Review of: **Kuiper**, de Bresser, Drury, Eichler, Pennock and Weikusat: "Using a composite flow law to model deformation in the NEEM deep ice core, Greenland: Part 2 the role of grain size and premelting on ice deformation at high homologous temperature"

By **Dave Prior** University of Otago.

This is an excellent paper. The quantitative analysis of potential balance of grain size sensitive and grain size insensitive deformation mechanisms in different grain size layers in the deeper, warmer section of the NEEM ice core is very important. The analysis shows that finer grained layers should deform more rapidly and with a greater proportion of deformation attributed to the grain size sensitive mechanisms. This is a crucial insight, as this deep core section may be representative of the warm basal regions of glaciers and ice sheets where strain is maximized/ deformation localized; such sections may dominate the rheology of glaciers and ice sheets. The paper does need some substantial re-writing, the area where the paper needs most modification is in discussion around premelt. Overall the paper is rather rambling and overlong and would benefit from significant shortening and tightening.

I have also reviewed the part one 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. There are a number of comments related to the Part 1 paper that are also applicable to this Part 2 paper. I have copied these to the end of the review.

I also have an annotated pdf the authors can have.

Premelting

The writing about premelting needs some significant modification. The key problem is that what you are describing, both in your own data and from the literature, is a change in the kinematics of processes as a function of temperature. These are observations. Attribution of these obeservations to premelt is an interpretation and much of your writing does not make this important distinction. I believe the premelt interpretation, but it is important that we make it clear that there is little direct evidence for premelt in ice. The best (almost) direct evidence I know for premelt in grain boundaries is the Raman spectroscopy data presented by (Hammonds and Baker, 2018) that shows an aqueous phase on triple junctions in sulphuric acid doped ice. The review by Dash et al (in your reference list) is extremely thorough. The optical measurements of liquid film thickness on the basal plane surface provide some direct evidence but most of the paper outlines reasonable physical inference of premelting. A good example of misleading writing in your paper is lines 27 to 29 on page 2: *"High temperature deformation tests on polycrystalline ice have shown that a small liquid-like amorphous layer at the grain boundary increases grain boundary mobility by two to four orders of magnitudes (Duval and Castelnau, 1995; Schulson and Duval, 2009)"*. Castelnau and Duval do not mention premelt or any related concept in their paper. They do discuss changes in recrystallisation behaviour at ~ -10C but they do not talk about "liquid -like" or "amorphous" layers. I have had a re-scan through the Schulson and Duval book and this is not a suitable reference for premelting. As far as I can tell the only explicit mention of premelting is on page 231 and is in the context of crack propagation. There is a brief discussion of the effect of a liquid phase on secondary creep on page 127, but this appears to be talking about ice-melt mix, rather than grain boundary pre-melt. The sections on recrystallisation and particularly on GBM (pages 130-138) make no mention of pre-melt or grain boundary properties. I have not had time to check each and every reference you cite. It's really important that the writing is clear and citations are used correctly. The writing should distinguish:

1. Direct evidence of premelt in the paper cited.
2. Indirect evidence in the paper cited, that the authors of that paper interpret in terms of premelt (e.g. Vaughan et al., 2016 ultrasonsonic attenuation see below).
3. Phenomena presented in a paper that you can reasonably interpret in a premelt framework. The Duval and Castelnau, 1995 paper would fall into this category.

There is an area of research that can be interpreted in terms of premelt that you do not mention. This is the work on attenuation of sound waves, where the attenuation is much more effective at warmer temperatures. Key references include (Peters et al., 2012), (Kuroiwa, 1964) and (Vaughan et al., 2016). In fact the Vaughan et al paper has been analysed in terms of the changing contribution to the bulk stiffness tensor of grain boundary elasticity by (Sayers, 2018): this can be interpreted as a parameterization of premelt related grain boundary properties. There is good discussion relevant to this topic in (McCarthy and Cooper, 2016).

An interesting additional discussion point is on whether premelting is a threshold. The splitting of activation energy into a high and low temperature values is rather artificial- it's based on limitations of the experimental data sets we have. I have a strong feeling that activation energy changes continuously from some low T value at ~-20C and lower through progressively higher values as T increases (see also discussion in the Cuffey and Paterson text book). If this is true and the T dependence is a proxy for grain boundary properties related to premelt then it suggests that pre-melt is not a simple threshold phenomenon.

A schematic overview at start

The paper needs an introductory schematic overview figure of the microstructures and grain sizes in the NEEM core with emphasis on the lower section. This can be used to highlight the structural and stratigraphic complexities, making the text easier to follow and can be used to put the layers with different grain size characteristics in context. Such a figure will increase the impact and uptake of the paper significantly.

Grain numbers in fig 1.

I am confused by the numbers of grains in the seteronets in fig 1, compared to the number I see in either the LM or the fabric image. For (c) I count about 40 grains, whereas the stereonet contains 200. For (a) I count about 70 grains, whereas the stereonet contains 382. For (b) I count ~14 for the single frame shown whereas the stereonet for six frames has 539 grains giving an average of ~90 grains per frame. Please explain these in the manuscript.

Woodcock parameter

You need to explain this a little more completely. It is not explicit from Nigel Woodcock's original paper what this parameter is (of course he does not name it that way). You need an equation that explains how this parameter relates to the principal eigenvalues.

Names for the modified flow laws.

This is repeated later in the cut from Part 1 – but I want to expand a little on it here.

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)

So in this Part 2 paper I think you need to list the original G&K parameters as well as your modified parameters.

Eemian Glacial Facies vs Eemian ice.

I got rather confused in the distinction or not of these terms. Please attempt to make this as clear and easy for someone to understand as possible. Also it is important that the reader can recheck their understanding of these terms quickly- i.e. not needing to read a length of text. The schematic figure I suggest would be a good place to make the meaning of these terms clear. Then there is a fast reference for the reader to go back to.

Strain energy during GBS

All of the micrographs show grains with irregular grain boundaries, suggesting that GBM is operative. In this context I think you need a short discussion on how you generate the internal strain energy to drive this under conditions where GBS contributes >90% of the strain rate.

Split Fig 2 into 2 graphs: one for first eigenvalue one for Woodcock parameter. Or abandon one of the measures.

Fig 4. Get rid of the box legend. Red, blue and green dots are already labelled by the horizontal axes. Dislocation Creep, GBS and Glen would be better labeled by words written along the trajectory of these lines. This is also a good example of a caption that is way too long.

Minor things: through manuscript

Page 2 line 1-2. CPO development and weakening are correlated in experiments, but there is no clear evidence that the CPO formation causes the weakening. Please be careful with the way you say this.

Page 6 line 4. The explanation here quotes units with joules. The actual units you use have the energy component in kJ (table). It would be simpler to keep the units the same.

Page 7 line 30. I think you would be better saying two classes of microstructure?

Page 8 line 30. Glen's T dependency is fairly crude so I don't think you can quote him for a change in Q at 263K. Cuffey & Paterson? Where you quote this kink please explain briefly then basis for it, rather than just quoting the reference. In Table 2 Q < 263 is quoted as 60kJmol-1. I thing originally this is from Paterson 1977, summary of field data, not from the text book.

Page 14, line 14. It is stated in various reviews that CPO controls weakening in different orientations. There is actually very little data that demonstrates this. Most that is commonly quoted either does not explore different orientations or has other changes (e.g. grain size) that can also contribute to weakening. For me the paper that gets closest do demonstrating the orientation effect is (Azuma, 1995).

<u>Comments copied and pasted from the Part 1 review: that are also relevant here.</u>

"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:

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

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.

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.

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.

CPOs during GBS in ice.

The discussion ocould 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.

Azuma, N., 1995, A flow law for anisotropic polycrystalline ice under uniaxial compressive deformation: Cold Regions Science and Technology, v. 23, no. 2, p. 137-147.

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.

Hammonds, K., and Baker, I., 2018, The Effects of H2SO4 on the Mechanical Behavior and Microstructural Evolution of Polycrystalline Ice: Journal of Geophysical Research-Earth Surface, v. 123, no. 3, p. 535-556.

Kuroiwa, D., 1964, Internal friction of ice: Contrib. Inst. Low Temp. Sci. Hokkaido Univ., Ser. A,, v. 18, p. 1-24.

McCarthy, C., and Cooper, R. F., 2016, Tidal dissipation in creeping ice and the thermal evolution of Europa: Earth and Planetary Science Letters, v. 443, p. 185-194.

Peters, L. E., Anandakrishnan, S., Alley, R. B., and Voigt, D. E., 2012, Seismic attenuation in glacial ice: A proxy for englacial temperature: Journal of Geophysical Research-Earth Surface, v. 117.

Sayers, C. M., 2018, Increasing contribution of grain boundary compliance to polycrystalline ice elasticity as temperature increases: Journal of Glaciology, v. 64, no. 246, p. 669-674.

Vaughan, M. J., van Wijk, K., Prior, D. J., and Bowman, M. H., 2016, Monitoring the temperature-dependent elastic and anelastic properties in isotropic polycrystalline ice using resonant ultrasound spectroscopy: Cryosphere, v. 10, no. 6, p. 2821-2829.