The Cryosphere Discussions

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Comment on tc-2021-240

Anonymous Referee #2

Referee comment on "Modeling enhanced firn densification due to strain softening" by Falk M. Oraschewski and Aslak Grinsted, The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-240-RC2, 2021

Review of: “Modeling enhanced firn densification due to strain softening” by Oraschewski and Grinsted

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.

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 NEGIS seismic data. However, for the latter the authors make big claims but do not validate their model in any meaningful way.

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.

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.

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).

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.

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.

Other comments

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

L29: remove "isotope"

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

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

L78: Replace "respectively" with another word.

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

L110: where did the exponent n go?

L221: The firn data in Fig. 2b suggest a lower value of <300 kg/m^3

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

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

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.

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

L299: remove comma after "No"

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.

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

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

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.

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.

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%.

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

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

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.

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?

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.

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.

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

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