow (suspension)

We will clarify the motivation for selecting these model approaches in the revised manuscript (see aims outlined in R1-1). In practice, users are often faced with choosing among different models without clear guidance on their applicability outside idealized conditions. This study aims to address this gap by providing a comparative assessment at high spatial and temporal resolution.

Regarding Sx parametrization we will clarify that we tested multiple radii and maximum search distances and selected $R = 10$ m for the TPI and $D = 8$ m for the Sx model, which yielded the highest average correlations across all events. As suggested by Reviewer 1 (see R1-9), we will move these results to the Results section and describe them more clearly.

R2-7: 320: usually automated wind measuring stations for avalanche forecasting locally (slope scale) are located at ridges to determine from what wind direction snow is blown into a slope, calculating e.g Sx those locations usually also work best. Flat field stations for meteorology are usually not able to represent local wind fields in mountainous terrain, is perhaps this discussion going their? Of course the location of such automated weather stations is dependent on application of the data and has to be carefully chosen.

We agree with the reviewer that ridge-based wind measurements often provide the most suitable data for slope-scale applications such as avalanche forecasting. However, we intentionally relied on numerical weather prediction data for several reasons:

- Measurement stations are often unavailable in close proximity to the area of interest, particularly outside well-instrumented regions.
- Even when available, station placement introduces representativeness challenges (e.g., exposure, elevation, terrain complexity).
- Wind measurements are prone to data gaps or errors (e.g., icing) during harsh weather conditions, which are particularly relevant for snow redistribution processes.
- Numerical weather prediction data provide spatially continuous input, enabling applications in remote, non-instrumented areas.

We will revise the discussion to better articulate these trade-offs and the reasons behind our approach.

R2-8: 330: Numerous studies have shown that the underlying DSM of surfaces with or without snow or different stages of the snow-pack have an impact using terrain based model approaches if a terrain feature is snowed in or not or to what extend as long as the terrain feature is represented in the DSM resolution and model settings are also matching (e.g. search distance for Sx). For a preferential deposition model the choice of the DSM is rather negligible as only large terrain features that are never fully covered by snow are represented in the resolution of the DSM used for the calculation. The discussion here seems a bit unspecific, please specify more and explain why the results show no difference in model performance.

We will clarify that in our implementation, the preferential deposition model is applied at the same spatial resolution as the TPI and Sx models, such that small-scale terrain features represented in the DSM also influence the results. The limited differences observed between snow-on and snow-off DSMs in our results are likely due to several factors:

- Large-scale terrain features that dominate the model behaviour remain largely unchanged between the used DSMs. In our case the snow cover mainly has a smoothing effect on the terrain, leading to similar effective surface representations.
- The snow-covered surface itself is dynamic and evolves during events, depending on the availability of erodible snow.
- Our modelling approach integrates multiple time steps. Although lidar data are available, their spatial coverage is limited and varies between time steps, particularly during snowfall periods. A consistent, temporally evolving DSM covering the full study area is therefore not available, which limits the feasibility of dynamically updating the surface representation within the current framework.

We will revise the discussion to better explain these aspects and to clarify why the expected differences are small in our case.

References

Kapper, K. L., Goelles, T., Muckenhuber, S., Trügler, A., Abermann, J., Schlager, B., Gaisberger, C., Eckerstorfer, M., Grahn, J., Malnes, E., Prokop, A., and Schöner, W.: Automated snow avalanche monitoring for Austria: State of the art and roadmap for future work, Frontiers in Remote Sensing, 4, URL `https://www.frontiersin.org/articles/10.3389/frsen.2023.1156519`, 2023.

Prokop, A. and Procter, E. S.: A new methodology for planning snow drift fences in alpine terrain, Cold Regions Science and Technology, 132, 33–43, doi: 10.1016/j.coldregions.2016.09.010, 2016.

Schön, P., Naaim-Bouvet, F., Vionnet, V., and Prokop, A.: Merging a terrain-based parameter with blowing snow fluxes for assessing snow redistribution in alpine terrain, Cold Regions Science and Technology, 155, 161–173, doi: 10.1016/j.coldregions.2018.08.002, 2018.