Review of: *On the Contribution of Grain Boundary Sliding to Firn Densification - an Assessment using an Optimisation Approach*
by Schultz, Müller, Gross, and Humbert

General Comments:

This manuscript is much improved from the previous version – the scientific arguments are clearer and the manuscript is much easier to follow. I thank the authors for their careful consideration of the points I raised previously. I am pleased to recommend it for publication in the Cryosphere after a few minor points are addressed.

My biggest remaining concern regards melt: the authors mention that their model does not include melt and as such it is not considered, which I think is appropriate for this study. However, despite the authors' response, "We assume that the relatively low number of firn profiles influenced by melt compared to the overall number of sites does not affect our results", a quick glance at the map indicates that quite a number of the sites (in Greenland, at least) that are considered in this study do in fact experience melt each year. This study specifically considers densification near the surface, and it makes a strong case that grain boundary sliding is an adequate descriptor of the physical processes at play. However, in melt areas meltwater refreezing in the near surface firn is an additional (and potentially large) densification mechanism. I do not expect the authors to adapt their model to include melt, to try to determine if grain boundary sliding is still the correct physical descriptor in melt zones, or determine which of their sites do and not experience melt. However, I do think it would be appropriate in the discussion section to add a paragraph of how consideration of meltwater refreezing affects the authors' results – would their conclusions be the same? Would they expect their correlations to be better if only dry sites were considered? I think it is appropriate to include just a short bit of discussion, leaving questions open to be further investigated.

Aside from that point, I have a number of line-by-line comments that I expect will be easy fixes that will improve the readability of the paper.

Line by line comments:

Line 10: parameter → parameters

L53: Grammar on point (iv) is incorrect; do you mean 'how a modification of the constitutive relation by Alley (1987) could lead to an improvement of the description'?

L58: comma after investigations

L59: approach → approaches

L60: Change to 'Along with a large number...'

L69: Take out 'which incorporates the factor D_{BD},' here because you describe later in the paragraph.

L73: I suggest changing to: "found in literature (e.g. Maeno and Ebinuma) and is further discussed in Section 2.2."

L86: Remove 'It has to be mentioned though, that' (colloquial phrase) – either just start with 'Alley (1987) …' or you could say, "We note that Alley…"

L87: Sentence is oddly worded; I suggest changing sentence structure to: "It is feasible that the strain rate due to grain boundary sliding decreases while it increases due to the influence of other physical processes"

Section 2.2, first paragraph: I appreciate the addition of this description of your study; it is succinct and clear.

L104: 'Further allows us' (word 'us' is missing)

L122-123: "The strain rate due to grain boundary sliding is therefore higher at the critical density when using the modification." → Because you have several variations yourself, please be specific describing this – I think you mean 'when using the Breant et al. (2017) modification'?

L134: Be specific of what you mean by constant values – constant values of temperature, accumulation rate, and surface density, I think? Are there others?

L134: How long is the spin up? What is the time step for the model runs?

I was initially concerned that your modeled profile would be affected by the steady-state spin up, but then I saw that you limit yourselves to 1958 and younger firn. Please expand on your method a bit here: I think that the 1958 surface is a modeled surface; is this correct? Or is the core dated and you know where the 1958 surface is in the observations? I am guessing that the 1958 date comes because that is when the RACMO RCM data begins; if so you should just state that clearly, e.g. something along the lines of: 'For all of our model-data comparisons, we limit our analyses to the firn shallower than the depth horizon of the modeled 1958 surface. This is because the climate data we use to force the model (RACMO2.3) begins in 1958. By imposing this limit, we ensure that the modeled firn profile used for comparisons is not affected by the spin up process." (and then continue your explanation of the alternate case of restricting to less than critical density). (and I do now see this in the Fig. 1 caption, but should be in the text)

L149: Remove 'as well'

L151: Change 'disregard' to 'exclusion'

L160: Change to: "This presents the problem of finding an appropriate surface-density boundary condition for the simulation"

L161: Offset “especially near the surface” with commas

L164: Remove: ‘This method proofed to work well throughout the study’

I think you could be a bit more explicit stating that you tried all 21 surface density values with each of the 250 values of C; this is no small feat so I think it deserves to be highlighted a bit more. I suggest something like: remove ‘following our approach’, and: “For each of the 250 values of C for each variation, we tested 21 different…”

L165: The sentence starting: “Applying the method to all…” – I am not sure what you are getting at with this; either remove or rewrite/clarify.

L179 – point 4 – should be: ‘must not exceed’

Figure 1 caption: specify that the colored dashed lines are model results, e.g. ‘Colored dashed horizontal lines show modeled horizons of firn deposited in the indicated years’

L180: Specify that it is mean surface mass balance

L189-L190: Does this mean you should restrict your analyses to firn younger than 1979 for Antarctic cores (as described in Section 2.2)?

L212: I realize here that you are responding to the other referee’s comment asking for a statement to this extent, but as it is written it sounds as if you are doing the spatial interpolation, where in reality is RACMO that is doing a spatial interpolation of ERA data – perhaps, “The spatially-interpolated RACMO fields have the potential to include systematic errors” or something like that – and you could cite (in which they specifically mention RACMO bias) e.g. *van Wessem, J. M., van de Berg, W. J., Noël, B. P. Y., van Meijgaard, E., Amory, C., Birnbaum, G., Jakobs, C. L., Krüger, K., Lenaerts, J. T. M., Lhermitte, S., Ligtenberg, S. R. M., Medley, B., Reijmer, C. H., van Tricht, K., Trusel, L. D., van Ulft, L. H., Wouters, B., Wuite, J., and van den Broeke, M. R.: Modelling the climate and surface mass balance of polar ice sheets using RACMO2 – Part 2: Antarctica (1979–2016), The Cryosphere, 12, 1479–1498, https://doi.org/10.5194/tc-12-1479-2018, 2018.*

L213: Can you be more specific about these systematic errors? I don’t think for this paper you want to go down the road of investigating systematic errors in RCM outputs; perhaps it would be more appropriate for you to simple state something like, “any error in the RCM forcing data will manifest itself as error in the modeled firn profiles; these error analyses are outside the scope of this paper”.

L220: change to: “We use second example to illustrate how…”

Figure 3 caption: The second sentence (‘Colour coded…’) does not make sense – reword/rewrite for clarity

L257: consider removing ‘easily’ – it would indeed be easy to add the Freitag impurity model, but I don’t think getting the model to realistically simulate layering is easy.

L259: I don’t follow what you are saying that the activation energy is temporally averaged? The Freitag activation energy, or the Arrhenius one? Didn’t you just say that you are not including the Freitag equation?

L262: I don’t follow here what you are trying to say about running mean – I agree that it neglects information, but you are simulating the firn at annual resolution, while the layered firn you are comparing to is much higher resolution than that. Are you assuming that deviations due to layering will be equally distributed positive and negative? I am not saying your method is wrong, but I think you need to explain your thinking more clearly. I would omit the part about the Freitag equations (there are some number of people in the firn community who don’t agree that impurities are the source of layering), and stick to a simpler story: layering exists in real firn, your model does not simulate that (most firn models do not), you are still comparing to the raw data, and here is why.

L266: either state here that the neumann condition is set to zero, or reference section A4.

L302: Dependency on what?