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

Ernst-Jan N. Kuiper, Johannes H. P. de Bresser, Martyn R. Drury, Jan Eichler, Gill M. Pennock, Ilka Weikusat

Reviewer: Adam Treverrow

General comments

The manuscript describes the use of two flow relations to calculate deformation rates at depths from 2000–∼2500 m in the NEEM ice core. The novel aspect of this manuscript is the application of the Goldsby and Kohlstedt (2001) flow relation, using ice microstructure and temperature data to investigate variablity in deformation mechanisms and strain rates over small spatial scales – and the factors that may contribute to this. In general this is a great paper which will be a valuable contribution to the the Cryosphere and I enjoyed reading it. This work should motivate further discussion and research into the effects of microstructure and temperature on ice deformation, and the extent to which localisation plays a role in large-scale ice sheet dynamics.

The paper is well written and thorough (good background and referencing), nicely presented and has few errors (grammar, syntax). The quality of the writing does decrease in sections 4.4 and 4.5 of the Discussion and the Conclusions. Primarily, these sections could be more concise, and more importantly, attention should be paid to being more explicit about which are the speculative aspects of the interpretation and discussion - at times speculative comments are presented in such a way that the casual reader may interpret these statements as observations or facts. I don't think this is deliberate, but the tone of the writing should to be consistent. Some specific examples of the issues noted above are given in the comments below.

Finally, where appropriate, the limitations of the study design need to be clearly addressed; these are also dealt with in the following specific comments. The paper is largely in good shape, so many of the following comments relate only to minor issues. However, these and the several more significant issues should be addressed prior to publication.

Specific comments

In the comments that follow text, where copied from the manuscript, is emphasised using italics.

P1 L2, L3: In the Abstract there are references to layers of *fine grained glacial ice* alternated with *coarser grained Eemian ice*. While these designations are described in the main text, the fact that they're not defined in the abstract will create uncertainty for readers not familiar with the use of climatic periods (e.g. *glacial* and *Eemian*) to describe types of ice, or those that don't immediately recall how the periods relate to one another. The abstract should be revised to improve clarity, e.g. *ice deposited during the last glacial period* (as used elsewhere in the text) would be an improvement. While I understand why the terms *glacial*, *Eemian* and *Eemian-glacial facies* were used throughout the manuscript, an alternative terminology based directly on impurity content or microstructure could be used and should at least be considered.

P1 L15-18: *The dominant deformation mechanisms between the layers is also different with basal slip accommodated by grain boundary sliding (GBS-limited creep) being the dominant deformation mechanism in the glacial layers, while GBS-limited creep and dislocation creep (basal slip accommodated by non-basal slip) contribute both roughly equally to bulk strain in the coarse grained layers.*

Do you really know this from direct observation or is this simply the result you get when you use the flow relation of Goldsby and Kohlstedt (2001) in conjunction with the microstructure observations? This sentence needs to be rewritten to make it clear this is the ‘predicted’ balance between deformation mechanism when you use the Goldsby and Kohlstedt (2001) flow relation and the NEEM microstructure data.

P1 L19: *...impurity-depleted...* is ambiguous since it also suggests that some depletion process that reduces the impurity concentration might have occurred. Since the impurity content of the Eemian ice is simply low relative to the *glacial ice*, it would be better to use ‘low(er) impurity Eemian ice’, or similar, throughout the manuscript.

P2 L1: The use of *...shallower ice...* is ambiguous since it is also suggestive of ice that is thinner, not just ice that is closer to the surface, which is what I think this sentence is about. As it stands there’s also some (small) potential for things to get mixed up in the concept of the shallow ice approximation (SIA).

P2 L2: Shear stress does increase with depth, but not all the way to bedrock. If it did then simple shear strain rates would increase down to the ice-bedrock interface, but strain rates derived from borehole inclination measurements show that they don’t....which is the point of the following sentence on P2 L3. Some revisions is required here for consistency with the sentence commencing at P2 L3. Also *bedrock variations* is somewhat vague. Is this a reference to bedrock topography, rock type and therefore geothermal heat flux, or both?

P2 L5: To the list of references *...Gow and Williamson, 1976; Paterson, 1983; Thorsteinsson et al., 1999; Weikusat et al., 2017* you could add Morgan and others (1998), which clearly shows the topographically-driven reduction in shear strain rates in the last few hundred metres of ice above the ice-bedrock interface near the summit of Law Dome where the ice thickness is $\sim 1200\,\mathrm{m}$.

P2 L8: I would suggest that ‘strain heating’ is a more widely accepted term than *shear heating*.

P2 L7-10: Surface meltwater that penetrates to the bed is not widespread in Antarctica. Refreeze of melt generated at the bed is probably more likely to be an issue there. So, in order for this sentence to apply equal well to both Greenland and Antarctica you could simply end at P2 L9 after *...water*.

P2 L11-22: In this paragraph the use of an Arrhenius relationship to describe the temperature dependence of ice strain rates is discussed. Mostly this discussion is fine; however, the paragraph requires some rewriting.

The fact that a simple Arrhenius relationship does not apply as temperatures approach the pressure melting point, unless the apparent activation energy is also a function of temperature, is discussed. Morgan (1991) acknowledge this fact, yet the actual citation for Morgan (1991) appears before this point in the discussion, leaving the impression that Morgan wasn’t aware of this issue.

I’m also surprised that Budd and Jacka (1989) is not cited in this paragraph. Data from their Tables 3 & 4 and Figure 7 make it clear that a simple Arrhenius relationship – or even one with a single temperature dependent switch in activation energies, as discussed by Paterson (1994) – is inadequate to describe the effect temperature on strain rates as temperatures approach the pressure melting point. Their Table 4 is particularly important in this respect. The requirement for a temperature dependent apparent creep activation energy is also discussed by Morgan (1991). As an aside, while it’s not clear – since there are no acknowledgements in Budd and Jacka (1989) – some of the data from Morgan (1991) are actually included in the compilation of Budd and Jacka (1989).

It’s not discussed why the temperature dependence specified by Paterson (1994) is used in preference to Cuffey and Paterson (2010), since the latter is a revision of Paterson (1994). The justification for using Paterson (1994) should be provided. Figure 1 (next page) is something I’ve used in my own work, but it’s useful here. It clearly shows the difference between the Paterson (1994), Cuffey and Paterson (2010) and Budd and Jacka (1989) specifications of the temperature dependence ($A(T)$ is the temperature dependent term from a Glen-type flow relation. It is clear that the Budd and Jacka (1989) values, which are based on a compilation of experimental data, display a much higher sensitivity of strain rates to temperature as the melting point is approached. Any of these descriptions of the temperature dependence could be used with a Glen-type flow relation – the point in relation to this paper is that the Glen flow relation may do a better or worse job when simulating strain rates simply based on specification of the temperature dependence. This is discussed later in relation to section 3.3 of the manuscript.

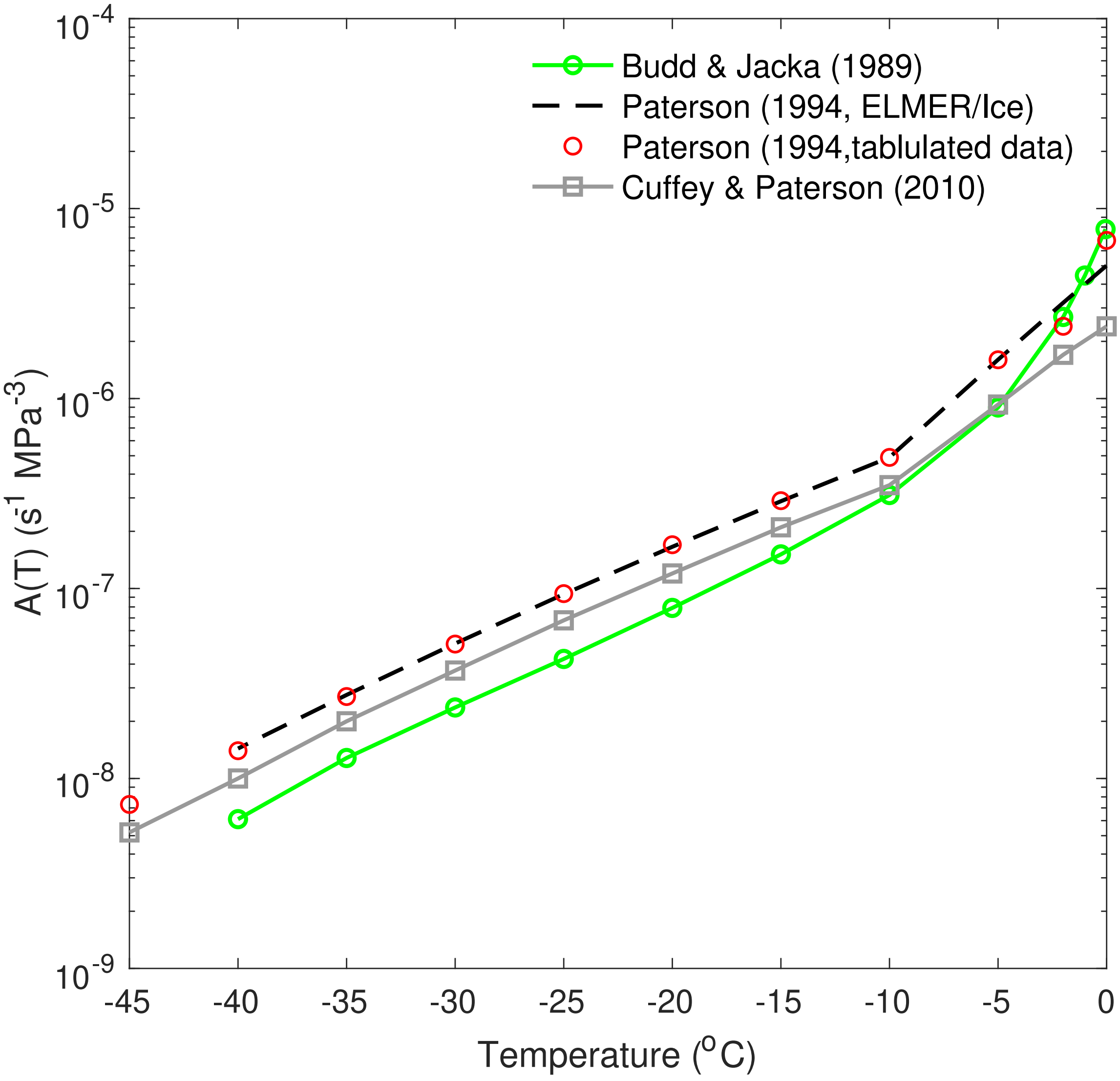

Figure 1: Variation of the temperature dependent term, $A(T)$ in a Glen-type creep power law. The temperature dependence schemes of Paterson (1994), Cuffey and Paterson (2010) and Budd and Jacka (1989) are plotted. For consistency, the conversion from octahedral shear stress and strain rates to equivalent effective stresses and strain rates has been made for the data of Budd and Jacka (1989)

P3 L15-17: *The coarse grains and multi maxima CPO are thought to be the result of rapid strain induced boundary migration (SIBM) in combination with the nucleation of new grains (SIBM-N).* Is an adjective required here to indicate how much nucleation might be expected? If grain coarsening is significant in these layers then SIBM is likely playing a more significant role than nucleation?

P3 L23 – P4 L3: In this paragraph the reasons for using the flow relation of Goldsby and Kohlstedt (2001) are well articulated, but some additional words to clarify why the Glen flow relation, with the Paterson (1994) temperature dependence, makes a meaningful ‘control’ would be helpful.

P4 L8-11: I’m not sure if I’m missing something in the following two sentences: *Ice deposited during the last glacial period (glacial ice) lies below reaching a depth of 2207 m. The layering below the glacial ice is strongly disturbed and four stratigraphic disruptions were identified at 2209.6 m, 2262.2 m, 2364.5 m and 2432.2 m of depth by discontinuities in oxygen stable isotope values...* Did you mean ‘The layering in the (underlying?) glacial ice is strongly disturbed...’? As it stands *The layering below the glacial ice is strongly disturbed...* suggests there’s another layer beneath the end of the (youngest) glacial ice, and that the glacial ice layer is at the most 2.5 m thick since by 2209.6 m we’re into another type of ice.

P4 L25-26: *The total area classified as ‘grain’ by the Ice-image software was divided by the number of grains and a mean grain diameter was calculated assuming circular grains.* A brief comment on whether this introduces any bias into the grain size estimates would be helpful here.

P5 L7-9: *For each orientation images the Woodcock parameter was calculated (Woodcock, 1977). The Woodcock parameter is often used in order to distinguish between cluster and girdle type of CPOs. A distribution with a Woodcock parameter >1 indicates a cluster, while a Woodcock parameter <1 indicates a girdle.*
Woodcock (1977) actually specifies two parameters for describing the CPO pattern and strength. I’m not sure how common it is, but I haven’t previously seen either referred to as a ‘Woodcock parameter’....not to say that you can’t, but it might be simpler and clearer to include the equation for parameter K, equation 5 in Woodcock (1977), since that gives you access to the relevant letter or symbol to denote the parameter.

P5 L11-15: *However, these microstructural parameters can differ significantly from the microstructural parameters obtained by the Ice-image software (www.ice-image.org) (Binder et al., 2013; Binder, 2014). For instance, the mean grain size derived from the orientation images in this study is systematically shifted towards lower values, which is mainly caused by the exclusion of grains with a grain diameter <0.3 mm in the LASM method as was described above.* This needs to be rewritten to make it clearer which method it is that underestimates grain size.Grain sizes determined via the LASM are an effective diameter, is this also the case for the FA measurements? From P7 L4-16 it’s not clear if this is the case. A clear statement that all grain sizes reported in the manuscript are effective diameters, if that’s true, would be helpful.

P5 L28-29: $\dot{\epsilon}_{basal}$ *refers to GBS accomodated by basal slip, while* $\dot{\epsilon}_{GBS}$ *represents basal slip accommodated by GBS.* I guess I’ve missed something but the difference between these two terms isn’t immediately clear to me. Perhaps this could be expanded on, even if $\dot{\epsilon}_{basal}$ is discarded. Also, there’s also a typo in the first ‘accommodated’.

P6 L6: *The value for p determines whether the creep is grain size insensitive* $(p = 0)$ *or grain size sensitive* $(p \neq 0)$. While it is noted later in the manuscript it would be worth mentioning at this point that when $(p = 0)$ the flow relation reverts to the Glen flow relation (even if the Glen flow relation is formally introduced in the following paragraphs).

P6 L16: *where* ρ_{ice} *is the density of ice* $(910\,kg\,m^{-3})$. The bulk density of ice (and snow and firn) varies considerably in ice sheets, so $910\,\mathrm{kg\,m^{-3}}$ is a value selected (or assumed) for the present calculations, so this should be made clear. Also, is there a density profile (modelled or measured) for the NEEM core? If so, how does the chosen value relate to that profile for the range of ice depth in this study.

P6 L29-30: *...a constant equivalent stress of 0.07 MPa was taken as input for Glen’s flow law and the composite flow law.* Even if it was explained in the companion paper, some commentary on how and why a stress of 0.07 MPa is appropriate for this study should be provided. The authors should also specify if it’s the effective stress of Nye (1957) or the octahedral shear stress (e.g. Jaeger, 1962), or something else again, that’s being used in the calculation.

P7 P17-18 & Figure 1: Based on the pole figure in Figure 1c it’s not clear to me that this is a partial girdle. It appears that broad single maxima could also be a reasonable description. Either way, it’s not that important

to the main focus of the paper; however, if the equations for the fabric parameters, K and C, as defined by Woodcock (1977) were included in the manuscript, the pole figures could be annotated with these values, which would help with interpretation of the pole figure.
In the Figure 1 caption and annotations the same acronyms for the image type, as defined section 2.1 should be used, i.e. LASM and FA. These acronyms should be made consistent throughout the manuscript. While the Figure 1 caption begins with *Part of...* it should be revised to more clearly state that the presented LASM and FA images have been extracted from larger sections. Figure 1 would be better if the LASM and FA image pairs were co-registered, i.e. they showed the same region of each section (perhaps this isn't so straightforward...and that's why it wasn't done?).

P7 L28: *...with a Woodcock parameter varying from 0.3-3.* It looks more like 0.3-10 to me.

P8 L4-19: In this paragraph the contribution of stress relaxation to the microstructural evolution should be included. Irregular bedrock topography disrupts simple shear dominated flow, leading to a reduction in the stress towards the bed while at the same time the temperature of the ice is increasing (i.e. maximum simple shear strain rates occur some distance above the ice-bedrock interface). As a first estimate the extent of the flow disturbance can be related to the effective wavelength and amplitude of the bedrock topography.

While the ice temperatures have been corrected for pressure, a second order effect when it comes to the different T^* at the transition for each ice core is the assumption of constant ρ_{ice} as a function of depth (for the NEEM site). Is this at all significant? Possibly not, but if different values of ρ_{ice} are used for each site, it may contribute to difference in T^* at the transition.

Table 1: I'm not sure how much value is added to the manuscript by the last column of data and the last sentence of the caption. Is it really necessary?

P8 L27-32 & Figure 3: The last sentence of this paragraph should explicitly state that the poor performance of the Glen flow relation in predicting the temperature dependence of strain rates (if that's how you want to describe it) is directly related to selection of the Paterson (1994) apparent creep activation energies and pre-exponential terms. Comments to this effect should propagate into the Discussion and Conclusions (where appropriate). From an ice sheet modelling perspective the Paterson (1994) (or Cuffey and Paterson (2010)) temperature dependent terms are easy to implement, popular, and as described in the manuscript, overly restrictive. As shown in Figure 1 of this review, a Glen type-flow can be made to be much more sensitive to increasing temperatures according to the data used to calibrate the temperature dependent term...it's possible the data of Budd and Jacka (1989) may even lead to a greater temperature sensitivity than that shown in Fig. 3 for GBS-limited creep when $T > 262K$.

I'm not sure about referring to GBS-limited creep and dislocation creep as *end members*. As used here there are two deformation mechanism 'components' in the flow relation of Goldsby and Kohlstedt (2001), and finding a way to refer to them in this manner would have the small benefit of saving a few words here and there in the main text, but also the figure captions. Since two of the original four mechanisms from the flow relation of Goldsby and Kohlstedt (2001) are not required in this analysis, what remains is a set with two members. While technically each is an end member, describing them as end members is both redundant and suggestive of there being more than two components to the relation. I only mention this as its best to avoid any hint or suggestion that all four components of the Goldsby and Kohlstedt (2001) relation were used here.

P9 L7-8: *Glen's flow law predicts a higher strain rate (about* $10^{-10}\,s^{-1}$*) than the modified composite flow law in the lower part of the glacial ice.* Based on inspection of Fig. 3 this was always going to be the case for terrestrial ice, i.e grain size of $\sim 5mm$. A broader question is how do the calculated strain rates, for both flow relations, compare to the expected in situ values.

P9 L8-9: *At the interface between the glacial ice and Eemian-glacial facies, the calculated strain rate for the composite flow law drops by about an order of magnitude.* There are some comments on localisation in the Discussion, but the question of how realistic such a rapid transition in strain rates of this magnitude is requires further attention. More on this later...

P9 L15-18 & Figure 4: *The strain rate produced by dislocation creep, which is not affected by the variation in grain size, steadily increases with depth throughout the Eemian-glacial facies. Glen's flow law, which is not affected by grain size variation either, predicts an increasing strain rate with depth and a higher strain rate than the modified composite flow law in the entire Eemian-glacial facies.* While this is the results section, I think some minor rewriting of these sentences to note the effect of temperature on the increase in strain rates with depth is required.

Figure 4 would benefit from an additional pane showing temperature as a function of depth for this part of the NEEM borehole - even temperature without the pressure correction would be very helpful.

P9 L24-26: *The difference in activation energy for Glen's flow law and the dislocation creep mechanism above their temperature thresholds is rather small (Table 2), which results in an almost similar strain rate increase with depth.* Looking at Fig. 4 I would say that these strain rate increases are not at all similar in magnitude...it's a log scale.

P11 L26-29: *The in-situ temperature at which coarse and interlocking grains start to appear in polar ice sheets occurs at* T^* *of about* $-11\,^{\circ}\mathrm{C}$ *(262K) (Table 1), falls within the temperature range (258K to 263K) of the transition to a more temperature sensitive deformation mechanism during deformation tests (Mellor and Testa, 1969; Barnes et al., 1971; Weertman, 1983; Paterson, 1994; Goldsby and kohlstedt, 2001).* This sentence needs to be rewritten. Also, Budd and Jacka (1989) should be included in this list of references since their data compilation clearly shows the likely effect premelting has on increasing strain rates above $\sim 263\,K$.

P12 L1-11: This discussion, particularly the final point that *the strain rate becomes progressively more temperature dependent when approaching the melting point.* highlights why it's a good idea to calibrate the Glen flow relation with a temperature dependence, other than of Paterson (1994). Barnes and others (1971) is cited here when discussing how the apparent activation energy for creep changes markedly towards the melting point – this also shown by the data of Budd and Jacka (1989) which covers a wide range of temperatures and stresses.

P12 L26: *Due to the limited SIBM...* Since this hasn't been directly observed it should noted that there is an 'expectation' of limited SIBM.

P12 L30: Rather than *...using the available creep laws...* this should be 'chosen', or 'selected' or similar.

P13 L5: *Alternatively, the original microstructure might have been obliterated by SIBM.* When? In situ, or post-drilling but prior to analysis?

P13 L10-12: *The CPO of these coarse grained ice core sections suggests that new grains with a high Schmidt factor nucleate continuously (e.g. Alley, 1988; Montagnat et al., 2015; Qi et al., 2017). A high Schmidt factor indicates grains with a high resolved shear stress on the basal plane, i.e. grains with a soft orientation.* At some point in these two sentences it needs to be made clear that the Schmid factor for the new grains is expected to be high based on an assumed or inferred stress configuration. Also, it's Schmid, not Schmidt.

P13 L10-12: *This strongly suggests that the multi maxima CPO in the premelting layer is linked to the formation of new strain free grains with soft orientations (high Schmidt factor), which grow at the expense of grains oriented in a hard orientation.* Should this be 'relatively high Schmid factor'? Given that ice with both a strong single maximum and multi-maxima CPO can co-exist in regions of the ice sheet where the large-scale deformation is dominated by simple shear, it would seem appropriate to classify the Schmid factor of the multi-maxima CPO as mid-to-high range, rather than high, which would apply for a single maximum CPO.

Section 4.5: As a general comment this section of the paper should be revised to make it more concise. The writing here is less focussed than elsewhere in the manuscript.

This entire section also requires revisions to make it clear that some of these interpretations, while completely valid, are speculative in the sense that they have been not verified by direct measurement. In reality this only means changing a few words here and there. For example, *This is also shown by the results using the modified composite flow law....*, might be *This is also suggested by the results using the modified composite flow law.....* Also, using the composite flow relation of Goldsby and Kohlstedt (2001) to conclude that *the fine grained ice is much softer than the coarse grained ice* isn't an independent assessment of viscosity (even if it is true), since this is answer you were always going to get using that flow relation.

P13 L26 – P15 L24: While the relative effects of different CPO's are discussed with regard to localisation, an omission from this section on CPO and its influence on strain rates is a clear link back to the predicted strain rates of Fig. 4. In particular, whether or not the strain rates are realistic and how they might vary if CPO effects were able to be accounted for in either of the selected flow relations should be commented on. Exclusion of CPO effects also misses the opportunity to assess the relative contribution of CPO and different mechanisms to the overall deformation story.

P13 L26 – P14 L3: Some care is required when discussing the strain rates measured for ice with different CPO's in the experiments of Russell-Head and Budd (1979) and Lile (1978). While the experiments of Russell-Head and Budd (1979) do show similar simple shear deformation rates for nearly-isotropic samples and those with a multi-maxima CPO, this isn't really a valid 'apples with apples comparison' due to the different in situ flow regimes of the near surface (∼isotropic) and multi-maxima (near bedrock) samples. The comparison of greater relevance to the present study, is the difference in the simple shear deformation rates of the single maxima and multi-maxima samples. This is also a more realistic comparison since the same large-scale in situ flow regime applies in each case.

In addition to (and inspired by) the work of Lile (1978) and Russell-Head and Budd (1979) the experiments of Treverrow and others (2012), Figure 8, also show how ice with a single maximum CPO deforms much more readily in simple shear than isotropic and weakly clustered samples.

P13 L32-33: All of the natural ice samples in Lile (1978) were from Law Dome ice cores, either from the dome summit, or a site near Cape Folger along a flow line from the summit, i.e. Lile's Dome Summit is simply the summit of Law Dome.

P14 L2-3: *Samples with a multi maxima CPO from Dome Summit showed a similar strain rate as laboratory-prepared isotropic samples during uniaxial compression tests (Lile, 1978).* If this is a reference to Figure 2 of Lile (1978) the sentence should be deleted since it is an incorrect interpretation. The samples from the dome summit site were from a depth of 318 m, where the total ice thickness is ∼ 1200 m. These samples have a small circle girdle CPO and are from a site where the in situ deformation regime is essentially uniaxial compression. Figure 2 of Lile (1978) clearly shows that these samples deform several times faster than isotropic ice when appropriately aligned to replicate the in situ orientation and stress configuration.

P14 L16-17: *while the coarse grained layers do not have a compatible CPO and are likely relatively stagnant or deform in coaxial deformation.* I think '....coarse grained layers have a (much?) less compatible CPO...' would be better.

I'm not sure what observations exist to support the claim that these layers deform coaxially. This statement should be either removed, or at least moved to the section related to borehole observations and revised.

P14 L22: *Since the CPO of this layer (Figure 1c) is relatively hard in simple shear.....* It's most likely still more compatible than an isotropic aggregate, so perhaps 'less compatible' would be an improvement here.

P15 L3-5: *Borehole data from Byrd station showed that the tilting rate (deformation rate) in the premelting layer (1810 m down to the bedrock at 2164 m of depth), where grains are coarse with a multi maxima CPO (Gow* *and Williamson, 1976), deformed much less than the remainder of the ice (Paterson, 1983)* See Morgan and others (1997, 1998) for more of the same.

P15 L3-5: *Borehole data from the deeper part of the EDML ice core showed that a coarse grained layer with a girdle type CPO deformed predominantly by pure shear, while the layers just above and below deformed predominantly by simple shear (Jansen et al., 2017).* Based on my reading of the Jansen and others (2017) abstract I thought that simple shear was dominant throughout the deeper part of the core...perhaps I missed something? Nevertheless, the suggestion of direct measurements of strain rate localisation consistent with changes in CPO is particularly interesting.

Some additional details regarding these measurements at EDML are required to add weight to the discussion of strain rate localisation and the possibility of coaxial deformation, in order to make it less speculative. Without adding too much to the discussion I think it's important for readers to know about the vertical resolution of the inclination and borehole closure measurements (and any other relevant details) if they are to support the occurrence of coaxial deformation within zones where the CPO is interpreted as being compatible with this mode of deformation.

In general I understood the borehole closure problem to be a fairly tricky one since the state of stress within the ice sheet in the immediate vicinity of the borehole is modified by the existence of the borehole, so one has to deal with secondary and tertiary creep effects (depending on the timescale of the closure measurements).

P15 L18-24: This paragraph needs to be revised to highlight the speculative nature of the discussion. The writing has a bias towards the presentation of facts rather than speculation. Preferably an explicit statement, along the lines of the last sentence, should appear at the start of the paragraph, rather than the end.

P16 L9-12: *Glen's flow law, which is grain size insensitive, is unable to predict the strong variations in strain rate in the Eemian-glacial facies and predicts a steadily increasing strain rate with depth caused by an increase in temperature with depth. Glen's flow law predicts a higher strain rate than the modified composite flow law along the entire lowest 540 m of depth of the NEEM ice core.* This should be revised to address the influence of the chosen method to calibrate the temperature dependence of the Glen flow relation on its capability to predict strain rates (e.g. Figure 1 of this review).

P16 L22-23: *while the coarse grained interglacial layers with a partial girdle type of CPO deform at much lower strain rates by coaxial deformation* This should be revised to note the speculative nature of the suggested localised coaxial deformation.

Technical corrections

Figure 1 caption, P26 L5. Typo at *Is parallel.*

P11 L29: Typo at *kohlstedt.*

P12 L32: Missing data at *...going up to 75% at xxx m depth....*

References

Barnes, P., D. Tabor and J.C.F. Walker, 1971. The friction and creep of polycrystalline ice, *Proceedings of the Royal Society of London, series A*, **324**, 127–155.

Budd, W.F. and T.H. Jacka, 1989. A review of ice rheology for ice sheet modelling, *Cold Regions Science and Technology*, (16), 107–144.

Cuffey, K.M. and W.S.B. Paterson, 2010. The Physics of Glaciers, Elsevier, 4th ed.

Goldsby, D.L. and D.L. Kohlstedt, 2001. Superplasic deformation of ice: Experimental observations, *Journal of Geophysical Research*, **106**(B6), 11017–11030.

Jaeger, J.C., 1962. Elasticity, Fracture and Flow, Methuen, London, 2nd ed.

Jansen, D., I. Weikusat, T. Kleiner, F. Wilhelms, D. Dahl-Jensen, A. Frenzel, T. Binder, J. Eichler, S.H. Faria, S. Sheldon, C. Panton, S. Kipfstuhl and H. Miller, 2017. In situ-measurement of ice deformation from repeated borehole logging of the EPICA Dronning Maud Land (EDML) ice core, East Antarctica., Geophysical Research Abstracts, EGU General Assembly, vol. 19.

Lile, R.C., 1978. The effect of anisotropy on the creep of polycrystalline ice, *Journal of Glaciology*, **21**(85), 475–483.

Morgan, V.I., 1991. High-temperature ice creep tests, *Cold Regions Science and Technology*, (19), 295–300.

Morgan, V., T.D. van Ommen, A. Elcheikh and J. Li, 1998. Variations in shear deformation rate with depth at Dome Summit South, Law Dome, East Antarctica, *Annals of Glaciology*, (27), 135–139.

Morgan, V.I., C.W. Wookey, J. Li, T.D. van Ommen, W. Skinner and M.F. Fitzpatrick, 1997. Site information and initial results from deep ice drilling on Law Dome, Antarctica, *Journal of Glaciology*, **43**(143), 3–10.

Nye, J.F., 1957. The distribution of stress and velocity in glaciers and ice sheets, *Proceedings of the Royal Society of London, series A*, **239**, 113–133.

Paterson, W.S.B., 1994. The Physics of Glaciers, Pergamon, 3rd ed.

Russell-Head, D.S. and W.F. Budd, 1979. Ice-sheet flow properties derived from bore-hole shear measurements combined with ice-core studies, *Journal of Glaciology*, **24**(90), 117–130.

Treverrow, A., W.F. Budd, T.H. Jacka and R.C. Warner, 2012. The tertiary creep of polycrystalline ice: experimental evidence for stress-dependent levels of strain-rate enhancement, *Journal of Glaciology*, **58**(208), 301–314.

Woodcock, N.H., 1977. Specification of fabric shapes using an eigenvalue method, *Geological Society of America Bulletin*, **88**(9), 1231–1236.