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, a figure showing the time evolution of modelled snow depth (highlighting snow layering and accumulation/erosion), and a two-dimensional binned plot that shows the relationship between air temperature, wind speed and erosion frequency
- we modified figures 2, 7, and 8, as well as the correlation matrix figure based on Reviewers' suggestions
- we removed figures 5, 9, 11 and replaced Table 2 with a new figure that visually shows the same results, improving their readability.

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.

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.

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 in the revised manuscript.

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, 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, we have better emphasized this result in the revised manuscript.

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, 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). Clarified in the text (Section 3.3).

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, as we write at line 201. 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.

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 the speed-up effect 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 text to 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 similarly 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. A figure was added to show the full meteorological input of the model.

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, we added a new figure displaying the modeled snow layering and phases of snow erosion.

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 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 and improved knowledge of snow erosion by wind is essential for the dating and interpretation of these paleoclimatic archives. We have better clarified the motivation and the speficic aim of this study.

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.

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 5 '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', 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.

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 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 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 figure 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). We try to make it more clear in the revised manuscript.

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.

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), 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).

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.

This figure was 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.

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.