Response to review 1 on tc-2021-240

Thank you for taking your time to thoroughly review this paper. Your comments were of great value for improving the communication of assumptions and the theoretical basis of the paper.

General remarks

This paper proposes a method to account for strain softening in firn densification models. Most of the densification models used to predict the evolution of density with depth in polar ice-sheets are based on the assumption that the horizontal flow can be neglected, assuming an infinitely large and flat firn. Doing so, the density is only function of depth (1d model). Such assumption might hold at places such as the central parts of these ice-sheets (domes, ridges), but is certainly not anymore valid closer to margins. The proposed method account for strain softening by including the horizontal strain-rates in the effective strain-rate. Horizontal strain-rates ($\dot{\varepsilon}_{xx}$, $\dot{\varepsilon}_{yy}$ and $\dot{\varepsilon}_{xy}$) are assumed vertically uniform and estimated from surface velocity. The overall paper is well written and the figures are of good quality. I have nevertheless a number of criticisms on the model itself and on some hidden assumptions used to derive the model.

> **Author response**:
>
> Thank you again for your comments. After reading your comments it has become clear that we must make a more clear separation between the theory of the strain softening correction and our application of that correction. It is only our application that relies on horizontal strain rates being uniform with depth, and $\dot{\varepsilon}_{xz}$ being zero. The correction itself will also work in the more general case. We have revised the manuscript to clarify this point.

Most of the derivations rely on the assumption regarding the **form of the strain-rate tensor** (Eq. (4)). It is assumed that the shear components $\dot{\varepsilon}_{xz}$ and $\dot{\varepsilon}_{yz}$ are negligible in comparison to $\dot{\varepsilon}_{xx}$, $\dot{\varepsilon}_{yy}$ and $\dot{\varepsilon}_{xy}$. This assumption might be true for an incompressible ice column if one looks only at the top of the ice column as shear deformation will mainly concentrate close to the bedrock. But this is not anymore true if the upper part of the column is composed by a compressible material like firn (see as an example the vertical profil of horizontal velocity in Fig. 9.16 of Greve and Blatter (2009), which indicates larger $\dot{\varepsilon}_{xz}$ close to the surface than at the base). At least, this assumption should be discussed!

> **Author response**: We originally assumed that horizontal strain rates were uniform with depth, and $\dot{\varepsilon}_{xz}$ being zero. However, these are not actually necessary from a theoretical point of view. So, we have revised the theory sections of the manuscript to clarify this. We still use these assumptions when we apply it to EastGRIP and the ice sheets. We have added some additional justification for why this is a reasonable assumption in the interior of the ice sheets (we refer to borehole deformation measurements).
>
> In this context we note that Fig. 9.16 in Greve and Blatter (2009) shows the deformation of a borehole from an alpine glacier. At such glaciers the surface slope, flow velocity and the velocity gradient in vertical direction and thus the vertical shear rate are much higher

than at the ice sheets. The main application area of our model extension however are the polar ice sheets and ice shelves rather than alpine glaciers, where full-stokes modeling with the approach by Gagliardini and Meyssonnier (1997) model is more suitable to capture the complex geometry of the glacier.

Moreover, Fig 9.16 particularly indicates larger $\dot{\varepsilon}_{xz}$ near the surface, but we only apply the strain softening model to stage 2 of the firn densification. I.e. below the surface layers, where dislocation creep becomes dominant. Therefore, strong vertical shear rates in the near-surface layers are in any case not captured by our model, and they will not affect the densification rate via strain softening.

There are a number of **implicit assumptions** in the derivation of the model that are not clearly stated. The unknown strain-rate components $\dot{\varepsilon}_{xx}$, $\dot{\varepsilon}_{yy}$ and $\dot{\varepsilon}_{xy}$ are derived from the surface velocity, which implicitly assumes that they are depth uniform. This assumption should be clearly stated (much before line 314) and discussed more deeply. How much the fact that the firn is compressible will contradict this assumption, as for $\dot{\varepsilon}_{xz}$ and $\dot{\varepsilon}_{yz}$? I think that there is an other implicit assumption to derive Eq. (5): the firn particules flow only vertically, which then contradict the initial assumption that $\dot{\varepsilon}_{xx}$, $\dot{\varepsilon}_{yy}$ and $\dot{\varepsilon}_{xy}$ are non zero. In fact, one should follow the particles trajectory and integrate density along these trajectories from the surface down to a given depth. In other words, it is assumed that the flow is not anymore only along a vertical flow line, but some equations are still derived using the opposite assumption. Eq. (15) is used to derive r_{V}, but I am wondering how $\dot{\varepsilon}_{zz,\mathrm{c}}$ is estimated? This is not clearly stated anywhere.

Author response: We agree, and we have revised our manuscript to be more explicit about our assumptions.

The assumption that horizontal strain rates are uniform with depth is again not necessary from a theoretical point of view, but required for our application of the model as data of how the velocities change with depth are not available. At sites where this information is known from e.g. bore hole deformation measurements, it could easily be taken into account with the model. Nonetheless, this assumption is justified for the polar ice sheets, where the flow velocity is determined by basal sliding (where the ice sheet is not frozen to the bed as it is suspected for ice streams for example) and by internal deformation which is strongest in the bottom part of the ice sheet. The firn in the top layers mainly flows with the movement of the underlying ice. See e.g. Fig. 7 in Gundestrup et al. (1993).

Following the assumption that horizontal velocities do not change with depth, we do follow particle paths in our EastGRIP densification modeling (red lines in Fig1). At every timestep we model the densification rate of the entire profile for a given location by applying the input parameters for that location and then advect the entire profile with the flow (using surface velocities). This process is realized by backtracking the flow path of the firn and recording the horizontal strain rates at each step. These strain rates are then used as input to our model in the reversed order. Temperature and accumulation rate are not backtracked as we assume that the temperature is constant over the region and that the accumulation rate given by radar surveying already is a mean value over the flow path.

$\dot{\varepsilon}_{zz,\mathrm{c}}$ is computed from the densification rate given by the classical (HL) model using Eq. 5. We will ensure that this becomes clear from the manuscript.

Minor remarks

- line 19: that that

  Author response: fixed.

- line 57: suggested by Alley and Bentley (1988) to accelerate

  Author response: fixed.

- line 70: it is not because it has never been applied to polar ice-sheets that it cannot! By the way, the density functions a and b in the model developed by Gagliardini and Meyssonnier (1997) are calibrated using the density profile of Site 2 in Greenland. I am wondering what should we conclude from this paragraph regarding the approach of Gagliardini and Meyssonnier (1997)? Be more specific.

  Author response: We can only speculate why people have not applied this nice model on a large scale. Our guess is because it is more computationally expensive, is more difficult to implement, and it has not been calibrated and tested on as wide a variety of different conditions as the old-school classical models. The success of the empirical Herron-Langway model in that regard is truly remarkable. We believe that with our strain softening correction the applicable range of these simple models will be even greater. It is also simple to implement as long as you assume horizontal velocities are constant with depth. This is clearly a big improvement, even if there are edge cases where these assumptions are not 100% valid.

  We have examined the Gagliardini and Meyssonnier (1997) formulation (GM97) in more depth in our preparation for this response and will adapt the paragraph accordingly. We find that the strain enhancement predicted by this model depends critically on a/b which, unfortunately, is poorly constrained. See our reply to the comment to line 107 for more details..

- line 103: it is not clear here if you are speaking about your model or previous models that only consider vertical compaction? The proposed constitutive relation only hold in the later case.

  Author response: We acknowledge that our presentation of the assumptions and the motivation behind the method was inadequate. We have therefore revised this entire section to make our reasoning more clear, and the inherent assumptions more explicit. See also our responses below.

- line 107: this sentence about incompressibility is not clear. Firn is compressible, so there is no need of an incompressibility assumption. For a compressible material, the trace of the strain rate tensor is not null anymore, such that the two constitutive relations should be set between (i) deviatoric stress and deviatoric strain rate and (ii) trace of the strain rate (change in volume) and isotropic pressure (trace of the Cauchy stress tensor). See equations (9.53) and (9.54) in Greve and Blatter (2009) for example. Using the formalism proposed by Gagliardini and Meyssonnier (1997), one would not obtain Eqs. (7) and (8). There are certainly some other implicit assumptions beyond these derivations that should be stated more clearly (replacing a deviatoric stress by a Cauchy stress should be justified and motivated).

> **Author response**: We acknowledge that our presentation of the assumptions and the motivation behind the method was inadequate. We have therefore revised this entire section and now argue for our strain softening correction from another angle.
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> There are two perspectives we could take when deriving the strain enhancement factor. We could consider firn as a compressible material where compaction is related to isotropic pressure (as in GM97), or we could consider firn as a complex geometrical structure of solid ice and consider the implications for the densification. We have tested both approaches, and prefer the solid ice perspective as explained in the following.
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> We now start from the perspective that firn is a mixture of air and solid ice, and that firn densification is rate limited by the strain rate of the solid ice. We end this revised section with a summary reiteration of our assumptions. We hope that this makes our reasoning more clear, and the underlying assumptions much more explicit.
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> The model with all of its assumptions of course needs to be tested against data. A key feature of our model is that it has zero free parameters. Even without any tuning it is able to reproduce the amplitude of the BCO contour at EastGRIP. We consider this to be a very strong validation of the model sensitivity.
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> We have also derived a strain enhancement from the GM97 model (Gagliardini and Meyssonnier, 1997) in preparation for this response[1]. You are correct that the GM97 model results in a strain enhancement with a different functional form. We find that the strain softening enhancement based on the GM97 model depends crucially on the ratio between the a and b empirical parameters. These two empirical parameteres are determined by calibration to the Site2 density profile. Unfortunately, this calibration is ill-posed in exactly such a way that the ratio between a and b is poorly constrained (See the final section in Gagliardini (2012) on a 'Porous Law for snow and firn in Elmer/Ice'). This in turn means that the GM97-based strain enhancement factor is poorly constrained, and that we should be vary of any quantitative predictions using GM97 at present.
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> In Fig. R1.1 we compare the impact of the strain enhancement corrections obtained from the GM97 model with the data of Riverman et al. (2019) and our simpler model

[1] In order to derive the strain softening enhancement from GM97 we had to assume: 1) that pressure is unaffected by horizontal strains; 2) that the incompressible part of the vertical strain rate is $\dot{\varepsilon}_{zz,i} = -(\dot{\varepsilon}_{xx} + \dot{\varepsilon}_{yy})$; and 3) that $\dot{\varepsilon}_{xz}$ and $\dot{\varepsilon}_{yz}$ are neglible.

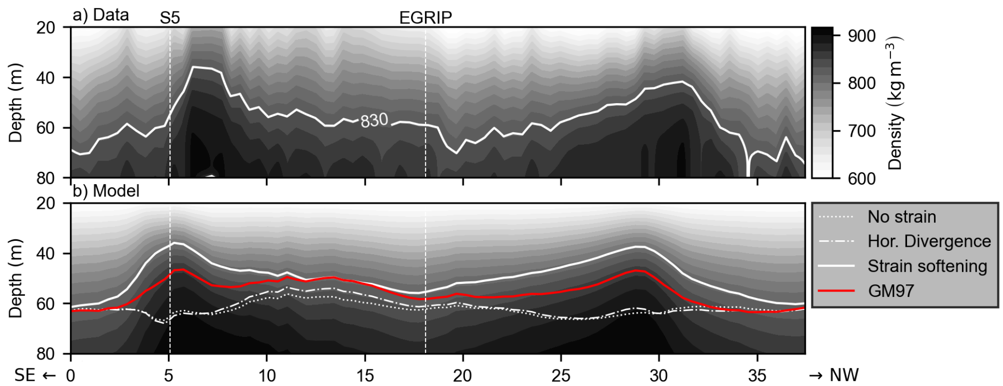

Figure R1.1: (a) Firn density profile across NEGIS recorded by Riverman et al. (2019, Fig. 9b). The white contour line indicates the firn-ice transition/BCO depth. (b) Modeled firn density profile for the same location using the corrected strain softening model. The contours show the BCO depth for the cases of no strain, horizontal divergence and strain softening model and the GM97 model.

across the NEGIS density profile. Qualitatively GM97 generally agrees with the results of our model - It also predicts a shallower firn pack in the shear margins. Quantitatively we can see that the effect is about half as pronounced when we use the strain enhancement derived using the GM97 model. Our model is better able to capture the observed amplitude of the BCO contour.

We note another issue with the GM97 model: It predicts a relatively higher strain softening contribution in areas with strong horizontal divergence (between 7km and 18km in Fig. R1.1). When the enhancement factor were increased to fit the data, the densification rates in areas of high horizontal divergence would be overestimated. Discussing this issue in full depth would exceed the scope of this manuscript, so to put it simple, it is caused by mixed terms that are introduced in the effective strain rate due to the more complex functional form of GM97 and the necessary assumptions. Deriving a GM97-based scaling factor therefore does not only require a refined calibration of a and b, but also requires additional theoretical work on how to deal with horizontal divergence as our current approach is incompatible with data.

So to summarize: Our model is simpler, fits reality better, and has zero free parameters. Thereby, it is valuable on its own for studying the impact of strain softening on firn densification and capturing the effect with little effort in good approximation. GM97 is a more complete (and really nice) physical description — but GM97 is also substantially more complex, and key parameters are poorly constrained. Our data comparison indicate that GM97 needs more empirical calibration before it can be widely applied (e.g. to shear margins near EastGRIP).

- line 144: as Oraschewski (2020) is a Master thesis, I think it would be better to include these results in an Appendix. Also, should write "derived in Oraschewski (2020).". Same line 158.

Author response: There are no results or conclusions in the present manuscript that rely on $n = 3$. So, it is not strictly necessary to include this as supplementary information. We point to the MSc thesis as a service to the reader. We note that the thesis has been archived at thesiscommons.org, has been assigned a doi, and is easy to access publicly. So we do not see any great value of adding the exact same information as a Supplement or an Appendix.

There are plenty of papers on TC that cite master theses. In our opinion this is good — as long as they are easy to access. It would be a shame if all that work is treated as lost unless it makes it into the peer reviewed literature.

The derivation of the approximate residual strain rate of $\dot{\varepsilon}_0 \approx -2 \times 10^{-4}\,\mathrm{yr}^{-1}$, as mentioned in line 158, will be added as supplementary material.

- line 144: it should be explained here how the different strain-rates in Eq. (16) will be estimated (from surface velocity for $\dot{\varepsilon}_{xx}$, $\dot{\varepsilon}_{yy}$ and $\dot{\varepsilon}_{xy}$ and some explanations are needed for $\dot{\varepsilon}_{zz,\mathrm{c}}$)

Author response: We will specify how the components can be obtained when discussing Eq. 14.

- line 179: the fact that $\dot{\varepsilon}_{xx}$, $\dot{\varepsilon}_{yy}$ and $\dot{\varepsilon}_{xy}$ are estimated from surface velocity and thus assumed depth uniform should be mentioned much earlier

Author response: The theory is more generally valid, but for our specific applications of the theory this assumption is necessary. We will address this issue together with the discussion of the strain rate components in Eq. 14.

- line 210: it is not clear if you account for temperature evolution with depth?

Author response: We do not account for temperature evolution with depth, as we force our model with constant temperature.

Temperature has a clear impact on the densification rate and the CFM framework does allow for detailed temperature evolution in the firn. However, this is particularly important in the surface layers where the temperature differs between seasons. Deeper in the firn the seasonal amplitude in temperature is much more attenuated. In this paper we are only concerned with firn stage 2 where dislocation creep is dominant and thus strain softening is active. In this stage the annual average temperature is adequate and we decided to simply use constant climatic forcing.

We note that in theory firn temperature can also be affected by internal heat production. Therefore, we also tested whether strain heating has a considerable impact on firn densification, but it proved to be negligible. See Fig. 5.5 in Oraschewski (2020).

- line 237: sensitivity to what?

Author response: Will be changed to ”sensitivity of firn densification to strain softening”.

- line 271: NEGIS by Riverman et al. (2019) are reproduced.

  Author response: fixed.

- line 286: I am not sure that this statement is true? Horizontal velocity from a circular ice cap are always divergent?

  Author response: We agree that the statement is not generally true. We will weaken it to ”If on a flat ice sheet velocities diverge at one place, they tend to converge elsewhere.”.

  We note that we have verified that this is true in the surroundings of EastGRIP — incl. the shear margins. See Fig. 4.1g in Oraschewski (2020).

- line 314: strange to discuss an assumption that is not even mentioned before

  Author response: Fixed. It is now clearly stated earlier.

- line 330 and below: units for year is yr. Some time, units are not italicized, some time they are (and should not be, as stated by TC rules). Check this over all the manuscript.

  Author response: fixed.

- line 356: over which period are you lookin for in term of climate forcing?

  Author response: We use the average over the entire RCM model period. For the Antarctic this is 1980–2017, and for Greenland it is 1980–2014. We clarify this by writing ”according to the multi year average of the HIRHAM5 output”.

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