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 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 report also the mass fluxes related to humidity effects. 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, adding references in support of the existence of the orographic increase of precipitation with elevation.

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 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. Please see also the reply to comments 1.6 and 1.7 (by the Reviewer 1).

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 and also quantified the related mass fluxes.

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 in the revised version of the manuscript, because the same information (normalized by the wind speed distribution as the Reviewer suggests), is already reported in Figure 8.

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 and discussed the additional insight it provides, 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 are 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, 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 several figures thanks to the suggestions of the Reviewers, including those that have a spreadsheet-like appearance. 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.

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 sentence to highlight this issue in Section 4.1, 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. Results are discussed in the revised text.

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.

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.

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.

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 and integrated the Table 3 adding the surviving snow percent as a new row.

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, information added and discussed in the revised manuscript.