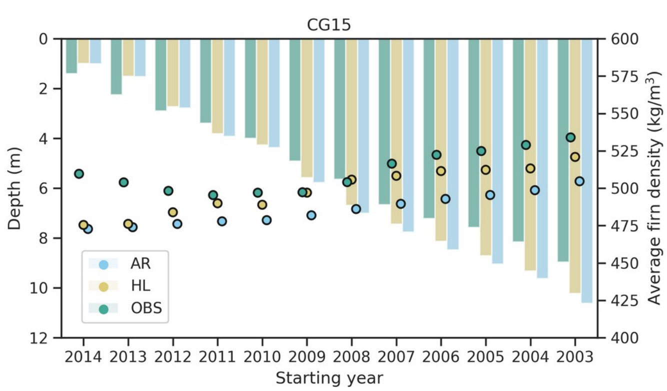
Dear Referee #2,

thank you for your comments. We will reply to the general comments first and then we will address the specific ones.

Regarding the problem of parametrization, we will state more clearly in the manuscript that most of the fluxes are modeled based on empirical approaches, and we will rewrite the conclusions Section, following the remarks you presented in both the general and specific comments. We will also address the issues you pointed out regarding the structure and the use of English and we will check the presence of commas through the text.
The choice to present the selected values of some parameters in Section 3.4 while others in Section 2 was done in order to distinguish between site specific parameters (reported in Section 3.4) and the parameters whose value was not selected depending on the specific case study. If you believe it not to be clear or useful, we will reorganize Section 3.4 as you suggested.

Regarding the computation and representation of the depth-density profiles we will discuss it in the followings. In the single layer version, the overburden stress for firn densification was computed considering half of the firn column weight plus the whole weight of the snow column, in order to have an average densification rate. We will state this clearly in the manuscript. Regarding the multilayer version, we discretized the equation modelling a layer for each water year. For each layer, we then computed the overburden stress as the weight of all the layers above and half of the layer considered.

We apologize for the lack of clarity of the figures. We thought of a new representation of the results of the single layer version of the model, reported in Fig. RC2.1. In the panels of the figure, each group of bars represents a different run of the single-layer version of the model. On the x-axis it is reported the starting year of each simulation or equivalently the age of the oldest part of the firn column. The age of the most superficial part, or the ending year of the simulation, is different for the two cores: 2015 for CG15 and 2013 for KCC. The length of the bars is equal to the depth of the firn column obtained from the single-layer version or summing the depths of the individual layers of the multi-layer version. Dots, instead, represent the average density of the firn column, again as obtained from the single-layer version or averaging the density of each individual firn layer obtained with the multi-layer version, weighted for their depth. Whenever a snow layer becomes firn, the average density of the already existing firn column is changed depending on the density of the new added mass. This implies that average firn density, locally, may decrease with increasing simulation time, while the general trend will still be an increase in average density. We hope this helped in clarifying the issue, elsewhere we will provide further explanations of the results.



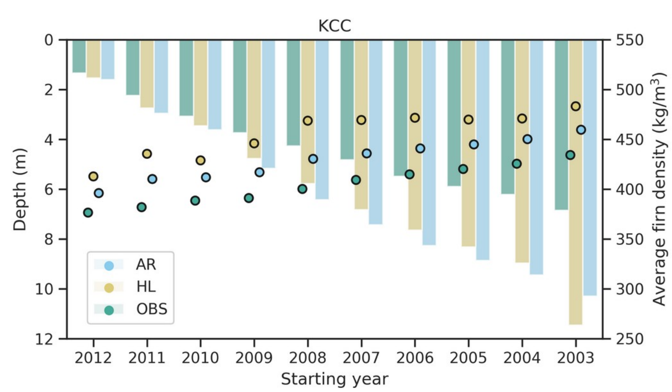


Figure RC2.1: Firn density and depth obtained from the single-layer version of the snow-firn model and observed, for ice core CG15 and KCC, using the model for firn densification of Arnaud et al. (2000) (AR) or Herron and Langway (1980) (HL).

We will also change the figures representing the results of the multi-layer version of the model, reporting a profile of depth versus density, as given in Fig RC2.2, instead of deposition year versus density. We are glad to receive any suggestions in order to further improve the representation of the results.

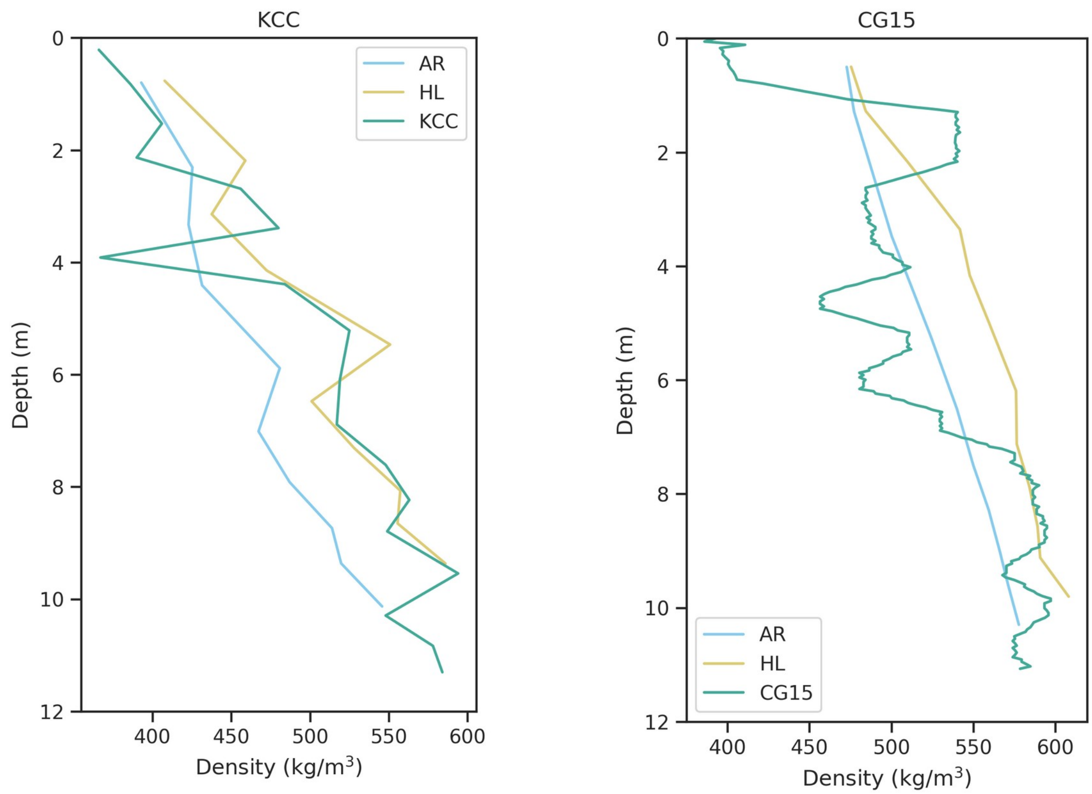


Figure RC2.1: Firn density and depth obtained from the multi-layer version of the snow-firn model and observed, for ice core CG15 and KCC, using the model for firn densification of Arnaud et al. (2000) (AR) or Herron and Langway (1980) (HL).

Here are the replies to the specific comments. Some comments are not reported, because we believed them not to need further discussions. They will, however, be integrated in the revised manuscript:

1. *P2 L50: "momentum balance and rheological equations" → I don't really see any equations related to the momentum conservation (divσ+ ρg = 0) or firn rheology ($S = \sigma + pI = 2\eta\dot{\varepsilon}$).*

    Following De Michele et al. (2013), to model snow densification, we have used the momentum balance equation in the integral form: $\sigma + \rho g h s = 0$, while concerning the rheology of the snow we have used the Maxwell law $\eta = \frac{\sigma}{\dot{\varepsilon}}$ where η is the coefficient of viscosity, and $\dot{\varepsilon}$ is the vertical viscous strain rate. The contribution of wind was then considered as proposed by Liston et al. (2007).
    Also the firn densification equation of Arnaud et al. (2000) is physically based. The first stage equation is obtained assuming linear-viscous boundary sliding, while the following equations were obtained assuming the constitutive equation of power law creep with a stress exponent varying from 3 to 1.

2. *P3 L84: "and metamorphosis of grains not driven by wind" → I don't think there is any representation of snow metamorphism in your model*

   You are right, the densification equation used models the evolution due to compaction, neglecting the metamorphism. We will correct it in the manuscript.

3. *P4 L113: Point (2) is not very clear, could you precise*

   We noticed that we reported incorrectly the point numbers, repeating two times point (2). We will correct this in the revised manuscript. If you are referring to the correct second point, the wind is measured at a given height from the ground, that is fixed when we have bare soil, but decreases when the snowpack is present. In the formula in which wind speed is modified so that it is referred to a specific height from the ground, we assumed the measure of the meteorological station to be referred always to 10 m above the surface. Regarding the third point, this assumption was introduced to model the role of melting in protecting snow from erosion. If snow melts and then freezes above the surface of the snowpack, it will create a hard layer that will prevent snow erosion. The presence of wet snow too makes the snow more difficult to be eroded. In the model, when snow melts, erosion flux is set to zero, independently of wind speed, until the deposition of new solid precipitation. We assumed, in fact, that, when a wet or icy snow layer is present, the snow below cannot be eroded.

   Regarding the last comment, after the list, we are able to compute the value of Q without running the snow-firn model, because it requires as input only the solid precipitation series, temperature and wind speed. Information about the snowpack height is not needed for point (2) and the amount of potential erodible snow can be inferred by solid precipitation alone if erosion after melting is not allowed i.e., point (3).

4. *P6 Eq (2b): I don't really understand how the last term of the right-hand side of this equation is treated as the firn is said to be impermeable. How do you transfer the remaining liquid water to the firn then?*

   The idea is that all the snowpack is moved into the firn layer at the end of each water year. Inside the firn, no water movement is then allowed, so that the water possibly present will remain confined in the firn pores.

5. *P8 Eq (4): In the text you are mentioning three densification regimes but there are four equations here. Could you clarified that point?*

   The last two equations belong to the same stage, i.e. after that close-off conditions are reached, when pores become isolated. The passage between the third and fourth equation is driven by the transition between cylindrical to spherical pores.

6. *P8 L196: It is not clear why the information regarding the transition density for HL is given here while the same information for AR is given at the end of Section 3.4. I would bring the latter back here (see Major Comments).*

The transition density in the model of Herron and Langway (1980) is given and it is not an adjustable parameter. On the contrary, in the model of Arnaud et al (2000), the transition density is one of the parameters whose value needs to be selected. Depending on the site, different values could be therefore chosen. As already reported above, we tried to separate the site-dependent parameters in the organization of the manuscript.

7. *P8 L201: "In this way the passage from snow to firn densification is driven by snow characteristics rather than snow age" → 1/ In this way, ... 2/ I don't really see where snow age appears as the driving factor for densification from Eq. (4) and (5).*

   We will reformulate it in the revised manuscript. Instead of moving from one densification equation to the other when the snowpack encounters the end of a water year, we linked the transition with the value of the average density of the layer. We believe it to be more accurate to use the information of snow density to guide the transition between the two densification equations.

8. *P8 L202: "corresponds to the average surface density" → Averaged over what ? Time ? Observations from cores ?*

   It is the average snow density, that we obtained extrapolating the most superficial part of depth-density profiles of ice cores. Steady-state firn densification models cannot be applied to the superficial snow where the metamorphism is more complex and significantly influenced by air temperature. The original model of Arnaud et al. (2000), for example, is applied only from a depth higher than 2 m. Therefore, in order to run them, it is necessary to specify a surface density. Different papers, studing the firn density at Colle Gnifetti, use different values of surface density, since the surface density is highly variable at the site.

9. *P10 Fig. 2: The map is a bit difficult to read. At least, I would remove the dotted lines (what is it ?) and the line made of crosses (boundary between Swiss and Italy ?) or add a legend to tell what they mean.*

   Thank you for your comment. We will change the basemap in order to make it clearer.

10. *P11 Fig. 3: It would be great to have a box on the background map to know exactly to what region the zoom-in of the lower right corner corresponds. Also, the fact that it is a zoom-in should be precised in the caption. You can also mention that CM appears on Fig. 2 as well, which will help the reader to localize the study area on this map*

    Thank you for your suggestion. We will add the box and rewrite the caption.

11. *P13 L283: "smoothing the snow depth with a moving average whose window size was calibrated" →Not very clear, could you clarified ?*

    Hourly snow depth series cannot be used to infer solid precipitation series by simply computing hourly differences; this because of the fluctuations of snow depth measurements due to air temperature variations, that introduce positive differences not related with solid precipitation events. We therefore processed the series of positive differences of hourly snow depth, applying a moving average to the series. To chose the best window for the moving average, for each water year, we selected several window sizes and we calibrated *a* for each of them. Among all the windows, for each year, we chose the one with the best value of the objective function in calibration.

12. *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?*

    The parameter *a* was calibrated to a lower altitude site, using data of a meteorological station located in a position sheltered from wind. For this reason, wind effect was not taken into account. Regarding the second point, you understood correctly. The comparisons were used to compute the performances of the model in validation, but in order to select the final value of the parameter *a,* as done also by De Michele et al (2013), we did not consider which parameter' value had better performances in validation.

13. *L304-306:"Its value is in fact chosen in order to have a continuous densification rate between first and second stage of densification. For each of the available ice cores, with the exception of CG11, we computed the parameter γ' running AR in a steady-state condition (Bader, 1954) using the mean accumulation reported in Table 2." → In other words, you are calibrating γ', right?*

    You are correct, we will change this in the revised manuscript.

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

    In Fig. 4, the purpose was simply to evaluate the performances of steady-state models, therefore we selected a value of surface density already used in literature to study firn at Colle Gnifetti. For the two cores, used to compare non steady-state results, we checked if the selected value was coherent by extrapolating the last part of the depth-density profile of firn. We consequently kept the value of 360 kg/m^3 for KCC and we estimated a new value, from the extrapolation, for CG15. Moved by your comment, we report here a comparison between the results of the snow-firn model for CG15 ice core with ρD = 360 kg/m^3 (one of the two values tested by Licciulli et al (2020)) or ρD = 470 kg/m^3, the value used in the manuscript.

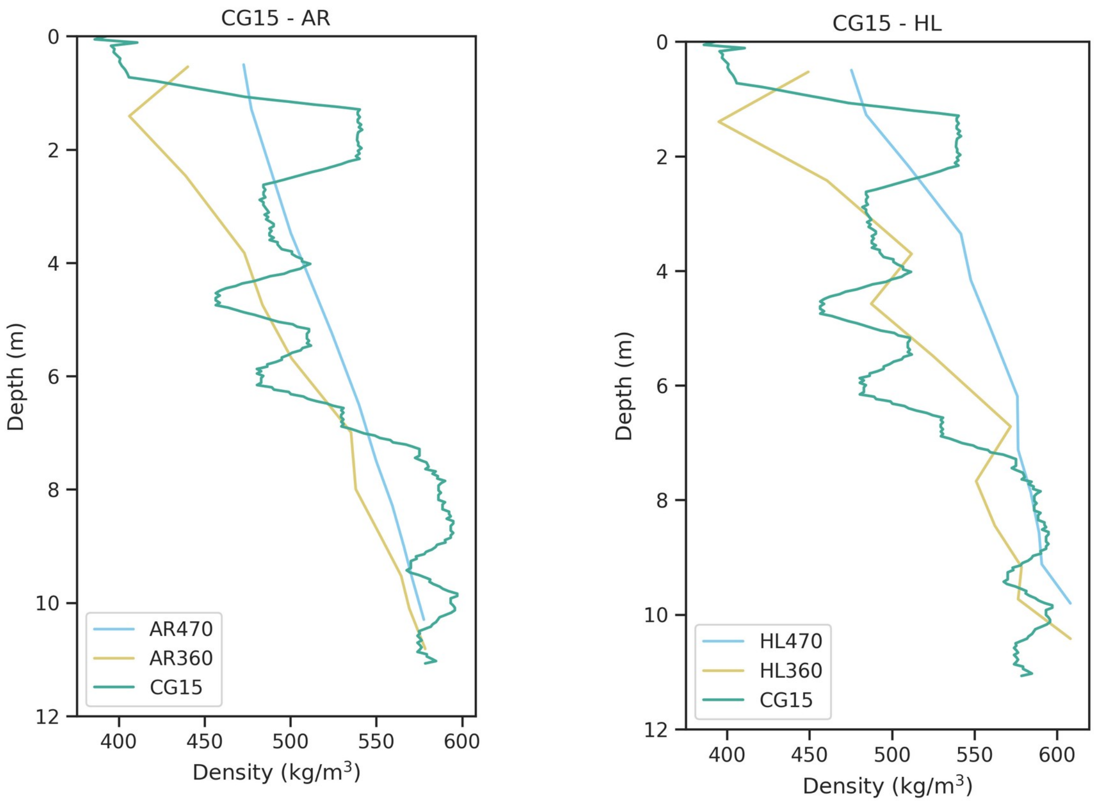


Figure RC2.3: Firn density and depth obtained from the multi-layer version of the snow-firn model and observed, for ice core CG15. In the left panel, using the model of firn densification of Arnaud et al. (2000) with a transition density between snow and firn of 470 kg/m3 (AR470) or 360 kg/m3 (AR360). In the right panel, same as left panel but for Herron and Langway (1980) firn densification model.

15. P14 L319: The parameterization of SSA given here is not consistent with the one given in Appendix A1.

    We are using two different parametrizations for SSA depending on the context. The one in the appendix is used to estimate the grain size of the whole snowpack. In order to compute the grain size in wind erosion formula, we adopted a parametrization proposed for recent snow, since we believe it to be more representative of the grain size of the snow mobilized by wind.

16. *P15 L323: Why does the unit of NSE, RMSE and MBE is different from the original unit of a?*

    Calibration and validation were performed using snow depth in the objective function so the error measures have unit of a depth.

17. *P15 L324: What is the SNOTEL network?*

    SNOTEL (Snow Telemetry) is a network of automated stations, located in mountain basins of western U.S., operated by the Natural Resources Conservation Service (NRCS). They collect data of precipitation, snowpack characteristics, temperature and other meteorological variables. We will clarify this in the revised version of the manuscript.

18. *P15 L329-331: I have the feeling that a re-calibration of parameter a would be required if the value of e is changed to be consistent. Is it not the case?*

Thank you for your remark. We will remove the sensitivity analysis of the parameter *e*.

19. *P18 Fig. 6: For the lower right Figure, I think what is represented is 100 × (Solid precip. - Eroded snow)/ Solid precip., am I correct ? If yes, you could write it explicitly.*

Yes, we will write it explicitly.

20. *P19 Fig. 7: I am not sure this Fig. brings relevant additional information.*

We will remove it in the revised manuscript.

21. *P20 L386: Why comparing HL and AR between each other and not to observations ? In addition, in its current state, this sentence seems to countradict the one just before.*

In Fig. 4 we report the performances of the steady-state firn densification models as they were proposed by the respective Authors, without introducing novelties. Nevertheless, it is important to understand their performances at Colle Gnifetti and also how the two models behave, before combining them inside the proposed snow-firn model. In this context, we included the comment about the differences between the two models. Comparing the two models, we can see that the profile obtained with AR is generally above the one obtained with HL before D_0 and below after D_0. This may give information about a tendency of one of the two models to overestimate/underestimate the density.

22. *P21 L408-409: How do we compare cm to kg m−2 yr−1 ? More broadly speaking, a choice has been made to give all results in terms of accumulation in kg m−2 yr−1, but I think that m SWE is often easier to apprehend. But that's only my opinion.*

This a second submission of the manuscript. The first TC Editor, that handled it, suggested us to change all results from m SWE to kg m−2 yr−1.

23. *P22 Fig. 10: The choice of the colors is not ideal.*

Thanks for the remark. We will change them.

24. *P23 L442:"The annual behaviour" → You mean the inter-annual variability, don't you ?*

Yes, we do. We will correct it.

25. *P23 L456-457:"along with the snow redistribution due to gravity" → What do you mean ?*

The spatial pattern of accumulation in the site may also be due to the movement of deposited snow along the saddle due to gravity. We will clarify this point in the revised manuscript.

26. *P23 L460-461:"We can instead see an improvement in the ability of the model to follow year-to-year variations in the period 2008-2014" → It is not very obvious.*

Thank you for your remark. We looked at the results of Fig. 11, plotted as a density versus depth, instead of deposition year, and we saw no improvements by changing T_t. We will remove this discussion from the manuscript.

27. *P24 L474:"Results in Fig. 9, on the contrary, show a better and more robust performance of the model with" → one cannot say that it is robust when it changes depending on the core being considered*

Thank you for your comment, we will change it. We will also change Fig. 9, substituting it with the profiles of density versus depth, reported above, and we will comment the performances of the model based on the new figures.

28. *P24 L485:"to asses the influence of meteorological variables on snow and firn characteristics." → (1) to assess; (2) How this could be helpful ?*

As stated before, the model links meteorological variables, used as inputs, to the firn characteristics. It can be therefore run with simulated meteorological series, as input, in order to understand how firn responds to changes in temperature or precipitation.

29. *P25 L486:"therefore moving the boundary of the model from surface accumulation and density to hourly meteorological series" → This is an interesting way of formulating what you have been doing in this paper. I think a similar formulation of the problematic you are addressing would be welcomed in the introduction as well.*

Thank you for your suggestion. We will introduce this formulation of the problem in the Introduction as well.