Response to Reviewer 2

We thank Reviewer 2 for his thoughtful and helpful review of our paper. The comments have helped us improve the manuscript significantly. Our reply to reviewer comprises two parts: (1) some short general statements and (2) point-by-point reply to comments from reviewer. Reviewers comments are in blue type. *Extracts from our revised manuscript are in italics.*

Section one: general statements

1. This work contains data which are completely new.

We would like to thank the reviewer for one particular comment: "This paper essentially presents a nice set of experimental data…. the authors provide a detailed analysis of the microstructure of ice grains and its evolution…". We would like to emphasize that the sequence of microstructures and CPOs developed with increasing strain has not been documented before for ice deformed at cold temperatures (-20, -30 °C).

2. The reviewer suggests rejecting this paper mainly because the interpretation of grain boundary sliding (GBS).

In our view, interpretations are not usually what make a scientific good paper. New data that is factually correct and will stand the test of time make a good paper. It is likely that the interpretations will change in the future as researchers gain new data or insight. We accept that the factual observations that we present and then to infer GBS could be interpreted in different ways. In the revision, we include some alternative interpretations (including "spontaneous" nucleation) of the data, with some discussion of the merits and drawbacks of each of these interpretations. We hope that we have kept the observations and interpretations clearly separated and we have reduced the emphasis on our preferred interpretation of GBS. We have also identified some of the tests that may facilitate distinguishing these different interpretations in the future. Some more details are included in answers to specific points.

The reviewer's comments highlight that our original manuscript did not really make clear that we do interpret intracrystalline dislocation glide that causes lattice rotation as one of the key processes controlling CPO development. We hope that we have made this much clearer in the revised manuscript. The operation of a GBS process, if this is correct, would be additional to the role of intracrystalline dislocation glide and associated recovery and recrystallisation processes.

Section two: point-by-point reply to comments

1. All along the paper, the authors state that grain boundary sliding (gbs) must be invoked to explain the observations. For my point of view, there is absolutely no proof here that gbs has been activated, even in the "small grains". The authors observe some correlations between grain size distribution, temperature, texture, but whether gbs is necessary to explain all that is another story. The evidences prone by the authors are highly speculative. Authors try to explain that gbs is necessary using arguments based on microstructure evolution. But many other parameters coming in play should be also considered, and mostly those associated with recrystallization (for which the micrographs show direct evidences unlike gbs) such as gbm rate, nucleation rate, stored energy, etc and their evolution with temperature and strain for which our actual knowledge is very limited. To prove that gbs has been active in the specimen, I would suggest the authors to (i) provide direct evidence of a sliding boundary and/or (ii) show that the associated viscosity is compatible with the one of the specimen (as gbs in a polycrystalline aggregate required associated diffusion, which is slow) and/or (iii) model microstructure evolution due to deformation + dynamic recrystallization to show that the observed evolution cannot be explained by these only mechanisms.

The key objective of this paper is to report the detailed changes in microstructures and CPOs to progressively higher strains at low and high temperatures, with the very new data being at lower temperatures. The interpretation of GBS is not central to this and we have downplayed that in the revised manuscript. We still wish to explain the weakening of CPOs in finer grain sizes and have presented two alternative interpretations; GBS and spontaneous nucleation.

We agree with reviewer's comment that our data cannot prove the existence of grain boundary sliding. We would love to have fiducial marker evidence to show directly the GBS effect (e.g.(Eleti et al., 2020; Schmid et al., 1977; Spiers, 1979): this is a significant technical challenge for now. The particular set of experiments presented in our paper does not include variable initial grain size. However, comparable experiments do show grain size sensitivity. The set of -10 °C experiments published by Qi et al 2017 have two different initial grain sizes. A plot of strain rate against the peak stresses (Fig. 3, copied below as Fig. R2.1) shows two different best fit lines for the two initial grain sizes. At peak stress (~ equivalent to min strain rate) grain size is unlikely to have changed substantially from the starting material (and we have some new experiments to peak stress only that show this to be correct). The easiest interpretation of the Qi et al (2017) mechanical data is that there is grain size sensitivity, which is consistent with the operation of GBS.

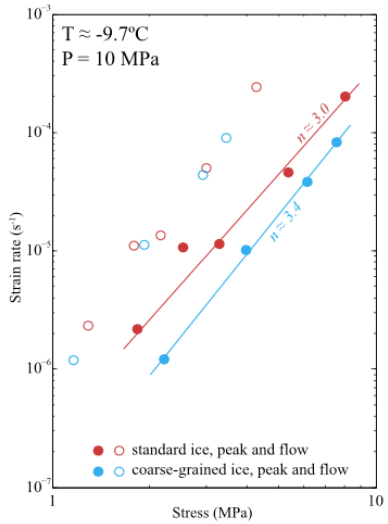

Figure R2.1. Plot of strain rate versus stress on logarithmic scales using data from Qi and others (2017).

If GBS does occur, our interpretation is that this is in addition to dislocation glide (Drury et al., 1985; Gifkins, 1976, 1977; Goldsby and Kohlstedt, 1997, 2001; Hirth, 2002; Hirth and Kohlstedt, 2003; Kuiper et al., 2019a; Kuiper et al., 2019b; Langdon, 2006, 2009; Warren and Hirth, 2006). Some authors term this dislocation accommodated GBS or "disGBS". In this mechanism, the total strain rate is the addition of a dislocation process (that changes crystal shapes and causes lattice rotation and internal distortion) and a GBS process that is probably controlled by a "viscous" mechanism within grain boundaries (small path length diffusion and/ or asperity plasticity: idea originally from (Gifkins, 1976)). This is not the same as diffusion creep, irrespective of whether that is controlled by lattice diffusion (Nabarro-Herring creep) or grain boundary diffusion (Coble creep). GBS is required as an accompanying mechanism to polycrystalline diffusion creep, but in that case grain shape change is facilitated by the diffusive mass transfer process. In diffusion creep, grain size sensitivity comes primarily from the increased path length for diffusion meaning that the change of shape of bigger grains takes longer. In "disGBS" the GBS itself is the prime source of grain size sensitivity. If there is a "viscous" grain boundary volume then the rheology will depend on the volume proportion of the sample that comprises grain boundaries: this proportion will increase with decreasing grain size.

CPO models certainly do not match observations fully for shear (see discussion in Qi et al., 2019) and that paper speculates that GBS may bridge the gap between the results of laboratory experiments and numerical models. Indeed, there is currently a major effort (led by Sandra Piazolo and colleagues) among the community that use the ELLE modelling platform to incorporate GBS: a difficult task. Microstructural modelling is beyond the scope of our paper.

2. The mechanical tests (figure 2) essentially show a dominant temperature effect (known since the early years of glaciology – do the associated activation energy, not calculated here, matches literature data ?) and a softening at strain larger than ~0.03.

Yes, the mechanical data match literature data. We have added a calculation of activation energy to the supplementary information and have referred to this in the text. Best fit to all data (-10, -20 and -30 °C) give activation enthalpies of 98 kJ/mol and 103 kJ/mol from peak and final stress data assuming n=3 and 131 kJ/mol and 138 kJ/mol from peak and flow stress data assuming n=4. These values are close to reported Q values of 71-124 kJ/mol (-5 °C- -30 °C) from Budd and Jacka (1989) and ~133 kJ/mol (-1.5 °C- -12.8 °C) from Glen (1955) and 64-250 kJ/mol from Kuiper and others (2019a, 2019b). Note experiments in this study only cover three temperature values. Hence, the calculated Q values are prone to error. More data points are needed for a more accurate Q investigation.

3. Along the same line, the sentence (p12 line 2) "gbs is kinematically required for all grain size sensitive mechanisms" is incorrect. For example, the Hall-Petch mechanism is largely used in metallurgy to explain size effect observed in many nanometric grains metallic alloys. Hall-Petch is based on the mean free path of dislocations, it explain very well many observations, and does not require any other mechanisms than dislocation glide (no gbs!). Could the mean free path of mobile dislocations have an influence of ice rheology at low temperature ?

Our apologies; the reviewer is correct. That statement does not apply to the full breadth of GSS mechanisms including classic Hall-Petch and also mechanical twinning (Rowe and Rutter, 1990) and we have removed the statement.

As an aside there is a very interesting ongoing discussion of the Hall-Petch (Weertman, 1993) relationship (with strength increasing with grain size) and the inverse Hall-Petch relationship

(Masumura et al., 1998) in the materials science literature (Pande and Cooper, 2009; Ryou et al., 2018; Sheinerman et al., 2020). Modelling of the inverse Hall-Petch relationship requires coupling of GBS to intragranular dislocation activity (Carlton and Ferreira, 2007; Ehre and Chaim, 2008; Padmanabhan et al., 2007; Padmanabhan et al., 2014; Ryou et al., 2018; Sheinerman et al., 2020) and the relationships are not very different to those described elsewhere as GBS accommodated by dislocation creep (Goldsby and Kohlstedt, 1997; Hansen et al., 2011; Langdon, 2006, 2009). In minerals, the normal Hall-Petch relationship (increasing strength with decreasing grain size) has only been documented at low homologous temperatures (Hansen et al., 2019; Koizumi et al., 2020) whereas weakening with reduced grain size is the norm at higher temperatures and lower stresses (Brodie and Rutter, 2000; De Bresser et al., 2001; Hiraga et al., 2013; Hirth, 2002; Hirth and Kohlstedt, 2003; Schmid et al., 1977; Ter Heege et al., 2005; Walker et al., 1990). Materials science work defines a material-dependent threshold grain size, above which the Hall-Petch relationship holds and with the inverse Hall-Petch relationship at grain sizes below the threshold (Pande and Cooper, 2009; Ryou et al., 2018). Recent work suggests that the threshold moves to larger grain sizes at lower strain-rates or stresses (Somekawa and Mukai, 2015). The rates that are considered very slow in these metallurgical analysis (e.g. 1 x 10^{-4} s^{-1}) are very fast in the context of geological or glaciological laboratory experiments and this may explain why we only see evidence of the Hall-Petch effect at low homologous T. Some recent work relates GBS associated with the inverse Hall-Petch relationship with amorphization of the grain boundaries (Guo et al., 2018) and a molecular dynamics modelling study of ice (Cao et al., 2018) generates an inverse Hall-Petch relationship that involves a combination of GBS, grain rotation, amorphization and recrystallization, phase transformation, and dislocation nucleation in both bicrystals and polycrystals.

4. Similarly, about the sentence (p15 line 6) “similarly, we suggest that grain size sensitivity of gbs favours a faster strain rate in small grains”: I find no fact in the results supporting this assertion. Strain-rate in various sets of grains is not measured nor estimated here. And also, in section 4.2, the authors make a correlation between the softening observed at -30degC and the grain size, and conclude that the observed softening should likely be attributed to gbs. Gbs could be a possibility, but among many others. For example, what do we know about the density of mobile dislocations ?? If it increases, the stress would decreases as observed. Increase of dislocation density is often used to explain the peak stress for materials with low initial dislocation density (eg. Si, . . .).

Please see our answer to point 1.

5. The statistical relevance of the performed mechanical tests and/or microstructural investigations can also be questioned. Figures 9, 10, 11, 13 show pole figures that do not, by far, exhibit the expected transverse isotropy (expected since the initial specimen are thought to exhibit random CPO with equiaxe grain shape, and since uniaxial compression is transverse isotropic). This severe lack of symmetry in the observed microstructure can originate from (i) initial samples that do not exhibit a random microstructure and/or (ii) mechanical tests that deviate from uniaxial compression (there could be many reasons for that) and/or (iii) the microstructure is not analysed on a sufficiently large material volume (volume smaller than the Representative Volume Element-RVE). Consequently, the global picture shown here (ex. texture strength as function of temperature, which is an interesting result) are probably correct, but I don’t think that, with the results shown, authors can dig deeper into the interpretation of active deformation mechanisms. If the lack of texture symmetry is present in the specimens, then the applied axial strain-rate would generate significant shear stress (or shear-rate, depending on the experimental boundary conditions), affecting of course the texture and microstructure evolutions (so-called out-of-axis tests). Is there any connection with the large

spread observed on the mechanical responses (figure 2) ? For example the peak stress at -10 C varies by almost a factor 2, which is considerable and should be discussed. One could expect some associated spread in the microstructure.

The reviewer is correct about symmetric incompleteness and we have added the following text to address this: *"Many deformed samples exhibit an incompleteness of c-axes cone (lack of cylindrical symmetry) (Fig. 8-10). The incompleteness of c-axes cone is more severe for 5 µm EBSD maps collected from a much smaller area than 30 µm EBSD maps (Fig. 12). These phenomena are common to all ice CPOs from measurements on a single sample planes (by EBSD or optical methods: see any of the papers cited), but are not so apparent in neutron diffraction data (Piazolo et al., 2013; Wilson et al., 2019), that sample a larger volume, suggesting that a single plane through a deformed sample does not generally contain sufficient grains for a fully representative CPO."*

The fact that neutron diffraction data gives CPOs that have close to the cylindrical symmetry, for samples that have fewer grains (initially) in an average cross section (Piazolo et al 2013 initial grain size 0.5mm whereas ours <0.3mm: samples in both cases 1 inch diameter) suggests that the sample as a whole has enough grains to b considered mechanically isotropic. In this case the incompleteness of CPOs is an analytical sampling issue and should not impact on mechanical data. A good example of where samples contain too few grains to be considered isotropic is the re-deformation of natural ice with a 20mm grain size (Craw et al., 2018): this gives rise to significant inconsistency in stress strain curves, although yield stress data correlate sensibly with strain rates.

The scatter of peak stress values we have is fairly typical of confined medium constant displacement rate experiments (data for comparison can be extracted from (Durham et al., 1983; Golding et al., 2020). Unconfined constant displacement rate experiments (Hammonds and Baker, 2016; Vaughan et al., 2017) have less variability and it is likely that some of the scatter in confined medium experiments relates to how stable the confining pressure is. Unconfined creep experiments (constant load) also show a range of minimum strain rates for a set of experiments at the same stress (Journaux et al., 2019; Montagnat et al., 2015; Treverrow et al., 2012). To compare constant rate vs constant load experiments, we can calculate the "viscosity" at peak stress/ minimum strain rate. Confined constant rate and unconfined creep tests both have "viscosities" that vary by up to about 2x for experiments at the same rate or stress. Unconfined constant rate experiments have peak stress "viscosities" that vary by up to about 1.1x. These statements are made on a relatively small data set as there seem to be few "repeat" experiments (in terms of load or rate) in the published literature. At the moment we don't have a full explanation as to what controls this variability. We have to account for the variability in studies where it becomes important (e.g. for calibrating flow laws). In this paper it is not so important and the aspect that is important to us – the curve shape with a peak stress followed by weakening is common to all experiments.

6. The discussion in this paper relies on a separation of the grain size distribution between "small grains" and "large grains", invoking a "bimodal" (p7 line 4) grain size distribution. In figures 3, 4, 5, I do not see any bimodal grain size distribution, but rather a unimodal one with a long tail. Therefore the size threshold (p7 line 16) used to separate small and large grains is completely ad hoc, and I am not sure about the effect of this particular choice on the provided discussion. I also don't understand why the authors state that "The small grains are likely include all the recrystallized grains" (p7 line 19, p8 line 8, . . .) as (i) if GBM occurs, recrystallization can also lead to large grains and (ii) the grain size distribution of the initial microstructure is not shown.

(1) We modified the description of grain size distribution in section 3.3.2: *"For samples deformed to ~3% strain, the grain size distributions are strongly skewed or possibly bimodal, with a clear main peak at finer grain sizes and a tail of coarser sizes with a broad, poorly defined secondary peak corresponding to the mean grain size of the starting material (Fig. 4(d), 5(d) and 6(d))."*

(2) We removed the statement of: "The small grains are likely include all the recrystallized grains." This is a very good point from the reviewer. We (who come from the rock deformation world) sometimes forget that at the high homologous temperatures in ice recrystallised grains can grow to a large size. In much lower homologous temperature experiments in quartz, for example, it is reasonable that recrystallised grains are small and remnant grains large (see for example (Cross et al., 2017; Hirth and Tullis, 1992). We still wish to segment the grain size on the basis of "big" and "small" grains and we hope that our presentation of this is now more robust and does not assign arbitrarily the status recrystallised or remnant on certain grain size populations. The precise threshold we use does not influence the difference in CPOs between "big" and "small" grains as shown in Fig. R2.2, extracted from new supplementary information.

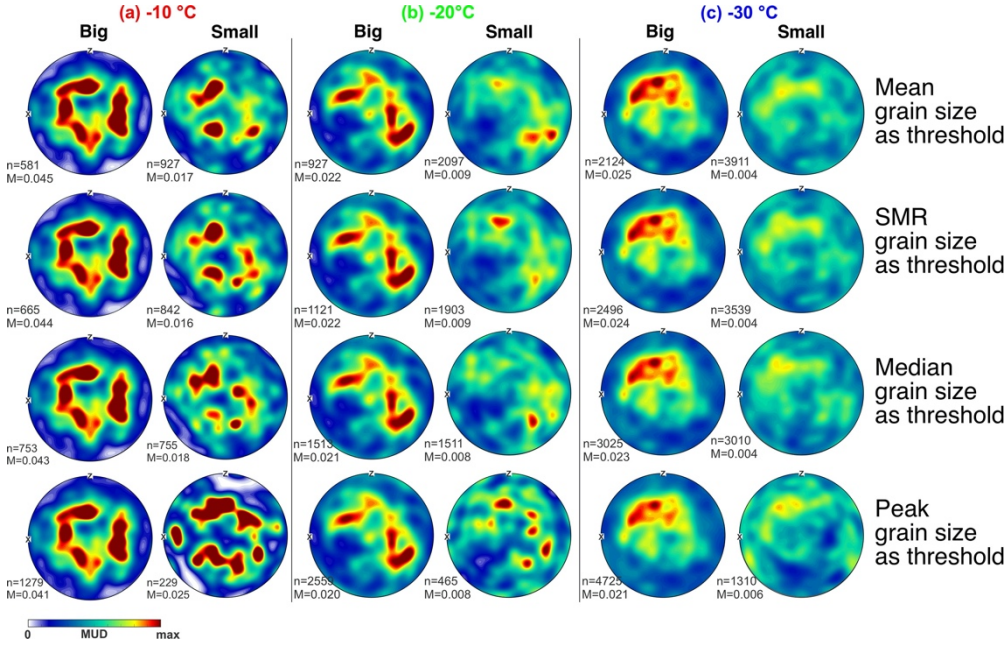

Figure R2.2. The contoured *c*-axis CPOs of "big" and "small" grains in samples deformed at **(a)** -10, **(b)** -20 and **(c)** -30 °C to ~12% strain. "Big" and "small" grains are separated using the threshold of mean grain size (row 1), SMR (square mean root) grain size (row 2), median grain size (row 3) and peak grain size (row 4). Number of grains and M-index value are marked at the bottom left corner of the corresponding *c*-axis CPO.

7. The discussion also largely relies of the size of subgrains. However, in figures 3, 4, 5, it is really hard to identify those subgrains in most of the grains. For example in figure 5 at 20% strain, one only sees some disconnected segments in the WBV map in the large yellow or pink-orange grains at the bottom of the micrograph. How do the authors identity the subgrains and calculate their size in such a case ?

We agree with the observation from the reviewer that suggests many of the subgrain boundaries are straight tilt bands or kink bands. The subgrain structure was revealed by Weighted Burgers vector (WBV) method, which picks up pixels with the WBV magnitude (||**WBV**||) higher than 0.0026 μm^{-1} (equivalent to misorientation angle between neighbouring pixels higher than ~0.7°). Therefore, many of the subgrain boundaries lower than 2° were selected and they might

contain non-neglectable errors (Prior, 1999). Moreover, we didn't make it clear that the measurement of subgrain sizes were not based on the data of WBV, and they were based on the misorientation between adjacent pixels. Therefore, the new subgrain boundary plots corresponds to the original subgrain calculations. We kept the WBV analyses based on the thinking that they might contain more information for further comparison. But the WBV analyses have now been removed completely from this paper.

The new maps (Fig. 4(c), 5(c), 6(c)) that show subgrain boundaries correspond to the much simpler misorientation threshold. We modified statements on section 3.3.1 to: "Distinct sub-grain boundaries can be observed in all the samples (Fig. 4 (c), 5 (c) and 6 (c)). Many of the subgrain boundaries appear to be straight, some with slight curvature. A small number have strong curvature. Interconnected subgrain boundaries can be observed in some of the grains. Subgrain boundaries subdivide grains into subgrains."

8. This experimental study cannot be used without very special care to infer deformation mechanisms occuring in "terrestrial and planetary ice flow" (1st abstract line), as (i) the grain size investigated (~200 microns) is one order of magnitude smaller than the natural one, and (ii) the strain-rate used during the mechanical tests (10-5s-1) is 3 to 6 orders of magnitude larger than in cold regions of ice sheets.

We modified the first line in abstract to: "To understand better the ice deformation mechanisms…" The reviewer raises the key problem that we struggle with, when we are working in the laboratory with application to natural ice. The absolute fastest documented natural terrestrial strain rates are in lateral shear margins ~ 10^{-9} s^{-1} (Bindschadler et al., 1996; Jackson and Kamb, 1997). Rates in basal ice is harder to estimate; most models would have strain rate maxima also around ~ 10^{-9} s^{-1}. Most parts of ice sheets and glaciers have strain rates that are up to 2 orders of magnitude slower than this. To run an experiment from to 10% strain (i.e something that may go from isotropic starting material to a "steady state" microstructure) will take three years at ~ 10^{-9} s^{-1}. (Jacka and LI, 2000) did an amazing job running experiments for long durations at low rates (down to 4 x 10^{-10} s^{-1}) but these are really the only experiments that achieve substantial strain at "natural" rates. Specific aspects of ice mechanics have been assessed by deforming natural samples to small strains (<1%) in the lab at relatively slow rates (10^{-10} s^{-1} to 10^{-8} s^{-1}) (Castelnau et al., 1998; DahlJensen et al., 1997; Jackson and Kamb, 1997). In general, it is virtually impossible to work at natural rates and we have to develop scaling relationships that involve strain rate, temperature and grain size.

9. I wonder whether there is no damage occurring at the high strain-rate considered, particularly at the smaller temperatures?

Stress-strain curves of all experimental runs show a smooth and continuous increase of stress as a function of strain before reaching the peak (Fig. 3). The stress-strain curves of experiments with a development of cracking during deformation normally show an initial yield point before reaching the peak stress (Mellor and Cole, 1982). The initial yield point is interpreted as a reflection of cracking on the mechanical data (Mellor and Cole, 1982). Such yield point is not observed in any of the experiments in this study.

The chief purpose of the confining pressure in these experiments is to suppress brittle phenomena including cracking and frictional sliding. Fig. R2.4 shows the experiment with the highest differential stress, plotted on a Mohr diagram for stress. The green circle shows the shear and normal stresses for surfaces of all orientations and the maximum (σ_1) and minimum ($\sigma_3 = \sigma_2$ = confining pressure) plot along the line of zero shear stress. Superposed are two failure envelopes. One is a Coulomb (frictional sliding) envelope using the friction coefficient

for ice-ice sliding from (McCarthy et al., 2017). Coulomb envelopes usually underestimate brittle strength. The second failure envelope is the composite envelope from (Beeman et al., 1988). Red and blue Mohr circles show the stress states needed for brittle failure at 20MPa pressure with each of these envelopes. Maximum differential stresses applied in our experiments are substantially below those for needed for brittle failure.

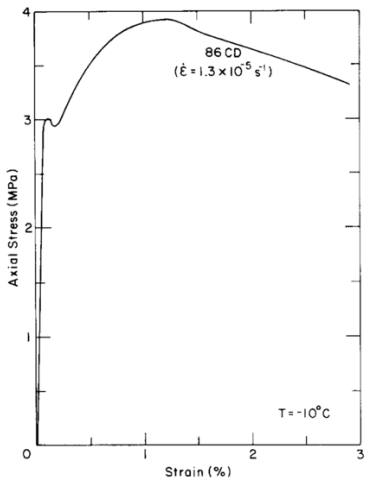

Figure R2.3. Typical stress-strain curve for deformed sample with cracking (from Mellor and Cole, 1982)

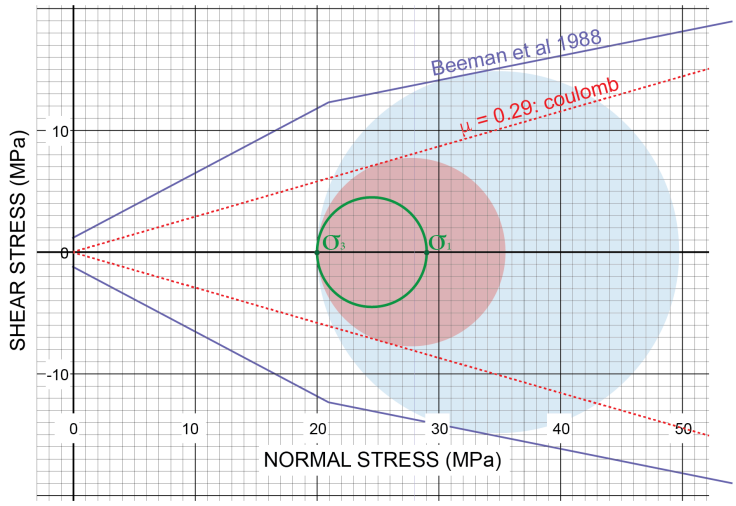

Figure R2.4. Mohr diagram showing stress state of sample PIL164 (the largest differential stress) in green. A coulomb failure envelope using a friction coefficient of 0.29 from (McCarthy et al., 2017) is shown with a red dashed line and the Mohr circle for failure at 20MPa confining pressure is shown in red. The blue lines show the (Beeman et al., 1988) failure envelope from and the Mohr circle for failure at 20MPa confining pressure is shown in blue.

10. p1 line 16 : "displacement rate" instead of "displacement"

Corrected.

11. p1 line 26 : invoking creep stages (secondary, tertiary) for the description of constant strain-rate experiments is misleading.

We have deleted these misleading wording.

12. P5 line 26, I don't understand what is meant with "The CPO data were contoured with half-width of 7.5deg" ?

We modified the statement in section 2.5.2: “The CPO data were contoured with a half-width of 7.5° based on the maximum of multiples of a uniform distribution (MUD) of the points, to more clearly show the CPO patterns.”

12. p7 line 26 : to the best of my knowledge, recovery, subgrain rotation and gbm are not deformation mechanisms ! If recovery and/or gbm are initiated, the specimen will not deform.

Thank appreciate the reviewer for pointing out this mistake. We have removed this sentence since we removed boundary hierarchy analyses.

13. eq. 3, how is R (grain radius) estimated for non-spherical grains ??

We have removed grain boundary lobateness analyses.

14. p9, line 1 : I think that calling “m” the 10-10 direction is not standard (m-axes pole figures). Should be clarified ?

We corrected “*m*-axes” to “poles to the *m*-planes”.

15. p11 line 26, the sentence “Much of the stress increase prior to peak stress relates to elastic strain” is wrong. First of all, there is no known yield stress for the high temperature rheology of ice, i.e. plastic strain starts as soon any stress is applied, as here in the first part of the loading prior to the peak stress. There are old published data (on single and polycrystals) showing that the initial slope depends on the strain-rate. Of course, there is always an elastic strain associated to the applied stress (Hooke’s law). On top of that, the measured slope (~1GPa) very probably also accounts for the way strain is measured experimentally: if it is not measured directly on the specimen (eg. with an extensometer or strain-gage), it is well known that very small modulus are obtained, due to machine rigidity and other artefacts.

intracrystalline dislocation slip, the porosity loss being very likely negligible.

Published literature labelled the stress increase prior to peak stress in constant displacement rate experiments as: “normally elastic” (Cole, 1987) and “quasi-elastic” (Kirby, 1987). The deceleration during primary creep in constant stress experiments was interpreted as effected by a “delayed elasticity”, with a recoverable component of time-dependent elastic strain and an irrecoverable viscous strain (Mellor and Cole, 1982), and “anelasticity” (Duval et al., 1983). The reason we chose to describe the behaviour as substantially elastic is that we have other experiments where we can show that this part of the deformation is recoverable. However, these other experiments are higher rate experiments with slopes on the stress strain curve approaching the 9GPa modulus. The reviewers are correct in pointing out that in the experiments presented in this paper the slope is substantially below modulus and the behaviour is not substantially elastic. We have modified the statement in section 4.1.1: “*This likely includes anelastic deformation related to intergranular stress redistribution used to explain primary creep in constant load experiments (Duval et al, 1983). The curvature of the stress strain line at the start of each experiment may relate to initial porosity loss as suggested by rapid increases in ultrasonic p-wave velocity in comparable experiments by Vaughan et al., (2017).*”

17. figures 3, 4, 5 : If I understand (this is not clear in the paper), the shown grain size distributions indicate the number of grains at a given size. It would be more instructive to show the volume fraction, not the number of grains, as the rheology is associated with the volume average of grain deformation.

Grain size distribution has been used to show generation of small grains after deformation. These grains are not observed in undeformed grains. We estimated grain volume for each grain

size class for modelling the effect of small grains on mechanical weakening. These grain volume data are subject to another paper.

18. figure 14 is interesting, as it shows that the strain-rate seems to have little effect. To my understanding, this is not expected for thermally activated mechanisms such as recrystallization, where time comes in plays. This figure could be more largely discussed, to my point of view.

We plotted data from this study and previous studies in a diagram of θ as a function of strain with data subdivided with different temperatures and strain rates to increase our understanding of the processes that might control the c-axes cone opening-angle (Table 4 and Fig. 13). The relation to strain rate within the broader data set in this figure is not very clear, because for any given temperature there is not a big range in strain rate. The exception is the data set plotted from (Qi et al., 2017) at -10 °C and ~ 20% strain which does show a rough decrease in θ as strain rate (or stress) increases (See Qi et al., 2017 fig 9. This fits with the Zener-Hollomon concept (Zener and Hollomon, 1944) that suggests that decreasing strain rate will have an equivalent effect to increasing temperature.

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