Comment on tc-2020-357

Anonymous Referee #2

This paper presents a model for time-dependent snow and firn dynamics, and then applies it to a particular site, for which weather-station data are extrapolate to provide the model forcing, and ice-core records are used to compare the model against.

Although the paper was quite interesting to read, I was left a bit underwhelmed by it and am not convinced by the widespread applicability or usefulness of this model. My concerns can be summarised as follows: (1) The model seems designed to be incorporated within a large-scale hydrological model, yet it is used for analysing a very specific location; the model/application do not seem appropriate to each other. (2) There are some problematic aspects of the model. (3) The model contains a lot of questionable parameters and only a very few of them are fitted, or have any discussion.

To elaborate,

(1) I ended up being a bit confused by the purpose of this paper. If the intention is to compare meteorological data with accumulation / density data from an ice core at a specific site, then it would make more sense to use a model that explicitly considers depth dependence in the snow/firn. In fact, the presented ODE model seems to be used in a convoluted way to try to do this, but it seems to me a model that actually resolves the variation of density with depth would be much more suitable for this (and there are various such models in the literature).

On the other hand, it seemed from the introduction that the purpose of this model is to provide a more streamlined approach to modelling snow/firn processes that might be included on the catchment scale to think about questions such as the timing of glacial runoff. I sympathise with this goal, since for such application modelling the details of things like firn compaction and glacial flow may not be feasible (or necessary). But the

merits of such a model would be that it could be applied spatially, and it therefore needs to do things like conserve mass - which the current model does not (the wind-eroded snow seems to disappear into thin air, as does the run-off). The calibration and test of such a model would also seem more appropriately done on the catchment scale.

(2) The model does not possess a steady state for the firn depth (the only term that can decrease the firn depth is melting and runoff of the firn, which can only occur if there is no overlying snow-pack). This seems problematic to me, since the results of the model will depend on how long it is run for. A term that allows for the firn to transition to glacial ice would be needed I think.

(3) The length of the notation table in the appendix (pages 23-26) speaks for itself. There are a huge number of processes that have been parameterised in this model, and given the minimal outputs that come out of it and are compared to the data - together with the fact that the model-data comparison in figure 6 does not actually look all that good - I was left unconvinced what we learn from this. I would have thought that at least as good a fit to the data could be achieved with a simpler model that involves many fewer parameters. It is stated in the conclusions that more sensitivity analysis is forthcoming, and it is possible that by combining this study with other data and/or model runs, a better agreement could be obtained and/or more robust conclusions could be drawn from the model.

Having written quite a negative review, I will note that I did learn something interesting from this paper - the fact that the majority of accumulation at this site occurs from summer snow-fall, due to the need for surface melt/refreezing in order to harden the crust and prevent wind erosion. The counter-intuitive response to warmer temperatures that could result from this was new to me, and is interesting. However, that does not seem to be a key result (or at least not a new result) of this study.

Specific comments

1. The paragraph starting on line 33 seems appropriate for a funding proposal, but seems a bit odd for a scientific paper. Why not let the scientific arguments speak for themselves? The absence of literature on a subject does not necessarily make it an important subject to study (it could be that there are relatively few studies precisely because this is a niche area that is not very important!). That being said, the number of papers with these very specific keyword combinations actually seems larger than I would have expected, so I don't think this paragraph makes its point very well in any case.

2. Line 68 - 'volume with unitary area' does not make sense. Perhaps you mean 'volume per unit area' ?

3. I am confused by the distinction between h_S and h, and it would help to label h in figure 1. Is h trying to account for the case when there is more water than would fill the snowpack, so a layer of water/ice forms on top of the snow? In that case, it seems odd to call it the 'height of the snowpack'.

4. Given the apparent intention that this model could be integrated in catchment models, the assumption that wind-eroded snow simply 'disappears' seems odd (i.e. there is no source term due to wind deposition). I can see that this might be appropriate for understanding an ice core site on the top of mountain, but it seems unlikely to be widely appropriate.

5. The description of the model in section 2.1 and 2.2 that is in the appendix (equations (A1a-A1d)) is, in my opinion, much clearer and more concise than the one in the main text. e.g. the meaning of each term on the right hand side of (1a) is not explained in the main text - such as that the third term represents a temperature-index model of melting. I'd suggest presenting the model in the main text more like the way it's presented in the appendix (while specific details of the parameterisations could be put in the appendix if desired).

6. The representation of runoff in (1b) as alpha*K_w seems highly questionable to me. Surely the value of alpha would vary depending on factors like the slope, the permeability of the firn underneath, and also the transport of water from other neighbouring areas of the snowpack?

7. Equation (1d) claims to include 'densification due to overburden stress'. I don't see how it does this, since there are only terms proportional to wind speed (i.e. 'drifting snow compaction') and due to the addition of new snow.

8. I could not follow the procedure described in the paragraph starting on line 108. Step (3) seems to be the same as step (1). I think this could be explained more clearly. It might be the sort of thing that could be put in an appendix, since it is very algorithmic, but it also seems to point towards a problem that the model is trying to do more that can really be accommodated with the ODEs in equations (1) and (2) - the need to keep track of time-stamps when each deposition event occurred effectively means trying to resolve different layers in the snowpack.

9. Line 136 - please give your precise dates of 'water year' in this context.

10. The use of the Dirac delta function in equations (2) is not really correct; the 1/dt should not be there (a 1/dt effectively arises from discretising the delta function in time,

but as written these equations are in the continuous time limit, and the delta function already has units of 1/time - note the delta function is really infinite when its argument is 0, not 1).

11. The firn layer in the model does not have any way to decrease in size other than through melting, which can only happen when there is no snow on top. Should there not be a term that represents the loss of firn into the (presumably) underlying glacial ice? The reason I think this is problematic is that a continued run of this model is simply going to result in h_F growing indefinitely - in (2f) that means the final term will have little effect and you will end up with the firn density becoming equal to the ice density (effectively you are modelling the whole ice column).

12. In (3) - how is 'overburden pressure' defined? The overburden pressure obviously varies with depth, but since you are trying to model a column of firn this must represent some average? There are a large number of parameters here - activation energies and so on - and given the lumped nature of the model it seems to me this model is trying to capture many more details than you need to. Could a simpler model with many few parameters not give a similarly good fit to the data as in figure 11?

13. I did not understand the discussion of temperature profile in section 2.2.2. The model does not resolve depth dependence (at least in the way it's described) so I could not see how the temperature profile is used.

14. Line 220 - what data actually come from the ice core? It sounds like one of them has a profile of density with depth, and figure 11 suggests that from the others you infer annual accumulation rates? But this was not clear.

15. I had quite some difficulty following the procedures for extrapolating the precipitation and wind-speed data to the study site. I think this was particularly not helped by some confusion between the acronyms CG and CM, which are perhaps used interchangeably for the same thing(?) - see particularly, for example, the paragraph starting on line 265.

16. In section 3.4, there is a quite important temperature index parameter (a) that seems to be calibrated by application to a site MRZ that has a very different elevation and geographic setting to the site to which the model is being applied (judging from the map in figure 3). Is there some reason to think that the same value would be appropriate?

17. In figure 5, I think the model for density must be being used in a different way to what's described in section 2, since it now seems to have depth dependence. I can imagine how it might be extended to account for depth dependence, but then given the number of parameters and processes in the densification model, this fit to the ice-core data seems quite disappointing to me. (Note gamma' only really controls the densification

in the upper part of the core, not the lower part, so perhaps considering the choice of other parameters would be worthwhile).

18. Figure 6 shows results for the annual accumulation obtained from the snow-firn model, but it is not clear what output of the model this actually corresponds to. Which terms in the equations are considered the 'accumulation' here? Is it net of the wind erosion and run-off?

19. The results in figure 6 almost seem to show an anti-correlation between model and the observations! This needs to be discussed more I think.

20. Figures 7-10 could usefully be combined into the same figure so they can more easily be compared with each other. I could not find an explanation of what the dots in these figures mean.

21. Line 441 - the expressions for E_1, E_2 and E_3 cannot be correct in their units, given the way that they appear in the expression for U.