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 4 (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, the information provided on the measurements is incomplete (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. 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.

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).

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 multiplicative factor for precipitation, as suggested. 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 rather close to Mt. Ortles (14 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.

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 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. We also added the missing information regarding the measurement height, and other sensors' characteristics, as suggested.

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 a figure would be easier to understand. 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, as suggested.

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 and accuracy, measurement height, methods of data processing, corrections and gap-filling in the text and in a new table.

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. Timing of snowfalls and snow erosion are reported in a new figure that displays the time evolution of snow layers. The timing of snow erosion (and melt) events was already reported in Figure 10, baseline conditions. Clarified also in the revised text.

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 a new figure, as suggested (see also reply 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 mentioned literature regarding the SNOWPACK model, in the revised manuscript we recall the most relevant procedures as suggested, in particular for the calculation of turbulent fluxes and of the roughness length.

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. 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.

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 frequency of 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. Snow erosion is represented in terms of hours (now reported ‘hours’ instead of ‘h’, 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, Figure 8) 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 with normalized frequencies and a modified version of Figure 8 (comment 2.17). Figure 9 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.

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 and how changes in air temperature are taken into account in the *simulations. 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 and replaced the term 'life' with 'duration'.

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 as much as possible in the revised version.

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

Ok, modified as suggested

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

Ok, modified accordingly

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.

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.

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 erosion hours and melt hours for the baseline condition, to be compared to the other temperature conditions.

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).