## General comments

Overall, the authors of 'A local model of snow-firn dynamics and application to Colle Gnifetti site' have taken my comments into account and have accomplished substantial work which has considerably improved the quality of the manuscript. In particular, I highly appreciate that the calibration procedure for parameters $a$ and $e$ was revised, with calibration performed at a station where wind erosion as a significative effect on total accumulation. I also think that the presentation of results regarding the evolution of the modeled firn density, both in the single-layer and multi-layers versions, is now much clearer. This is for a large part due to the new Figs. 7 and 8 which, from my point of view, are much easier to interpret than the ones on which the previous version of the manuscript was relying.

Regarding the structure of the paper, I like the fact that there is now a Section 'Calibration of model's parameter' and another one 'Site specific parameters'. The manuscript is much better organized this way. Many commas have been added, but some are still missing here and there.

Overall, I think the manuscript is almost ready for publication, I have simply a last handful of minor comments that are listed below. Note that page and line numbers refer to the version of the manuscript with tracked changes.

## Specific comments

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

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'.

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.

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

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.

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.

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.

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.

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.

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.

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.

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 ?

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.

P26 L534: reasonable