Response to review 2 on tc-2021-240.

Thank you for taking your time to thoroughly review this paper. Your comments were of great value for improving the discussion of the implications of our work and for assuring that the message of the paper becomes clear.

General remarks

The authors present a simple modification that allows for the inclusion of strain in calculating firn densification rates. The model provides a satisfactory fit to available active seismic firn density estimates through the NEGRIS shear zones. The model as formulated further has large implications for firn densification away from high-shear environments, although this aspect of the model is not validated in a meaningful way. The modeling approach is clever and interesting, and overall the paper is well written and illustrated. The paper is suitable for publication in TC after some modifications.

> **Author response**: Thank you. In this paper we focus on the theoretical development of the strain softening correction. This is a correction factor which models how much faster the firn is densifying when subjected to horizontal strain. We develop the theory, make sensitivity tests, validate, and study the implications on continental scales. We do not re-calibrate the particular densification model that we apply the correction to. It is, however, only a correction factor and not a complete densification model. This means that any validation must focus on changes rather than absolute values — e.g. $\Delta\rho$ vs ρ or Δz_{BCO} vs z_{BCO}.
>
> In our validation we demonstrate that our model can fully explain the shallower firn pack observed in the shear margins of NEGIS. The fit is remarkable considering there is no free parameters in the model at all. This strongly indicates that the physical mechanism is real, and that our model has the correct sensitivity.
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> The strain softening mechanism will of course also be active at low strain sites. However, applying our correction to low shear sites will not automatically yield better fitting profiles than the classical 1D models, as they have been calibrated against real world profiles from low shear sites. Thus they already implicitly account for some small amount of strain softening in their empirical parameters. For this reason it is not very useful to validate our model against $\rho(z)$ at a low strain site without re-calibrating the underlying firn model. This is a big task in itself and would require a whole database of firn profiles. This is beyond the scope of this paper, where we focus on the theory and implications, but it is certainly something that should be done in future work.
>
> This is also why we frame our continental scale results in terms of changes rather than absolutes.

The model suggests strong impacts on firn densification rates for both high- and low-strain environments. For the former, the authors provide some validation using the NEGRIS seismic data. However, for the latter the authors make big claims but do not validate their model in any meaningful way.

Author response:

We find that the effect is substantial and important even at low strain rate sites, but we also emphasize that the effect is implicitly included in the empirical calibration of existing models (specifically Herron-Langway). So, when we estimate a 10% reduction in BCO depth due to strain enhancement in the interior of Antarctica, then we are claiming that old-school models like Herron-Langway would have errors of that magnitude. We believe this to be at the root of a misunderstanding concerning just how big our claims are.

We demonstrate that the physical mechanism is real. We also show that the sensitivity of the correction is correct in the high strain rate margins of EastGRIP. All physical reasoning tells us that the same mechanism would also be active at low strain rate sites, but we would expect the effect to be less pronounced.

Our model takes the form of a correction to classical models such as Herron-Langway (HL). For low shear environments the strain softening effect is smaller and can be easily compensated for by slight adjustments to the empirical constants in the classical models. Indeed, the empirical constants in a HL model have been tuned to environments with a small amount of horizontal strain, and so those constants already do account for some average strain softening across those sites (as discussed in the paper). So, for low strain environments we expect the HL to already provide a reasonable fit on average. However, being good on average is not good enough for many applications (e.g. ice core interpretation), and so it is common practice to re-calibrate a new densification model at every site. Our model explains how surface strain rates could possibly explain some of the inter-site variability. We therefore argue that all classical 1d models should be recalibrated with this effect in mind.

Therefore, cores from low shear sites are not a good validation of the correction in our model. We would have to start by adjusting the constants in the Herron-Langway model to compensate for the fact it has not been tuned to a zero shear environment. That would slow the densification of the HL model. Then we would apply our strain softening correction which would speed it up. In our opinion it would simply not be a convincing validation to show that our corrected model fits better than the slowed HL model. We would need at least two profiles from the same site but subjected to different strain rates in order to isolate the effect of the correction.

I request that the authors us firn density data to validate their model, because such data are the only true way to test the model validity. Unfortunately, a firn core from the NEGIS shear zone (EGRIP S5 2019) is not available due to COVID-19 restrictions in field work. Were there no density data taken in the field? Usually this is the first thing that is done as it is easy and requires no high-tech equipment.

Author response: We do not know the details of why density of the S5 core was not measured immediately when logging the core as is standard practice. We have however triple checked that this is indeed the case with the AWI people involved in the drilling. So, we just have to accept that this data not exist. We do, however, have the seismic density data that passes right next to S5 core. So while S5 density data would be nice to have, then data would provide little extra information for validation.

As we have explained above we have a model for a correction factor, and so a validation must focus on whether it can model the density difference between sites, rather than a good density profile at a given site. The seismic density profile (Fig. 3) is near ideal for this purpose. Our model reproduces the change in BCO depth over the shear margin near perfectly, with no free model parameters. How is this is not a true way to test the model validity? We consider this to be a very strong validation of both the physical mechanism and the model sensitivity.

The authors suggest the model has implications for firn densification modeling across all of Greenland and Antarctica. For example, the authors suggest the impact may be as much as 30% on the Delta-age calculated in WAIS Divide (WD). Such claims are important, and should not be made without any validation. Their claims ignore the fact that conventional firn models provide a good fit to the WD empirical Delta-age, and the WD firn density data.

Author response: We want to emphasize that our WAIS divide results are intended as a sensitivity test, and should not be taken as a new age model for the site. Our sensitivity test highlights the potential magnitude that strain softening could have at a site like WAIS Divide. We want to stress that we do **not** say that existing gas ages are wrong by xyz years — precisely because we realize that the WD density model is relatively tightly calibrated to local observations. This is why we carefully use words like "may", and emphasize that the estimated change in BCO age is conditional on a particular change in the horizontal strain rates.

Our results show that even a modest change in strain rate can have a substantial impact on gas age. We therefore argue that strain softening is a complicating factor in paleoclimatic interpretation. Ice flow velocities, and thus strain rates will almost certainly have been different in the past — especially at a site like WD. As an example Buizert et al. (2015) finds that "WD $\delta^{15}N$ starts to decrease around 20.5 ka BP, suggesting a thinning of the firn column". This is interpreted to be a response to early Antarctic warming. While this is completely reasonable, and we do not argue with the interpretation, then our results demonstrate that it could also be due to changes in ice flow.

At the only site where density data are shown (the EGRIP site, Fig. 2b), the "no strain" model actually provides the best fit to the data. The Antarctic map (Fig 5f) suggests several locations where the impact is 10% or more. This should easily be visible in the available firn density data (for example from South pole, EDC, EDML, Dome F and WAIS Divide).

Author response: Our model is a correction factor to existing densification models. It is not a complete densification model in itself. To isolate and validate the correction factor we should focus on changes to the density profile when exposed to differences in horizontal strain. We disagree that EastGRIP is the only site where data is shown. We also show the entire profile in Fig. 3, which arguably is a much stronger validation as it directly shows the change in density due to the effect we model.

We agree that the un-corrected HL model fits EastGRIP better, but it has a massive misfit in the NEGIS shear margins. So the correction is clearly a necessary improvement if you want the model to fit both sites. Further, the empirical calibration of the HL model will account for some amount of horizontal strain as it has been calibrated to sites that are affected by some small amount of strain.

By chance the effective horizontal strain rate at EastGRIP is approximately as big as the average effective horizontal strain rate of the firn core sites that were used for tuning the HL model. It is therefore expected that the HL model provides the best fit to the data. The mismatch exactly shows that the HL model needs to be recalibrated with strain softening being taken into account. For this reason we introduce $\dot{\varepsilon}_{\text{cor}}$ as a first-order correction for the mismatch and the corresponding implicit strain softening contribution in the HL model.

The authors should either (1) demonstrate that their strain-enhanced model indeed improves the density data fit at various low-strain sites where such data are available, or (2) remove statements about the impact of their model on low-strain sites.

Author response: We do not argue that the strain enhanced models fit low strain sites better out of the box. Most existing models probably already account for some small amount of strain softening as they have been calibrated to data from real sites (usually with low-strain rates). So the existing models may work reasonably OK at an average low strain rate site, but for the wrong reasons. Even if existing models already can fit a low strain site, then that does not mean that the strain softening is not active at the site.

Correcting models for an effect that is already included will of course not improve the fit. This does not mean that the correction factor is wrong, but rather that the existing models need to be re-calibrated with the strain enhancement accounted for, so that the model is more generally applicable. Indeed, we spend considerable space arguing that existing models need to be re-calibrated to account for this known effect.

The authors suggest a threshold (epsilon_cor = 4E-4) below which strain has no impact on densification rates. Establishing the value of this threshold seems important to how the model performs. However, the authors use a very arbitrary definition of epsilon_cor, namely the average strain rate at the calibration sites of the HL model. It seems to me that average HL value gives a lower bound estimate on epsilon_cor, but that it could easily have a much higher value. If we use epsilon_cor is 1E-3 for example, the HL model would still fit its calibration data set equally well – while presumably also fitting the NEGIS data.

Author response: We do not argue that strain softening is irrelevant when $\dot{\varepsilon}_{\text{E,H}} < \dot{\varepsilon}_{\text{cor}}$. Rather, we argue that the original classical Herron-Langway model has been calibrated to a situation with this much strain softening on average. So, that the particular model (=HL) probably works best at sites with exactly that value.

The idea of the correction factor was to subtract this mean value from the total horizontal strain rate at a specific site in order to avoid taking this small contribution into account twice. This approach however does not work at sites with lower effective horizontal strain rates as it cannot become negative. Therefore, these values were simply fixed to 0. Nevertheless, after revising this problem now, we will change the methodology of the correction factor such that it also takes into account smaller values:

The densification rate output of the HL model can be interpreted as the densification rate by temperature/accumulation rate forcing times the scaling factor for strain softening due to an effective horizontal strain rate of $\dot{\varepsilon}_{\text{cor}} = 4 \times 10^{-4}\ yr^{-1}$. To determine the densification rate for the case of no strain we therefore need to divide the densification rate give by the HL model with the $4 \times 10^{-4}\text{yr}^{-1}$-scaling factor before multiplying it with scaling factor for the strain rate that is present at the site that we model. In this way we can also achieve scaling factors smaller than one at sites where the effective horizontal strain rate is lower than $4 \times 10^{-4}\text{yr}^{-1}$.

In this way, the corrected strain softening model matches the HL model at EastGRIP where $\dot{\varepsilon}_{\text{E,H}} \approx \dot{\varepsilon}_{\text{cor}}$. Outside the ice stream – where effective horizontal strain rates are even lower – the firn thickness is now increased. Thereby the model fit is increased compared to the no-strain model which is a further indicator that the correction factor and the sensitivity of the model are accurate. The adapted correction now also has a stronger effect in the high-strain shear margins and again increases the firn thickness here, as well. The discussions of the correction factor in the manuscript will therefore be adapted accordingly.

Minor remarks

- L16: "when old snow, respectively firn" The grammar seems off here. What is "respectively" mean here?

  Author response: fixed.

- L29: remove "isotope"

  Author response: fixed.

- L29: Delta-age is determined by the ice age at the lock-in depth, not the BCO

  Author response: That is correct and we have rephrased the sentence to be more clear on this point. Nonetheless, the age at the lock-in depth and the BCO age are closely related. The lock-in depth is affected by the occurrence of high-density layers that are caused by seasonal variability of precipitation and surface densities. As we do not take these into account, we do not model the lock-in depth and therefore look at BCO age instead.

- L40: some models further consider grain size/growth (such as Arthern) or dust loading (such as Breant)

Author response: We mention impurity content as an additional possible input parameter now. Grain size and growth however are again parameterized by temperature and accumulation rate and would fall under physical descriptions of firn processes.

As the majority of the classical models still only takes temperature and accumulation rate into account, we keep denoting them as climate-forced.

- L78: Replace "respectively" with another word.

  Author response: fixed.

- L108: Firn exhibits very strong horizontal density layering. So isotopic is not really true.

  Author response: We agree and only meant the crystallographic isotropy. Currently the sentence is removed after changes for R1.

- L110: where did the exponent n go?

  Author response: The exponent n went into the nonlinear strain dependence of the viscosity. See Eq. 8.

- L221: The firn data in Fig. 2b suggest a lower value of $< 300\mathrm{kg/m}^3$

  Author response: For the model runs in the NEGIS region the surface density is changed to 295m. This is approximately the density of the top 0.1m observed by Schaller et al. (2016) at EastGRIP.

  Fig. 2b still shows lower density values observed in the firn core. Near the surface – where the firn is still soft – material of the core is easily lost before weighing the core section and hence a bias of the density towards lower values is plausible. Therefore, we follow the dedicated surface density studies by Schaller et al.

- L260: again, "respectively" is used in a way I don't understand

  Author response: fixed.

- L264: data is plural ("are not available")

  Author response: fixed.

- L267: This is an interesting observation. At even higher strain rates, do you obtain a kink in the opposite direction? This is a good target for model validation.

Author response: Yes, for very high strain rates (at approx. $\dot{\varepsilon}_{\mathrm{E,H}} > 10^{-2}\ \mathrm{yr}^{-1}$) our model predicts a kink in the opposite direction. The kink is however rather week and might require firn density measurements with high accuracy to be unambiguously observable. A more pronounced kink potentially requires unrealistically high strain rates as the sensitivity of firn densification to strain softening decreases at high strain rates. To our knowledge at the moment no firn density data from a region with sufficiently high strain rates exist.

- L298: I am confused. What data and what model? I assume this is fig. 3b?

  Author response: Correct. We will add that figure reference.

- L299: remove comma after "No"

  Author response: fixed.

- L300: is the movement of the ice stream a reasonable explanation here? The firn is only 200 years old, so that would imply a lateral movement of 2km / 200 yr = 10 m per year (ballpark estimate). That seems like a lot to me – please comment.

  Author response: We know of two other EastGRIP manuscripts currently in the works which demonstrate that the shear margin is not stable now, and has not been stable in the past (using completely different data). So, Yes. With this in mind it seems very reasonable to us. Especially considering there are no other candidates for a physical mechanism that could explain the shallow firn thickness at x=7km. We cannot cite these new studies yet, and so are careful to only offer it as a potential explanation. Our only other candidate explanation would be that there is a misalignment of the gps-positions and the seismic data. We have however, double checked this with Riverman and she assures us that it is correct.

- L322: What kind of firn air processes are you talking about? Firn air diffusion?

  Author response: Yes. It could be diffusion and gravitational fractionation. But we are sure that there are other firn air processes that we have not thought of that could benefit from having a natural laboratory like this: Two neighboring sites with essentially the same surface climate, but with different densification profiles. We do not want to be too specific in this sentence as this is beyond our expertise. We just want to highlight the potential because we hope that somebody will think of ways to exploit this in future work.

- L332: Is that indeed the horizontal strain rate at WAIS Divide, or is this just an example?

Author response: We only have the strain rates derived from the ice sheet wide velocity fields. It is a value representative of the immediate vicinity of WAIS. However, there is some of scatter when looking at the pixels around WAIS. Further, the WAIS core is of course influenced by upstream past strain rates rather than the strain rate right at WAIS. For that reason we do not claim that our value is the WAIS strain rate, and instead frame the entire WAIS analysis as a sensitivity test.

- L333: this is much larger than the WD Delta-age, which is 205 years at present as determined from firn air sampling (Battle et al., 2011). The WD firn density model by Buizert et al. (2015) fits this empirical constraint well, suggesting strain can safely be ignored at WD. To get a meaningful Delta-age the authors should use the ice age at the lock-in depth, and correct for the gas age at that depth.

  Author response: Those data only shows you that you can ignore strain softening at present. Partially because existing models implicitly account for a small amount of strain softening. However, the flow regime at WD could easily have been different in the past. Our point is that this has to be considered when modeling the past. This is what our sensitivity test shows. A small change in strain rate can have a substantial effect on BCO age (and by extrapolation also on gas age). Thus you cannot simply conclude that the effect can safely be ignored at WD. That would be exactly like assuming that you can safely ignore changes in temperature, accumulation, or impurity content just because the model fits well at present.

  In this paper we are not modeling the gas age, and we want to keep the manuscript focused on the correction itself. So we will not use lock-in depth. Our goal is not to make a new model of the gas age at WD. We are only making a sensitivity test. For this purpose it is entirely sufficient to look at the **change** in BCO. Our assumption is that if the BCO age changes by some percentage, then that will be reflected in a corresponding change in the gas age.

  We will revise this section to stress the fact that this is a sensitivity test and not a correction to the WD gas ages. We will also highlight the difference between BCO age and gas age.

- L334: But his changes the Delta-age in the wrong direction. To obtain a smaller bipolar phasing (122 vs. the original 219) would require making WD Delta-age LARGER. So the strain correction you suggest works in the exact opposite direction of the observed correction.

  Author response: This is an interesting point, and we plan to add a sentences to highlight this. However, the strain softening correction can work both ways. It depends on whether you go from low-to-high, or from high-to-low strain rates. Could past strain rates be smaller than present. The point of this entire section is to make a sensitivity test.

  We will revise the section to make it absolutely clear that this is a sensitivity test, and that the results are just intended to gauge whether the effect should be considered or not. We are not making a new model of WD.

- There are firn density data available for WAIS Divide. I would recommend you try to actually fit the firn density data (and the observed empirical Delta-age) before claiming that the established Delta-age of an ice core is incorrect by 33%.

  Author response: First we want to stress that we are NOT claiming that the delta age is wrong by 33%. We have apparently not been sufficiently clear that this is only a sensitivity test. Our intent is to demonstrate that the effect can potentially be large, and must be considered. We will revise this section very carefully to ensure that this is 100% clear.

  It would make sense to fit WD data if we were trying to make a new gas age model for WD. But we are not. As we argue elsewhere: Fitting the WD firn profile would not be a test of the strain softening correction, but just a test of whether Herron-Langway fits. For that reason it would strongly deviate from the focus to add the WD data to this paper.

- Line 337: again, this is in the wrong direction. To reduce the Greenland-WD phasing, one would need to INCREASE the WD Delta-age

  Author response: It can be either direction depending on whether past strain rates were greater or smaller than present-day strain rates. Further, it is interesting and important regardless of the direction.

  But we agree that this is a really important observation: "To reduce the Greenland-WD phasing, one would need to INCREASE the WD Delta-age" - We will revise the manuscript to explicitly highlight this.

- Line 341: Note that Buizert et al. (2021) also rely on borehole thermometry, and that the past temperature estimates from both methods agree well.

  Author response: We are not saying that the Buizert2021 approach does not give reasonable estimates. We are only saying that strain softening is a complicating factor (like impurity loading). The really nice thing is that if the two methods agree then that should place limits on how large the strain rates can possibly have been in the past.

- Line 341: What do you expect strain rates during the LGM to be like? I would expect them to be lower, as the acc rates, surface slopes and velocities are all expected to be lower in the interior. In that case, wouldn't this make the firn column thicker during the LGM? To fit the data constraints with a more viscous firn column, one would have to make the LGM temperatures even warmer than Buizert et al. (2021) do.

  Author response: We agree that this is a reasonable speculation. Lower accumulation must tend to give smaller velocities just from a naive flux balance consideration. If that is the case then strain softening would be reduced in the LGM compared to present. So if that speculation holds then the reduction in strain softening would increase the WD Delta-age and thus reduce the Greenland-WD phasing.

If we look further back in time, then we know that the bottom of the WD core is quite young, and there are some studies that suggest that WAIS might have been much smaller during the LIG. Those observations suggests that the flow patterns at WD could have been very different in the oldest part of the core. That could potentially have a large effect on the strain softening.

- L349: Can you please clarify your approach? Are you making some kind of look-up table to then interpolate to get the values in the GIS an AIS? Why not just use the gridded spacial forcing and strain rates to force the model?

  Author response: The two methods are equivalent. We do it this way as it is much more computationally efficient.

- L404-405: This important conclusion is based only on the model, but not validated with any data. I think validation is necessary before making claims about the validity of the method outside of high-strain environments.

  Author response: The fact that the sensitivity at high strain rate sites is accurate constrains the sensitivity at low strain rates. We know the effect is zero at zero strain rate and we have validated that the sensitivity is good at high strain rate as we are able to reproduce the peaks in Fig. 3b. The correction curve between these high and low strain rate sites has to be monotonically increasing (see Fig. 2). This therefore sets a lower boundary for the strength of strain softening at sites with lower strain rates. Moreover, it is physically reasonable that the sensitivity decreases with increasing strain. The driving force of densification is the load of the overlying ice. Strain softening only speeds up the process. Because the load needs to build up first, the maximum reduction of firn thickness by strain softening must be limited. Physical reasoning now tells that the corresponding value must be approached asymptotically and that the sensitivity must be decreasing.

  We can also verify whether the low strain rate sensitivity is reasonable by examining if we can reproduce the BCO contour gradient between $x = 17$km (where $\dot{\varepsilon}_{E,H} \approx 0$) and $x = 23$km (where $\dot{\varepsilon}_{E,H} \approx 1\text{kyr}^{-1}$) while $\dot{b}$ is relatively constant (Fig. 3b). In our opinion this is a good fit and demonstrates that the sensitivity of the model at low strain rates is adequate.

- L417: what do you mean be "synchronizing with gas isotopes"? Do you mean d18O-O2? This is unclear to me. synchronization is often done with CH4, not with isotopes.

  Author response: Agreed. This sentence will be fixed.

- Fig 2b: Your value of the surface density is too high.

  Author response: The surface density for this plot is changed to 295kg/m^3. See reply to L221.

- Fig. 5d: why are there white patches in the Antarctic firn thickness? Did the climatic conditions go outside your look-up table? Please fix.

  Author response: Fixed for the EAIS. The white patches in the polar hole are caused by a lack of ice velocity data at these locations.

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