Review **tc-2021-240** *"Modeling enhanced firn densification due to strain softening"* by Oraschewski and Grinsted

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 ($\dot{\epsilon}_{xx}$, $\dot{\epsilon}_{yy}$ and $\dot{\epsilon}_{xy}$) 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.

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 $\dot{\epsilon}_{xz}$ and $\dot{\epsilon}_{yz}$ are negligible in comparison to $\dot{\epsilon}_{xx}$, $\dot{\epsilon}_{yy}$ and $\dot{\epsilon}_{xy}$. 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 $\dot{\epsilon}_{xz}$ close to the surface than at the base). At least, this assumption should be discussed!

There are a number of **implicit assumptions** in the derivation of the model that are not clearly stated. The unknown strain-rate components $\dot{\epsilon}_{xx}$, $\dot{\epsilon}_{yy}$ and $\dot{\epsilon}_{xy}$ 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 $\dot{\epsilon}_{xz}$ and $\dot{\epsilon}_{yz}$? 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 $\dot{\epsilon}_{xx}$, $\dot{\epsilon}_{yy}$ and $\dot{\epsilon}_{xy}$ 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 $\dot{\epsilon}_{zz,c}$ is estimated? This is not clearly stated anywhere.

Minor remarks:

- line 19: that that
- line 57: suggested by Alley and Bentley (1988) to accelerate
- 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.

- 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.
- 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).
- 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.
- line 144: it should be explained here how the different strain-rates in Eq. (16) will be estimated (from surface velocity for $\dot{\epsilon}_{xx}$, $\dot{\epsilon}_{yy}$ and $\dot{\epsilon}_{xy}$ and some explanations are needed for $\dot{\epsilon}_{zz,c}$)
- line 179: the fact that $\dot{\epsilon}_{xx}$, $\dot{\epsilon}_{yy}$ and $\dot{\epsilon}_{xy}$ are estimated from surface velocity and thus assumed depth uniform should be mentioned much earlier
- line 210: it is not clear if you account for temperature evolution with depth?
- line 237: sensitivity to what?
- line 271: NEGIS by Riverman et al. (2019) are reproduced.
- line 286: I am not sure that this statement is true? Horizontal velocity from a circular ice cap are always divergent?
- line 314: strange to discuss an assumption that is not even mentioned before
- 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.
- line 356: over which period are you lookin for in term of climate forcing?