Response to Anonymous Referee #2, 24 May 2021

Dear colleague,

We thank you for the careful reading of the manuscript and the constructive remarks. We have discussed all points raised by you and accordingly changed the manuscript. In the following there is a point-by-point response and discussion of the issues addressed in the review.

There is one thing we want to mention. It seems to us that we mislead the reviewers in that respect that we give the impression that we are up to determining one perfect coefficient C, which can then be used for all future simulations of firn densification. This is not the case. We are using the optimisation approach to test if the constitutive relation by Alley (1987) in its particular form can be used to simulate firn density profiles fitting observed ones. We then analyse if the magnitude of factors C leading to the best optimisation results exhibits a dependence on another system variable (e.g. surface mass balance), to see if in future, one could extend or improve the constitutive relation. Alternatively a better representation of the stress could lead to better results in the future. The study is not about finding the best factor C, but that a minimum in the optimisation process exists and how much off density profiles simulated using the constitutive relation by Alley (1987) are compared to measured firn density profiles.

Sincerely,

Timm Schultz on behalf of the co-authors

Summary and general comments:

The manuscript purports to be based on the grain boundary sliding model presented by Alley (1987), but the authors wrap all of the grain structure parameters from Alley into a single parameter C, which makes their model quite similar to other macroscale firn models for stage 1 (albeit with a different dependence on the current density). The authors do point this out eventually, but I would like more discussion about this assumption. Is it still a grain boundary sliding model, and why?

The aim of the study is not to develop a new firn densification model. This is not possible with the methods introduced and/or contradicts the idea of a physics based densification model. In this sense it is wrong to attribute the model used throughout the study to the authors. The aim of the study is to answer the following questions:

(i) Is a constitutive relation in the form of Alley's constitutive relation convenient to simulate firn densification below the critical density?

(ii) How does the modification of the constitutive relation introduced by Bréant et al. (2017), which alters the domain in which grain boundary sliding occurs, affect simulation results?

(iii) Can we identify hidden or additional dependencies in the description of grain boundary sliding?

(iv) How could an improvement of the constitutive relation by Alley look like?

In order to clarify this, we explicitly formulate these aims at the beginning of the revised version of the manuscript and discuss them at the end.

Related: The authors use RMSD to quantify the misfit between their model variants and firn-depth-density profiles – but are any of these variants significantly better than previously published firn densification models for stage 1? I think it would be a relatively simple exercise to run, e.g. the Herron and Langway (1980) or another firn model, for these sites to show that the authors' model provides an improvement.

It makes no sense to compare our simulation results to results from another model. Our model results would most certainly show better agreement with the measured firn profiles than any other firn densification model, because the optimisation process provides an optimal factor for every firn profile. The aim of the study at this point is not primarily to find a global factor C fitting all climatic conditions. We would be happy if we found one, but to evaluate if it would be possible using the constitutive relation by Alley (1987). We point this out more vividly in the revised version of the manuscript.

A pesky problem for the firn modeling community is layering in the firn. The core the authors chose to highlight shows significant layering. Like other models, their model does not capture the layering; but, I suggest that a physically based model should be better at simulating layering than the macroscale models. As the authors are claiming that their model formulation is more physically based, a bit of discussion about this model deficiency would be welcomed.

The modelling of firn layering is indeed a problem, highlighted by the growing amount of high resolution measurements, but often neglected in firn density modelling. We assume impurities influencing the deformation mechanisms in firn to drive layering on the small scale like pointed out by Freitag et al. (2013). This influence is reflected in the activation energy used to describe either the different deformation mechanisms driving the firn densification or in the activation energy used in empirical models, describing the average of all processes.

Therefore layering could be included in the presented model via the activation energy for the process of boundary diffusion. We actually conducted tests regarding this topic in the past. However, this requires information about the impurity concentration, which is not available on a global scale. The used constant value of the activation energy can therefore be understood as an temporally averaged value, leading to a mean density. By the use of the root mean square deviation as cost function, the optimisation result is however not affected when using layered firn profiles as a basis.

We address these points in the revised version of the manuscript. Additionally we include a running mean density for core ngt03C93.2 in Figure 2 a) to illustrate how the optimisation results relate to the mean density.

I would like to see a more detailed uncertainty analysis as a part of the discussion (or as its own section). Not until the end of the conclusion do the authors mention that the results will be affected by biases in firn model forcing data that comes from a regional climate model; this is true, and it should come sooner. Additionally, how do other assumptions contribute to uncertainty, e.g. the assumption of surface grain size and Arrhenius activation energy? To what extent is C just accommodating other errors – e.g. the authors assume that the model calculates grain size r accurately using the Gow formula – but errors are introduced by the boundary condition, and this formula is just an empirical relationship itself. The authors propose a linear relationship to predict C from SMB. If this relationship exists, they should include the equation for the line, and use the value of C predicted by it to run their model. How do those results compare to the outputs from the optimized model? Are they acceptable? Are they better than predicted by a previously published model?

Possible errors are indeed accommodated for in the factor C using the presented optimisation scheme. This includes random errors as well as systematic errors. The idea is to suppress random errors by using a great amount of firn density profiles and then searching for systematic errors to evaluate the constitutive relation by Alley (1987). However, systematic errors can also be introduced by other relations incorporated in the model, such as the Arrhenius equation describing the process of boundary diffusion or the formula by Gow (1969).

As the influence of the activation energy is rather great, we neglected the Arrhenius equation in two of the variants to determine its influence on the resulting factors C. We discussed its influence on the results briefly in the current version of the manuscript and will discuss it in more detail in the revised version. It has to be mentioned though, that the value of the activation energy is not determined by empirical measures. It is relatively well known as it describes the physical process of boundary diffusion. We further discuss the influence of the grain radius and its evolution in the revised version of the manuscript.

We don't want to give the impression that the factor C can be predicted from the surface mass balance. Using the optimisation scheme we generated firn density profiles matching the measured firn profiles as good as possible using our particular model setup. We then evaluate the site specific values of the factors C to assess the constitutive relation for grain boundary sliding and to find possible hidden or additional dependencies that may improve the relation. We found a possible additional dependency of the strain rate on the surface mass balance and interpret this as an insufficient description of the stress regime.

As these points were not formulated clearly enough in the original manuscript, we highlight them more clearly in the revised version.

Specific Comments

Introduction

Line 15: the first paragraph should be rewritten – it is filled with grammatical errors and does not contain citations. I would argue that cold content (Vandecrux et al., 2020) and ice lenses (MacFerrin et al. 2019) are just as important for governing melt water retention. I do not disagree that firn densification is important for meltwater retention, but I do not think this paragraph adequately makes a strong case that it is. The last sentence implies that current models are not adequate but does not provide evidence that they are not.

> We reworked the paragraph for grammatical errors. Indeed, we do not cite papers on melt water percolation and retention, but this is due to the fact that the entire work does not deal with melt water - the problem is even in cold conditions tricky to solve! Many thanks for raising to cite Vandecrux et al. (2020), we will be citing the newest work, from 2021 (Vandecrux et al., 2021), which was actually published a month after our submission, but contains very valuable data for our discussion.

L22: I would not say that the other models neglect the overburden stress exactly – for models operating at small-ish (sub annual) timesteps, they use the mean accumulation rate over the lifetime of a firn parcel (Li and Zwally 2011, 2015), which is effectively the stress, rather than the mean site accumulation rate.

> The addressed models are based on the Robin hypothesis (Robin, 1958) which states that the density change is proportional to the change in stress resulting from accumulation. This hypothesis is basically an interpretation of the linear momentum balance in the form of the one dimensional continuity equation. Hence the stress is implicitly integrated in these descriptions. Li and Zwally (2011) however point out very clearly that the static model approach by Herron and Langway (1980) will not lead to densification if there is no accumulation which is wrong and therefore needs further adjustment. Namely the integration of the accumulation rate over the firn column, which again is an interpretation of the momentum balance in a one dimensional case.
>
> In fact many of the models following the modelling approach by Herron and Langway (1980) use a description more directly linked to the stress. To clarify this and avoid misunderstandings the sentence under consideration and the following one are removed from the manuscript.

L26 – 35: Since your paper is about modeling grain boundary sliding in stage 1, I think it would be prudent for you to focus on how the models handle densification in stage 1. That is, you talk about sintering before you talk about grain boundary sliding (paragraph starting on line 32) – this is just distracting.

As we focus on grain boundary sliding the paragraph about sintering and hot isostatic pressing is removed in the revised version of the manuscript.

L29: Arnaud and Goujon are actually the same firn-densification model, Goujon just added heat conduction to the Arnaud densification physics. I think it is worth noting at this point that Anaud/Goujon used Alley (1987) equations for stage 1 densification because that is what your paper is about. Their equations for stage 2 densification are based on Maeno and Ebinuma.

Besides the models by Arnaud et al. (2000) and Goujon et al. (2003) also the models by Arthern and Wingham (1998) and Bréant et al. (2017) used the description of grain boundary sliding by Alley (1987). The authors intended to highlight this fact with the sentence beginning in line 37 of the manuscript.

"Since then the description of this process was used in various other firn densification models (Arthern and Wingham, 1998; Arnaud et al., 2000; Goujon et al., 2003; Breant et al., 2017)."

To make this even more distinct, the sentence is adjusted in the revised version of the manuscript.

L36: change to "first applied grain boundary sliding theory . . . "

The formulation is changed accordingly in the revised version.

L36: be specific of what you mean with "low density" – less than the critical density, I think.

In fact density values below the critical density are meant here. We specify this in the revised version.

Alley (1987) first applied the theory of grain boundary sliding, adopted from Raj and Ashby (1971), to firn densification at densities below the critical density of $\rho_c = 550\,\mathrm{kg\,m^{-3}}$.

L39: numerous times: cite numerous studies

The revised version of the manuscript includes additional references.

L40: "tried to" – colloquial, vague language. Hypothesized? Showed?

The sentence is changed accordingly.

For example Theile et al. (2011), by conducting experiments on a small number of snow samples, suggested that the densification is more likely driven by processes within the grain than by the inter granular process of grain boundary sliding.

L48: "in the sense of Alley" → "Following Alley,"

The sentence is changed accordingly.

Section 3.1

My general feeling with Section 3.1 (and much of section 3) is that it is distracting from the what the authors state is the goal of their paper (at the end of the introduction), which is to evaluate the efficacy of a grain-boundary sliding densification model. I think that these details of the numerical treatment help the paper achieve that goal. My suggestion would be to move this section to an appendix at the end of the paper. Perhaps I feel this way because I got lost in some of the details, which I attribute to several instances of imprecise language (See below comments). I am admittedly not familiar with the updated Lagrangian formulation, but I don't think that understanding the numerics of that numerical scheme are important for understanding the results (hence my suggestion that this section be moved to an appendix). The meat of the paper begins with section 4, so reduce section 3 to only what is necessary.

The authors would like to maintain the structure of the manuscript in its current form, but if recommended by the editor, we are flexible to adopt a different structure. Sections 3.1 and 3.2 are serving the understanding of the method as they describe the numerics and the constitutive relation for grain boundary sliding, while the remaining sections are rather short.

As the authors all have some kind of background in applied mechanics, we see an importance of a precise explanation of the numerical method and its basis in continuum mechanics. With regard to some of the results from the study an understanding of the treatment of stress and its link to the quasi static momentum balance within the model should be provided.

Furthermore we see that in other studies the description of large parts of the used models are not explained. This may become a problem in our opinion as model results are not reproducible. For example the presented method is most likely used with other model approaches but not specified as such properly. The term "Lagrangian" is often not used properly.

A precise and comprehensive model description also helps other researchers to get started in firn densification modelling in particular and in modelling in general. The cryospheric sciences attract researchers from all kinds of research fields, often not used to the established practices. This also includes for example mathematicians or computer scientists interested in efficient implementations and numerical schemes.

The description of the constitutive relation for grain boundary sliding by Alley (1987) is of great importance as it is the central part of the manuscript. However, we will revise Section 3 to establish a more precise language and improve its understandability.

Equation 1: you say you let $v_b = v$, but when you do that, the left term implies that the time derivative of the density (i.e. the densification rate) is zero – is that what you mean? I am not sure what you are getting at with this.

> "the left term implies that the time derivative of the density (i.e. the densification rate) is zero"

This might be a misunderstanding. The integral of the mass density over a given control volume is the mass, not the density itself. Therefore the time derivative of the mass in just that control volume is zero. Conservation of mass, one of the principles of continuum mechanics, is given.

In a Lagrangian or material based description the density can change by change of the control volume. Mass can not be created or disappear. In contrast to this in an Eulerian or location based description the control volume is fixed and mass inside the volume can change due to inflow and outflow through its surfaces. Hence the density changes accordingly. Conservation of mass is then established with respect to the overall system described.

As these are fundamental principles of continuum mechanics we don't see any benefit in repeating them in the manuscript, especially as the referee notes the section is quite large already. The book by Haupt (2002) on continuum mechanics, which we refer to, allows the reader to get basic information about the addressed topics if desired. The described method however is not entirely a Lagrangian description as the physical properties are solved in an Eulerian reference frame for every time step. We will revise section 3 to improve the understandability for the reader.

Line 71: I am confused a bit with v and v_b – can you clarify the language? You use multiple terms ("flow velocity", "boundary velocity" and "material velocity", and later "grid point velocity"), but only have the two variables. Please choose one term for each variable and stick with that. It is also not clear which velocity is which – e.g. with "boundary velocity", are you talking about the boundary of one grid point/control volume, or the boundary of the entire model domain (i.e. downward advection due to underlying ice dynamics?).

> As this is definitely true, we will refer to the velocity v_b as "grid point velocity" in the entire section in the revised version of the manuscript. Additionally we will consequently add the symbols v and v_b in the text, to make the distinction between the different velocities easier.

L74: "build" → built

> The mistake is corrected.

Equation 2: please use a different variable for stress – you already use t for time (eq. 1). This gets especially confusing because Alley (1987), equation 5, matches your equation 5; but your t has a different meaning than the t in the Alley paper. I don't think it is sufficient to differentiate between t and t_{zz}. Alley used P for pressure; Morris and Wingham (2014) and Arthern et al. (2010) used σ.

We introduce the symbol σ_{zz} for the stress in vertical direction in the revised version of the manuscript to avoid confusion with the time t.

Nevertheless we want to hold on to the differentiation between $\boldsymbol{\sigma}$ and σ_{zz}. In a Cartesian system stress can be represented by a tensor of rank two, commonly the Cauchy stress tensor $\boldsymbol{\sigma}$. Pressure is usually defined as one third of the trace of this tensor. Alley used the symbol P for the "overburden pressure" defined as the product of the accumulation rate, acceleration due to gravity and time since deposition of the described firn parcel. This reveals a common inaccuracy as this definition does not describe some kind of pressure but the stress in vertical direction σ_{zz}. We want to emphasize that σ_{zz} is the stress in vertical direction as our interpretation of the study results states other entries of the stress tensor $\boldsymbol{\sigma}$, describing stress in horizontal directions, may be considered in future approaches to firn density modelling.

Equation 4: With this formulation, it seems that in the accumulation zone the model domain will continue to thicken indefinitely?

This is correct. In this study we address this problem by removing grid points within the computational domain if a certain limit of grid points is reached. This limit however is chosen in a way that guarantees a high quality representation of the modelled domain. In the revised version of the manuscript we will add a short comment noticing this point and a description of how this problem is solved.

This process results in a growing number of grid points. Therefore grid points are removed from the extending model domain at its base when a maximum number is reached.

Section 3.2

write Equation 5 as a part of a sentence (as you have done with other equations)

Equation (5) will be integrated in the first paragraph of the section in the revised version of the manuscript.

Stress term in Eq. 5 – Alley (1987) makes a steady-accumulation assumption. In this paper it appears that you do not – see comment regarding this at Section 4 comments.

This is true, Alley (1987) uses a steady-accumulation assumption for his simulations. The derivation of the model however does not need this assumption. The densification rate depends on the average force per grain, linked to the so called overburden pressure, which itself is derived from the accumulation rate. A changing average force per grain due to a changing accumulation rate does not conflict with the model.

Just afterwards the derivation of the constitutive relation Alley makes the assumption that certain quantities, including the accumulation rate, "are constants at any site assuming isothermal conditions". It can be assumed that Alley used this formulation because high quality time series of accumulation data were sparse in 1987. Nowadays we have access to such data and can use it to improve firn densification models.

However the revised version of the manuscript will address the point that the formulation in the original paper is different. Please see also our response to your comments regarding this difference to the original work by Alley (1987) in Section 4.

Line 107: "seeming arbitrary at first" is a subjective comment (is it more arbitrary than the 5/3?); remove it.

Both 2/15 as well as 5/3, as pointed out later in the paragraph, are not arbitrary. The authors intended to highlight this fact by using this specific wording. However as it seems to be mistakable it will be removed in the future version of the manuscript.

Do not use "term" and "factor" interchangeably. (e.g. you use "term" to describe $8D_{\mathrm{BD}}\Omega/k_bTh^2$ but this is a factor, not a term.)

In the revised version of the manuscript the word is used consistently.

Line 117: "kind of fade out behavior" – please use specific language and avoid colloquialisms – "fade out" is used several more times, but I think you can be more precise, e.g. how does it fade out? e.g. "the influence of grain boundary sliding on the strain rate decreases asymptotically as the density approached the critical density".

We reformulated the parts under consideration in the revised version of the manuscript. The term "fade out" is avoided in the revised version of the manuscript.

The vertical strain rate ε_{zz} decreases with increasing density ρ, until it becomes zero at the critical density ρ_c.

Line 120: previously you said T "resembles the temperature" (what does that mean?), and here you explicitly say that T is the temperature.

> The improper formulation "resembles" is removed from the revised version of the manuscript.

Line 120: you previously said r is grain radius, and here you redefine it slightly differently ("grain size" is a broader term than "grain radius"). Which is it? Please be more specific of what you mean by grain radius, because the grains are not perfect spheres and different people mean different things when they talk about grain radius.

> The revised version of the manuscript solely contains the term "grain radius". Furthermore we added a definition of the grain radius. The model by Alley (1987) indeed assumes perfectly spherical grains. As though this is not true for firn, the assumption of such geometry seems suitable as a model approach. As this also affects the simulation of the grain radius, more details are presented in section 3.6.

3.4

L128: Again, I think you have just not explained your method clearly – what is a "test volume"? This is the only time you use that phrase. Upon first glance, this sentence says that the densification rate is zero; the next sentence clarifies this a bit – but not until the third sentence do I figure out that your control volumes are changing. The upshot is that I don't doubt that your numerical scheme is correct; I think it is not explained clearly.

> In the revised version of the manuscript we will take heed of consistent wording regarding control or test volumes.
>
> The integral over the density is the mass, not the density itself. Please see our answer to your comment on Equation (1) for a detailed explanation. The authors think that the terms "Langrangian description", "material fixed coordinates" as well as "control volume with moving boundaries $z_1(t)$ and $z_2(t)$" (see beginning of section 3.1) pretty distinctly introduce the concept of a changing control volume. We will revise section 3 to establish better understandability.

3.5

Line 142: This is an incomplete sentence. Discretized by what? Or maybe it is just written oddly?

We reformulated the sentence to make it better understandable.

As pointed out in Section 3.1 all advection in the model domain is represented by the moving grid. Therefore the description of temperature evolution reduces to simple heat diffusion:

Line 145: please cite page numbers with textbooks.

In the revised manuscript we added page numbers when citing monographs.

Equation 11: Is your equation for conductivity your formulation? I do not see it in Paterson 1994; please either cite source material or state that you came up with this parametrization (and how).

In fact a mistake happened here. During the development of the study we tried different descriptions of the thermal properties for firn and things got mixed up. The relation shown for the thermal conductivity is adapted from Arthern and Wingham (1998). However, this is not the description for the thermal conductivity we used throughout the study. We followed the example of Zwinger et al. (2007) and used the description by Sturm et al. (1997). The new version of the manuscript features the correct description and the corresponding citations.

3.6

Please provide what you use for a surface boundary condition for the grain size.

At this point of the manuscript we want to show the equations solved during the simulation process, mainly independent of the use case. We mention that a suitable boundary condition for the grain radius is needed in order to solve the equation and provide a reference to Section 5.2.

"This is suitable as the grain radius is used in the constitutive equation by Alley (1987)" – please justify this assumption. Also, second use of word "is" appears to be a typo.

The mentioned formulation has been rewritten as part of the revision of section 3.6 to clarify the computation of the grain radius. This includes the definition of the grain radius and the relation of the grain radius to the mean grain cross-sectional area as defined by Arthern et al. (2010).

The evolution of the grain radius r is simulated using the well known description of Stephenson (1967) and Gow (1969) as given in Arthern et al. (2010). Stephenson (1967) and Gow (1969) describe the grain size in means of the mean cross-sectional area. Arthern et al. (2010) however assume the mean cross-sectional area to be $A = (2/3)\pi r^2$ and formulate the grain growth rate as ... This formulation allows for simple calculation of the grain radius r.

Section 4

Alley (1987) did not include heat diffusion (he assumed isothermal conditions), and he assumed steady accumulation. How does your inclusion of it affect your results vis-à-vis those of Alley? Please provide justification that it is appropriate to "test the concept of the material model developed by Alley.

The authors see no reason why a more detailed description of the temperature and the stress due to accumulation should not be justified. The model by Alley describes the process of grain boundary sliding which depends on temperature and stress. It is in no way adjusted to average values or steady state conditions. This is one of the advantages a model describing the physical processes of firn densification would provide in contrast to empirical models.

Furthermore the model was used in this way before (Arthern and Wingham, 1998; Goujon et al., 2003). Arthern and Wingham (1998) tested how the model responds to varying boundary conditions at the surface in contrast to steady state conditions. They used random generated values as "information about the spatial scale of climate fluctuations is limited by the scarcity of meteorological stations and the large distance between core drilling sites". Data products like RACMO now provide that data. Using constant temperatures like Alley did in 1987 would provoke the question why available forcing data was not being used. The same is true regarding the grain radius.

Nevertheless we address the fact that transient boundary conditions are applied to the model before in Section "5.2 Boundary Conditions and Forcing".

L172: How does lumping these parameters together affect the results? I would expect that they will vary by site/climate, and so what does it mean for your grain boundary sliding model that you remove the specific parameters that describe grain geometry?

> The approach decouples the resulting depth density profiles from the parameters. Indeed the optimisation result in the form of the factors C is site specific. This is the point of the study. We test if Alley's description of grain boundary sliding is convenient to model the density within the first stage of firn densification, if optimal parameters were used. And we analyse the resulting site specific factors afterwards regarding possible hidden dependencies.
> As this idea is not intelligible to the reader from the first paragraph in section 4, the authors point it out more clearly in the revised version of the manuscript. Please also see our answers to your comments on Section 6 regarding this topic.

Eq. 14 and 15: You are adding an Arrhenius term, which was not included in Equation 5 (but is of course included in many other firn models. Given that your paper is about optimizing the grain boundary sliding approach, can you justify this addition, or add text describing why you felt it was appropriate?

> In fact the same Arrhenius term is indeed included in Equation (5) (D_{BD}). To highlight it the revised version of the manuscript includes a description of D_{BD} in Equation (5) in the same manner as in Equations (14) and (15).

L187: what does "its" refer to? i.e. to test what's influence?

> To clarify the sentence its structure is changed in a future version of the manuscript.
>
> *To test the influence of the Arrhenius law it is disregarded in Variants 3 and 4 as shown in Equations (16) and (17):*

L191: It is not clear to me why a possible dependency (of the strain rate?) will reflect in the optimal values of C_{v_3} and C_{v_4}? There is still a factor of $1/T$ in the equations.

> As the addressed sentence is mistakable and provides no important information it is removed from the manuscript in the revised version.

L193: → "The aim of the optimization . . . "

> The missing "The" is added in the revised version of the manuscript.

L195: Where is the Wilhelms core taken from? Please mark in on the map (figure 3) in a different color and provide coordinates. I suggest coming up with an easier-to-read name (i.e. state the full name once here when introducing and citing it, and give it an easy-to-recognize abbreviation to use for the remainder of the paper). I suggest this because I get concerned when I am reading if there are multiple cores with similar names that I need to be differentiating between.

Figure 3 features the location of ice core "ngt03C93.2" in the revised version of the manuscript. Additionally the coordinates are provided.

Consistent and unambiguous naming of datasets, no matter what kind, is of great importance. Therefore, the authors would like to stick to the name "ngt03C93.2", although we note the name is somewhat cumbersome. The likelihood of confusion would be greater when using an abbreviation because the abbreviation could for example just as well refer to ice core "ngt06C92.2". Aside from that "ngt03C93.2" is the only core featured explicitly in the manuscript.

L200: this implies that Lundin and Verjans did not use objective measures for their studies, which is not the case. Verjans did in fact use RMSE. I will also suggest that RMS deviation is not wholly objective in this scenario (which is not to say it is not useful) – imagine that you had a scenario where you were able to model the density perfectly at all depths except a single depth where it is off by $20\,\mathrm{kg\,m^{-3}}$. If this misfit point is near the surface where the density is low, it is a much higher percent misfit than at greater depths where the density is greater.

Both studies, the one by Lundin et al. (2017) and the one by Verjans et al. (2020), used the depth integrated porosity (DIP) for comparison. Verjans et al. (2020) in fact used the RMSE, but to express differences between different values of the DIP.

There is one drawback of the DIP: Let's assume the DIP of a firn profile has the value d. When one flips the same firn profile vertically, the highest density now at the top, the lowest at the base, the DIP is still d. Actually, when one imagines the firn profile discretized at certain points in space, one can rearrange the density values at these points in every possible way, the DIP remains d. DIP only works, because the density profiles are always rather similar to each other, but is not unambiguous.

Nevertheless we agree with the reviewers point that the RMSD is also not entirely objective. In the revised version of the manuscript the references to the studies by Lundin et al. (2017) and Verjans et al. (2020) (great study by the way) will be removed. The root mean square deviation will not be called objective anymore.

L209: It occurs to me at this point that you have not described how you are modeling densification in stage 2 – ostensibly your model goes beyond stage 1 densification because this is important for getting the temperature profile/heat transfer correct. With the Arnaud/Goujon/Breant models, there is a factor introduced to allow the densification rates to match at the critical density – are you doing something similar? I do not think you need to provide a lot of detail but you should add a sentence describing what you do for density greater than 550.

In this study, we focus only on the first stage of densification, therefore, we do not compute the densification further than the end of this stage. Indeed, if we expand the simulation into further depth, the transition is more of an issue.

What is important from our perspective is that our focus is indeed on assessing the process of grain boundary sliding and not (yet) the best approach for the transition. From our experience with running simulations incorporating the following stages this is the most tricky point to solve. If the goal is to get the best estimate for delta age, or the best close-off density depth, this is by far more of an issue than only checking if the constitutive relation for the first stage is appropriate. Therefore, we think that we are basically ok, due to the low depth at which the first stage ends.

We do not develop the density beyond the first stage and the critical density. Regarding the computation of the temperature a spin-up and consideration of the following stages of densification would be the best choice. We have not done that in this study, because not many of the analysed firn profiles extend below the depth of 10 m to 15 m, which is the depth at which the mean annual surface temperature is established (e.g. Cuffey and Paterson, 2010). We are not investigating seasonality at this point.

In the revised version of the manuscript we cite the article by Vandecrux et al. (2021) which has been published after our submission. The study presents a firn temperature profile at "Camp Century", which shows that a Neumann boundary condition at the depth we are applying it is not an issue, as the temperature change below the depth of 5 m is insignificant for our approach. The study by Orsi et al. (2017) shows that the temperature change in the upper 150 m of firn is only about 1 K, which is also small in context of our work.

In order to assess what influence this rather small temperature change has on the outcome of our study, the revised version of the manuscript contains a simulation in which we have reduced and raised the temperature at one site. We check whether the ability to find an optimal value throughout the optimisation process is reduced. As expected, we find that it has little influence on the assessment of the constitutive relation.

L214: Please be more specific about what tests you did.

The sentence is altered in the revised version of the manuscript so that the process leading to the presented range boundaries is comprehensible.

To ensure optimal factors are found within the presented range boundaries the all simulations were performed multiple times.

L215: A general note: Thank you for consistently including the units with your variables and equations.

L223: It would be helpful to explicitly say that there are 21 densities you tested, e.g. "we tested 21 different values of the surface density . . . "

We include the number "21" in the revised manuscript.

Section 5

Section 5: I suggest moving section 5 to before section 4. At this point my feeling as a reader is that I have made it to the 12th page and am finally getting to the part of the paper where you tell me more specifics of the science you did (this is also related to my suggestion that you move a majority of section 3 to an appendix.)

The authors would like to maintain the current structure of the manuscript. In terms of Section 3, please see our response to your comment, regarding the suggestion to move it to an appendix.

In our opinion the order of Sections 4 and 5 should not be changed to first introduce the methods, and then present the data used within the study. Furthermore, the choice of data is based, at least to some degree, on the used method. The main scientific idea is related to the optimization scheme in conjunction with the physically based model of grain boundary sliding. The considered data was gathered by other researchers and is not in the focus of the present investigation.

Figure 2: Since you include the SMB and temperature in a plot, it might be useful to also include the mean of those as predicted by RACMO.

Panel c) of Figure 2 also features the mean values of the surface mass balance and surface temperature over the course of the simulation time in the revised version of the manuscript.

L232: Be specific of how many sources, or leave that out. Probably you could just remove your short description of what SUMup is (the fact that is is available online and has a lot of measurements is not germane for the point of your paper)

The sentence is removed from the revised version of the manuscript as suggested by the reviewer.

L241: Positive SMB and melt are not exclusive - quite a few sites experience significant melt in Greenland yet still have positive mass balance. Quite a few of your sites, as far as I can tell on your map, are in wet firn locations. How did you handle these sites with your simulation? Your model handles densification due to compaction but not densification due to meltwater percolation and refreezing. How does this assumption affect your results? Do you see different optimal values of C for sites where melt > 0? Or higher optimal surface densities?

We are aware of the fact that a positive annual surface mass balance may include periods of melting. The data basis for this study is mean annual data from RACMO. A discrimination of those sites including melt and those which do not, is not possible using these data. 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, as we do not aim to find a specific value C representing all firn profiles (see also our responses to your comments regarding the discussion section). In the revised version of the manuscript we comment on this problem.

L255: Except it appears that the cold, low-accumulation center of the East Antarctic ice sheet is not represented, and this is one place of high interest for firn studies because of its relevance to ice core studies. (The Arnaud model specifically used Vostok as a test site).

This is most certainly a problem, we address it in the sentence just before. The Vostok core used by Arnaud et al. (2000) is not included in the SUMup dataset. Arnaud et al. (2000) cite Barkov (1973) as the original source. It seems, Koenig and Montgomery (2019) were not able to get hold of the original report and more relevant the data. Many cores from this era are not published in a usable way. The authors strongly encourage everyone in the possession of unpublished firn density data to contribute to the SUMup dataset.

However the authors consider the SUMup dataset the most extensive dataset of firn density data available at this point and decided to focus on these data if they match the criterions presented in Section 5.1.

Section 5.2: I suggest rewriting the first paragraph of this section for clarity.

As suggested by the referee the paragraph has been rewritten.

To force the firn densification model, surface values for density, temperature, accumulation rate and grain radius at the locations of the 159 firn profiles are needed. Although Alley (1987) used constant forcing we follow the example of Arthern and Wingham (1998) and Goujon et al. (2003) performing transient simulations.

As measured firn density profiles represent past climate conditions, the choice of forcing data is crucial for the presented method. Uncertainties in the forcing will reflect in the simulation results and therefore in the comparison with measured firn profiles. Neither the model formulation nor the optimisation scheme can compensate for that. We use data provided by the regional climate model RACMO2.3 (Van Wessem et al., 2014; Noël et al., 2015)5. RACMO2.3 provides forcing data for the Greenland ice sheet covering the period from 1958 to 2016. In case of Antarctica the time period is shorter, starting in 1979 and ending in 2016. Data for the mean annual skin temperature and surface mass balance for the Greenland ice sheet are available at mean spatial resolutions of 11.3 km *and* 1.0 km *respectively for this study. Mean spatial resolutions for Antarctica are* 8.0 km *and* 28.5 km *for mean annual skin temperature and surface mass balance. Spatial interpolation of the fields leads to forcing data for the locations of the investigated firn profiles.*

L259 – 268: I am confused here. You said earlier that your model time resolution is 48 steps per year (roughly weekly), but you say here that you are using annual data from RACMO. Why do you not use the daily RACMO data downsampled to weekly resolution? Are you using the same value for the temperature and accumulation for each of the time steps during a given year? On Line 260 you say you are interpolating, but what are you interpolating (spatially? temporally?), and how?

We clarify that spatial interpolation is meant here in the revised version of the manuscript. We found that using forcing data with a higher temporal resolution affects the resulting firn density profile little compared to the results using mean annual data. However, we have not included these findings in the manuscript. We add them in the revised version. The time resolution of 48 time steps per year ensures for high spatial resolution of the simulation results, helping to compare results with measurements, and guarantees for numerical stability. We address this in the future version of the manuscript. Despite being used in many studies, data from RACMO are not freely 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?

Even when near surface density values are available, it is not trivial to determine a surface value. Strategies could incorporate the computation of mean values over certain depths, fitting of more or less complex functions over certain ranges, combinations of both, and so on. Difficulties arise when applying one method for a great number of profiles because profiles start at different depths, the spatial resolution differs, the density varies. Firn profiles are very unique especially at low density.

We have worked out during our study that the approach presented in the manuscript works well. In case near surface values of the density are available the surface density resulting in the best match between simulation result and measured firn profile represents these values well due to the use of the root mean square deviation as cost function. For the reasons pointed out before it is difficult to quantify this. We do not distinguish between those profiles from which near surface values are available and those starting at greater depth, to maintain comparability. The surface density is constant with respect to time for a given site.

The revised version of the manuscript addresses these points.

L270: What is the basis of your choice of 0.5 mm for initial grain size? Is it a realistic assumption to assume it is the same everywhere? (I would think not – southern Greenland is quite different than e.g. the South Pole). How sensitive is your model to this parameter choice?

The choice of using the value 0.5 mm for the initial grain radius is mainly based on the study by Linow et al. (2017). The effective radius defined in this study can be understood as radii of spheres having the same specific surface area as measured throughout the study. Linow et al. (2017) derive an empiric relation for the surface grain radius from their measurements. It depends on the mean annual temperature and mean annual accumulation rate.

Applying this empiric relation to the RACMO data representing the locations of the analysed firn profiles leads to a mean surface grain radius slightly below 0.5 mm. 0.5 mm is also the grain radius used in the study by Arthern and Wingham (1998).

Nevertheless data for the grain size at the surface are sparse. Even the study by Linow et al. (2017) is based on only a few firn cores and features only one core from Greenland. The choice of a value constant in space and time, might not be realistic, but pragmatic. Tests throughout the study have shown the model is not very sensitive to this parameter. Furthermore because of the optimisation approach the initial grain size is less relevant. The change of the grain radius with depth is the important part here.

In the revised version of the manuscript we will address these points.

L274: But aren't the surface densities in good agreement because the range you chose for them is based on what the literature says in the first place? This seems circular.

This is in fact true. In the revised version of the manuscript we dismiss the comparison with other studies.

L277: I think there is literature that you could find to cite regarding Antarctic vs. Greenland surface densities. Or you could query SUMup and find all measurements shallower than 50 cm or 1 m for Greenland and Antarctica.

The number of firn profiles incorporating shallow density data would not be representative for either Greenland or Antarctica. The comparison of such data is difficult. We neglect the discussion of different surface densities at this point on purpose, as we believe it is out of place in this section.

L279 – 280: This seems out of place – should it be in the temperature section in section 3? Also you need to specify what your model domain depth is since you are using a Neumann condition.

Yes, we agree, and it moved to Section 3 in the revised version and we also specify the depth explicitly. Please see also our response to your comment on line 209.

Section 6

L290: "even better agreement" – this implies that that core has good agreement in the first place – but does it? You do not provide context for whether $\approx 23\,\mathrm{kg\,m^{-3}}$ is actually a good agreement. I suggest that RMSD is a good metric to intercompare the models' performance – v2 is doing better in general than v3. But it does get murkier when actually deciding if this is a "good" fit to the data – so this would be better if you could normalize the RMSD somehow, and provide a quantification of what you consider to be a good fit (any why).

The question of what is considered good and what is considered bad is difficult to answer. The authors think that the root mean square deviation actually is descriptive and understandable for the reader as it describes, as the name states, a mean deviation given in the unit of the density itself. A simulated density profile with a root mean square deviation of for example $20\,\mathrm{kg\,m^{-3}}$ is maybe off by $20\,\mathrm{kg\,m^{-3}}$ at every point. A value of the depth integrated porosity which is off by a certain value is much harder to grasp.

Along with the revised version of the manuscript we publish plots of the analysed firn density profiles and our optimisation results along with the root mean square deviation in the manner of Figure 2 (a). This allows the reader to get an overview of all results. Together with Figure 5 we think this will help to put the values of the root mean square in perspective.

Figure 5: please label which variant is which in a more obvious manner (the v1 is small and hard to spot).

In the revised version of the manuscript the four subplots contains an additional label.

L291: Isn't this more a function of the SMB (from RACMO) being accurate than it is the strain rate being correct? If you were to optimize your model to fit the depth-age profile best (rather than depth-density), would the same optimal parameters be the same?

> Optimisation for the age results in different optimal parameters. The quality of the depth-age profile does not only depend on the quality of the surface mass balance forcing but to some extent on the quality of the model. The model is driven by the surface mass balance forcing, so that things can not be examined separately. As age data is not available for the entire dataset, and for simplicity, we drop these considerations in the revised version of the manuscript.

L296: This is confusing. Alley (1987) as far as I can tell does not include an Arrhenius term. (Although he did derive an activation energy for grain boundary sliding). This makes me wonder: the activation energy you use is notably smaller than recent studies have implied it should be (Arthern et al. 2010; Morris and Wingham 2014) – how does your choice of activation energy affect your results?

In fact the model by Alley (1987) at first sight does not contain an Arrhenius term (see Alley, 1987, Equation (5)). It does however include the "boundary viscosity" ν. As pointed out in the introduction of the manuscript, Alley (1987) has not simulated the four firn profiles described in his study. He fitted the curves resulting from his model to the firn profiles and evaluated the value of the boundary viscosity ν leading to his fitting results. He then proposed to use a description of the boundary viscosity ν provided by Raj and Ashby (1971) (see Alley, 1987, Equation (7)). This description includes an Arrhenius term describing the rate of boundary diffusion.

The activation energy of $44.1\,\mathrm{kJ\,mol^{-1}}$ is the activation energy for the physical process of boundary diffusion in ice. This value results from measurements of the self diffusion of ice (for example Maeno and Ebinuma, 1983; Itagaki, 1964). Alley (1987) suggests the slightly different value of $42\,\mathrm{kJ\,mol^{-1}}$ citing the text books by Hobbs ("Ice Physics", 1974) and Paterson ("The Physics of Glaciers", 1981), which should show this value was established in 1983.

The values of the activation energy resulting from the studies of Arthern et al. (2010) and Morris and Wingham (2014) (and many more) do not describe the process of boundary diffusion. These values result from the empirical approach of fitting a model of a certain form, often including an Arrhenius law, to a certain dataset. These activation energies might be understood as an average over all physical processes leading to firn densification. Including boundary diffusion, lattice diffusion, dislocation creep, vapour transport and maybe more (see Maeno and Ebinuma, 1983; Arthern and Wingham, 1998). The aim of this study however is to reassess the description of the physical process of grain boundary sliding by Alley enabled by the process of boundary diffusion.

To clarify these facts the authors mark the relevant activation energies and corresponding pre factors with the index "$(\cdot)_{\mathrm{BD}}$", to point out the process of boundary diffusion is meant. Furthermore the study design of Alley (1987) and the relevance of the boundary viscosity ν within the study will be repeated briefly in Section 3.2. The used values for activation energy Q_{BD} and pre-factor A_{BD} are derived in more detail by inclusion of more appropriate literature especially in section 4.

Note on figures in general (especially Fig. 6): the colors you have chosen can be challenging to see (especially the yellow) when text is written in those colors.

In the revised version of the manuscript the yellow color #EBCB8B is changed to purple #B48EAD for better readability.

L305/Figure 7: To me these data do not appear to be linear, and the lines do not appear to be good fits (especially at low and high temperatures). A Pearson correlation coefficient is based on the assumption that the data are linear, which I am not convinced they are, so I think it is deceiving to include that metric. To me the most obvious conclusion is that there is not a clear linear relationship between any of the C parameters and temperature. If this is not what I should take away from figure 7, you need to be more thorough convincing me.

The intention in providing the Pearson coefficient and a linear fit was to give the reader an orientation to determine whether the factors C show a dependency on the mean annual temperature or not. This dependency has not necessarily to be of linear kind as mentioned in the text. However, we acknowledge that this impression could arise.

The main problem of the Pearson coefficient is, that even if it becomes zero, a dependency of higher order may be present. However, it can be used to determine to which extent two vectors are linear. The correlation with higher order functions is difficult to quantify if no concrete function predicting the observed values is known. Nevertheless we will introduce the distance correlation (Székely et al., 2007) in the revised version of the manuscript. It was designed in recent years to explicitly overcome the problems of the Pearson coefficient.

Furthermore we removed the linear fits from Figures 7 and 8 and adjusted the text to clarify our thoughts presented to the reader.

Figure 7: what does a negative value of C indicate? (it is hard to tell if there are any; but figure 7's y axis extends to -2, and it looks like there could be? Make the axis the same as figure 8.)

The figure is altered as proposed by the referee.

310: Again, "even higher" is implying that you had a good fit between temperature and C, but the a high Pearson is meaningless because the data in figure 7 are not linear.

The addressed term is changed in the revised manuscript with respect to the other changes in this section.

L312-315/Figure 8: These do appear to be more linear than the C vs. temperature plot, but I think it would be appropriate to do a statistical test to actually show that they data are linear (e.g. a statistical model with higher order terms that have coefficients not significantly different than zero, or some other test). I am concerned with the values at the high accumulation sites for variants 1 and 2 – there are clusters that are half of what would be predicted by your linear model. And, what is going around 0.4m w.eq. a^{-1}? (And 0.6?) These vertical series of dots would indicate to me that there is no correlation between SMB and C – i.e. you can have a number of sites with SMB = 0.4 and C can vary by a factor of 10.

We want not necessarily to imply a linear dependency, but a general correlation. In the revised version of the manuscript the distance correlation provides a correlation measure independent of the assumption of a linear dependency (see our answer to your comment regarding line 305 of the manuscript). Sites with a mean surface mass balance around $0.4\,\mathrm{m\,weq.\,a^{-1}}$ show indeed peculiar results. These sites are located in a cluster (hence the very similar conditions) in western Greenland. These sites are influenced by melt. We address this in the new manuscript.

Your discussion should include discussion of why the optimal C values vary by site and how that influences our understanding of grain boundary sliding.

The idea is to generate the best possible density profiles for each one of the four variants of the constitutive relation and each site. No matter which variant of the constitutive relation is used, the density profile always matches the measure density profile, due to the optimisation. As the resulting density profiles are similar, because the general structure of the constitutive relation is maintained, we can compare the factors C leading to these density profiles.

The differences in the factors C do not primarily arise because of the differences in the resulting density profiles, as these differences are small, but from the differences in the variants of the constitutive relation used throughout the simulation. Nevertheless, also the small differences between the resulting density profiles matter.

We will point out this idea at the beginning of the discussion section in the revised version of the manuscript.

Section 7

L323: Again, I suggest that getting depth-age correct is more dependent on getting the accumulation rate correct – and, since the accumulation rate is often determined by counting annual layers, the science can become a bit circular.

As pointed out before, we neglect these considerations in the revised version of the manuscript.

L326: I agree entirely with what you say in this paragraph – your model formulation is not too different than the other models that have been published in recent years, but up until this point you have been claiming that you are optimizing a grain boundary sliding model (including in the title). I think it would be appropriate for you to rework the text a little bit to be more forthcoming with this, rather than waiting until the very end to point it out.

We do not want to formulate a new model for the process of grain boundary sliding nor do we want to optimise the existing model by Alley (1987). Hence we are not claiming to optimize a grain boundary sliding model. We think the title states clearly that we do an assessment by using an optimisation scheme. We assess an existing model, the model by Alley (1987). We can not and we do not want to claim this model for ourselves. We assess if the constitutive equation for the process of grain boundary sliding by Alley from 1987 is convenient to model firn densification below the critical density using an optimisation scheme and data not available back in 1987. In the revised version of the manuscript we address the point that we assess the constitutive relation and it's format in the introduction more clearly.

L338: Morris (2018) did in fact create a "transition model" to move from stage 1 to stage 2.

This is true. Morris (2018) addressed the problem that (semi)empricial models based on the approach by Herron and Langway (1980) a distinct kink in resulting density profiles, because of the different functions describing stage 1 and 2 of the densification by applying another empirical function for the transition.

"Other functions that produce a smooth transition from one value to another exist and produce much of the same effect."

This is most certainly an improvement for those kind of models. In the present investigation we are interested in the description of processes leading to firn densification, particularly in grain boundary sliding.

L345: I am confused here – are you suggesting that because you do not have a separate Arrhenius factor in equations 16 and 17 there is implicitly an Arrhenius factor wrapped into C_{v_3} and C_{v_4}? If this is the case, say so clearly in Section 4. It seems that you have a baseline assumption in your study that firn strain has an Arrhenius temperature dependence. But, if Variants 3 and 4 give good answers, couldn't this just as easily indicate that firn does not have an Arrhenius dependence? Part of my point here is that you have coefficients, C_{v_3} and C_{v_4}, that have all sorts of "physics" wrapped into them, and I don't think you can just decide what those physics are.

We do not have a baseline assumption that a firn densification model has to contain an Arrhenius equation, although it is likely meaningful. The description of grain boundary sliding by Alley (1987) in combination with the description of the bond viscosity by Raj and Ashby (1971) however does. We want to assess this constitutive relation.

The idea is to strip the constitutive relation down to its core. We neglect the Arrhenius law in two of the four variants. Due to the optimisation however, we produce very good results in terms of the density. These results, the depth density profiles, are very similar for all four variants. For the depth density profiles it does not really matter if we include the Arrhenius equation or not. But, because the results are so similar, we can afterwards compare the factors these results. This is the idea of the presented method.

By using one variant incorporating the Arrhenius equation another neglecting it, leading to the same depth density profiles but different factors, we can see the missing dependency on the temperature in the factors. However, as though the results improve when using the Arrhenius equation in terms of the determination of a global perfect factor (which does not exist), we still see a dependency on the temperature to some degree for Variants 1 and 2. This tells us that either the existing temperature dependency is flawed, for example by wrong parameters of the Arrhenius equation, or there's another dependency, so far not respected in the constitutive relation.

However, the paragraph is indeed confusing and does not really address these points. The revised version of the manuscript therefore features a much more detailed description of these thoughts.

L349, 352: "resulting factors" – do you mean the 4 values of C for your variants? C and the surface density? Please use more specific language, e.g. "indicate a clear dependency of C_{v_3} and C_{v_4} on the mean surface mass balance".

This expression is removed in the revised version of the manuscript.

The determined factors C_{v_3} and C_{v_4}, resulting from variants without the Arrhenius equation for boundary diffusion D_{BD}, show a stronger dependency on the mean surface temperature than factors C_{v_1} and C_{v_2}. At the same time, factors C_{v_1} and C_{v_2} show less dispersion than factors $C_{\mathrm{v}3}$ and C_{v_4}, as is shown in Figure 6. The inclusion of the Arrhenius equation D_{BD} in the constitutive relation leads to better determination of these factors. It is therefore a meaningful description within the constitutive relation. Although the inclusion of the Arrhenius equation results in better determination of factors C_{v_1} and C_{v_2}, we still see a dependency on the mean surface temperature to some degree. A better determination of the parameters of the Arrhenius equation may result in resolving this dependency. If this is not the case another dependency on the temperature may be introduced to improve the constitutive relation for grain boundary sliding.

L349: The first two sentences of this paragraph need to be rewritten to improve clarity. After multiple readings it is still not clear to me what you are trying to convey.

These sentences are removed from the future version of the manuscript. For reasons see our response to your next comment, addressing the same paragraph.

L350: This is not obvious to me – are you saying that the C factors are functions of temperature, and since temperature and SMB are correlated, then C must also be a function of SMB?

The point the authors wanted to address here is that one could argue that the dependency on the surface mass balance is just a hidden dependency on the temperature because colder sites usually also show less accumulation. As the paragraph is a little abstruse and the referee seems to doubt this point, the paragraph is neglected in the revised version of the manuscript.

L353: What do you mean by "Dependency of the mismatch"?

This formulation is indeed not chosen well. The sentence is reformulated in the revised version of the manuscript.

We interpret the dependency on the surface mass balance such, that the load situation is currently not represented well.

L354: You can at least do a scale estimate of the effect that horizontal stresses would contribute – see Horlings et al., 2020. Your paper is about stage 1 densification/grain boundary sliding – on the timescales of firn densification on ice sheets, is horizontal stress going to make a significant difference? I am also skeptical that an 3D model incorporating horizontal stresses would significantly reduce your misfit – at least in the example core you highlighted, the layering of the density profile likely contributes to a significant portion of the misfit.

Horizontal divergence due to the movement of the underlying ice as for example investigated by Horlings et al. (2020) is not what is meant here. The mention of a three dimensional problem also does not necessarily imply a full three dimensional model approach.

As pointed out before, the stress tensor is a tensor of rank 2. Therefore the term "overburden pressure" is misleading as the pressure is a scalar property. An isolated firn column, would not only compress in vertical direction, but also elongate in horizontal directions. But there is no such thing as an isolated firn column, a firn column would be confined in horizontal directions by other firn. One also could imagine a firn column in a pipe. As this firn column is now not able to elongate in horizontal directions, the horizontal components of the stress tensor are not zero, as the firn kind of presses against the walls of the pipe. We therefore propose that in a purely one dimensional model approach the stress might not be represented well.

To solve such a case is not as trivial as it sounds, as the material in question, firn, is compressible. However, model approaches can be found at Zwinger et al. (2007), (Greve and Blatter, 2009, pp. 224–230), Salamantin et al. (2009) and Meyer and Hewitt (2017). The revised version of the manuscript will address these points.

Section 8

L367: not represented well in the model – are you effectively saying here that the stress should have an exponent other than 1?

The revised manuscript features a more detailed discussion of our assumptions on the representation of the stress in Section 7. This, together with additional changes in Section 8, clarifies that we don't consider the stress exponent to be wrong.

L371: I agree with this paragraph; I think it should be moved into the discussion section along with a more complete uncertainty analysis.

We moved the paragraph in question to the discussion section in the revised version of the manuscript. We discuss the uncertainties within the forcing and other error sources and their influence on the results in more detail.

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