Review of: **Kuiper**, Weikusat, de Bresser, Jansen, Pennock and Drury "Using a composite flow law to model deformation in the NEEM deep ice core, Greenland: Part 1 the role of grain size and grain size distribution on the deformation of Holocene and glacial ice"

By **Dave Prior** University of Otago.

This is an important paper and an excellent piece of science. The manuscript needs some significant modification to help readers understand the paper, to highlight its importance and improve potential impact. The significance of this paper is that it demonstrates that a composite flow law that involves both grain size sensitive and grain size insensitive deformation mechanisms can be used to model the behavior of polar ice deforming at natural conditions. Moreover the analysis suggests that under low stress conditions (ice divides) the grain size sensitive mechanisms could dominate the rheology. I have also reviewed the part two paper and I think the authors decision to separate the two papers is a good one. The outcomes are clearer and impacts are more effective as two papers.

I also have an annotated pdf the authors can have.

The mistake in the Goldsby & Kohlstedt composite flow law.

The paper corrects a mistake in the dislocation creep component of the Goldsby & Kohlstedt composite flow law. The validity of the correction is nicely illustrated in Fig 6a. The way this correction is used is convoluted and I think a reader who is not aware of this issue will be thoroughly confused. There are two problems:

1. The manuscript does not make it clear this is a mistake in the Goldsby & Kohlstedt data analysis, so the reader will be wondering is this just an alternative analysis. Maybe you are trying to be too polite: don't worry everyone makes errors and when we identify them they need to be corrected. You need to be absolutely upfront about this being a mistake. E.g. "We and other researchers (Goldsby pers comm, Prior pers comm) have spotted an error in the fit of the <258K dislocation creep flow law to the data in Goldsby and Kohlstedt. We have recalculated the flow law based on the original data…".
2. Figure 7 and the discussion around it are pointless. You are using a flow law (the original G&L <258K dislocation creep flow law) which you show is wrong. This serves no purpose and it just makes the paper really really confusing. You can make the general point that the re-fitted composite flow law tends to decrease the importance of dislocation creep relative to GBS, in the section where you discuss the re-fitting of the flow law.

I think you need short names that clearly distinguish the different flow law fits. This becomes particularly important when one considers the two parts of your work as the second paper has a different fit (for justifiable reasons). Something like

- G&K: original Goldsby and Kohlstedt flow laws.
- $G\&K_{corr}$: Goldsby and Kohlstedt flow laws corrected as in part 1.
- $G\&K_{262}$: Goldsby and Kohlstedt flow laws with best fit for 262K switch (related to the part 2 paper)

I'm sure you can do better than this suggestion- but it needs something otherwise we will all be very confused.

A schematic overview at start

The paper needs an introductory schematic overview figure of the microstructures and grain sizes in the NEEM core: basically, an annotated depth profile. Readers are busy and you cannot rely on them looking up the source literature so having this figure up front will increase impact and uptake. Most readers will be unfamiliar with NEEM. The figure could include the T and grain size profiles currently in fig 7,8 (enabling these figs to be simplified) as well as the stratigraphic info. The images in current fig 1 could potentially be incorporated in this.

"Accommodated" by

Expressions such as "grain boundary sliding accommodated by easy slip" are commonly used by the rock deformation community. The problem is that this terminology is not used consistently. I find this language highly uninformative. If it is used to indicate a mechanism dependency then which is the "dependent" mechanisms depends on how you understand the English: different readers interpret it in opposite ways. Furthermore some use this terminology to indicate the mechanism within the grain boundary (as opposed to a kinematically required partner mechanism) as discussed in some of the original GBS literature from Michael Ashby (see for example fig 6.1 in Schulson and Duval, 2009). In your paper the language becomes particularly confusing through variation of language used - especially bearing in mind that many of the readers are not from the rock deformation community. This language discussion arises repeatedly and I recall a meeting back in 2006 where I was involved in extensive discussions with at least for the two co-authors on this topic. There are several statements that inform what language might be useful:

- Grain boundary sliding of a polycrystalline material (where pore spaces are not allowed) requires that the individual crystals change shape.
- Where a polycrystalline deforms by a mechanism that restrict the shape change of each individual crystal (e.g. glide on one crystal plane and homogenous bulk strain), grain boundary sliding is required.
- Diffusion creep in a polycrystalline material requires grain boundary sliding.

You are primarily trying to explain the flow law form:

$$\left(\frac{1}{\dot{\varepsilon}_{basal}} + \frac{1}{\dot{\varepsilon}_{gbs}}\right)^{-1}$$

embedded within equation (2). The explanation on lines 6 and 7 of page 5 are not going to help the reader understand this. The way I usually explain this mechanism is that GBS is accompanied by basal slip. The two mechanisms are dependent upon each other, one cannot proceed without the other. The explanation on line 7 is particularly confusing as it indicates (wrongly) that both of the inverse terms inside the brackets each involves both basal slip and GBS.

$\frac{1}{\dot{\varepsilon}_{basal}}$ is just the inverse of the strain rate related to basal slip. GBS is not involved.

$\frac{1}{\dot{\varepsilon}_{gbs}}$ is just the inverse of the strain rate related to GBS. Basal slip is not involved.

It is the expression as a whole that provides the rate dependence. So that if

- $\dot{\varepsilon}_{basal} \gg \dot{\varepsilon}_{gbs}$ then $(\frac{1}{\dot{\varepsilon}_{basal}} + \frac{1}{\dot{\varepsilon}_{gbs}})^{-1} \approx \dot{\varepsilon}_{gbs}$ ie GBS limits the strain rate

- $\dot{\varepsilon}_{basal} \ll \dot{\varepsilon}_{gbs}$ then $(\frac{1}{\dot{\varepsilon}_{basal}} + \frac{1}{\dot{\varepsilon}_{gbs}})^{-1} \approx \dot{\varepsilon}_{basal}$ ie basal slip limits the strain rate.

You use the “rate limiting” terminology (in addition to the accommodation terminology) and this language is much more satisfactory to me. I think that you can make the paper much clearer by abandoning the “accommodated by” expression and describing the mechanism balance in terms of rate limits. In the discussion around lines15 to 21 on page 5 you could usefully incorporate the two bullet points listed above. That then gives a much clearer basis for the simplification to equation 4.

The “Glen” law

I think you need to take care with the language used related to the Glen law. Citing Glen (1955) for a Glen law with n=3 does a disservice to John Glen. Glen’s three key papers have n values of 4 (1952), 3.3 changing to 4 (1953) and 3.2 to 4.2 (1955) respectively. As far as I know Glen has not written that one should use an n=3 relationship; if anything, he suggests that n values for naturally deforming ice should be around 4. So, the n=3 is a simplification of Glen’s work that is in common use (I’m not really sure who did this first). The Glen law in common use has n=3 but it was not Glen who set this value. It would be nice if your introduction of the Glen law made this subtlety clear.

Discussion

The discussion is too long and rather rambling. I have some specific suggestions that follow but I would suggest some significant shortening beyond these points. A rambling discussion just weakens a paper’s impact.

Put all the discussion of the modified flow law in one place

As commented earlier, the modification is to correct an error. So it is not really a discussion point. Put all or the discussion of this issue in the text where the error is corrected. E.g. move page 11 L8-L14 to earlier.

Put the discussion of the micro scale constant stress and constant strain rate models in one place.

This is an excellent piece of work, but looses coherence by being spread through the manuscript. I would suggest that fig 9 and ensuing discussion goes before figure 8 (figure 7 should be axed). This will make the paper easier to follow and will mean an explanation is already at hand for the strain rate variation of dislocation creep in fig 8.

Grain size: mean diameter vs mean area

The exploration of using grain size distributions rather than means in flow laws is excellent. One thing that is probably worth mentioning is that the convention in the glacial literature is to use the mean area. Since this is measured by counting the number of grains in an area, backing out a mean diameter is more or less impossible (needs standard deviation of the normal distribution to do this). For normal distributions of diameter or of log diameter (as is common for recrystallized grain size distributions from experiments) the equivalent diameter calculated from mean area will be larger than the mean diameter. Application of the GSS flow law elements to mean area data would need this to be considered.

Girdle

When you use the term girdle to describe a CPO element can you describe this more completely. Girdle covers a wide range of things on a stereonet. I restrict the term for great circle distributions, but many include small circle distributions under this name. Even if more restricted some information on “girdle” orientation would be useful.

Recovery and recrystallisation

These are very important processes in deforming glacial ice. They get very little space in this paper?

Strain rate

The layer thinning basis (page 7 line 32-33) for strain rate estimates needs explaining a bit more completely so the reader understands the basis of the strain rate estimates.

CPOs during GBS in ice.

The discussion on page 14 lines 23-25ish could make reference to a paper by one of my students.(Craw et al., 2018) show incredibly strong CPOs develop at relatively low strain (20% shortening) in large grains. In this case the large grains are not strongly strained (they do not have elongated shapes) and the large grains are surrounded by a network of fine recrystallized grains that have an equivalent but much weaker CPO. In that paper we suggest that GBS is an important mechanism controlling the microstructural evolution but some slip on the basal plane of the large grains is needed to develop such a strong CPO.

Figure Captions

Generally figure captions are way too long and include discussion elements that should be in the main text. The role of the figure caption should be to explain what is in the figure, where that is not clear from the figure itself. Discussion of the significance of a figure should be in the text.

Figure 8 layout

The layout of figure 8 can be improved significantly.

- If the G-size and temperature are in a schematic at the start of the paper they can be omitted here. The GBS and composite flow laws mirror the G-size profile so well that it does not need to be on the same fig. Similarly the acceleration at the bottom of the hole clearly corresponds to temperature so the depths and the stratigraphic labels give enough cross reference.
- The reason for removing T and G-size is that a much neater figure is possible if you stack a,b and c vertically above each other. This makes the strain rate position of lines much easier to compare.
- Label the axis of the right-hand graph as "GBS contribution (%)" rather than the label "percentage".
- Make all the lines solid (dashed lines do not work for wiggly lines) and label them with rotated vertical text next to the line, in the same colour as the line. This and the last point mean that you can get rid of the boxed legend.
- Make the colours bold and clear. The yellow (GBS) is not good.

Some refs I think you should have in there:

(Durham and Goetze, 1977; Durham et al., 2010; Durham et al., 2001; Pettit and Waddington, 2003; Pettit et al., 2011)

Craw, L., Qi, C., Prior, D. J., Goldsby, D. L., and Kim, D., 2018, Mechanics and microstructure of deformed natural anisotropic ice: Journal of Structural Geology, v. 115, p. 152-166.

Durham, W. B., and Goetze, C., 1977, Plastic-flow of oriented single-crystals of olivine .1. mechanical data: Journal of Geophysical Research, v. 82, no. 36, p. 5737-5753.

Durham, W. B., Prieto-Ballesteros, O., Goldsby, D. L., and Kargel, J. S., 2010, Rheological and Thermal Properties of Icy Materials: Space Science Reviews, v. 153, no. 1-4, p. 273-298.

Durham, W. B., Stern, L. A., and Kirby, S. H., 2001, Rheology of ice I at low stress and elevated confining pressure: Journal Of Geophysical Research-Solid Earth, v. 106, no. B6, p. 11031-11042.

Pettit, E. C., and Waddington, E. D., 2003, Ice flow at low deviatoric stress: Journal of Glaciology, v. 49, no. 166, p. 359-369.

Pettit, E. C., Waddington, E. D., Harrison, W. D., Thorsteinsson, T., Elsberg, D., Morack, J., and Zumberge, M. A., 2011, The crossover stress, anisotropy and the ice flow law at Siple Dome, West Antarctica: Journal of Glaciology, v. 57, no. 201, p. 39-52.