Manuscript review: tc-2018-275

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

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Reviewer: Adam Treverrow

General comments

The authors have made a thorough response to the reviews and the corresponding changes to the manuscript have improved it greatly. It should be published subject to the minor issues described below being addressed. As an aside, I was appreciative of the colour-coded formatting of the revised manuscript and accompanying response documents. They made navigating through the updated manuscript quite simple.

Lastly, I apologise for this review being overdue.

Specific comments

P1 L13, P2 L23, P9 (Section 4.2): Both 'liquid-like' and 'water-like' are used throughout the manuscript to describe the occurrence of liquid water at grain boundaries and at impurity interfaces. These should all be liquid-like since water-like is ambiguous.

P4 L34: Additional detail required. $\lambda_1, \lambda_2, \lambda_3$ are normalised eigenvalues of the 2nd order order orientation tensor.

Elsewhere the expression 'c-axes eigenvalue' is used, when specific reference is being made to λ_3. This should be corrected.

P5 L26: A further relevant citation is Greve and others (2014).

P6 L3: Recently Bons and others (2018) have pointed out that the experimental basis for $n = 3$ is debatable. I understand and agree with the intent of this sentence, but as it's incorrect as written and should be revised.

Over a limited range of stresses there is a very good experimental basis for a creep power law where $n = 3$ – provided you're only interested in predicting secondary creep rates for initially isotropic ice. Since this clearly is not the case for the vast majority of ice deformation in polar ice sheets, it's then true that using $n = 3$ probably isn't the best idea for an ice sheet model, even if there's historical inertia associated with it's usage.

So while some folks would consider that selection of the flow relation stress exponent is a solved problem, there's clearly evidence suggesting that $n \neq 3$. The actual problem is what value to use in its place - there's not a lot of data around. From our analysis of new and existing experimental data in Treverrow and others (2012) we speculate that $n = 3.5$ may be an improvement...our experimental studies exploring this effect are ongoing.

So, the P6 L3 sentence needs some minimal revision to suggest that $n = 3$ is not appropriate for the type of high-strain (tertiary) creep that is characteristic of polar ice masses. Since the sentence describes the existence of experimental evidence for $n \neq 3$, you should probably explicitly cite some of these, e.g. Goldsby and Kohlstedt (2001) and Treverrow and others (2012)...there are others too.

P6 L25 typo at 'delta O18' – elsewhere δ^{18}O is used.

Figure 1: There are no axis labels indicating the range of δ^{18}O values.

Table 1:From the author's response: *Table 1: We added the last column and the last sentence in the caption to show that the sudden change in CPO and grain size close to the bedrock does not always coincide with a transition from glacial ice to interglacial ice or vice versa. We think this shows that the sudden change in microstructure is*

not only caused by glacial or interglacial ice, but that a temperature 'threshold' also plays a role in this sudden change. We therefore prefer to leave the Table and the caption as it is.

I agree – the motivation for including the age data in the table is completely valid. However, getting back to my initial comments, what I should have said is that expressing the age in terms of MIS makes this information rather impenetrable to those readers who will need to seek out the corresponding reference to decode the marine isotope stages and interpret this data column.

P10 L25: A minor point – the discussion in this paragraph would be much clearer if the following sentence was slightly altered.

Therefore, it is argued that the temperature threshold should have been 261.7K instead of 258K

Perhaps something like: 'Therefore, it is argued that the corresponding temperature threshold should have been 261.7K instead of 258K, if a confining pressure of 50 MPa is also assumed'.

Table 3: There should be a citation to Goldsby and Kohlstedt (2001), since that's where the $n = 1.8$ and $n = 4$ values originate. It's also the basis for values of the pre-exponential term and activation energy for the GBS-limited and dislocation creep.

Section 4.5 The text in this section should be consistent. At **P11 L40** it is noted that:

CPO is thought to strongly influence strain rates,

while at P12 L11 there is:

it is well known that the CPO has a weakening effect on ice depending on its orientation.

The latter is the more correct statement, since there is evidence for this, so P11 L40 should be changed accordingly. Also, in both cases the same citations are used, (e.g. Alley, 1988; Hudleston, 2015), so the statements can't be conflicting. And finally (a minor point) these references are probably acceptable here, but personally I think that Budd and Jacka (1989) and/or Faria and others (2014), would be better placed here than Alley (1988).

P12 L15: Here (and elsewhere) I found usage of 'premelting layer' confusing. This is because premelt is discussed over various spatial scales throughout the manuscript. There is premelt at grain boundaries and impurity sites, but also a zone within the ice, where due to temperature, premelt may be occurring. Overall, the discussion would be improved if terminology such as *the temperate layer where premelt may occur** was used when discussing the large-scale zone where this may be important.

* OK - this example is rather verbose. You'll find your own way to describe this region within the ice sheet, which also clarifies the issue of scale.

P12 L25-32: The last sentence of this paragraph narrowly misses an opportunity to make an important point that's relevant to this discussion. The Law Dome DSS record is your friend here.

The DSS strain rate profile is adequately described in the manuscript. There is a broad maximum in the shear strain rate at $\sim$ 1000 m and then with increasing depth the strain rate begins to decrease, even though the ice is getting warmer. As described, this is due to a large-scale reduction in stress. Within this zone, where coarse-grained ice with a multi-maxima CPO predominates, there is narrow band where strain rates are high. This is correctly identified in the manuscript as ice from the last glacial maximum (LGM). What's important to this manuscript is that relative to the ice immediately above and below this LGM layer (where stresses and temperature are otherwise similar) significantly higher strain rates occur. These must be associated with the fine grain size and significantly different CPO of that layer. The fact that fine grained ice with a strong single-maximum CPO exists at this ice depth is most likely a consequence of the manner in which impurities influence the rate, or way in which microstructure can evolve. Immediately above and below the LGM spike there is no longer any sign of this strong CPO and fine grain size. Within the LGM layer, the single maximum and fine grain size are likely remnants of a microstructure that developed upstream of the borehole site when this ice passed though the zone where shear rates are generally higher.

So, getting back to the sentence at P12-L31-32, that the highest simple shear strain rates *over the entire depth profile* occur in a zone where the strain rates are otherwise decreasing points to the combined influence of temperature and grain size on strain rates. Importantly, recall that the stresses are lower in this zone than at ~ 1000 m where the dominant, yet broad maximum in simple shear strain rates occurs.

References

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