On the contribution of grain-boundary sliding to firn densification - an assessment using an optimisation approach. Schultz et al

1 General Comments

This paper revisits a physics-based model for the densification of dry snow by grain-boundary sliding (Alley, 1987). The model contains a number of parameters which can, in principle, be determined *a priori* and requires a knowledge of temperature, accumulation rate and grain size in order to calculate the densification rate in the upper part of a firn layer. Alley tested his model on 4 profiles, starting at 2 m depth, and optimised one parameter, the grain-boundary viscosity, ν, for each profile. He then found that the local best values for ν fitted an Arrhenius law with an activation energy of 41 $\pm$ 2 kJ mol-1. The 1987 paper does not show explicitly how well the model works if values of ν are calculated using this global equation, but the results given look promising. It is certainly very worthwhile for the authors to use the mass of new density profile data available from the SUMup data base to make a more complete assessment of the Alley model.

In general the presentation is clear, although the English needs some editing, and the results are potentially of interest to others working in the field. However, the paper could be much improved by a deeper discussion of the assumptions made and the significance of the results. Some suggestions follow:

(1) The SUMup data base is an on-going community effort which sets a very clear condition for use of the data. The guidelines state "When using this data set please cite both the individual researchers who provided the data as listed in the Citation column as well as the SUMup dataset". There is a citation for one profile in this paper (ngt03C93.2, shown in a figure) but there are no citations for the 158 other profiles used. This may seem like a tedious task, but it is essential that researchers are properly acknowledged. It may be that the editor will accept a table of profiles with citations as Supplementary Material, but whether this is adequate needs to be confirmed with the SUMup team.

(2) The authors have constructed a numerical model for the evolution of temperature, density and grain radius in firn using the Alley densification equation and forced by annual values of surface temperature and accumulation rate from the RACMO2.3 meteorological model. The advantages of using the RACMO data are (i) that profiles for which *in situ* climate data are not available can be simulated and (ii) that inter-annual climate variations can be taken into account. The disadvantages are that (i) local data have to been found by interpolation and (ii) systematic errors may be introduced. It would be helpful if the authors could provide an assessment of how accurate the forcing data are likely to be.

(3) Since the input temperature is only available at yearly intervals, the annual variation of temperature in the top layer of the firn cannot be simulated. Alley dealt with this problem by excluding the top 2 m of the firn, which experiences the greatest temperature fluctuations. In l.203 the authors state that they

optimise in a domain "bounded by the surface and the oldest horizon within the profile affected by the forcing". In fact some records, including ngt03C93.2, shown as an example in the paper, do not extend to the surface. I assume the authors actually use an upper bound as near to the surface as possible. In any case, it is quite important to discuss why they choose not to follow Alley's example. The reader will need some convincing that inclusion of data from a region with strongly-varying temperature does not affect the optimised parameter values.

(4) One of the factors in the Alley equation (α') describes the effect of spherical averaging of the sliding velocity along variously-oriented grain boundaries to obtain the mean vertical velocity. Based on geometric arguments Alley suggests $\alpha' = (1 - N/6)$ where N is the coordination number and notes that, by observation, $N \approx 10\rho/\rho_i$. The authors make the interesting comment that $\alpha' \to 0$ for $\rho = 550$ kg m^{-3}; in other words the Alley equation implies that grain boundary sliding must stop at this density. An alternative expression for α' from Bréant implies that grain boundary must stop at a higher density of 596 kg m^{-3}. The authors choose to restrict their optimisation domain to densities below 540 kg m^{-3} " due to the asymptotic characteristic of the resulting density profiles using the... constitutive equation" (l.209). But the reader might wonder why the cut-off should not come when densification by grain-boundary sliding is no longer much greater than densification by other processes (e.g. dislocation creep). There is a brief mention of the possibility of competing processes in the discussion section of this paper (l. 328) but this comes only as a suggestion for future consideration. I think the reader would appreciate a much deeper discussion of the implications of this choice of cut-off density, earlier in the paper.

(5) Another problem arises because the authors have not chosen to discuss the question of layering in firn density profiles, even though their example profile clearly shows large fluctuations in density superimposed on the general increase in density with depth. They use a microscale, physics-based model, which applies to a small element of snow, and a grid spacing of 48 points per annual layer, so it would be possible to investigate the effect of annual layering. The authors may choose not to do this, but it is important then to bring out their underlying assumption, which is that the Alley equation can be applied on the macroscale by simple substitution of the macroscale mean density. Given the work that has gone into writing the model and selecting profiles to simulate, it would a pity not to take the opportunity to include some discussion about the problem of up-scaling.

(6) In the paper 4 versions of the Alley model are tested by optimising one parameter C_ν per version. The reader will want to know how the values of ν implied by values of $C_{\nu 1}$ compare with the Alley values and also what the individual values of the cost function RSMD are. These could be included in a table of sites, input data and results in the Supplementary Material.

(7) The authors conclude that their results show that "the description of grain boundary sliding introduced by Alley (1987) is suitable for the simulation of firn densification at low density" (l. 362). In fact this would only be true if the value of parameter C_ν could be specified *a priori*. The authors do not

say that the linear relations with mean annual temperature and accumulation shown in Figures 7 and 8 could be used to determine global values of C_ν but, if this is what they mean, then we need to know what the cost functions are when profiles are simulated using these global values.

(8) On the whole the reader is left with the impression that the Alley model is inadequate, because the supposedly-fixed parameter C_ν varies with the input conditions. This may be somewhat unfair. The variation may arise because the model has been applied too near the surface, layering has been ignored or indeed, as the authors remark, the input meterological data are inaccurate. But if there is a real dependence on accumulation rate this can be traced back to the calculation of stress t_{zz} (Equation (6)). Do the authors think that their results show that the vertical grain sliding velocity is not linearly related to the vertical force on the grain? Could this be because other non-linear processes are acting as well as grain-boundary sliding? Or is the Alley (1987) analysis too simple?

2 Other Comments

- l.2. "first introduced by Alley (1987)" Perhaps better to say that the theory of grain-boundary sliding in snow was developed by Alley in this paper? The concept that this process might occur was already in the literature.
- l.15. "a couple of research fields" implies two fields, whereas the authors list three
- l.20 "The first and greater (category)". This sounds like a value judgement; better to say that the majority of models fall in the first category
- l.22 "this approach neglects". Do these models really neglect overburden stress or is it implicit rather than explicit in the model equations?
- l.32 onwards "which applies for firn" . The paragraph goes on to say the theory does **not** apply to firn below the critical density. This is confusing. Does the theory not apply because the snow is not compacted enough? Or because the grain geometry is not regular?
- l.40. Theile et al (2011) "tried to point out" or "did point out"?
- l.43 Are these 4 profiles also simulated in this paper? Which are they?
- l.101 m w.e.a^{-1} is used elsewhere; why does m w.e. s^{-1} have to be used here?
- l.107. Why does 2/15 seem an arbitrary number? To say it results from the geometric deviation does not really explain to the reader where it comes from. Since the paper is based on the Alley model, expanding the derivation would be worthwhile. Then the reader would understand the physics behind the model without having to go back to the original paper
- l.109. Is the word "resembles" right here? Maybe "represents"?

- l.128. Is “test volume” right? Maybe “volume element”?
- l.150 Would it be worth pointing out that Alley used observed values of r?
- l.200 This sounds as if the authors do not think the Lundin and Verjans papers use an objective cost function. The sentence could be altered so it is clear that the authors are explaining that they have used a different objective cost function.
- Figure 2. Is 2(c) a true representation of the input to the model? Or is the accumulation constant for a given year?
- l.279. It is not clear here whether the lower boundary for the simulation is the same as the lower boundary for the domain used to calculate the deviation between observed and modelled densities. The lower boundary for the simulation needs to be deep enough for the temperature to be constant, whatever the temperature variation at the surface.
- l.278 The difference in surface density between Greenland and Antarctic profiles merits further discussion. To say merely it is “plausible” misses an opportunity to comment on different types of snow (see Salamatin for example) and whether the densification rates might be different for Greenland and Antarctica.
- l.320 This would be a good point to tell the reader whether using the Alley model, with one parameter optimised, gives a better fit than using a semi-empirical model (e.g. Herron and Langway) with the densification rate optimised. And “ a reasonable good match” needs to be defined in terms of the RSMD
- l.330 This paragraph should come much earlier in the paper because it is key to understanding the physics of the densification problem. The ideas expressed are well-established in the literature, so should be reviewed before the authors proceed to describe their work. It would then be easier to discuss the choice of lower boundary condition, the role of layering and other factors in the light of current understanding.
- l.353 The question of whether horizontal divergence affects the creep densification rate was raised by Alley in the 1980s. It is not clear here whether the authors are suggesting that basal-sliding is also affected
- l.371. This paragraph should come earlier, when the input data are introduced, so the reader knows their limitations from the start