Dear Editor, dear Referees,

thank you for your comments. Here are our replies.

Referee #1

Thank you for your suggestion of integrating the results of the multi-layer version of the model in order to improve the performance of the single layer one. We will address it in the future.

Referee #2

- *P2-3 L58-59: This new sentence lacks of clarity and would deserve to be reformulated.*

  Thank you. We reformulated it (ll 55-57).

- *P4 L92: Change slightly the sentence to avoid the repetition of 'compaction'. I think it is correct to use 'settlement' instead of the second occurence of 'compaction'.*

  Done.

- *Eq (1d) p4 and Eq (3f) p6 L23-25: Following your answer to my comments about the momentum balance and firn rheology, I checked the paper of Di Michele et al. (2013) to see how expressions of Eq (1d) and (3f) were derived from mass/momentum conservation and Maxwell law. It turns out that in their formula the density in the factor before the exponential is raised to the square, whereas it is not in your expression. Please double-check this point, both in the manuscript and in the code of the model.*

  For the dry snow densification, to take into account wind effects, we used the equation proposed by Liston (2007). In the equation, strains are still governed by the same coefficient of viscosity, but the stress driving strains is parameterized as a function of wind speed so in the equation there is not the term (ρ_S * h_S).

- *P8 L191: 'that may be assimilated to the conditions' → 'assimilated' sounds a bit too strong to me. What about 'that resemble conditions' ?*

  We changed it as you suggested.

- *P8 Eq4 Top rhs member: I keep thinking that the 'Pa' should be removed as P is already given in Pa as you precise it at l. 195. Therefore, for reason of homogeneity of the equation, the second member of the max operator is naturally also given in Pa. This is only a formality and I leave the decision to the editor.*

  We do not have a specific interest in keeping the unit 'Pa', we simply believed it not to be clear otherwise. But, since from your comment I understand this is not the case, we removed it in the revised version as suggested.

- *P8 L208: One comment: I have the feeling that because the density increases non-linearly with depth, there are more mass in the lower half of the firn column than in the upper half. Therefore, considering the overburden pressure of the snowpack plus the upper half of the firn*

column is probably not a very good estimate of an 'averaged pressure' that would apply to the whole firn column. And therefore, calculating the densification rate from this overburden pressure value does not necessarily give an 'averaged densification rate of the firn column'. However, one could probably argue that this is somewhat counterbalanced by the fact that firn deforms less readily at higher densities.

Thank you for your comment. We will explore it in future developments of the model.

- *P12 L292: There is an inconsistency here as you are saying just above that for gaps longer than 24h, you adopt the long-term lapse rate approach and not the MicroMet procedure.*

  The MicroMet procedure consists of three sub-methods depending on the gap size, where the last one was substituted in our work with another approach. Nevertheless, we preferred to report the complete MicroMet procedure. To avoid confusion, we added a sentence (l.284) to make it clearer that the last step was not used here.

- *P14 L314-318: Despite the effort you have made to make it clearer which I acknowledge, I stil have a hard time to understand what you have been doing here. But if the editor finds it clear enought, then you can leave it as it is.*

  The procedure used is very similar to the one reported in the paper by Avanzi et al. (2014), with the exception reported in the manuscript. For this reason, we did not want to dedicate too much space for its explanation, to not extend the manuscript with information already published. However, we will explain it with more details if the editor believes it necessary.

- *P16 L380: "the three NSE" → It took me a little while to understand that the three NSE were refering to the combined NSE as well as the one for snow depth only and the one for SWE only. Maybe it is worth to repeat it.*

  Done

- *P18 L406: The quantities you are representing as monthly box plots are not listed in the text in the same order as they appear in Fig. 6, which is somewhat confusing, especially since the way yo are referring to them in the text does not correspond to the titles of the subplots.*

  Thank you for noticing it. We changed the order in which they are listed in the text so that now it matches the one in the figure.

- *Fig.7 p21 and Fig.8 p22: It is really great to have made these new figures that are much clearer than the older ones. However, I have one small comment: in Fig. 7 rows are for the densification model and columns for the considered core, while it is the other way round in Fig. 8. I think it would be better to have it the same in both Figs.*

  Done

- *P24 L472-480: Are you talking about the change in densification rate which is very obvious in, e.g. cores CG03,CC and KCI ? If yes, is it not rather due to the switch toward another densification regime at high density that could correspond to the third densification stage that you mentionned in p.7 ?*

Taking as an example CG03 ice core, I am referring to the change occurring at a density of around 700 kg/m^3. I believe this not to be the case, for two main reasons. First, I expect the close-off density, i.e. the passage between second and third stage, to be associated with a lower density. Luthi and Funk (2000), for example, for Colle Gnifetti, assumed it to be between 780 and 917 kg/m^3 . Secondly, when the close-off density is reached, densification rate decreases, because it is now associated only to the compression of the bubbles of air. Hence, the third stage is characterized by a steeper profile of density with depth. On the contrary, the change, pointed out in the manuscript, results in a profile that is more horizontal, and therefore in a greater variation of density with depth with respect to a smaller depth.

- *P25 L516: This sentence needs to be checked for meaning. In addition, I would insist on the fact that this sentence refer only to the configuration of CG or similar configurations, i.e. that in general higher temperatures are not expected to lead to higher snow accumulation as I feel that this sentence, in its current formulation, is suggesting.*

  We rephrased the sentence, and we hope that in this way we made it clear that we are discussing the specific response of Colle Gnifetti under climate changes (ll 459-462).

- *P26 L534: reasonable*

  Done.