Dear Editor, Dear Referees,
thank you again for your evaluation of our manuscript. Working on the revised version of the manuscript and reflecting further on your comments, we decided to perform some changes with respect to what already stated in the replies. In particular, we would like to make additional comments to the following points of the evaluation by Referee #2:

- *P8 Eq (4) Top rhs member: Remove the Pa (if that stands for Pascal, pressures should naturally be given in Pascals) and check wether the 10 4 has to stay or not*

  The equation needs the maximum between the overburden stress and 0.1 bar, i.e. 10000 Pa. In order to better model the densification of the most superficial part of firn, Bréant et al. (2017) imposed a fixed constant pressure of 0.1 bar, that should approximate the pressure at 2-3 m depth, when overburden stresses are lower. We reported the unit of measure because we believe it to be clearer, since this value can be assimilated to the reported values of constants.

- *P14 L296-301: The procedure you are following to calibrate a is not very clear. First, I don't understand why the wind contribution is not accounted for as it should explain part of the ablation, the other part being due to melting. Then, if I understand well, you are calculating one optimal value of a per hydrological year, and then, for each thus obtained value of a, you compare observed snow depth to snow depth modelled for all the hydrological years except the one from which the considered value of a has been derived. Is that correct ? If yes, I think that this procedure makes sense, but what I find surprising is that you don't use these comparisons between model outputs and observations to set the final value of a but you simply take the median of all the a you have calculated. Why is that ?*

  For the calibration of the two parameters, *a* and *e*, that was performed in a different site in the revised manuscript, we accounted for wind contribution, running the snow model with the addition of wind erosion and the influence of wind on snow density. We also changed the adopted procedure with which we selected the optimum parameters, as reported in the revised manuscript. In particular, due to the low amount of available snow water equivalent data, we calibrated the two parameters considering together all the years, instead of performing a different calibration for each year. We then performed the validation on a site closer to Colle Gnifetti, in order to check the performance of the selected parameters and their transferability. The choice of the calibration and validation station was made so as to calibrate the parameters in the site where SWE measurements were performed closer to the meteorological station. This because the solid precipitation input series was reconstructed from the snow depth series measured at the meteorological station; hence, having measured SWE data in a site characterized by different snow depths is likely to introduce more errors in the model.

- *P14 L308-312: The way you are setting the value of ρ D for the various cores sounds a bit random as presented here.*

  In the revised version, we tested three different values of ρD looking for values already adopted in the literature at CG.