Dear Editor,

we would like to thank the three Reviewers for the reviews and suggestions that helped us improving our manuscript.

We have addressed all the points highlighted by the reviewers and we modified the manuscript accordingly. In particular:

- we better framed this work in the context of ice/firn core studies, clarifying that the results and their relevance are specific for this and other similar drilling sites of the European alps
- we modified the text integrating all the missing information, details and discussion, as highlighted by the Reviewers and detailed in the reply to the comments below
- we better motivated our approach regarding model calibration and validation, and temperature sensitivity analysis
- we reduced from two to one the calibration factors, constraining one of them (the precipitation factor) by means of independent meteorological observations
- we added a figure presenting the entire meteorological dataset (new Figure 3), a figure showing the time evolution of modelled snow depth (highlighting snow layering and accumulation/erosion, (new Figure 4), and a two-dimensional binned plot that shows the relationship between air temperature, wind speed and erosion frequency (new Figure 10)
- we modified figures 2, 3, 4, 7, and 8, based on Reviewers' suggestions (new figures 2, 6, 8 and 9)
- we removed figures 5, 9, 11 and replaced Table 2 with the new figure 5 that visually shows the same results, improving the readability
- We have checked the paper thoroughly; in particular, we have harmonized the verb tenses

In the following, we answer in detail to the specific comments made by the reviewers. The author responses are reported in blue colour right below the reviewers' comments. Line and page numbers indicated by the reviewers are referred to the submitted paper, whereas line numbers indicated in the authors' replies are referred to the revised paper (clean, non-track change version).

Reviewer 1 (anonymous)

Comment 1.1 - General comment

The authors investigate the snow cover at one AWS location at Mt. Ortler (on the glacier) for the 4 year period 2011-2015 using the SNOWPACK model. Main focus of the study is the assessment of wind erosion of snow depending of snow conditions and potential future changes. While the general topic is very interesting and the approach taken understandable, several aspect remain unclear to me. In the following, a list of main and specific comments.

Main comments

Regime shift

In my understanding, the described shift from an erosion- to a melt-dominated regime at Mt Ortler is not a new phenomenon, but is happening since the accumulation areas of the glaciers are decreasing and the glaciers are retreating. This shift seems to now also have arrived at the investigated site fairly high up close to the peak. In case I understand this correctly, why is it important that this regime shift also has arrived at the investigated site?

Because the investigated site can be considered as representative of high-elevation accumulation areas of alpine glaciers, and this shift in regime is an important feedback that affect mass balance dynamics of these *areas that are still poorly investigated and whose dynamics are still difficult to model. In addition, this regime shift affects the formation and preservation of ice core archives, so the current and past similar shifts are of high relevance for the interpretation of these archives. The text has been edited to better explain this point in the abstract (line 15-17) and at the end of the Introduction (line 91-93), where the specific aims of the study are reported.*

Comment 1.2 - Ice core, paleo-climatic reconstructions

Reading the manuscript, it seemed that the authors are very closely linked to the ice core community. A lot of parts of the manuscript are designated to the description of ice cores and glacier mass balances in general. Especially the introduction focuses a lot on theses topics. However, in my opinion, this study is not about ice cores or paleo-climate investigations and also not about glacier mass balances. Snow simulation at one point are conducted, which in this case was on a glacier. I think that similar investigations on snow erosion also could have been conducted at a non-glacieted site. The focus of the introduction should be more shifted to snow modelling and the modelling of snow redistribution processes, I think.

Our focus is on snow redistribution processes at highest elevations on alpine glaciers, and as we write the main motivation of this study was to better understand how snow redistribution by wind regulates glacier mass balance in response to air temperature fluctuations, because it is related to the the formation and preservation of ice core archives and their paleoclimatic interpretation. The introduction is therefore focused on these topics. We agree that a similar study could have been carried out at lower elavation and at non-glacierized site, however our interest was for high elevation areas where ice cores are retrieved. Obviously, the results are of broader interest, not only for the glaciological and/ore the ice core communities. We have clarified these points at the end of the Conclusions section (line 592-594).

Comment 1.3 - New insights, innovative aspect of study

The authors state that already in the 80s and 90s it was reported that temperature regulates the susceptibility of the snowpack to wind erosion. As far as I know, following this type of knowledge snow erosion processes where implemented into SNOWPACK. So the model includes a temperature-dependence of wind erosion of snow. So when your results point out that dry snow erodes more than wet snow, it is because that it is defined like this in the model.

The modelling approach is not empirical or conceptual. Instead, SNOWPACK is a physics based model that accounts for all processes and meteorological variables (including air temperature) that affect the metamorphism of snow and its susceptibility to be transported by wind. The innovative aspect, which is now better highlighted (in the Conclusions section, line 560-563), is the quantification of the snow erosion process and its partitioning due to air temperature fluctuations at an high-elevation glacier accumulation area.

Comment 1.4 - Please point out what are the new insight from your study. To me the most interesting aspect is the quantification of the negative temperature feedback on the snow erosion (more melt due to higher temperatures, but less loss due to erosion). This aspect is a bit lost in all the other information, I think. Maybe it is possible to more focus on this aspect?

Yes, thank you for highlighting this aspect. It already mentioned in the Abstract, Discussion and in the Conclusion sections; we have better emphasized this aspect and the research gap in the Introduction (line 82-86).

Comment 1.5 - Precipitation correction, model adjustment

It is not entirely clear to me what correction and adjustment steps with regard to the precipitation were taken. As you take precipitation from a valley station nearby (~1500 m below the site) you first correct for undercatch (according to Kochendorfer?), then adjust lapse-rate based and then multiply everything with a constant factor, right? Please clarify.

As reported in Section 3.1, line 149 (in the submitted manuscript), we corrected precipitation data measured at the Solda village following the procedure described in Carturan et al. (2012). The corrected precipitation is then multiplied by a factor that account for the vertical gradient in precipitation, no other multiplication/correction is applied (please see Section 3.3, lines 196-197 in the submitted manuscript). Clarified in the text (Section 3.3, line 224-226 in the revised manuscript).

Comment 1.6 - Please also justify why you selected the constant correction factors for precipitation and wind to calibrate your model. Please also discuss the issue of equifinality. Are there other combination of the two correction parameters that also result in good results?

Constant correction factors were the best choice considering the time resolution of available mass balance observations for parameter calibration. The alternative would have been to calculate a seasonality of these two factors, which would have required higher time resolution field observations of snow water equivalent (not feasible at this study site). We agree with the reviewer on the possible issue of equifinality, and for this reason we constrained one of the two factors (the precipitation factor) using lapse rates calculated for this geographic areas with independent meteorological and snow observations. For this reason, only the wind multiplicative factor was adjusted iteratively to minimize the Root Mean Square Error (RMSE) between measured and modelled mass balance at the simulation site. Clarified in the text in Section 3.3 (line 226-230).

Comment 1.7 - If I understand correctly, you correct the measured wind speed with a factor of 0.7. Why should the at the site measured wind speed constantly have been too high so you have to correct them with this factor?

The adjustment factor for the mean wind speed is a calibration factor to account for the specific setting in the close-ridge complex terrain of the measurement station. While the exact choice of this factor has been determined by calibration, we can physically justify a wind speed reduction of the measured values. Consistent with knowledge on speed-up effects over ridges (Raderschall et al., 2008), the sensor at the mast was exposed to a higher relative effect of speed-up than what occurred on the snow surface. Literature reports speed-up ranging between 30-100% in these settings at the elevation of the wind sensors, and a much steeper wind gradient from this elevation to the surface compared to the logarithmic wind profile (Taylor et al., 1987; Pellegrini and Bodstein, 2004). Reference and details have been added in the Section 5.1 (line 439-444) to discuss and support our approach.

Lemelin, D. R., Surry, D., & Davenport, A. G. (1988). Simple approximations for wind speed-up over hills. Journal of Wind Engineering and Industrial Aerodynamics, 28(1-3), 117-127.

Pellegrini, C. C., & Bodstein, G. C. R. (2004). The height of maximum speed-up in the atmospheric boundary layer flow over low hills. Journal of the Brazilian Society of Mechanical Sciences and Engineering, 26, 249-259.

Raderschall, N., M. Lehning, and C. Schär (2008), Fine-scale modeling of the boundary layer wind field over steep topography, Water Resour. Res., 44, W09425.

Taylor P.A., Mason P.J., Bradley E.F., 1987. Boundary-layer flow over low hills. Bound.-Layer Meteorol. 39:107–32.

Comment 1.8 - Optimization of a highly complex snow model as SNOWPACK only based on six snow depth measurements between 06/2012 and 09/2014 seems questionable to me. Can it be that you adjust for the wrong reasons and compensate for errors, particularly when only adjusting two constant multiplicative factors. Please consider to show precipitation and wind measurement before and after correction and if possible also show the full four years simulated including the time steps with observed values using for model optimization.

The model was calibrated using available measurements of snow water equivalent and seasonal mass balance, which are generally feasible twice per year, at the beginning and at the end of the ablation season, at this and other similar remote sites. The available high-time-resolution AWS data (snow depth measurements) could not be used for parameter calibration without converting them into water equivalent, which would have required the separate estimation of the unknown snow density, introducing additional uncertainty. Discussion on this point are reported in the manuscript at the beginning of Section 5.1. As in the reply to previous comments, there is now only one calibration factor. A figure was added to show the full meteorological input of the model, new Figure 3. In this figure we did not add the corrected precipitation and wind speed data because it would compromise the readability of respective data series. In addition, because the multiplicative factors are constant in time, the corrected data series would be parallel to the original data series, adding little overal information in our opinion. In agreement with comments by this and other reviewers (especially comment 3.6), we replaced Table 2 with a new figure, new figure 5.

Comment 1.9 - Please also consider to show results of modeled snow layering. Due to your modeling approach you have very detailed insights and the exemplary presentation of an erosion event could be very insightful.

Thanks to this comment, in the new figure 4 we now show the modeled snow layering and an example for an erosion event in detail. A description of what is show by this figure was added in the text at the beginning of Section 4 (line 270-278).

Comment 1.10 - Selection of site

You only have very little data (four years of temperature and wind from one site more than ten years ago). Please justify better why you select this site and data to investigate temperature-dependence of snow erosion. Aren't there better sites and data sets to investigate this aspect? I think that a continuous time series of SD and/or SWE would be very beneficial for the evaluation of the model results.

In our view (and also according to Reviewer 3, comment 3.1), a 4-year continuous dataset of 15-minute nivo-meteorological observations from this remote site is a remarkably long dataset, and is quite rare. The objective was to investigate and quantify the temperature-dependence of snow erosion at this and similar sites in the Alps, because the characteristics of this site are representative of other ice/firn drilling sites. Improved knowledge and quantification of snow erosion by wind was missing and is essential for the dating and interpretation of ice core paleoclimatic archives. We have better clarified the motivation and the speficic aim of this study in the Introduction (line 82-87 and 92-93) Please see also the replies to comments 1.1 and 1.2. Limitations related to the relatively short period were already discussed in Section 5.1 (line 395-399 of the submitted manuscript).

Comment 1.11 - Warming/Cooling

The authors choose a very simple a basic approach to assess the impact of temperature changes by simply adding/subtracting 1/2/3 °C to/from the temperature time series. This results in physically in-consistent model input and I am not sure this is a good idea when modelling the show cover with a physically-based snow model. To me the combination of the very simple temperature change approach for only four years of data and the SNOWPACK model needs to be justified better. I am not sure if the robustness of the provided numbers can be guaranteed. I am not sure if the results from 4 years can be generalized considering the complexity of the area. As long-term historical and future climate data is available, I find it difficult to understand why no such data has been used. Please argue why the selection of this simple approach is justified.

Using a scenario of changed temperature is a simplification of future warmer climates but represents the main effect as climate models predict small changes in other parameters such as precipitation or radiation (Kotlarski et al., 2023). The main additional effect is through increased longwave downwelling radiation, which is captured in our simulations as this forcing is parameterised in SNOWPACK (Schmucki et al., 2015). Relative humidity can be assumed unchanged as suggested by secular meteorological observations at high elevation in the Alps, which show the absence of clear trends (Brunetti et al., 2009). In addition, a minor (2-4%) decrease in relative humidity is indicated by future projections at the end of the 21th century in the European Alps (Gobiet et al., 2014), that are highly uncertain due to the relationship between the relative humidity and precipitation, and to the high uncertainty in future trends of precipitation in the study area (Kotlarski et al., 2023; Pepin et al., 2025). A small increase in winter precipitation is predicted by most (but not all) climate models and could have been implemented, but would not change the conclusions of our analysis. Discussion and references were added in the text (Section 5.1, line 470-478).

Brunetti, M., Lentini, G., Maugeri, M., Nanni, T., Auer, I., Bohm, R., & Schoner, W. (2009). Climate variability and change in the Greater Alpine Region over the last two centuries based on multi-variable analysis. International Journal of Climatology, 29(15), 2197-2225.

Gobiet, A., Kotlarski, S., Beniston, M., Heinrich, G., Rajczak, J., & Stoffel, M. (2014). 21st century climate change in the European Alps—A review. Science of the total environment, 493, 1138-1151.

Kotlarski, S., Gobiet, A., Morin, S., Olefs, M., Rajczak, J., & Samacoïts, R. (2023). 21st Century alpine climate change. Climate Dynamics, 60(1), 65-86.; doi: 10.1007/s00382-022-06303-3

Pepin, N., Apple, M., Knowles, J., Terzago, S., Arnone, E., Hänchen, L., ... & Zardi, D. (2025). Elevation-dependent climate change in mountain environments. Nature Reviews Earth & Environment, 1-17.

Schmucki, E., Marty, C., Fierz, C., & Lehning, M. (2015). Simulations of 21st century snow response to climate change in Switzerland from a set of RCMs. International journal of climatology, 35(11); doi: 10.1002/joc.4205.

Specific comments

Comment 1.12 - Line 15 'unique dataset': As far as I know, a climate station on a glacier is not unique. There are other sites with longer measurements and more measured variables. Why are measurements of snow depth once per year useful to investigate snow erosion? Wouldn't continuous measurements of SD and SWE be required?

This sentence was reworded and 'unique' was replaced by 'rare' (line 17), considering the location and the physical characteristics of the study site. Snow depth was measured continuously over four years, but could not be used to investigate snow erosion because the snow density is known only for the dates of the field visits, at the beginning and at the endd of the ablation season. The snow erosion was investigated obtaining a continuous SWE series by means of the SNOWPACK model. We have specified this in the revised manuscript (Section 3.3, line 185-188).

Comment 1.13 - Line 33-34: I am not sure I understand why 'elevation' represents a positive feedback mechanisms? How is snow accumulation due to avalanches a negative feedback mechanisms? Maybe the wording 'feedback' is a bit misleading here. High elevations, topographic shadowing and accumulation of snow due to avalanches favor the formation of a glacier, but I would not call it feedback. Maybe it is possible to re-phrase these sentences to avoid misunderstandings.

These are well-known feedbacks during deglaciation (described in the studies already referenced in this part of the manuscript):

i) elevation feedback is a positive feedback because during deglaciation glaciers loose mass and their surface undergoes lowering, that can exceed tens or hundreds of metres. The result is a progressive lowering ot the glaciers' mean elevation, which causes additional warming due to the vertical temperature laspe rate

ii) avalanche feedback: shrinking avalanche-fed glaciers benefit from an increse of the accumulation area ratio because they lose part of the ablation area, while the avalanche deposition area keeps unchanged. This causes an increase in the accumulation area ratio

iii) topographic shadowing: similar to avalanche, shrinking glaciers that benefit from shadowing tend to lose the portions that are less shaded by terrain, and this results in a lowering of the mean (clear-sky) shortware radiation received by the glacier

Comment 1.14 - Fig 1: From what year is the glacier extent? Glaciers seem still fairly large compared to the current state. Please consider to update to a current state.

The glacier extent is from the 2017 outlines published by the Province of Bolzano. This is now specified in the caption of figure 1.

Comment 1.15 - Fig 1: The map takes the entire page. Please consider to optimize the figure size, e.g. by including panel b into a.

We prefer to keep Fig. 1 as is if possible, for a clearer view of the study site.

Comment 1.16 - Fig 2: As it is only 4 years of data, please consider to show the entire time series of the measured data (not monthly values only). Also consider to mark the periods which were gap-filled. Please also clearly show in the figure that precipitation is coming from another location.

We have modified Figure 2 based on this suggestion and indications of Reviewer 2 (comment 2.1) and added a new figure (Figure 3) that shows the entire nivo-meteorological record, specifying that precipitation comes from another location.

Comment 1.17 - Line 188: I am not sure where all the other meteorological variables used to force SNOWPACK at the selected site were coming from. Please clarify where e.g. humidity and the radiation components where measured.

They were all measured at the automatic weather station installed at the study site, as detailed in Section 3.1 (line 124-131 of the submitted paper), with the only exception of precipitation (line 139-145 of the submitted paper). A sentence was added to clarify this point in Section 3.3, lines 214-216.

Comment 1.18 - Section 3.1: I think that the description of the AWS site fits better to the data description. Please consider to move.

In our understanding, the Reviewer suggests to move the description of the AWS site from the section 2 to the section 3.1. In our opinion the description of the AWS site should be part of the description of the study area, so we prefer keeping it in section 2 if possible.

Comment 1.19 - Line 219: Why not using liquid water content directly to assess if the snow was wet or dry?

The aim was to separate snow that has already experienced surface wetting conditions from snow that has not experienced wetting conditions. The liquid water content of the surface layer can be zero in case of surface wetting followed by refreezing, so in that case the surface layer would be considered dry, even if it has already experienced wetting (and the deriving metamorphism that affect snow redistribution). We motivate better our choice in the revised manuscript, Section 3.4 (line 243-247). For improved clarity, we replaced 'wet snow' with 'wetted snow' in the entire manuscript.

Comment 1.20 - Fig 3: I am not sure what to think about the fact that a very low correlation of 0.09 is considers highly significant. Is it possible to plot the values, e.g. for SWE_eroded and T_Ereosion, so the reader gets a better visual impression of the correlation?

The high significance (low p-value) derives from the high sample size (this is now reported in the text, Section 4.2.1), however in the text we mention these low correlations between WE_Eroded and T_Th, T_Erosion, T_Life as non meaningful (line 257 of the submitted paper). We think that adding a scatterplot only for these two variables does not add much to what the correlation coefficient already shows, also considering that we have now increased the number of correlation matrices in agreement with the suggestions of Reviewer 2 (comment 2.13). This part was edited to better clarify how we interpreted these results and to describe the results of the two additional correlation matrices that were added in the new figure 6 (Section 4.2.1, specifically line 308-213 for what regards this comment).

Comment 1.21 - Fig. 7 and 8: Figures take a lot of space and it is all white on the right side. Please try to optimize the figure quality.

These figures are concieved to occupy a single column, so there should not be empty space on the right side. These figures have been modified as suggested by reviewer 2, and are now numbered as Figs. 8 and 9. The related text was modified according to the new evidence obtained by the improved figures (Section 4.2.2, line 349-364).

Comment 1.22 - Line 359: Which snow depth sensors?

The snow depth sensor was a Campbell Scientific SR50A, as reported in Section 3.1, line 130-131 of the submitted manuscript. We installed two sensors in order to have a backup. Clarified in the text. In addition we inserted a new table reporting all sensors' characteristics (new Table 1) as recommended by Reviewer 3, comment 3.8.

Reviewer 2 - Luis Durán

Comment 2.1 - Figure 2. I recommend restructuring this figure into three separate panels.

First, a combined plot displaying temperature and relative humidity, using a dual-axis format, provided that relative humidity data are available.

Second, a precipitation plot using stacked bars, incorporating a distinct color scheme to indicate precipitation events occurring when the mean temperature is below 0 °C.

Third, I suggest including a wind rose diagram, divided into 12 directional sectors and approximately four wind-speed bins, to provide a clearer representation of wind patterns.

We have improved this figure making the adjustments suggested by the Reviewer.

Comment 2.2 - Line 124. Air temperature and relative humidity are known to be strongly anticorrelated, and analyses based on air temperature are therefore often assumed to implicitly account for humidity effects. However, in the present context relative humidity may exert an independent control on mass balance and snow dynamics, particularly through its influence on sublimation and ablation processes. It is therefore not immediately clear why relative humidity was not included in the analysis. If its influence is negligible compared to that of temperature and wind, this could be explicitly mentioned.

Thanks for highlighting this point, in the revised manuscript we added monthly mean relative humidity in Figure 2. We also added the mass fluxes related to humidity effects (new Table 3) and comment the results in the text (Section 4.3, line 385-386 and 398-400). Please see also the reply to comment 1.11.

Comment 2.3 - line 195. States: "The snow accumulation was simulated using the Solda precipitation, multiplied by a correction factor to account for the increase of precipitation with elevation (vertical precipitation lapse rate).". Could you please give some basic information or a reference about this method?

Please see the reply to comments 1.5, 1.6, 3.4. We have edited the text in order to clarify what we did, adding references in support of the existence of the orographic increase of precipitation with elevation (Sections 3.3 line 223-226, and Section 5.1 line 436-438).

Comment 2.4 - line 200. When you state that "the multiplicative factors for precipitation and wind speed were adjusted iteratively to minimize the RMSE between measured and modelled mass balance at the simulation site", you are implicitly assuming that these calibration factors do not compensate for other structural deficiencies or errors in the snowpack model itself. Could you clarify whether this assumption has been evaluated? In other words, how do you ensure that the tuning of precipitation and wind-speed multipliers is not merely correcting for biases arising from other processes (e.g., snow densification, redistribution, sublimation, melt parametrization, or energy-balance components) rather than improving the physical representation of precipitation and wind inputs?

We have modified this part (Section 3.3, line 225-226) and clarified that the multiplicative factor for precipitation was derived from independent observations in this geographic area (see reply to comment 3.4). The only calibration parameter is therefore the wind factor, whose physical meaning and requirement is now better explained in the text in Section 5.1 (line 439-444). Please see also the reply to comments 1.6 and 1.7.

Comment 2.5 - line 207 Maybe this was mentioned or implicitly assumed, but what is the treatment of bidirectional sublimation in the model? Is the contribution of sublimation—both mass loss (sublimation) and mass gain (deposition/subsublimation)—considered negligible compared with other processes, or is there a reason why it cannot be distinguished from wind-driven snow erosion in your results? In other words, how do you ensure that sublimation/deposition is not being misattributed to wind erosion in the mass-balance interpretation? A basic clarification might be needed here.

The mass flux due to wind erosion can be clearly distinguished from mass fluxes due to sublimation, because they are separated outputs of the SNOWPACK model. We have added this information in the text (Section 3.5 line 267, and Section 4.3, line 385-386 and 398-400) and also quantified and added the related mass fluxes in Table 3.

Comment 2.6 - Line 318. If I understand this figure correctly, this represents the frequency of each threshold wind speed for dry and wet snow. It seems hard to interpret since higher winds are less frequent than lower winds, and there might be a high correlation between high speeds and lower temperatures. It is hard for me to interpret the peak at 9 m/s for dry snow mentioned in Line 312. However, this peak is largely influenced by the higher occurrence of moderate wind speeds in the dataset, rather than indicating an intrinsic preference of the event for this velocity. To better isolate the physical relationship between wind forcing and event occurrence, the event frequency should be normalized by the wind speed distribution. Figure 7 partially solves this issue. A more informative representation is therefore the conditional probability of the event given wind speed, which quantifies the likelihood of the event occurring for a given wind regime and removes the bias introduced by the uneven frequency of wind speeds. Maybe something in this direction should be done or a proper explanation in the text.

We have removed this figure (and the related text) in the revised version of the manuscript, considering also observations from Reviewer 3.

Comment 2.7 - Following this line of reasoning, an additional informative visualization would be a two-dimensional scatter (or binned) plot using wind speed and air temperature as the horizontal and vertical axes, respectively, with the size (or color intensity) of each marker representing the frequency of wind erosion events. This representation allows the combined influence of thermal and dynamical conditions to be assessed simultaneously, highlighting preferential regimes where wind erosion is most likely to occur and revealing potential interactions between temperature and wind forcing that are not evident in one-dimensional distributions.

We have added this figure (new figure 10) and described the additional insight it provides (line 375-377), thanks for the suggestion.

Comment 2.8 - Line 363. I understand that Table 2 is in some way examining the performance of SNOWPACK during this period. The analysis seems to assume that the model reproduced the snowpack accurately, yet I wonder if a higher frequency validation could be shown to support this assumption apart from Table 2.

The model can only be validated using field observations of SWE and mass balance, which were carried out at the beginning and at the end of the ablation season. Considering the remoteness of the study area (that can be reached only using helicopters) and the resources available, it was not feasible to organize field visits with higher frequency. Please see also the reply to comment 1.8 for considerations regarding the (high frequency) snow depth series for model calibration and validation. Discussion on this point is reported in the manuscript at the beginning of Section 5.1.

Comment 2.9 - Line 359. This paragraph provides a very clear explanation of the study's scope and limitations. These ideas might have been helpful at the beginning of the paper. I may be mistaken, and they may already appear in the introduction, but they are particularly clear here.

We have detailed the objective of this study at the end of the introduction (improved in the revised version) and now we have added a short sentence describing what the EISMODEL was used for at the beginning of Section 3.3. Here we shortly recall what was done and mainly describe the limitations of our approach. In our opinion this part fits better here.

Comment 2.10 - This may be a minor point, but the figures have a spreadsheet-like appearance that could be distracting to the reader, especially Figure 13.

We have improved figure appearance thanks to the suggestions of the Reviewers, including those that have a spreadsheet-like appearance (e.g. Figure 2). In our opinion, Figure 13 is simple and clear, but we are available to further improve it based on specific suggestions.

Comment 2.11 - I have some particular comments to add to my previous list. I hope they are helpful.

Fig 2: Maybe too simple for the information available. More information could be included especially about the year to year variability. For example max and min values could be shown with a shaded area or swarmplots could be employed. Also other variables such as radiation and relative humidity were recorded by the AWS and used within the model but are not shown here.

Based also on suggestions from the Reviewer 1 (comment 1.8), we have added a figure displaying the full meteorological dataset, covering the entire period of observation. This is the new Figure 3.

Comment 2.12 - Section 4.1 and Table 2: It seems that the model has some problems reproducing the measured SWE especially during summers. This is later explained in the Discussion but maybe a sentence could be included here also. As of now one can see this discrepancy in the Table but no mention is found within the text until the Discussion.

We have added a comment regarding the different behaviour in summer and winter balance simulations in Section 4.1 (line 288-290), as suggested.

Comment 2.13 - Figures 3 and 4: here no distinction is made between wet and dry snow erosion whereas later important differences are shown. Could it not be misleading to calculate these correlations without making the distintition.

In the revised manuscript we split the correlation matrices making the suggested distinction between wet and dry snow (new figure 6). Results are reported in the revised text (Section 4.2.1, line 300-323).

Comment 2.14 - Lines 271 and 272: Maybe a better description of the statistical goodness of these fits is necessary. Also it is said that "the dry snow fit lies significantly above" but no indication of this significance is made in terms of statistical tests.

We have removed Figure 5 in the revised version of the manuscript, because of the large scatter and considering also remarks from Reviewer 3 (comment 3.12).

Comment 2.15 - Line 292: "dry snow erosion accounts for 91% of the total modelled erosion" Could you please provide information about how much time you have wet or dry snow conditions? Right now it is unclear whether this difference is just because of erosion being less likely to occur with wet snow or also because dry snow conditions are significantly more common in your studied period and site than wet snow conditions. Line 297: same as the previous comment. It would be useful to know how much of this could be because of dry snow conditions being far more common than wet in your site.

We fully agree with this remark and added this information in the revised manuscript, as suggested (Section 4.2.2, line 349-355). Please see also the reply to the two following comments 2.16 and 2.17.

Comment 2.16 - Fig 7: again this Figure may be affected by the different amounts of wet vs dry snow conditions in your datatset. Maybe you could include a version where the values are normalized by the number of hours with wet or dry snow conditions in each case?

We have added this figure in the revised manuscript and discussed the results in the revised text. This is now Figure 8. Figure 8b reports the normalized frequency as suggested, and it actually helped us highlighting how the normalized frequency of erosion increases with increasing wind speed (not visible with former analyses) and confirming that dry snow is eroded with higher frequency (about three times higher, on average) compared to wetted snow. Reported in the text, line 349-355

Comment 2.17 - Fig 8: in a similar manner as previous comments maybe this Figure would be easier to interpret if you represente it by stacked area instead of lines. That way it would be easier to interpret the sum of wet and dry cases as a general case distribution.

We edited this figure as suggested in this comment, thanks for the suggestion that significantly improves the readability of the figure. This is now a stacked-area chart, new Figure 9.

Comment 2.18 - Fig 11: This Figure could be made much smaller without hindering its readability. Its data is also already displayed within Table 3 (except the surviving snow percent that could be easily added as a new line at the end). Maybe it would be better to reduce or delete it in favor of Table 3.

We deleted this figure considering also comment from other Reviewers, and integrated the Table 3 adding water vapor fluxes.

Comment 2.19 - Line 485: Again it would be best to know the amount of times where wet and dry snow conditions are met within your data. As now this sentence could be influenced by your site characteristics (not much wet snow conditions) rather than it being more difficult for erosion to take place with wet snow.

Please see the reply to comment 2.15 and 2.16, information on the higher frequency of dry snow erosion vs. wetted snow erosion at the same wind speed (normalized frequency) was added also in the Conclusion Section at line 571-572, as recommended by the reviewer.

Reviewer 3 (anonymous)

Comment 3.1 - The study investigates snow accumulation and wind-driven erosion on a high-altitude Alpine glacier using nivo-meteorological and mass balance observations collected on Mt. Ortles (Eastern Alps) and a physics-based snow model. The simulation results are compared with mass balance measurements and indicate that wind erosion is the dominant ablation process. According to the simulations, wind erosion is most effective in cold, dry winter conditions and is strongly influenced by air temperature.

Indeed, snow accumulation and wind-driven erosion processes are difficult to measure and model on mountain glaciers, and the article addresses scientific questions within the scope of TC. The measurement dataset covers a long period (4 years of 15-minute averages) and appears to be of high quality for a such challenging environment. However, <u>the information provided on the measurements is incomplete</u> (see comments below).

We have addressed this issue thanks to the Reviewer's indications (see the reply to the following comments 3.5 and 3.8).

Comment 3.2 - However, some questions arise regarding the methodology and arguments used to support the conclusions. I think the interpretation of the results is not really convincing, so that the discussion does not reach substantial conclusions. These points are detailed below. Thus, the manuscript does not provide much innovative inputs on the subject and does not represent substantial progress beyond current scientific understanding.

Based on the suggestions provided by the Reviewer in the specific comments, we have improved the manuscript. We think that this work is relevant because it provides a quantification of snow erosion on sites selected for firn/ice coring that have characteristics similar to the Mt. Ortles drilling site. The results are highly relevant for the dating and interpretation of ice/firn cores archives, and because they provide a quantification of a feedback that has long been suggested by other authors. The need for quantifying post-depositional mass exchanges (melt, but also erosion) for improving the accuracy in ice core dating and interpretation was already highlighted in our previous work (Carturan et al., 2025). We have better emphasized these concepts in the revised manuscript (Line 15-17 and 82-86).

Carturan, L., Ihle, A. C., Cazorzi, F., Zendrini, T. L., De Blasi, F., Dalla Fontana, G., ... & Gabrielli, P. (2025). Reconstruction of mass balance and firn stratigraphy during the 1996–2011 warm period at high altitude on Mount Ortles, Eastern Alps: a comparison of modelled and ice core results. The Cryosphere, 19(9), 3443-3458.

Comment 3.3 - Major comments

- *Lack of validation of the snow model and blowing snow simulations*

In section 3.3, the text mentions that two "multiplicative factors for precipitation and wind speed were adjusted iteratively to minimize the Root Mean Square Error (RMSE) between measured and modelled mass balance at the simulation site" (lines 199-201). However, calibrating the model on a variable such as the mass balance, which results from all the (complex and interdependent) processes simulated in the snow, undermines the benefits of using a physical model due to error compensation issues. This physics-based snow model is based on equations of mass and energy conservation and the parameters should have a physical interpretation, so they can be linked to measurable physical quantities. Such a calibration procedure on the mass balance breaks the links between the parameters and the physical processes or quantities they are supposed to represent.

In this comment the Reviewer refers to internal validation, which involves testing the model's performance on separate internal state variables—such as snow accumulation and snow erosion in our case. We agree with the observation of the Reviewer, and modified the manuscript because there was an inconsistency regarding the multiplicative factor for precipitation in the submitted version (see the reply to the next comment). Separate observations of snow accumulation and snow erosion were not feasible at the study site, due to its remote location. The snow depth data collected with high frequency by the AWS could not be used for assessing snow (mass) accumulation and erosion because the snow density was unknown, and its independent estimation would have implied the introduction of additional uncertainty. The snow density calculated by SNOWPACK could not be used for converting snow depth change into mass change, to be used for parameter calibration and validation, because it would have implied circularity (snow density depends e.g. on wind speed, which depends on the multiplicative factor). The availability of detailed mass balance observations at the beginning and at the end of the ablation season is quite rare on Alpine sites with such high elevation and difficult access. The timing of field observations helps in addressing how the model performs in periods with prevailing high (summer) or low (fall, winter and spring) air temperature. We agree that using SWE observations for calibration is somehow a 'lumped' calibration, but we also think that it is the best compromise in study sites with characteristics similar to Mt. Ortles. Uncertainty and equifinality is now reduced because there is only one calibration factor (see the reply to following comments). The physical justification and meaning of the two multiplicative factors is now discussed in Section 5.1 (line 436-444).

Comment 3.4 - A multiplicative factor for precipitation measured in the valley at Solda of 1.55 (Section 4.1) makes sense as we can expect higher precipitation amounts in the mountains (however a discussion about the value of the parameter is missing). Precipitation is an important input data in snowpack mass balance simulations, but little is known about it in high mountain areas. Are there other measurements that could be used to better constrain this precipitation correction factor? For example, mass balance measurements in snow pits dug in areas of the glacier less exposed to snow erosion?

We have added a discussion on the requirement for the multiplicative factor for precipitation, as suggested (Section 5.1, line 436-438). In the revised version of the manuscript, this is no longer a calibration factor. There was an inconsistency in the submitted version of the paper, this factor was not calibrated, it is fixed and derives from precipitation measurements and vertical precipitation lapse rates calculated from raingauge data located at lower elevation. Specifically, it derives from the mean vertical precipitation gradient calculated from the Malga Mare and Careser diga weather stations, between 1940 and 1984, which amounts ot 28% km^{-1}. This dataset comes from the weather station network of the neighbouring Trento Province (Meteotrentino.it) and is useful because the two weather stations are only 1.4 km apart, and their catchment is rather close to Mt. Ortles (10 km SE). The dataset is unpliblished but the vertical gradients for precipitation were used in Carturan et al. (2019). Details have been added in the revised version of the manuscript (Sections 3.3, line 223-226).

Carturan, L., De Blasi, F., Cazorzi, F., Zoccatelli, D., Bonato, P., Borga, M., & Dalla Fontana, G. (2019). Relevance and scale dependence of hydrological changes in glacierized catchments: Insights from historical data series in the eastern Italian Alps. Water, 11(1), 89.

Comment 3.5 - The use of a calibrated multiplicative factor for wind speed is even more questionable. The authors argue: "... the wind speed was also adjusted by means of a multiplicative factor, to account for the fact that on top of Mt. Ortles the wind is not expected to follow strictly a vertical logarithmic profile". As, if I correctly understand, this multiplicative factor is applied to the wind speed measured by the weather station on the glacier, I don't understand why this measurement needs such a correction. If the authors are referring to stability corrections of neutral logarithmic wind profiles, this correction is certainly small for measurement heights below 2 m (the measurement height is an important information that is missing in the manuscript). In addition, the study focuses on strong wind conditions associated with blowing snow events, during which wind profiles are close to neutral. As a result of the calibration, the multiplicative factor for wind speed is fixed to 0.70 (Section 4.1). the authors should use physical arguments to explain this 30% reduction in measured wind speed (or one might think that this factor is used to compensate for other simulation errors). The use of these two correction factors should be examined in more detail and, for example, the sensitivity of the model results to these parameters could be investigated.

The use of a calibrated multiplicative factor for wind speed is not referred to stablity corrections. Instead, it is physically justified by the location of the study site, close to the isolated top of a high mountain. Please, see the reply to the comment 1.7 and the related references. The magnitude of the correction factor (which means a 43% acceleration of wind speed at the sensor, compared to undisturbed conditions) lies in the range 30-100% reported in the literature for similar sites. This is now the only calibration factor used. In the revised manuscript we have added a discussion on the physical meaning and on the need for this wind factor (Section 5.1, line 439-444). We also added the missing information regarding the measurement height, and other sensors' characteristics, as suggested (new Table 1).

Comment 3.6 - The validation of the snow model results relies on Table 2 comparing observed and modelled snow water equivalent, but this table is difficult to read <u>a figure would be easier to understand</u>. Furthermore, the titles of Table 2 are not clear to me. I do not understand the cumulated balance: shouldn't it be the cumulated sum of the data in the previous column? And given the expected measurement uncertainties, is it appropriate to display the mass balance values to the nearest millimeter w.e.?

We agree and we have replaced this table with a new figure (Figure 5), as suggested. As pointed out by the reviewer, the data presentation was a little redundant and she/he is right stating that the second column is the sum of the first one.

Comment 3.7 - Given that the conclusions of the article are based primarily on the results of the model, the inappropriate method of validation of the snow model is a critical problem in this study.

We think that reducing the calibration to a single parameter (the multiplicative wind factor), and providing information that supports its physical meaning clearly improves the robustness of the results. Please, see also the reply to comment 3.3.

Comment 3.8 - *Lack of information on the meteorological measurements and calculation methods of the surface energy fluxes*.

In Section 3.1., very little information is given concerning the measurements of the meteorological variables. The text mentions that details on sensors and measured data can be found in Carturan et al. (2023) but this data paper only details the air temperature measurements. Thus, information is missing about the sensor characteristics and accuracy, measurement height and so on… The methods of data processing, corrections and gap-filling should also be presented. This information on the snow model input data is necessary for a correct interpretation of the model results (particularly in terms of uncertainties).

We have added information regarding sensors characteristics, accuracy and measurement height in the new Table 1. Gap-filling methods were further detailed in the manuscript in Section 3.1 (line 161-166), as requested.

Comment 3.9 - I suppose that the sensor measurements (in particular, wind speed, air temperature and humidity) are corrected according to the changes in height above the snow surface (measured by the SR50). This is an important correction that should be detailed. In fact, the authors do not use the SR50 measurements. Indeed, interpreting these measurements is difficult in very windy environments. However, as very little information is available on snowfall and snow erosion processes, some useful information could certainly be derived from these measurements, for example on the timing of snowfall or snow erosion events.

Yes, the sensor readings are corrected in function of sensors' height and we have added this information in the text (Section 3.3, lines 217-219). The timing of snow erosion (and melt) events is already reported in Figure 10 (now Figure 11), for the baseline conditions. Clarified also in the revised text of Section 4.3, lines 393-394. A new figure was added (new Figure 4) that displays the time evolution of snow layers in the simulation period, as requested also by the Reviewer 1 (comment 1.9).

Comment 3.10 - To get an idea of the meteorological context, it would also be useful to see time series of some meteorological variables (e.g., temperature, wind speed, radiation fluxes).

We have reported the complete series of the meteorological data in the new figure 3, as suggested (see also replies to comments 1.8 and 2.11).

Comment 3.11 - The calculation methods of the surface energy fluxes must be presented, particularly for the turbulent fluxes. I suppose that a bulk-aerodynamic profile method is applied. In this case, how are determined the roughness lengths for momentum, temperature and humidity? Are these parameters important in the blowing snow simulations? This information is also necessary to interpret the sensitivity analysis of snow erosion to changes in air temperature (Section 3.5).

The calculation methods for the surface energy fluxes are detailed in the reported literature regarding the SNOWPACK model; in the revised manuscript we added information regarding how energy fluxes are calculated (Section 3.3, line 197-199)

Comment 3.12 - Insufficient interpretation of results and too vague discussion

The interpretation of the modelling results is sometimes insufficient. For instances: Due to the large spread of data, Figure 5 is difficult to interpret. For instance, for the no snowfall cases, does it make sense that the fitted curve for wet snow (orange) is above that for dry snow (light blue)? At the same wind speed, would the "w.e. eroded" be larger for wet snow than for dry snow? Furthermore, the numbers of dry/wet conditions during/after snowfall are certainly very different (these numbers should be indicated), which complicates the statistical interpretation of the graph. I think that this graph provides no useful information.

Based on the comments from this and the other two reviewers, we removed this figure in the revised version of the manuscript.

Comment 3.13 - Lines 289-290 and Figure 6: For wet erosion events, what could explain such different cumulative relative frequency distributions of snow erosion events and cumulative relative SWE eroded? Can the sudden increase in "swe eroded" when the age reaches 900-1000 hours be explained?

The shape of the wet snow cumulative relative frequency distribution is affected by the smaller number of events, compared to the dry snow distribution (number of events is now added in the text). In particular, the sudden increase at 900-1000 hours depends on the erosion of an old snow layer which was covered by new snow, and re-emerged at the surface after the complete erosion of the new snow. Explanation added in the revised text (Section 4.2.2, lines 340-343).

Comment 3.14 - p.17, Figure 7: for dry snow, why the frequency of SWE eroded peaks at larger wind speed that the frequency peak of snow erosion? what exactly represents "snow erosion" in Figure 7? Units?

The SWE eroded peaks at larger wind speed that the frequency of snow erosion because at lower wind speed there is frequent erosion of small quantities of snow, whereas to remove larger mass of snow a higher wind speed is required. This figure aims at highlighting this aspect. Explanation added in the text (line 356-358).

Snow erosion is represented in terms of hours (now reported 'hours' in figure caption for clarity) and mm SWE, as reported in the Y axis. This figure was also modified following suggestions from Reviewer 2 (comment 2.16).

Comment 3.15 - There are very few wet snow erosion events: "dry snow erosion accounts for 91% of the total modelled erosion" (lines 292-293). Are the statistical results for wet snow events presented in Figures 7, 8, and 9 significant? In particular, how can the two peaks in the frequency distribution of the threshold wind speed for wet snow erosion shown in Figure 9 be explained?

There are 155 hourly wet erosion events compared to 1731 hourly dry erosion events. The sample size is rather different but we do not consider a sample size of 155 events to be not significant, in particular when comparing frequency distributions. The objective here was to highlight the different behaviour of wet and dry snow to wind and air temperature (and in particular possible threshold effects) and the different relevance of wet and dry erosion in terms of absolute frequency and total eroded SWE. Following recommendations from Reviewer 2 (comment 2.16) we have added a version of Figure 7 (now Figure 8b) with normalized frequencies and a modified version of Figure 8 (now Figure 9, comment 2.17). Figure 9 of the submitted paper was removed in the revised version of the manuscript.

Comment 3.16 - The discussion remains rather vague (especially in Section 5.1), mainly due to the lack of field observations concerning snow accumulation and erosion processes (see comments on snow model validation issues).

This section discusses several points raised by the reviewers, namely the low time resolution of field measurements used for calibration, the need for adjusting measured wind speed, the use of constant correction factors instead of e.g. monthly variable ones, uncertainty in mass balance measurements, and the simplified approach in testing the temperature sensitivity. We have modified this section improving the discussion, based on the various inputs by the Reviewers.

Comment 3.17 - The text states (p.28-29): "This specific study highlights the high sensitivity of glacier mass balance to snow erosion, and of snow erosion to air temperature fluctuations. These interactions may lead to a non-linear response of glacier mass balance to climate change and to negative feedbacks that should be taken into account when projecting the future behaviour of the alpine cryosphere at high elevation". However, this point-scale study indicates local snow loss. Eroded snow can be transported and deposited elsewhere on the glacier, so information is missing to consider the local eroded snow as a loss in the glacier mass balance.

According to this comment we highlighted better in the paper that the results are specific for this study site, and usefull for other sites with analogous characteristics (i.e. wind exposed saddles at high elevation) selected for firn/ice drilling operations in the Alps (line 29, 92, 542-545, 549, 592-594).

Comment 3.18 - *The sensitivity analysis of snow accumulation and erosion to changes in air temperature is not convincing and does not provide much innovative insights*.

There is insufficient information on how changes in air temperature are taken into account in the simulations. For example, does air humidity remain unchanged in relative or specific values? How are calculated the turbulent fluxes (see comment above)? It is important to know whether a stability correction is applied in the calculations of the turbulent fluxes (negative feedback).

We added information on how turbulent fluxes are calculated in SNOWPACK (see reply to comment 3.11) and how changes in air temperature are taken into account in the *simulations (Section 3.5, line 263-267), adding also discussion in Section 5.1 (line 470-478). Please see also the reply to the comment 1.11 for considerations regarding humidity.*

Comment 3.19 - Since the scenarios presented here only take into account changes in air temperature (all other meteorological variables remain unchanged), the physical consistency between the input meteorological data is lost and the benefit of using a physics-based model is greatly reduced. The most problematic point is certainly not taking into account changes in precipitation for such significant changes in air temperature (up to 3°C). For instance, the authors consider that “the -2°C scenario should represent conditions at the end of the Little Ice Age (LIA) and the -3°C scenario could be representative of the coldest part of the LIA” (p.28). However, there are numerous studies on glacier extents during the LIA in the Alps, and they generally indicate an increase in precipitation (e.g., Vincent et al., 2005). Thus, it is probably not fair to consider that the -2°C or -3°C scenario should represent conditions of the LIA.

We have removed any reference to the LIA climatic conditions in the manuscript. Our intention is to test and isolate the sensitivity of the model output to changes in air temperature, not to different climatic scenarios, because it would be beyond the aims of this work. Please see also the reply to the comment 1.11.

Comment 3.20 - Other comments

- Section 3.4: please clarify the definition of “the life” of a snow layer. If I understand correctly, the “life” of a snow layer ends when it is removed by wind erosion and it starts when “snow accumulation exceeded 0.1 mm h-1” (Lines 210-211). Thus, the life of a snow layer should also end due to a snow accumulation event (not only due to snow erosion). However, the interpretation of the results (e.g. Section 4.2.1) seems to consider that the variable “Age” is only limited by snow erosion (?). This point should be clarified. Furthermore, the term “life” is not appropriate to characterize a snow layer (it is not a living being); a term such as “duration” would be more appropriate.

Ok, we have better clarified what we mean (Section 3.4, lines 239-242) but we preferred keeping ‘life’ (as an abbreviation meaning ‘lifetime’) instead of duration. Lifetime is a common term referred to snow, e.g. Colbeck (1991).

Colbeck, S. C. (1991), The layered character of snow covers, Rev. Geophys., 29(1), 81–96, doi:10.1029/90RG02351.

Comment 3.21 - The wording is not always specific enough, for instances:

- too much use of the names of the simulation variables in the text (e.g. MS_wind, WS_Th…), it's not easy to understand, especially in the results section

We agree that acronyms can make some parts of the paper difficult to understand, and we tried to limit their use or recall their meaning along the text (especially in Section 4.2.1).

Comment 3.21 - Figure 1 should be cited in the first paragraph of the Section 2 (Study Area)

Ok, modified as suggested (line 98).

Comment 3.22 - line 340: Figure 12 should be cited before, at the beginning of the paragraph.

Ok, modified accordingly (line 401).

Comment 3.23 - Specific comments:

- Line 53: “this work”: self-citation?

Ok, removed.

Comment 3.24 - Line 104-105: also due to the influence of spatial variations in precipitation.

We added this sentence in the text (line 109).

Comment 3.25 - Lines 183-184: I do not understand what the fetch length represents here.

The fetch distance is an arbitrary length scale for the conversion of a horizontal mass flux (which is the primary model output) to a snow depth change. The assumption is that the mass that is transported (e.g. over a mountain ridge) is then distributed along the fetch distance in the lee. Explanation added in the revised text (Section 3.3, line 208-211).

Comment 3.26 - Tables and Figures

Figure 10: the lowest panel on the left showing the baseline scenario is not useful (it is shown in all the other panels).

We think it is usefull because it reports the time of erosion and melt for the baseline condition, to be compared to the other temperature conditions that have different erosion and melt hours (now figure 11). See also the reply to comment 3.9

Comment 3.27 - I'm not sure that Table 3 is necessary. Furthermore, is it appropriate to show precipitation, snowfall, or melt amounts in mm (w.e.?) with two decimal digits?

According to this comment and also to comment 2.18, we removed Figure 11 and kept only Table 3, removing the two decimal digits.

References

Vincent, C., Le Meur, E. & Six, D. Solving the paradox of the end of the Little Ice Age in the

Alps. Geophysical Research Letters 32, doi:10.1029/2005GL022552 (2005).