Answer to Editor decision, 28 Oct 2021

Dear Kaitlin,

Thank you again for editing our manuscript. As noted by you and the referees the grammar of our manuscript needed major revision. We have commissioned a professional language editing service to help us revise the manuscript. The new version is greatly improved with regard to the language. We also discussed all further remarks raised by the referees. Due to the comment of Referee #1 regarding the title of the manuscript and how it could result in confusion, we decided to change it.

On the contribution of grain boundary sliding type creep to firn densification – an assessment using an optimization approach

With the new title we want to highlight that the paper is not about an investigation of the process of grain boundary sliding itself, but about the functional relation describing this type of creep. We hope you approve of this.

Sincerely,

Timm Schultz on behalf of the co-authors

Response to Anonymous Referee #1, 01 Oct 2021

Dear colleague,

We thank you for reading our manuscript carefully again. We discussed all of your remarks. You can find point-by-point responses to them below. We contracted a professional language editing service to improve the grammar and readability of the manuscript. We thought a lot about the title of the paper and decided to change it to avoid confusion about the topic of the manuscript as you suggested.

On the contribution of grain boundary sliding type creep to firn densification – an assessment using an optimization approach

With this change we want to clarify that we don't investigate the process of grain boundary sliding itself, but its description. The micro-mechanical description of grain boundary sliding, like other forms of creep, for example Coble creep (Coble, 1970), is established by a functional relation of a certain type (see for example Goldsby and Kohlstedt, 2001). Alley (1987) adopted grain boundary sliding to firn. We assess the functional relation used for the description of this type of creep. We hope you approve of this new title.

Sincerely,

Timm Schultz on behalf of the co-authors

General Comments

This is a greatly improved version of the original paper. The authors have certainly put a lot of effort into answering the points made by both referees. There are still, however, places where further editing is needed so that the research is presented clearly. Part of the problem is that the English still needs a lot of improvement, but I think there is also an underlying confusion about what the paper is really about. For example, in the Abstract (lines 1 to 4) the authors draw a distinction between the Alley model for stage 1 densification, which they describe as a model for grain-boundary sliding, and models such as the Herron and Langway model, which they imply are not descriptions of grain-boundary sliding. But in fact the stage 1 expressions in Herron and Langway type models are generally considered to be models for grain-boundary sliding, they are just not developed by consideration of the detailed physics of the process. The distinction the authors are actually making is between an upscaled microphysics model (the Alley model) and macroscale Herron and Langway type models for the same process. The paper is not about the contribution of grain boundary sliding to firn densification - it is about whether the Alley model is a good representation of this process. I think the authors know this; it is just a question of editing the text and changing the title to avoid confusing the reader. I would suggest that the authors work through the paper, look at each mention of "grain boundary sliding" and consider whether they really mean "the Alley equation". For example at line 50 it would be better to write "In this study we aim to evaluate (i) whether the Alley equation is suitable for the simulation of firn densification at low density (ii) how the Breant et al. (2017) modification to this constitutive relation affects simulation results etc". In general the whole paper needs to be reviewed carefully to make sure that the text always says what the authors want it to say.

> As the referee suggested we worked again through the manuscript and reviewed in which way the term "grain boundary sliding" was used. Where it was necessary, we revised the manuscript to distinguish more clearly between the process of grain boundary sliding and Alley's description of this process. This includes the example in former line 50 mentioned by the referee. We further changed the title of the manuscript to avoid confusion.

Specific Comments

l. 45 the authors seem to imply that if the Alley equation does not reproduce stage 1 densification exactly this might indication that grain boundary sliding is not the only process acting in stage 1. But it could also indicate that the Alley equation is not an adequate description of grain boundary sliding.

> We are not aware of any firn densification model that reproduces stage 1 densification exactly nor are we aware of any model which reproduces nature exactly. As the addressed paragraph leads to confusion we decided to remove it from the manuscript.

l.144 This is a somewhat confusing sentence and needs to be rewritten. The authors have mixed up two things (i) that the factor 5/3 is derived from N/6 where N is the number of bonds and is approximately equal to 10 rho/rhoi and (ii) that the factor (1-5rho/3rhoi) implies that the densification rate goes to zero at a critical density rhoc = 550 kg m-3. Note also that relative density could mean rho/rhow so "density relative to ice" would be better.

> We note that the derivation of the factor 5/3 is somewhat different than described in this paragraph. Especially as theoretical densest packing, hexagonal or cubic, would result in a higher value of the critical density and a coordination number of $N = 12$. As established by Anderson & Benson (1963) the critical density corresponds to "close random packing". Our description was imprecise there.
>
> However the factor $1 - (5\rho/3\rho_{\mathrm{ice}})$ leads to a strain rate of zero, when the critical density is reached. We narrowed our description to this point. The reader might look up the exact derivation of the factor in the original publication. We further established that density relative to ice density is meant here.
>
> *The factor of* $1 - (5\,\rho/3\,\rho_{\mathrm{ice}})$ *causes the strain rate due to grain boundary sliding to decrease with increasing density until it vanishes at the critical density of* $\rho_c = 550\,\mathrm{kg\,m^{-3}}$. *When the critical density is reached, close random packing is established (Anderson and Benson, 1963), and grains can no longer slide against each other; thus, the process of grain boundary sliding ends. Other deformation processes, in particular dislocation creep (Maeno and Ebinuma, 1983), result in further densification with increasing stress.*

l.285. It might be worth mentioning here that Alley specifies model density equals observed density at 2 m depth whereas the authors choose to treat surface density as another parameter to be optimised.

We established this in the revised version of the mansucript.

Although Alley (1987) simulated the density starting at a depth of 2 m below the surface, we included this domain in our simulation so that we could apply transient surface forcing to our model. To find suitable values of the surface density, we included this parameter in our optimization.

l. 400. Are the authors saying the geothermal heat flux is negligible for all sites, hence the Neumann lower boundary condition on temperature can be used?

While the geothermal heat flux is certainly not negligible for all sites in general, its influence on the uppermost 25 meters of firn is indeed negligible to the best of our judgement. The geothermal heat flux acts on the ice base several hundreds of meters up to thousands of meters beneath the analysed firn domain and is thus a kind of far field boundary conditions of the ice sheet, whereas we study here the firn column only. In order to support this assumption, we evaluate observed firn temperature profiles that are found in the literature.

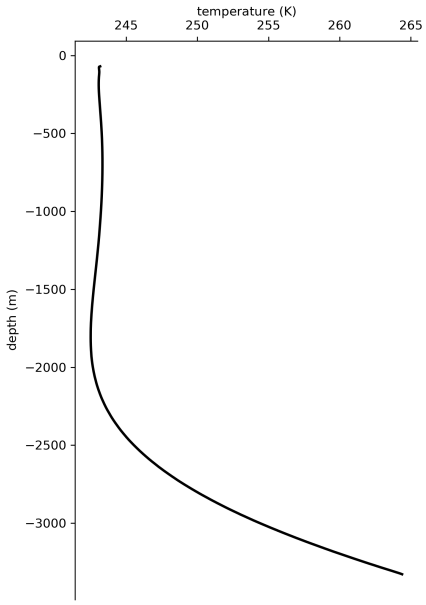

Figure 1: Figure 1. Temperature profile of the West Antarctic Ice Sheet Divide Deep Borehole (Cuffey and Clow, 2011). The data were acquired in December 2011 at the WAIS–D (N -79.4676°, E -112.0865°).

Figure 1 shows the temperature profile for the Deep Borehole at the West Antarctic Ice Sheet Divide measured in 2011 (Cuffey and Clow, 2011). Starting at a depth of about 2000 m below the surface the influence of geothermal heat is obvious. The ice temperature rises from about 242 K to 264 K at the ice base. However, in the upper part of the ice column the influence of the geothermal heat flux declines fast. The temperature is determined by the mean surface temperature. We therefore conclude that the geothermal heat flux from the ice base is negligible in our study focussing on the upper 25 m of the firn column.

On the other hand the question can be raised to which extent the temperature differs at the depth of 25 m due to the influence of varying surface temperatures. The following figure shows temperature profiles of different locations. They all indicate, as well as the results shown in Orsi et al. (2017) and Vandecrux et al. (2021), which are already cited in the manuscript, that the change in temperature is small at a depth of 25 m below the surface. Note that even in Figure 3 (d), at the Styx location, where there is strong dry ablation predominant, the change in the temperature profile at a depth of 25 m below the surface is relatively small. Therefore we believe that the use of a temperature boundary condition as described in the manuscript is well justified. This is especially true as we restricted the domain of comparison to the firn influenced by the surface forcing.

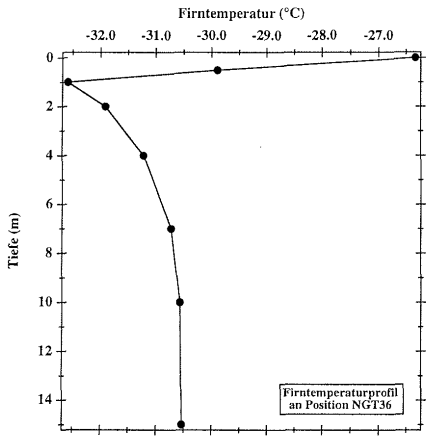

Figure 2: Temperature profile measured at position NGT36, Greenland, N 77°29', W 47°28', (Schwager, 2000)

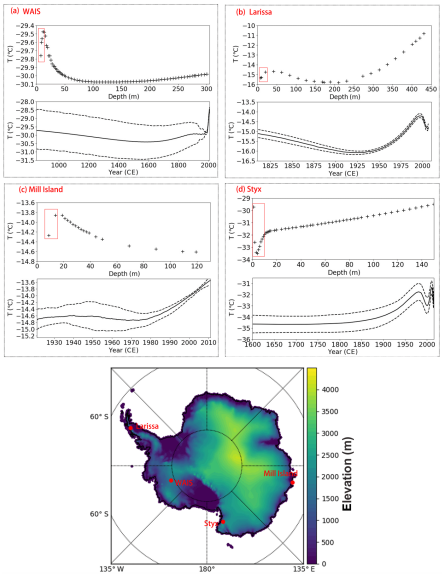

Figure 3: Observed borehole temperature and and reconstructed past surface temperatures at four antarctic sites, from Lyu et al. (2020).

Cuffey, K. and Clow, G. D. (2011). Temperature Profile of the West Antarctic Ice Sheet Divide Deep Borehole, Version 1. NSIDC, Data Set ID: NSIDC-0550, https://nsidc.org/data/NSIDC-0550/versions/1 [accessed: 12.11.2021].

Lyu, Z., Orsi, A. J. and Goosse, H. (2020). Comparison of observed borehole temperatures in Antarctica with simulations using a forward model driven by climate model outputs covering the past millennium. Climate Past, 16, 1411-1428, https://doi.org/10.5194/cp-16-1411-2020.

Orsi, A. J., Kawamura, K., Masson-Delmotte, V., Fettweis, X., Box, J. E., Dahl-Jensen, D., Clow, D., Landais, A. and Severinghaus, J. P. (2017). The recent warming trend in north Greenland. Geophysical Research Letters, 44, 6235-6243, https://doi.org/https://doi.org/10.1002/2016GL072212.

l. 560. The authors state that the use of a global value for the Alley parameter leads to worse simulation results than existing firn density models (by which they presumably mean the Herron and Langway type models for stage 1 densification). This is an important result, even if it is not what the authors wished to discover. The reader is left wishing that instead of only comparing results for the 4 variants of the Alley model the authors had included a benchmark macroscale model (for example Herron and Langway) and compared all 5 models.

Actually the presented study started with the idea of checking if we were able to find a global factor for the constitutive relation by Alley (1987), which led to good simulation results. This was motivated by the method used by Alley (1987) to determine the value of the amplitude of grain boundary obstructions. Instead we found that the optimal values for the individual sites scatter in a relatively wide range. So we asked ourselves why this is the case. We found indications that the optimal values depend on the mean surface temperature and the mean surface mass balance to some degree. In our opinion this is an interesting finding. This result can stand for itself.

We see no advantages in running the model with a global factor for the constitutive relation by Alley (1987). In this case the advantages of the micromechanical modeling approach would be lost. A global factor, for example the mean or median value of the location specific factors, would lack physical meaning.

Response to Referee #2, 11 Oct 2021

Dear Max,

Thank you for revising our manuscript carefully again. A special thank you for your comprehensive remarks regarding the grammar. They helped to improve the readability of the manuscript greatly. Additionally we have commissioned a language editing service to help us improve the manuscript You can find point-by-point answers to all your remarks below. Due to the review of the other referee we decided to change the title of the manuscript.

On the contribution of grain boundary sliding type creep to firn densification – an assessment using an optimization approach

With the new title, we want to clarify that we don't investigate the process of grain boundary sliding itself, but the functional relation describing this type of creep. We hope you approve of this.

Sincerely,

Timm Schultz on behalf of the co-authors

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.

> We agree with your point that surface melting, melt water percolation through the firn body, refreezing and the general interaction of melt water with the firn is an important part within the field of firn densification modelling. As suggested by you we added a paragraph on this subject in the discussion section.
>
> *This study analyzed only dry firn densification. The current model cannot handle melting. We accommodate this feature by setting the annual mean surface mass balance at the investigated sites to be strictly positive (Sect. 3.1). However, this limitation means that we cannot ensure that no melting occurs over the course of a year. The results shown in Fig. 8 illustrate how this limitation affects the optimization results. The limitation is problematic, especially in recent years, when melting occurred over almost the entire Greenland Ice Sheet (e.g. Nghiem et al., 2012). The simulation of meltwater percolation through the firn and its interaction with firn densification is important, especially in the upper part of the firn body (e.g. Vandecrux et al, 2020). The proposed method could be improved by the application of this model approach in future investigations. However, we identified some correlations between the optimization results and the surface mass balance.*

Line by line comments

Line 10: parameter → parameters

Corrected as suggested by the referee.

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"?

We reformulated the sentence (L39). It now reads:

. . . and (iv) how the constitutive relation of Alley (1987) might be improved.

L58: comma after investigations

We added the comma after "investigations" in line 44. We expect this line was meant.

L59: approach → approaches

It seems some mix up happened with the line numbers. We assume the comment refers to the sentence in line 44, which we reformulated. It now reads:

In contrast to these experimental investigations, a data-driven model approach is used in our study.

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

We decided to change and split the addressed sentence for better understandability and readability.

Since the original study of Alley (1987) was published, the amount of available data has increased greatly. The data include a large number of firn profiles and forcing data, and they allow us to simulate firn profiles at very high quality using additional modeling techniques.

L69: Take out "which incorporates the factor DBD," here because you describe later in the paragraph.

Corrected as suggested by the referee.

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

We altered the sentence as suggested by the referee.

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..."

We adopted the suggestion by the referee.

Alley (1987) suggested that additional processes contribute to densification below the critical density.

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"

We reformulated the sentence. It now reads:

Alley (1987) suggested that additional processes contribute to densification below the critical density. It is feasible that the effect of grain boundary sliding on the strain rate decreases, whereas that of other processes increases. The studies of Arthern and Wingham (1998) and Bréant et al. (2017) use ...

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)

Corrected as suggested by the referee.

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"?

We edited the sentence to clarify the modification introduced by Bréant et al. (2017) is meant here.

It was introduced to obtain a better transition to the second stage of firn densification. The strain rate due to grain boundary sliding is therefore higher at the critical density when this modification of Bréant et al. (2017) is used.

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

We added a sentence to briefly describe the forcing.

Every simulation begins with a spin-up period in which constant values are used for forcing. The model is forced with prescribed values of temperature, accumulation rate, firn density, and grain radius at the surface.

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)

The length of the spin up is determined by the time it takes to reach steady state, as we use constant forcing during the spin up. We determine steady state conditions by evaluating the change of density between consecutive time steps. If the maximum change in density between two time steps is smaller than $0.1\,\mathrm{kg\,m^{-3}}$ we assume the profile will not change significantly anymore and steady state is reached.

For spin up and transient simulation runs we use a constant value of 48 time steps per year. This information was temporarily lost due to the fact the model description moved to the appendix.

You are right about your assumptions on the domain used for comparing simulated and measured firn densities. We wanted to include only those results for which we know about past climate conditions. In this way the spin up plays a subordinate role.

We edited the section based on your suggestions to include this information and to highlight more clearly what we wanted to express there.

L149: Remove "as well"

Corrected as suggested. The sentence now reads:

As the implementation of our model is efficient and the approach is simple and reliable, we decided to determine the best factor C_v for the four variants of the constitutive equation by simply testing 250 values within certain ranges.

L151: Change "disregard" to "exclusion"

We reformulated the sentence it now reads:

These ranges are shown in Eqs. (6) and (7), which include and exclude the Arrhenius factor, respectively.

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

The sentence was changed as suggested.

L161: Offset "especially near the surface" with commas

Corrected as suggested.

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..."

We removed the sentence in line 164. Furthermore we altered the following paragraph based on your suggestions.

For each of the four variants and 250 factors C_v, we tested 21 values of the surface density between $\rho_0 = 250\,\mathrm{kg\,m^{-3}}$ and $\rho_0 = 450\,\mathrm{kg\,m^{-3}}$, using steps of $\Delta\rho_0 = 10\,\mathrm{kg\,m^{-3}}$.

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.

We added this part in response to a comment we received from you after the first review which raised the question why we haven't used measured near surface density values when available.

L269: In the cases where you have cores that have measurements from the near surface, why don't you just use the measured surface density from the cores? Are there cases where the surface density from the optimization scheme is significantly different than the observations from SUMup? It seems possible that your scheme could allow a surface density that is different than the observation in order to get a better overall RMSD fit. Do you use the same density at all time steps for a given site/simulation?

With the addressed sentence we wanted to justify why we haven't used measured near surface values directly. By applying the testing method to all profiles we establish comparability. However, density profiles reaching to the surface are well represented. Furthermore the use of measured values always requires some kind of adjustment of the measured density values. One can not simply pick the first measured value because the variability of the density is high at low depths.

We have adjusted the addressed sentence to clarify our point.

We used the method of testing 21 surface density values for all the analyzed firn profiles. We included profiles including measurements of the density at small depths. In this way, we established that the results are comparable. Profiles including near-surface density values are, however, well-represented.

L179 – point 4 – should be: "must not exceed"

The additional "to" was deleted.

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"

We altered the addressed part within the caption according to the suggestion for clarification.

Colored dashed horizontal lines show horizons obtained in the simulations. Horizons plotted in gray (to the right of the vertical dashed line) represent the same surfaces as those determined by Miller and Schwager (2004) during analysis of the core.

L180: Specify that it is mean surface mass balance

We have adjusted the bullet point as suggested.

The annual mean surface mass balance at the profile locations must be strictly positive.

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

Yes, this is true and we considered it in our optimisation and analysis. As this wasn't formulated clear enough before, we added some context in Section 2.2.

For firn profiles retrieved in Antarctica, climate forcing from RACMO2.3 begins in 1979. Thus, only those results located above the simulated horizon of 1979 are considered for comparison with the Antarctic firn profiles.

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.

Indeed we have performed a spatial interpolation as the profile sites do not accidently match the grid of RACMO output. We thought for completeness this should be mentioned within the manuscript. As it is an ongoing topic of confusion we deleted the sentence regarding this point in the revised version of the manuscript. It is kind of self-explanatory.

To serve the demands of Referee #1 we still mention that RACMO data may contain systematic errors. We will cite the paper suggested by you. Thank you for this useful suggestion.

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".

Please see our response to your comment on line 212.

L220: change to: "We use second example to illustrate how..."

We changed the sentence as suggested by the referee. The sentence now reads:

We present a second example to illustrate the effect of the temporal resolution of the forcing on the optimization results and why we used yearly averaged data provided by RACMO2.3.

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

We rephrased the addressed sentence.

Colored lines show the optimal simulation results for four tested variants of the constitutive relation.

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.

As suggested we removed "easily" from the addressed sentence.

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?

We reformulated the addressed paragraph as it was not clearly understable before. Please see our answer to your comment on line 262 below.

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.

We fully agree with you regarding this point. However, both referees stressed the point that small scale layering is not covered within the model before. We therefore added this paragraph. However, we reformulated it to address the problem of simulating small scale layering in firn in a more general form.

Ice core ngt03C93.2 (Fig. 1a) is an example of a high-resolution density measurement showing extensive small-scale layering. Only a few of the 159 firn profiles are of such high quality and include this type of layering. Although our proposed model works at high temporal and spatial resolution, it does not cover layering, as shown in Fig. 1a. The density profile retrieved at site 3 of the iSTAR traverse (Morris et al., 2017) (Fig. 3) illustrates that the model, if it is forced with data of higher temporal resolution, still does not cover the measured density variability. Small-scale layering of firn appears to be driven by a number of different processes (Hörhold et al., 2011). An extension of the model to cover such processes may be introduced in the future. We would prefer the approach of Freitag et al. (2013) who introduced the concept of impurity-controlled densification. Forcing data for this model are not globally available. However, the model in its current form does reproduce the mean density well, as demonstrated in Fig. 1a.

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

We added the information that the Neumann condition prescribes the temperature gradient to be zero at the profile base.

This choice raises the question of whether the use of a Neumann boundary condition set to zero at the profile base to solve for the temperature is justified for this particular model setup (Sect. A4).

L302: Dependency on what?

We reformulated the addressed sentence.

To check for possible mean surface temperature dependence of the 159 factors found by optimization, these values are plotted against each other in Fig. 7. The values of the mean surface temperature were calculated from the forcing data for each firn profile site.