General comments

The paper 'A local model of snow-firn dynamics and application to Colle Gnifetti site' by Fabiola Banfi and Carlo De Michele is a study based on the development of a snow/firn numerical model to investigate the evolution of density and snow accumulation at Colle Gnifetti. The model essentially relies on mass conservation of the various components of the system (i.e. dry snow, liquid water, firn), but it includes several fully empirical or semi-empirical laws to simulate processes such as wind erosion, firn densification, melting, snowpack densification due to wind... On the other hand, several important processes for snowpack evolution are not taken into account: wind re-deposition, water percolation, heat transport, radiative transfer, snow metamorphism... The flow of firn/ice is also not included. There are two versions of the model: one considering the firn column as a single layer, and for which the output in terms of density is an averaged value (which rises questions, see below); another one in which the firn is decomposed in multiple layers which enables to get a vertical density profile. Two models of firn densification are also tested. The model is forced with meteorological data (air temperature, wind speed, precipitation) recorded at near-by weather stations. The data recorded at one of the considered weather station are used to calibrate some of the model parameters. The two versions of the model, with the two approaches for firn densification, are then tested at Colle Gnifetti. Model outputs are compared to observations of density profile and snow accumulation from cores drilled on site. The modelled order of magnitude are correct but it is difficult to draw firm conclusions regarding the capabilities of the model due to high spatio-temporal variability of density profiles and accumulation observed at the site. One interresting result of the study is that, counter-intuitively, the positive contribution to surface mass balance at Colle Gnifetti occurs in summer, as all winter snow is blown away by wind.

As stated in the introduction, including the contribution of glacier run-off when projecting the future evolution of hydrological regimes in mountainous regions does indeed sound to be of prime importance in the context of global warming. Although I am not very familiar with hydrological models, I can easily imagine that this requires the development of simplified models of snow/firn able to simulate the amount of Snow Water Equivalent (SWE) stored in the system and its evolution over time, disregarding all the complexity associated to the most detailed models such as SNOWPACK or Crocus. This makes this study relevant and the paper probably ought to be published. This being said, I have a fair amount of major criticisms to formulate, both on the methodology and on the conclusions that are drawn from the numerical experiments.

First of all, I think that the model is much more empirical than what is claimed in the paper. Of course the core of the model relies on mass conservation, but most of the sink/source contributions are based on empirical approaches. This clearly appears when reading appendix A: melting is based on a temperature-index approach and does not care about the actual distribution of enthalpy within the sytem; the run-off relies on a matrix flow approach; the wind-drift is also based on laws relying on several parameterizations; on the contrary to what is written in the paper, I don't think that snow densification is modelled through the iterative resolution of mass and momentum conservation (i.e. $\partial\rho/\partial t + \mathrm{div}(\rho v) = 0$ and $\mathrm{div}\,\sigma + \rho g = 0$), but via a semi-empirical formula; same goes for firn densification (see next paragraph)... This results in a large amount of parameters, whose values have to be set. You claim that only two parameters, a and e, require calibration as all other values can be found in literature. I found this argument rather poor as, most of the time, values proposed in the literature are poorly constrained and are usually valable only in a very specific context which does not necessaily correspond to yours. In a way, this point is somewhat addressed for what regards the firn densification models that were derived from polar cores when you say that they can be apply in your context due to low firn temperature and low accumulation, mimicking polar conditions (l. 176). I am not saying that these empirical approaches make the study worthless, but I would assume more clearly this state of fact in the manuscript and avoid arguing that it "has the advantage not only that the model is less site specific but also that it can be applied to sites with a small amount of data, like most of the high-altitude sites". Regarding the calibration of parameter a, I find the description of the calibration procedure a bit confused (see specific comments). Also, you claim that "the parameter γ' that governs firn densification rate in AR, instead, does not need calibration since it is fixed once the mean accumulation and MAFT are set". But then, you explain that the value of this parameter was fixed by running AR in steady-state condition such that there is no discontinuity of the densification rate between the first and second stage of densification. But this is a form of calibration, right ?

My second main point regards the firn densification. I really have a hard time to understand how the density of firn and its evolution is actually computed. If I understood well, you get a single value of density per layer of firn in the multi-layer version and one single value for the whole firn column in the one-layer version ? But, in the AR model, density is an explicit function of overburden pressure, which itself should be a continuous function of depth.

Therefore, you should get a continuous profile of density with depth, which seems to be the case when looking at Fig. 4. I can somehow understand that, in the multi-layer case, the density of a given layer is obtained considering the weight of overlying layers. But how do you get a value for the single layer case ? How is overburden stress defined in that case ? If it is an averaged value, then how is the firn column over which the average is made defined ? Is it the whole column from firn/snowpack interface down to the bedrock ? I think this needs to be clarified in Section 2.4. At the very least, you should state explicitly how overburden pressure is calculated.

One of the consequence of my lack of understanding of how firn densification is actually computed is that I do not understand any of the Figures where density is plotted as a function of the layer deposition year (i.e. Figs 8, 9, 11). I can understand a plot of density versus depth (as in Fig. 4), but the plot of density versus year of deposition does not really make sense to me. Indeed, if one considers the density of the layer corresponding to snow deposited on a given year, this density will keep increasing over time due to the ever-increasing weight supported by this layer.Thus, the density of layer deposited, say, in 2005 will be smaller if assessed in 2015 than if assessed in 2020. There is an attempt of explanation of this point in l.355 to 359, but it is not very clear to me. I would reformulate to make it much clearer.

Regarding the conclusions of your study, I think that your main result, i.e. that the snow that is preserved and contribute positively to mass balance is the snow that fall in summer months as winter snow is eroded by wind, is an interesting result, but most likely very site-specific and cannot be extended to the scale of a whole massif or mountain range. Therefore, I think that the sentence (l. 421 to 424) "This, in fact, could lead, as already suggested by Alean et al. (1983), to a counterintuitive response in a scenario of higher temperatures, since the increased melting and the reduced fraction of solid events with respect to total precipitation will be accompanied by a reduced snow erosion. An increase of the positive net balances may in turn influence ice avalanche activity" should be reformulated to make it clear that this is specific to Colle Gnifetti. In addition, I don't really see the link with ice avalanche activity.

Furthermore, I find some of your statements in the conclusions Section a bit too strong. I think this Section requires a fair amount of rewriting.

On the structure of the paper, I think it is generally well-structured apart for Section 3.4 which gathers information regarding the calibration of parameters a, e (which is not calibrated in the end), and γ' (which is allegedly not calibrated but actually is), as well as values of model parameters that are fixed based on the literature. My opinion is that the latter should rather go in the corresponding subsections of Section 2 (especially Section 2.4) where most of the chosen values for the other model parameters are already given. I would keep section 3.4 to describe the calibration procedure of a and γ' only. And I would rename it as "Calibration of model parameters", or something like that.

Finally, regarding the form of the paper, although I am not a native English speaker myself, I think that the paper is rather well-written, although some sentences sound a bit odd to me (see specific comments). For example, the way you use "Accordingly," (lots of occurrences in Appendix A among other) to specify that you are following the approach of a previously mentioned reference does not sound natural to me. But I might be wrong on that point. Also, I think that a lot of commas are missing. I have noted down some of them in the specific comments but it is far from being exhaustive.

Specific comments

P1 L8: "We observed" → I would reformulate to make clear that this is a modelling result and not an observation.

P1 L18: polar regions.

P1 L23-25: This sentence is a bit complicated. I would reformulate.

P2 L26: glacier melt

P2 L29: "and release with implications" → and release, with implications

P2 L31: assess

P2 L40: "both components also in basins that in the present are highly glacierized." → both components, even in basins that are currently highly glacierized.

P2 L45: "In addition they are" → In addition, they are

P2 L47: "to reproduce the important processes" → to reproduce important processes (?)

P2 L47: "In the present work we give" → In the present work, we give

P2 L50: "momentum balance and rheological equations" → I don't really see any equations related to the momentum conservation ($\mathrm{div}\boldsymbol{\sigma} + \rho g = 0$) or firn rheology ($\mathbf{S} = \boldsymbol{\sigma} + p\mathbf{I} = 2\eta\dot{\boldsymbol{\epsilon}}$)

P2 L50: "and they estimate the snowpack and firn characteristics" → and they govern the evolution of the snowpack and firn characteristics

P2 L55: "it provides more insight on" → it captures better (?)

P2 L59: "In order to test the model a high" → In order to test the model, a high

P3 L62: "in the following way" → as follows

P3 L62: "we provide" → we present *or* we describe

P3 L63: "and discussion" → and discuss them

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

P4 L100-101: "the version presented in this work includes the effect of wind both on mass balance and densification" → For mass balance, it is only as a sink term (erosion) as wind re-deposition is not represented

P4 L105: properties

P4 L113: Point (2) is not very clear, could you precise ?

P5 L122: I would stress the fact that multi-layer refers to multi-layer of firn and that the snowpack is always treated as a single layer (on the contrary to more detailed snow models such as SNOWPACK or Crocus).

P5 L119: "In both cases we neglected" → In both cases, we neglected

P5 L119: "In order to separate snow from firn we refer" → In order to separate snow from firn, we refer

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 ?

P7 L170: "HL was optimized" → HL model was derived from cores...

P7 L171: "AL" → AR

P7 L171: "AR was optimized" → AR model was derived from cores...

P7 L176-177: I would reformulate this sentence. What you mean is that the low accumulation and low MAFT observed at CG justifies the use of the AR and HL models as such, despite the fact that they were derived for polar conditions while CG is an alpine site.

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

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

P8 L184: "The overburden P" → The overburden pressure P

P8 L186: "(see Appendix A for the expression of a_c and Z)" → You are referring to Appendix A3 (be more specific about the appadendix, here and elsewhere in the text). Also a_c is denoted a in Appendix A3, which is not consistent.

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).

P8 L199: Idem, I would put the information regarding the value of ρ_D here instead of Section 3.4

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).

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

P9 L207: "At depths higher than z_M" → This formulation is confusing. I suggest 'deeper than z_M' or 'below z_M'

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.

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.

P11 L260-261: "datum" → Although, it might be correct from a grammatical point of view, this formulation sounds quite odd to me. I would suggest data or information

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

P14 Section 3.4: See Major Comments

P14 L295: "The parameter a" → I would remind the physical meaning of this parameter

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 ?

P14 L304-306:"Its value is in fact chosen in order to have a continuous densification ratebbetween 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 ?

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

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

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

P15 L324: What is the SNOTEL network?

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 ?

P15 L345: "are provided in Fig. 6" → You should state or remind the time period of simulation from which Fig. 6 is obtained.

P17 L351: I am not sure that the median of wind speed gives a good picture of how windy a month was. But I have nothing else to propose.

P17 Section 4.4.1: As explained in the Major Comments, I think that this Section needs a fair amount of rewriting and, therefore, I do not make specific comments here.

P18 Section 4.4.2: Same as above.

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.

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

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.

P21 L395: "to fully represent the site, besides, ice core..." → to fully represent the site. In addition, ice core...

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

P21 L415:"On the contrary, it is less acknowledged in other fields" → I would remove this sentence.

P21-22 L.421-424: See Major Comments.

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

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

P23 L451:"that causes the two..." → that are very likely responsible for the significantly different behaviors of the two ice cores...

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

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.

P24 L473:"optimized" → derived from (?)

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.

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

P25 L486:"therefore moving the boundary of the model from surface accumulation and density to hourly meteorological series" → This is an interresting 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.

P25 L489:"Both the two model's versions have a parsimonious parametrization, with only two parameters (a, e) to be estimated." → I find this sentence largely overstated. Your model (both versions) largely relies on parameterization, see my Major Comments.

P25 L490-491:"All the other parameters were, instead, taken from literature" → Maybe, but most of them are far from being well-constrained !

P25 L491-493:I find this argumentation a bit awkward. It is like if you were arguing that it is better to have a model that performs badly whatever the conditions than to have a model that has good performances only when used in its domain of validity. It is not very convincing.

P25 L495:"we presented a general modelling that includes all the selected processes" → That is a tautology, isn't it ?

P25 L506:"and the spatial variation of solar radiation" → Not only the spatial variation but solar radiation at all.