

This article presents a method for deriving the two rheological functions of the firn law, originally proposed by Gagliardini and Meyssonier (1997), based on two density profiles measured in northeast Greenland. Noting correctly that one-dimensional model does not accurately reproduce the strain-rate and stress history experienced by the snow, the authors developed a strain-rate and stress model based on the observations. My first major criticism is that, due to the ice dynamics at the sites, the assumptions made to infer strain-rate and stress history might strongly influence the results. Furthermore, the assumed state of stress, unconfined compression, is not appropriate for a sample of firn confined within an ice-sheet. Confined compression, assuming no lateral deformation, would be more appropriate (a standard assumption for one-dimensional vertical model). To what extent are these hypotheses responsible of the differences obtained with the rheological functions of Zwinger and others (2007)? I develop a bit more these two points in the list of comments below.

I have compiled a list of comments below that I hope will help improve the current manuscript (listed in order of appearance).

- lines 39: you could list some of the papers that have used this law (at the end there is quite a number of applications using this law, but not in the context of interpreting ice-sheet density profile).
- Caption Fig. 1: You should mention that it is **surface** effective strain rates. Strain-rate at depth will be different because of the complex dynamics at these two sites. This should be discussed later in the model assumption. Same for the back-trajectory, they are reconstructed from the present day flow from the observed surface velocity ? It then does not account of the real trajectory of the snow/ice particule.
- Caption Fig. 2: from the text one understands that it is along the core. How do you link a given depth in the ice core with a given point on the back-trajectory? How do you account for the vertical velocity? This should be clearly stated in the text.
- line 94: a uniform temperature? What is important is how evolve the temperature with depth. If it is not uniform with depth, which I suspect is the case, then the evolution with depth should not be neglected as it will change the ice viscosity with depth and can explain, as the evolving anisotropy discussed later, the mismatch of the rheological a and b functions.
- line 103: Glen-Nye rheology for ice
- line 124: Is an elementary volume of snow within the ice sheet subject to unconfined compression? I believe it is actually confined compression, since the medium is infinitely large and the assumption is that lateral deformation is therefore zero. Unconfined compression would apply to a snow sample with free lateral sides under a compression apparatus. Since the solutions for unconfined and confined are very different, this choice strongly influences the derived rheological functions a and b. In reality, an elementary volume of snow/ice is subject to neither an unconfined stress nor a confined state of stress. Only a three-dimensional model would adequately resolve the history of stress and strain-rate history experienced by the snow/ice as it flows within the ice-sheet. This strong assumption should be at least discussed. Furthermore,

since it acts as a bound, the solution for the opposite bound (confined compression) could also be derived.

- line 139: this is a strong hypothesis? Especially regarding all the critics to the 1D model in the introduction and the choice of the site for which the horizontal velocity is not vertically uniform because of basal friction.
- lines 163-164: why not using the more classical Cuffey and Paterson 2010 formulation? How much the results are sensitive to the choice of the parameters in the Glen-Nye flow law? Also, I understand that the temperature was constant from what is mentioned above (line 94)?
- line 175: in the original paper by Gagliardini and Meyssonier (1997), the parameters a and b have were not calibrated solely using Ezz at single sites, but also based on mechanical tests for which the two parameters could be derived simultaneously. The various formulations are more on certain relationships that the parameters a and b had to satisfy and that had not been taken into account in the initial work by Gagliardini and Meyssonier (1997).
- line 193: the density profile is not a flow line as far as there is horizontal velocity.
- line 215: history experienced by a firn volume along a flow line.
- line 248: because the model is forced to fit this profile, this is then not a validation in itself.
- Figure 3: There is an issue here. As stated in the text, the limite of the parameter a for ice should be 1. Here you obtain 0.1!
- lines 265-271: all the discussion suffers from the strong assumptions made before. The non-monotonic variation with density of the viscous Poisson ratio is difficult to understand. What would in the microstructure evolution explains that? Can it be that the difference with Z07 on the functions a and b is not a result of the the applied strain-rate and stress history?
- line 274: Z07 was already defined above
- lines 294-301: Do you really expect a factor of 10 due to the anisotropy at 24 m depth? At least this should be quantified but the anisotropy at 24m depth is not so marked? Physically, the inferred parameters a and b are then of no use for an other sites as parameter a does not fulfill $a > 1$ when the density approaches that of solid ice. Then it does not make any sens to compare it to Z07 which, by construction, fulfill this limit.

References

- Gagliardini, O. and J. Meyssonier. 1997, Flow simulation of a firn-covered cold glacier. *Ann. Glaciol.*, **24**, 242–247.
- Zwinger, T., R. Greve, O. Gagliardini, T. Shiraiwa and M. Lyly. 2007, A full Stokes-flow thermo-mechanical model for firn and ice applied to the Gorshkov crater glacier, Kamchatka. *Ann. Glaciol.*, **45**, 29–37.