Response to Anonymous Referee #1, 29 Mar 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

1   General Comments

(1) The SUMup data base is an on-going community effort which sets a very clear condition for use of the data. The guidelines state "When using this data set please cite both the individual researchers who provided the data as listed in the Citation column as well as the SUMup dataset". There is a citation for one profile in this paper (ngt03C93.2, shown in a figure) but there are no citations for the 158 other profiles used. This may seem like a tedious task, but it is essential that researchers are properly acknowledged. It may be that the editor will accept a table of profiles with citations as Supplementary Material, but whether this is adequate needs to be confirmed with the SUMup team.

> We agree with the referee that proper acknowledgement of the data sources is important. For the revised version of the manuscript we will provide a document in the supplementary material that lists not only all factors leading to the best simulation result together with the corresponding RMSD for all profiles and tested variants, but also the reference to the original data for each firn core.

(2) The authors have constructed a numerical model for the evolution of temperature, density and grain radius in firn using the Alley densification equation and forced by annual values of surface temperature and accumulation rate from the RACMO2.3 meteorological model. The advantages of using the RACMO data are (i) that profiles for which in situ climate data are not available can be simulated and (ii) that inter-annual climate variations can be taken into account. The disadvantages are that (i) local data have to been found by interpolation and (ii) systematic errors may be introduced. It would be helpful if the authors could provide an assessment of how accurate the forcing data are likely to be.

> The authors have no background in regional climate modelling. We use RACMO as a data product, which is well established in the community. Therefore we can not assess the accuracy of data provided by RACMO. Further information on RACMO can be found in corresponding publications, cited within the manuscript, or on the project's website hosted at Utrecht University:
>
> https://www.projects.science.uu.nl/iceclimate/models/index.php, 25.06.2021

(3) Since the input temperature is only available at yearly intervals, the annual variation of temperature in the top layer of the firn cannot be simulated. Alley dealt with this problem by excluding the top 2 m of the firn, which experiences the greatest temperature fluctuations. In l.203 the authors state that they optimise in a domain "bounded by the surface and the oldest horizon within the profile affected by the forcing". In fact some records, including ngt03C93.2, shown as an example in the paper, do not extend to the surface. I assume the authors actually use an upper bound as near to the surface as possible. In any case, it is quite important to discuss why they choose not to follow Alley's example. The reader will need some convincing that inclusion of data from a region with strongly-varying temperature does not affect the optimised parameter values.

The simulation of the very first, near surface layer of firn is a general problem. However, tests have shown that a seasonal temperature signal has very limited influence on the resulting firn density profile compared to the same model setup using the mean annual temperature. Variations of the density, especially on a small scale, are also driven by wind and changing impurity contents (e.g. Freitag et al., 2013). We will include such considerations in the revised version of the manuscript.

Furthermore we found that the constitutive relation for grain boundary sliding performs quite well in the upper two meters of the firn column. For example in the study by Arthern and Wingham (1998) a constant value for the strain rate was used in the upper two meters. We don't see any advantage in this. Most likely the optimisation for a site specific surface density also compensates for an insufficient model approach. We will also include these points in the revised version manuscript. However, are convinced that the general idea of the optimisation is not significantly affected by this.

The domain for the computation of the root mean square deviation between simulation results and measured firn density profiles is indeed bound by the upper most point of the measured profile. We will clarify this in the revised version of the manuscript.

(4) One of the factors in the Alley equation (α') describes the effect of spherical averaging of the sliding velocity along variously-oriented grain boundaries to obtain the mean vertical velocity. Based on geometric arguments Alley suggests $\alpha' = (1 - N/6)$ where N is the coordination number and notes that, by observation, $N \approx 10\,\rho/\rho_i$. The authors make the interesting comment that $\alpha' \to 0$ for $\rho = 550\,\mathrm{kg\,m^{-3}}$; in other words the Alley equation implies that grain boundary sliding must stop at this density. An alternative expression for α' from Bréant implies that grain boundary must stop at a higher density of $596\,\mathrm{kg\,m^{-3}}$. The authors choose to restrict their optimisation domain to densities below $540\,\mathrm{kg\,m^{-3}}$ "due to the asymptotic characteristic of the resulting density profiles using the... constitutive equation" (l.209). But the reader might wonder why the cut-off should not come when densification by grain-boundary sliding is no longer much greater than densification by other processes (e.g. dislocation creep). There is a brief mention of the possibility of competing processes in the discussion section of this paper (l. 328) but this comes only as a suggestion for future consideration. I think the reader would appreciate a much deeper discussion of the implications of this choice of cut-off density, earlier in the paper.

> Our study focuses on grain boundary sliding, its description by Alley (1987), and the assumption that it is the leading process in the first stage of firn densification. We do not incorporate other processes at this point. The restriction of the optimisation domain to densities below $540\,\mathrm{kg\,m^{-3}}$, in certain cases, is a pragmatic choice. A criterion as suggested by the referee, considering the contribution of different processes, would be the better one. However, it would require the knowledge how these different processes actually act in the upper firn. This is not given at this point. We will address these points briefly in the revised version of the manuscript.

(5) Another problem arises because the authors have not chosen to discuss the question of layering in firn density profiles, even though their example profile clearly shows large fluctuations in density superimposed on the general increase in density with depth. They use a microscale, physics-based model, which applies to a small element of snow, and a grid spacing of 48 points per annual layer, so it would be possible to investigate the effect of annual layering. The authors may choose not to do this, but it is important then to bring out their underlying assumption, which is that the Alley equation can be applied on the macroscale by simple substitution of the macroscale mean density. Given the work that has gone into writing the model and selecting profiles to simulate, it would a pity not to take the opportunity to include some discussion about the problem of up-scaling.

Layering in firn is a topic highlighted by the growing amount of high resolution firn density data. However, it is not frequently addressed in firn density modelling. We follow the assumption of for example Freitag et al. (2013) that small scale layering in firn is driven by a changing concentration of impurities, leading to softening. This can be expressed in a dependency of the activation energy, used for the description of deformation mechanisms, on the impurity concentration. In the presented model this would be possible via the Arrhenius equation describing the description of boundary diffusion.

However, a suitable relation would be needed as well as data describing the impurity concentration (probably by a proxy). These data are not available on a global scale, so we (and the entire community) are lacking forcing data and can consequently not represent the short scale layering, but the average density over a larger spatial scale. By the use of the root mean square deviation as cost function, the optimisation result is not affected when using layered firn profiles as a basis.

We will address these points in the revised version of the manuscript and will add a running mean density profile for the example core ngt03C93.2 in Figure 2 a) to illustrate how the optimisation results relate to the mean density.

6) In the paper 4 versions of the Alley model are tested by optimising one parameter C_v per version. The reader will want to know how the values of v implied by values of C_{v1} compare with the Alley values and also what the individual values of the cost function RSMD are. These could be included in a table of sites, input data and results in the Supplementary Material.

> In the revised version of the manuscript we will provide a list containing the best parameters resulting from the optimisation approach and the corresponding values of the root mean square deviation in the supplementary material. However, it is difficult to compare the resulting factors to the boundary viscosity used by Alley (1987). The boundary viscosity depends on the temperature which is neither constant with respect to time nor along the firn profiles. One would have to assume appropriate effective parameters. Furthermore the resulting factors C may depend on other effects like the improper description of the stress, as pointed out in the conclusion section.

(7) The authors conclude that their results show that "the description of grain boundary sliding introduced by Alley (1987) is suitable for the simulation of firn densification at low density" (l. 362). In fact this would only be true if the value of parameter C_v could be specified a priori. The authors do not say that the linear relations with mean annual temperature and accumulation shown in Figures 7 and 8 could be used to determine global values of C_v but, if this is what they mean, then we need to know what the cost functions are when profiles are simulated using these global values.

> In the opinion of the authors, the determination of global values for the factors C is not useful at this point. Such assumption of global values would not lead to better simulation results compared to other published firn densification models. However, we think the constitutive relation for grain boundary sliding by Alley (1987) is a good basis for the description firn densification at densities below the critical density. A determination of a global factor, or rather of better parameters incorporated in the factor for this study, may be possible if an improvement of the constitutive relation can be achieved. We see a possible improvement in a better description of the stress regime, also mentioned in the manuscript.
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> We use the site specific values of C, found using the presented optimisation scheme, to identify differences in the variants of constitutive relation. Due to the fact that the general structure of the constitutive relation is maintained and the simulated density profiles are similar, because of the optimisation, we analyse the differences in the resulting factors, representing site specific ideal parameters, to identify possible additional dependencies and improvements.
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> This was not formulated clearly in the first version of the manuscript. We will improve it to point out the idea of the method.

Comment (8) On the whole the reader is left with the impression that the Alley model is inadequate, because the supposedly-fixed parameter C_v varies with the input conditions. This may be somewhat unfair. The variation may arise because the model has been applied too near the surface, layering has been ignored or indeed, as the authors remark, the input meterological data are inaccurate. But if there is a real dependence on accumulation rate this can be traced back to the calculation of stress t_{zz} (Equation (6)). Do the authors think that their results show that the vertical grain sliding velocity is not linearly related to the vertical force on the grain? Could this be because other non-linear processes are acting as well as grain-boundary sliding? Or is the Alley (1987) analysis too simple?

> The study aims to determine a site specific ideal factor C for every analysed firn profile. Even if for example layering is neglected, using the optimisation scheme, we are able to produce simulated firn density profiles matching measured ones well. As the amount of analysed firn profiles is large, random error should play a minor role when comparing the resulting values of the site specific factor C to other environmental properties.
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> Other processes than grain boundary sliding could act in the first stage of firn as well. We do not necessarily think the analysis by Alley (1987) is too simple, but the general assumption of a one dimensional model approach neglecting horizontal boundary conditions and lateral components of the stress tensor. We hereby do not mean so called "horizontal divergence" (e.g. Horlings et al., 2020), but stresses occurring due to lateral confinement. As these points were not explained clearly enough in the manuscript, we will revise it to highlight the aim of the study and its results.

2 Other Comments

l.2. "first introduced by Alley (1987)" Perhaps better to say that the theory of grain-boundary sliding in snow was developed by Alley in this paper? The concept that this process might occur was already in the literature.

> In the revised version of the manuscript the formulation will be altered as proposed by the referee.

l.15. "a couple of research fields" implies two fields, whereas the authors list three

> To avoid confusion, the wording will be changed in the updated version of the manuscript.

l.20 "The first and greater (category)". This sounds like a value judgement; better to say that the majority of models fall in the first category

> We will change the sentence following the suggestion of the referee.

l.22 "this approach neglects". Do these models really neglect overburden stress or is it implicit rather than explicit in the model equations?

> 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 its quasi-static form. Hence the stress is implicitly integrated in the descriptions. To clarify this and avoid misunderstandings the sentence in question and the following one will be removed from the manuscript.

l.32 onwards "which applies for firn". The paragraph goes on to say the theory does not apply to firn below the critical density. This is confusing. Does the theory not apply because the snow is not compacted enough? Or because the grain geometry is not regular?

> The theory of hot isostatic pressing as described for example by Kang (2005) was developed based on very different conditions than those found in firn below the density of $550\,\mathrm{kg\,m^{-3}}$. Both the compaction as well as the grain geometry contribute to these differences. As pointed out, the study focuses on grain boundary sliding, therefore the theory of sintering and especially hot isostatic pressing is not relevant at this point. Therefore the addressed paragraph will be removed from the manuscript.

l.40. Theile et al (2011) "tried to point out" or "did point out"?

> We will formulate this sentence in a more appropriate way.

l.43 Are these 4 profiles also simulated in this paper? Which are they?

> The four cores Alley used in 1987 are not part of the SUMup dataset. So they are not included in this study, but it is interesting to re-evaluate these data. The problem of many of the "old" firn cores is that the original data are not published. To our knowledge the 1987 paper by Alley is in fact the original reference for two of the cores.
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> The study by Spencer et al. (2001) features the four cores. There actually exists a dataset on the internet based on the 38 firn cores used in this study and some more cores. The authors are in fact in possession of this firn core data. However the origin of these data is not really traceable nor citable. Spencer et al. (2001) received many cores due to personal communication.
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> The authors strongly encourage everyone to contribute to the SUMup dataset to build a database as complete as possible. We will address the fact that the four profiles used by Alley (1987) are not featured in this study in Section 5.1 of the revised version of the manuscript.

l.101 $m\,w.e.\,a^{-1}$ is used elsewhere; why does $m\,w.e.\,s^{-1}$ have to be used here?

> The unit of $\mathrm{m\,w.e.\,a^{-1}}$ is usually better comprehensible for readers than $\mathrm{m\,w.e.\,s^{-1}}$ and is therefore used throughout the manuscript, for example in Figure 2. It is absolutely possible to use the unit of $\mathrm{m\,w.e.\,a^{-1}}$ in Equation (4) and the following paragraph as well. Nevertheless it would require an additional conversation of units. The authors prefer to use SI-units in all equations and model implementations and would therefore hold on to the existing formulation.

l.107. Why does 2/15 seem an arbitrary number? To say it results from the geometric deviation does not really explain to the reader where it comes from. Since the paper is based on the Alley model, expanding the derivation would be worthwhile. Then the reader would understand the physics behind the model without having to go back to the original paper

> The word "arbitrary" will be removed from the revised version of the manuscript. Although the derivation of the factor 2/15 is not difficult per se, it results from the combination of different surfaces describing intersections of grains, the coordination number of those grains and the correlation of the coordination number with the density derived by Alley (1987). The authors see no benefit to reproduce the geometrical considerations of Alley (1987). The interested reader can easily follow the derivation of the factor 2/15 by taking a look at Equations (1) to (4) in the original paper.

l.109. Is the word "resembles" right here? Maybe "represents"?

The mistake will be corrected as suggested by the reviewer.

l.128. Is "test volume" right? Maybe "volume element"?

In the revised version of the manuscript the term "control volume" is used consistently.

l.150 Would it be worth pointing out that Alley used observed values of r?

Although Alley (1987) Alley used observed values for the grain radius, these data are not published nor are ranges given. The authors address this issue briefly in the revised version of the manuscript.

l.200 This sounds as if the authors do not think the Lundin and Verjans papers use an objective cost function. The sentence could be altered so it is clear that the authors are explaining that they have used a different objective cost function.

In fact the authors see difficulties in the use of the depth integrated porosity as cost function. Nevertheless the revised version of the manuscript will no longer feature the studies by Lundin et al. (2017) and Verjans et al. (2020) at this point.

Figure 2. Is 2(c) a true representation of the input to the model? Or is the accumulation constant for a given year?

Panel (c) of Figure 2 shows the true representation of the model input. In the revised version there is a detailed discussion of the model input and forcing terms.

l.278 The difference in surface density between Greenland and Antarctic profiles merits further discussion. To say merely it is "plausible" misses an opportunity to comment on different types of snow (see Salamatin for example) and whether the densification rates might be different for Greenland and Antarctica.

We will neglect the discussion of different surface densities at this point, but not because it is not important, only because we do not have the means to contribute something on the surface density from our work. From the mechanical perspective it is an input parameter that is unfortunately poorly constrained and we would be delighted to see more studies upcoming on the issue of surface density. A database similar to SUMup would be of great value.

l.279. It is not clear here whether the lower boundary for the simulation is the same as the lower boundary for the domain used to calculate the deviation between observed and modelled densities. The lower boundary for the simulation needs to be deep enough for the temperature to be constant, whatever the temperature variation at the surface.

The lower boundary for the simulation does not match the domain used for the computation of the deviation. The lower boundary of the comparison domain is either limited by the length of the measured density profile, the oldest horizon influenced by the forcing or the density of $540\,\mathrm{kg\,m^{-3}}$ (see also our response to your comment (4) regarding the optimisation domain).

However, 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 $10\,\mathrm{m}$ to $15\,\mathrm{m}$, 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 $150\,\mathrm{m}$ of firn is only about $1\,\mathrm{K}$, 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.

l.320 This would be a good point to tell the reader whether using the Alley model, with one parameter optimised, gives a better fit than using a semi-empirical model (e.g. Herron and Langway) with the densification rate optimised. And "a reasonable good match" needs to be defined in terms of the RSMD

Both referees gave us the feedback that an optimisation of the constitutive law by Alley (1987) using a single factor, should lead to the determination of a global factor C, which then could be applied to all analysed and probably more firn profiles. Such an approach would naturally lead to an evaluation of this factor C by comparing the different model approaches with the new optimised one.

This is not what we intended to do. However, we admit that the reader could get this impression, simply because we failed in making it clear enough. With our approach we wanted to evaluate the ability of the constitutive relation by Alley (1987) to reproduce measured firn density profiles and to identify possible hidden dependencies and improvements. At this point we see no possibility to determine a global factor C, which would result in a firn densification model performing better than other existing ones. We propose that, although the constitutive relation for grain boundary sliding is a good basis, an improvement, especially by a proper incorporation of the stress state, is needed.

The revised version of the manuscript features this more clearly. Additionally we point out that the formulation of a single value for the factor C makes no sense at this point in the discussion section.

l.330 This paragraph should come much earlier in the paper because it is key to understanding the physics of the densification problem. The ideas expressed are well-established in the literature, so should be reviewed before the authors proceed to describe their work. It would then be easier to discuss the choice of lower boundary condition, the role of layering and other factors in the light of current understanding.

In the revised version of the manuscript we establish the points addressed in this paragraph at an earlier point, when explaining the structure of the constitutive equation in Section 3.2 and the optimisation scheme in Section 4.

l.353 The question of whether horizontal divergence affects the creep densification rate was raised by Alley in the 1980s. It is not clear here whether the authors are suggesting that basal-sliding is also affected

> The point the authors intended to make here is not formulated clearly enough. Horizontal divergence due to movement of the underlying ice is not the process in question. We would rather suggest that when we consider a firn column, this firn column is confined in horizontal directions. Therefore a deformation in these directions is not possible. While the firn compresses in vertical direction, it can not elongate in horizontal directions. This means that the horizontal components of the stress tensor, even in a one dimensional approach, are not zero.
> Because firn is a compressible material, it is not trivial to compute these stresses. Simulation approaches considering the horizontal components of the stress tensor can be found for example in 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 in more detail.

l.371. This paragraph should come earlier, when the input data are introduced, so the reader knows their limitations from the start

> In the revised version of the manuscript we address the problem of uncertainties within the forcing data, as suggested by the reviewer, in Section 5.2, when introducing the boundary conditions and forcing data.

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