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Interactive comment on "Microstructure and texture evolution in polycrystalline ice during hot torsion. Impact of intragranular strain and recrystallization processes" *by* Baptiste Journaux et al.

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We thank the reviewer for the detailed comments as we think it helped to greatly enhance the quality of our manuscript. We provide here answers to all the reviewer's comments, along with the revised manuscript and Supplementary materials.

Scientific Discussion 1.a: The <11-20> and <10-10> in the high strain sample (γ=1.96) are not randomly distributed within the girdle. The <11-20> and <10-10> both have broad maxima, parallel to the shear direction, of $\sim$ 4x m.u.d. and $\sim$3 x m.u.d. respec-

tively. These compare to minima within the girdle of $\sim$ 2x m.u.d. This level of <a> and <m> alignment is comparable to that shown for the highest shear strain data at -5C in fig 4 of (Qi et al., 2018). Additionally the ratio to the <c> axis maximum (max <a> $\sim$ max (c)/y where y is between 2 and 4) is very similar to the highest shear strain data at -5C and all data at -20C and -30C in fig 4 of (Qi et al., 2018). The alignment of <a> and <m> orientations is important. This might provide a cool tool for assessing shear directions in the analysis of naturally deformed ice so it needs to be documented. <a> and <m> being co-aligned matches our data and is intriguing. At present I do not have a coherent explanation for this. I'd be interested to hear your views on this.

Response : Our data presents indeed a preferred orientation of <a> and <m> directions within the girdle, as reported by Qi et al. (2018), instead of being randomly distributed within the girdle. We edited the figure 4 to adapt the ODF intensity to make the distribution easier to see, and added the following description in the manuscript:

Added Text (pp.11 L.11): For the high shear strain sample at γmax=1.96, the <10-10> and <11-20> axes (<a> and <m> axis) form a girdle, which tends to align in the shear plane. Within this girdle, there is a preferred orientation of both <a> and <m> directions toward the shear direction. The present CPO is similar to those formed in direct shear experiments (Qi et al., 2019). It is consistent with equivalent contribution of the three <a> axes in accommodating shear on the (0001) plane.

Scientific Discussion 1.b: You have not commented on the shape of the M1 and M2 maxima. In virtually all experimentally sheared polycrystalline ice samples these maxima are elongated in a direction perpendicular to the shear direction (see discussion in (Qi et al., 2018) and in our response to a Maurine Montagnat comment on this in the discussion section). Sometimes the elongated maxima (both M1 and M2) are actually each double maxima, with the profile plane as a mirror plane. The vast majority of naturally sheared ice samples do not have elongated maxima, the contours of the maxima match small circle distributions (e.g. (Hudleston, 1977)). This point of difference between experiment and nature is important and as such it is important that the shape of the M1 and M2 maxima from experiments is described. The high strain (γ=1.96) M1 is clearly elongated in the direction perpendicular to shear. I have superposed small circles, with their cone axes on the primitive, on the figure above to emphasise this point. M1 in the lower strain experiments is not so clearly elongated. In the annealed experiment the contours match the small circles, and it looks like this is the case for the lower strain experiments. In our experiments (Qi et al., 2018) elongation increases with shear strain. M2 in the γ=0.42 experiment is elongated, with a double maximum (labeled above max1, max2), with the profile plane as a mirror plane. The γ=0.42 experiment may also show this but I can't tell from the figure. Interestingly M2 in the annealed sample does not look elongated. This could be an important point. Does annealing remove the cluster elongation? One of the reasons we adopted a different reference frame in (Qi et al., 2018), with the pole to shear plane in the middle of the stereonet, is that it makes it easier to see cluster shapes, as shown below in a re-analysis of the (Bouchez and Duval, 1982) data. The highest and lowest strain samples in these data have elongated M1, the medium strain sample does not.

Response : The reviewer pointed out an aspect of our results that was understated in the initial version of the ms. We included the following text in the manuscript:

Added Text (pp.11 L.16): Some elongation of the distribution of the M1 and M2 submaxima towards the Z direction (Figure 4.b), which is the normal to the shear direction in the shear plane, is visible in our results. This elongation is best expressed for the M1 maximum in the highest strain sample TG1.96, for which pole figures for <0001> <10-10> and <11-20> lattice vectors are now represented in two perpendicular reference frame in figure 4.b for better readability. Similar elongated distributions of <c> axes have been reported in direct shear experiments by Qi et al. (2019). Some elongation of the M1 maximum is also observed in the highest shear strain sample (gamma = 2) of Bouchez and Duval (1982) as well as in other shear experiments in Li et al. (2000), Wilson and Peternell (2012) and Budd et al. (2013). However, most naturally sheared ice samples do not have elongated <c> maxima (Hudleston, 1977).

Scientific Discussion 1.c: I think you need to be a little more precise in description of the symmetry of the M1, M2 maxima pair with respect to the finite elongation direction. I think this is a cool observation and potentially of some value, but the symmetry is far from perfect. Below I have plotted up some traces for M1 and M2 (red lines), with angles measured from the top of the stereonet. The green line has equal angles to the two red traces. Superficially this green line is close to the finite extension direction (ED), but if I plot the expected M2 trace (yellow line) assuming it has the same angle to ED as M1 (and adjusting ED for for M1 not being at 0 degrees in the two lowest strains) then the observed M2 is anticlockwise of the yellow line for the three lowest strain, most markedly for the annealed sample. The symmetry you describe is approximate. Another way of looking at this is to plot the angle between M1 and M2 against shear strain. Below is a modified version of fig 8 from (Qi et al., 2018) with the addition of your data (big red dots) and a line (pink) that predicts the position of M2 if it has the same angle to the finite extension direction as M1. This is quite an interesting addition to the plot as very broadly the red data points (high T experiments: not just yours) do follow the path of the pink line, but at slightly lower angles? Is M2 at high T and low shear strain (<=$\sim$2) related to the orientation of the finite strain ellipsoid?

Response: The reviewer was right in noticing that the symmetry is not perfect. We included in figure 2.b the directions and angles of the M1 and M2 submaxima, along with the phi angle, for ease comparison with results from Qi et al. (2019). We also reported in the manuscript that the symmetry between M1 and M2 relative to the finite extension direction was not perfect. M2 maxima are at larger angles to the finite extension direction (ED) than what would be expected for a perfect symmetry. Nevertheless, the residual M2 angle relatively to the perfect symmetrical direction with M1 is rather small (between 1 and 3°), except for the annealed sample TGI0.71, which has an angle of 17°. This deviation could be due to annealing processes and is now discussed in a bit more details in the discussion of the new manuscript. Since the number of experiments performed in the present study is too small for a statistical analysis, we prefer not to discuss this point into more detail in the current manuscript. We refer therefore the interested reader to the discussion in Qi et al. (2019) paper for more details on this point. We would be happy in any case to share our data, if the reviewer was willing to use it to complete his data set, as exemplified in the figure presented in the review.

Added Text (pp.10 L.1): “Nevertheless the symmetry between M1 and M2 around the finite extension direction is not perfect. The angle between M2 and ED is generally larger than the angle between M1 and ED by 1 to 3âŮę. The exception is the annealed sample TGI0.71, where the difference between the two is 17.4âŮę, with M2 closer to ED than it should have been for a perfect symmetry. This change may be due to a post-deformation CPO evolution, due to grain growth during annealing. A small lag in the reorientation of the M2 submaximum relative to the M1 submaximum is also observed in other simple shear experiments (Bouchez and Duval, 1982; Qi et al., 2019). The limited number of experiments performed in the present study precludes a statistical analysis of this behavior. The evolution of the angle between M1 and M2 (φ) with increasing shear strain is, nevertheless, discussed in more detail in Qi et al. (2019), which present a comparison between observations in ice shear experiments results at different temperatures and numerical modeling.

Added Text (pp.17 L.17): “Furthermore we report an offset angle between the M1 and M2 submaxima of 17.4âŮę greater than what would be expected for a perfect symmetry to the finite extension direction. Other experiments, which didn’t undergo annealing show a difference in angle of only 1-3âŮę with the perfect symetry. In the TGI0.71 annealed sample, M2 is much closer to M1, as would be expected for a higher finite shear strain. This could be interpreted as a sign of preferential growth of bulging nucleii with orientation closer to M1 than the bulk CPO of the sample before annealing. ”

Scientific Discussion 2 The description/ documentation of the experimental set up needs to be improved. Please provide some key diagrams that show the experimental set up. Torsion is an important deformation kinematic and the torsion experiments you show here and the classic work of (Bouchez and Duval, 1982) represent significant

contributions to our understanding of ice with direct application to polar ice sheets and glaciers. I believe that torsion is an important defomation kinematic to explore more fully in the future. The picture in (Duval, 1976) and the words in (Bouchez and Duval, 1982), (Duval, 1976) and presented here are insufficient for someone to reproduce the experimental set up. It would be great if you could present (maybe in supplementary information) some diagrams that show the mechanics of the deformation apparatus. There is one particular aspect that I think is of paramount importance. I think that this apparatus is constrained to deliver simple shear, with no shortening or extension normal to the shear plane. If this is the case I presume that the “platens”, that deliver the torque, are fixed so that they cannot move normal to the shear plane. This is important so that we can be clear which experiments are simple shear only, and which comprise simple shear with a component of shortening (or extension). This is not necessarily the same as having zero normal stress on the shear plane. (Li et al., 2000) (a key paper that is not cited in your work) point out that direct shear experiments using a “Jacka” rig, with the normal load set as zero still experience shortening/ extension normal to the shear plane (and that the magnitude depends on sample geometry). Furthermore they suggest that an experiment with fixed platens will generate shear plane normal stresses of 0.1 to 0.2MPa. In my view a constrained (by fixed platens) simple shear experiment is great - it’s a clear kinematic end member. We do need to be absolutely clear about the experimental kinematics and the implications the kinematics have for stress, rheology and microstructure. What are the kinematics and dynamics of naturally deforming ice systems is yet another matter. I can imagine some scenarios (e.g. ice stream margins) where perfect simple shear may occur and others (e.g. basal zones) where shear with shortening parallel to the shear plane occurs.

Response: We added a scheme of the experimental setup in the supplementary material. The presented experiment is indeed a simple shear setup with fixed plates. The reviewer is right, the “platens”, that deliver the torque, are fixed so that they cannot move normal to the shear plane. Furthermore, the sample is held horizontally. Therefore, no extension of compression component of stress are delivered.

Added text : (pp.4 L.18): The design of the torsion apparatus does not allow for displacements parallel to the rotation axis; the imposed deformation is therefore perfect simple shear. During the experiments, the evolution of the CPO under these fixed-end boundary conditions might produce axial stresses (Swift, 1947). The latter cannot be measured in the present apparatus, but polycrystal plasticity models indicate that these axial stresses may attain values similar to those of the shear stresses when the CPO is oblique to the imposed shear (Castelnau et al., 1996). A more precise description of the apparatus is provided in Supplementary.

Scientific Discussion 3 The mechanical data are a bit puzzling. The focus of this paper is the microstructure, and I don't think the questions about the mechanical data affects substantially the microstructural observations and interpretations, but I would like to see a bit more analysis of the data. The key problem for me is that the applied shear stress should be the dominant control of the shear strain rate (whether secondary, tertiary of at a $\sim$ given strain in transient creep), given that your temperature and starting materials were nominally the same for all experiments. A shear stress of 0.6MPa vs 0.50.5MPa should give a $\sim$ doubling of strain rate (for n between 3 and 4). The secondary creep rate for TG10.42 (0.6MPa) is slower than that for TG10.71 (0.5MPa) and faster than TG10.2 (also 0.5MPa). In the text this is attributed to "variability of grain size and textures". This could be true, but in needs to be unpicked in a bit more detail. The method used to fabricate the starting material sounds the same as that we use (except that we do not anneal) as described in (Stern et al., 1997). We have looked at >10 samples of starting materials made by the same methods in four different labs (Otago, MIT, UPenn, UCL) and all have very very similar grain size distributions, mean grain size and random CPO; an example is in fig 1a in (Qi et al., 2017). I cannot see that the annealing will affect the CPO and annealing at consistent T and time should give the same grain size distribution. Do you have initial g size data from more than one sample? We can estimate what grain size differences would be needed to explain the variations in secondary creep rate. The ratio of secondary creep rates of the two samples deformed at 0.5MPa is about 2 (estimated from slopes on fig: would be good

to provide an enlargement of secondary creep region, as you have done for primary creep region). Using the grain size exponent (-1.4) from (Goldsby, 2006; Goldsby and Kohlstedt, 2001)jb this would require the relative mean grain sizes of the two samples to be $\sim$ 1.7. (e.g 1.5mm and 0.9mm). This grain size exponent may be a bit large. A more conservative estimate (related to similar starting materials) comes from using the peak stress (= secondary creep) data in (Qi et al., 2017), fig 3. This gives an $\sim$ grain size exponent of -0.8, requiring a grain size ratio of $\sim$2.3 (e.g 1.5mm and 0.65mm) to explain the strain rate differences at 0.5MPa. I am pretty sure that your original grain sizes do not vary by a factor of $\sim$2, so grain size is unlikely to provide an explanation for the variability in mechanical data. Although it seems likely that your bulk CPO is random in all starting materials, it is worth considering whether the sample cross section contains enough grains to give the mechanical properties of a random CPO. This was clearly an issue for us deforming 1 inch diameter samples with a $\sim$5mm grain size (Craw et al., 2018): in this case a cross section may contain only 10 or 20 grains and the peak stress (= secondary minimum) data do not have a systematic relationship to strain rate. In your case there should be $\sim$ 500 grains in a 35mm diameter cross-section so I would have thought this effect is unlikely to be significant. It seems unlikely to me that the variations in strain rate relate to variability in the starting material. In this case it's worth looking back at the experimental set up. How is stress transferred from the rotational drive platens (this needs describing- see point 2) to the sample? Is there a possibility that there is some slippage (frictional loss) or other parameter that varies from one sample to the next so that the torque is not all transferred to shear stress on the sample?

Response: We have been a bit fast in proposing that initial grain size and texture could be at the origin of the difference in mechanical response between our different tests. In fact, although some slight variations of those two parameters are expected to occur from sample to sample, they are probably too small to justify the measured differences in strain rate. Unfortunately the starting CPO and grain size distribution was not measured for each sample. Nevertheless, we thank the reviewer for the comment on the number of grain limitation. Indeed, with a starting grain size of 0.7 mm we can expect 45-50 grains in diameter in our samples (and not 500). If we talk in terms of radii (were the gradient in shear is applied, the picture gets even worse, with only 23-25 grains. This could indeed have as strong influence on the strain rate which could explain the difference we see here. We included in the new version of the manuscript a note to use these curves with cautions because of the points discussed above.

Added text (pp.7 L.19): The significant variations observed in strain rate evolution with time between the different runs cannot be attributed to a variation in initial grain size, CPOs or in the applied torque alone (Table 1), but rather to coarse-grained microstructure of the samples, which resulted in less than 25 per radii. The strain/time curves presented in figure 1 are therefore useful to characterize each run creep regime independently, bur should be used with care in comparison between different samples or with other experiments.

Scientific Discussion 4 The discussion of modeling is rather black and white and superficial. Numerical models and physical experiments all have limiting boundary conditions. All models and experiments show us something and none match nature, primarily because we cannot access natural conditions and have uncertainties about natural boundary conditions. Linking physical experiments to numerical models is important as we have much more control on the boundary conditions in both cases: so we learn more about our understanding of processes. However the crucial thing for both experiments and models is that we are clear about what we learn from them. I think having a model that is able to simulate fully CPO and microstructure evolution at high strains is still a way off. All steps on the way to achieving this are valuable and a discussion that implicates that one model is right and another wrong is inappropriate: in demeans what we learn from the models. I agree that the Etchecopar model as used in (Bouchez and Duval, 1982) matches quite well your data and most of the “hot” shear data (see the red symbols in M1-M2 angle vs shear strain graph posted earlier: Etchecopar model also plotted on this as hollow black squares). The problem is that these are the only

data it fits, so if this model is applicable it tells us only part of the story. The model does not predict the drop off to single maxima by shear strain of 2 in (Li et al., 2000); maybe this is a kinematic difference between simple shear and simple shear plus some strain normal to shear. The model does not match the minimal “colder” data we have, most particularly the -30 data from (Qi et al., 2018). The FFT model (Lebensohn, 2001) gives a remarkable match to experimental observations of intragranular deformation at low strain (Grennerat et al., 2012; Lebensohn et al., 2009). This is the code used to simulate shear deformation in the models by (Llorens et al., 2016; Llorens et al., 2017). The fact that the same model works well at low strain and less well at high strain tells us something. The bulk CPOs in Lloren’s models do not have double maxima, but the double maxima are there when only the high strain rate data are used (see Llorens, 2017 fig 5i) and the angle between maxima in the deformation only models evolves in a way that matches the -30 experimental data we have (Qi et al., 2018). Addition of recrystallization into the model changes the result, although not in a way that gives a really clear match to observations. There is no real conclusion here apart from this: both models and experiments are important. Probably most important is to design experiments that enable clear boundary condition matches to numerical models. That is the really beautiful thing about the columnar ice work at low strain e.g. (Grennerat et al., 2012). At high strain and in shear matching of model and experimental boundary conditions is rather harder.

Response: This response also takes into account the remark by Griera, Bons & Llorens,. We made use of the Etchecopar model just to highlight the likely role of subgrain boundaries in the process of accommodating basal glide of dislocations during simple shear of ice. To our point of view, this model can only explain that strain incompatibility accommodation processes are required to obtain the strong single maximum observed in the laboratory and in the field (Hudleston et al. 1977). We will be clearer about that in the text. Considering FFT homogenization schemes as the one used by Llorens et al. (2017) and the one that we used in Grennerat et al. (2012) or self-consistent viscoplastic models (Castelnau et al. 1996), they stand on strain being produced by the activity of slip systems only. For ice, the only slip system for which there is experimental evidence of easy activation is the basal system. However, the basal slip system cannot, alone, produce a general type of deformation. Thus for maintaining strain compatibility, these homogenization approaches require the activation of the non-basal systems, namely, prismatic and pyramidal systems. Activation of these systems induces specific rotations of the crystals. This is the main reason why, unless extra mechanisms (which mimic the role of dynamic recrystallization in helping to enforce strain compatibility) are added to these models, such as in Wenk et al. (1999) or Signorelli and Tommasi (2015), the crystals never reach the stable position observed experimentally or in naturally deformed samples, in which the main slip system is parallel with the imposed macroscopic.. Models, which do not include any strain compatibility relaxation process, as Llorens et al. (2017) produce a strong clustering of c-axes, similar in intensity to the one observed experimentally or in the field, but offset from the normal to the shear plane. We do not pretend that polycrystal plasticity models are not useful. We just discuss that, by construction, the vertical single maximum cannot be reproduced in a model where deformation is fully accommodated by dislocation glide. In the experiments, other mechanisms do come into play. By consequence, yes, the comparison is very helpful to quantify the role of these mechanisms. The text has been modified to be more clear about this point. We also noticed a mistake. The reference about simple shear modeling of ice in simple shear is Castelnau et al; 1996, JGR. It has been modified in the text.

Added text: (pp.18 L15): Pioneering work on 2D modeling of polycrystalline aggregates under simple shear by Etchecopar (1977) was able to repro- duce the sub-maxima M1 and M2. This was simply done by considering a single slip system (basal slip system for ice) and adding an accommodation process by allowing cells to subdivide (polygonization) and undergo rigid body rotation. The very good agreement of this simplistic model with evolution of textures observed experimentally for ice under shear was empha- sized by Bouchez and Duval (1982), who hypothesized that the polygonization processes in ice would be formation of GNDs and kink-bands. In our results few kink

bands were observed, but the prevalence of GNDs at most finite shear strains suggests that Bouchez and Duval (1982) supposition is reasonable. Although Etchecopar (1977) is too simplistic to pretend reproduc- ing every shear-induced textures in ice, it was useful to raise the likely role of polygonization as an efficient accommodation mechanism for solving strain incompatibility problems. Modeling of shear in ice has been done by mean-field approaches as in (Castelnau et al., 1996) or more recently by full-field modeling as in (Llorens et al., 2016). Both works reproduced the formation of a strong single maximum texture from shear strain of about 0.4 and above. Nevertheless, neither orientation of this single maximum normal to the shear plane, nor the existence of two submaxima observed at lower strains in the field or experimentally are correctly reproduced. The fact that the single submaxima prescribed is inclined from the tangent to the shear plane is significant, and stands from the fact that these homogenization techniques require the activation of non-basal slip systems. The activation of secondary slip systems, whose contribution to strain has never been proven experimentally, induces a geometrical rotation of the crystal, that is responsible for the modeled inclination of the clustered CPO compared to the vertical. The activity of these secondary slip systems relative to the basal ones is controlled by a parameter that is arbitrarily defined (it has been defined in comparison to experimental observations in Castelnau et al. (1997), using the mean-field VPSC approach, and values different than the one chosen is the previously cited studies were obtained). The higher is non-basal activity, the softer is the mechanical response of the crystal to accommodate the imposed conditions. The geometrical constraint of crystal rotation under shear, owing to the activity of non-basal slip systems, can be artificially relaxed, such as in Wenk and TomeÌĄ (1999), by forcing the growth of selected grains, or as in Signorelli and Tommasi (2015), by an association of polygonization and local (within a grain) relaxation of the strain compatibility constraints. By comparing these various modeling approaches, and their inclusion of recrystallization mechanisms, it appears that accommodation mechanisms, other than non-basal slip systems, must come into play to explain recrystallization induced shear textures in ice. Although we consider that fast grain boundary migration might be an efficient strain accommodation mechanisms, we suggest here that an efficient additional contribution to the texture reorientation, at the high homologous temperatures of our experimental studies (and the ones of Bouchez and Duval (1982) or Qi et al. (2017, 2019)), might well be nucleation assisted by polygonisation (or sub-grain boundary rotation).

Clarity of writing 1/The bulk of the text is well-written. The clarity of the writing is not as good in the discussion and not good at all in the conclusions. The discussion would benefit from some shortening and restructuring. The discussion starts with a reminder of the key observational data and I think it would be very helpful to the reader if you added a schematic diagram to highlight these key observations. This would then give a clear framework for ongoing discussion. The conclusions needs to have clear statements on what are the new factual observations and what are the interpretations of those observations.

Response : We edited the discussion and conclusion to enhance the clarity. We have included bullet points in the conclusion

2/The abstract should be a concise summary of the new findings and some short statement about importance. The abstract contains an extended statement of background that is better placed in the introduction (it is in fact already in the introduction).

Response : We feel that a very short background in the abstract can be relevant for some readers not coming from an Earth Science or glaciology background. To shorten it we edited the abstract to remove the background statement on modeling.

3/I would go for a simpler title: “Evolution in polycrystalline ice microstructure during progressive high temperature shear” ????

Response : We thanks the reviewer for this suggestion and we changed the title to : “Recrystallization processes, microstructure and texture evolution in polycrystalline ice during high temperature simple shear”

Technical/ terminological/ picky things 5. It would be great if you could show full grain size distributions (frequency plots). You are correct that the mean is not a great scalar to represent recrystallized grain size statistics. Grain size distributions could be represented as an extra row in figs 2 and 4 (it would be nice to compare the AITA and EBSD measures- I don’t expect them to be the same: see (Cross et al., 2017))

Response: During the analysis of the data we found that due to the small number of experimental runs (3 with gamma_max > 0.2 without annealing) made in this study the comparison of grain size frequency plots was not providing enough clear information to be included in the main manuscript. We have added the grain size frequency plots for both AITA and EBSD as supplementary material.

6. Please put the number of grains that correspond to each pole figure on figs 2 and 4 or in a table. This is important in comparing data sets.

Response: We have included the number of segmented grains in figure 2 and 4.

7. If you can, show point stereonets as well as contoured nets. The contoureing hides a lot of information.

Response: We added point stereonets in supplementary materials and kept the contoured ones in the main manuscript to maintain the readability of figures 2 and 4.

8. The statement on page 2, line 26 states that the “texture can increase shear strain rate (word “rate” missing) by a factor of ... “. There is a clear correlative relationship of weakening and CPO but a causative relationship is not established. Weakening in ice from secondary to tertiary creep correlates with development of a CPO. It is intuitive that the CPO developed in shear facilitates further shear. However similar weakening occurs in cold axial shortening where the CPO (cluster of c-axes parallel to shortening) would intuitively make further axial shortening harder e.g. -30 experiments right hand column of fig 3 in (Craw et al., 2018), mechanical data in fig 10. Other changes correlate with weakening, most particularly grain size changes (as documented in your

paper and elsewhere). In the geological literature grain size reduction is often thought of as the main cause of weakening. In reality CPO, grain size and other microstructural parameters all change in correlation to change in mechanical behavior. It is unlikely that the mechanical evolution is caused by changes to just one of these sample parameters.

Response: We agree with the reviewer and have changed the text accordingly to underline that weakening does correlate with the evolution CPO as well as other factors like the grain size.

9. I don't think that Kamb's idea that CPO is independent of of T, strain rate or stress is confirmed (P3, L11). The data in (Qi et al., 2018) show that in shear the CPO changes with T. (Qi et al., 2017) show that in axial shortening CPO is sensitive to stress or strain rate (the two cannot be separated). It is reasonable that the stress/ rate effect will also apply in shear. Using Huddleston's data in comparison to experiments is complex as both T and rate change. The lower rate has a similar effect to deforming hotter.

Response: Kamb (1972) states on his simple shear results on pp.233 : "Texture is sensitive to temperature, whereas fabric is not: recrystallization gives a distinctly coarser texture at the melting point than at temperatures only a few degrees below, whereas the fabrics developed under the two conditions are nearly the same." With the "texture" corresponding to the geoscience definition of grain shape and spatial relationships of grains, and "fabric" to CPO. We agree with the reviewer nonetheless that this conclusion is based on a small amount of results and on a limited temperature range (0 to -4°C in Kamb's work) and we rephrased this part of the text to address this point and refer to Qi et al. (2017,2019) work that shows a temperature and stress dependence of the CPO in both uniaxial and direct shear experiments.

Added text (pp.3 L7): Most of the knowledge on the microscopic processes occurring in polycrystalline ice under simple shear deformation is still mostly limited to deformation results from to data published over 30 years ago (Kamb, 1959, 1972; Duval, 1981;

Bouchez and Duval, 1982; Burg et al., 1986). The tools and methods used in these studies to analyze the CPO were often manual and highly dependent on the operator experience. Electron Back-Scattered Diffraction (EBSD) and Automatic Ice CPO Analyzer (AITA) can now provide high spatial and angular resolution quantitative data, enabling a global and statistical study of the processes accommodating strain at the micro-scale. Recent experiments (Qi et al., 2017, 2019) using these new characterization techniques have shed new light in some aspects of the question. They have, for instance, disproven the hypothesis by Kamb (1972) that CPO evolution in ice mainly depends on the finite shear strain and is not sensitive to temperature, strain rate, or stress. Indeed, Qi et al. (2017) that showed that during axial compression the final CPO is sensitive to stress or strain rate, and by Qi et al. (2019) which showed that the rate of evolution of the CPO in simple shear is sensitive to temperature.

10. The statement on page 5, line 8 is incorrect. Cryo EBSD of ice is not (in general) limited to samples of $\sim$10 by 20mm. In terms of published data there is a map in (Prior et al., 2015) (fig 12) of 80 by 30mm, the data in (Wongpan et al., 2018) has maps up to 40 by 40mm etc. Most of the CPO data we publish from experimental samples come from 25.4 by 40mm samples, our shear data CPOs in (Qi et al., 2018) are from elliptical shear surfaces of 25 by $\sim$ 30mm. For natural samples we routinely work on samples of $\sim$60 by 40mm and with suitable cold stage modifications I don't see why 100 by 50mm is not achievable. EBSD maps with the same dimensions as your AITA maps are possible now. If the Montpelier machine has a sample size limitation and this limitation is important to the paper, then link the limitation to that instrument, otherwise just delete the statement about size limitation. I guess if it the Montpelier machine does have a limitation it must be to do with cold stage tethering (gas pipes) or camera position limiting WD, as the sub-stage is designed for very large stages/samples (Seward et al., 2002).

Response: The reviewer is right and we changed the text accordingly.

11. Please provide enough information for the reader to understand how surface sub-

limation is managed. What I mean by this is; how is frost removed from the sample. There will be a frost layer on the sample surface as it goes into the SEM that would prevent EBSD (needs only ∼ 10-20nm to do this). The two main ways of removing the frost are to heat the stage (Iliescu et al., 2004; Weikusat et al., 2011) or to cycle through pressure (Prior et al., 2015). I recall Andrea Tommasi telling me that the sample is just put in the SEM and it works. In this case I infer that the sublimation to remove the frost occurs on the down pressure cycle and that the sample is warm enough when put in the SEM to give a path through PT space where the sample goes into the vapour field (see fig7 in (Prior et al., 2015). In this case it would be useful to know the sample temperature on insertion and the pressure sequence: do you go to high vacuum then to controlled gas pressure or directly to controlled gas pressure?

Response: We use a different technique for surface preparation than the ones described by the reviewer, where we carefully remove the initial frost by carefully shaving the surface of the sample using microtome blades at -60°C before rapidly putting the sample in the SEM. We do not cycle in pressure or temperature and we never observed any issue with either sublimation or frost if we keep the sample below the sublimation temperature, which is at -60.6°C at 1 Pa. We feel that we provided already all the details in the manuscript and also in other manuscripts like Montagnat et al. (2015).

References: Montagnat, M., T. Chauve, F. Barou, A. Tommasi, B. Beausir, and C. Fressengeas. 2015. "Analysis of Dynamic Recrystallization of Ice from EBSD Orientation Mapping." Frontiers in Earth Sciences. http://www.gm.univ-montp2.fr/PERSO/tommasi/publications.html.

12. Please say in figure captions if pole figures are equal area or equal angle. I think they are equal area from the shapes of maxima (the projection affects shape analysis of maxima).

Response: We changed the captions in figure 2 and 4 accordingly.

13. It would be really cool to see a radial section of the sample: to see how microstruc-

ture changes with strain in a single sample (e.g. see (King et al., 2011). I'm not suggesting this is needed for this paper- just something cool to do.

Response: This response is similar to the one given to reviewer 1. One axial section was measured with EBSD, but due to the coarse grain size discussed above, it was impossible to fractionate the section in different segments with 'supposed' constant finite shear strains and still have a high enough number of measurements to obtain representative estimates of the CPO intensity. Therefore, we could not use radial sections to estimate the CPO evolution with increasing strain.

14. There are a few key references on experimental shear of ice that are missing and should be cited. These include (Budd et al., 2013; Li et al., 2000; Wilson and Peternell, 2012).

Response: We included these in the revised version of the manuscript.

Added text (pp.3 L.2): "A similar evolution was observed in more recent shear experiments on artificial ice polycrystals by Li et al. (2000) and Budd et al. (2013), as well as by Wilson and Peternell (2012) which analyzed the influence of the initial CPO and the importance of recrystallization processes on the CPO evolution. "

15. There are several published papers that show a lack of CPO change in rocks during annealing. Some of these should be cited.(Augenstein and Burg, 2011; Heilbronner and Tullis, 2002; Ree and Park, 1997). I know there are others in calcite and olivine but can't find them just now.

Response: We thank the reviewer and included these in the revised version of the manuscript.

16. Throughout this paper the term "texture" is used with the meaning common in metallurgy and materials science. There is a very small community of geoscientists who use "texture" in this way and no glaciologists that I know of. For the vast majority of the geoscience community "texture" means the spatial relationships of phases and grains

and their internal structures. To most geoscientists, texture is what you would see down a microscope (in a petrographic examination for example) and is broadly synonymous with the term microstructure. The terms “crystallographic preferred orientation” (CPO: which you use in the intro) or “lattice preferred orientation” (LPO) are much better as they are explicit. If you want this paper to have wider readership/ uptake, remove the word texture throughout and replace with CPO. It is also worth (in the intro) relating this terminology to the word “fabric” and/or the acronym “COF” (crystal orientation fabric) as commonly used in glaciology. I avoid using the term fabric (except in explanations of how terminology matches up) as metallurgists use this term to mean microstructure.

Response: We agree with the reviewer and replaced texture by CPO in the entire manuscript.

17. It is not really clear what are the observations you use to constrain the dimensions of the bulging nucleus.

Response: We use the similarity in the microstructures observed in the present experiments with those described in Chauve et al. (2017) to suggest that bulging associated with formation of low angle grain boundaries may be an efficient nucleation mechanism. The increase in the c-axis component in the WBV of the low angle boundaries in the first 100 μm from the grain boundary is an indicator of the presence of sub-grain boundary loops with c-component GNDs, which as described in Chauve et al. (2017) play an essential role in closing the bulges. Thus the width we extrapolate for a maximum bulging nucleus of 100μm, which is controlled by the length scale over which the stresses are high enough to activate the hard non-basal slip systems and close bulging grains.

18. I don’t follow the discussion related to nucleation in the section where the annealing is discussed. Grain size increases during the annealing so nucleation is unnecessary. If you are talking about relationships that might be relevant to nucleation prior to the annealing then this needs to be made clear.

Response: We do not suggest that nucleation necessary occurs during annealing. We just highlight the fact that grain boundary migration during annealing does not drastically modify the texture. Therefore, new grains present prior to annealing, with low defect density and greater chance to grow through GBM, must have had orientations close to M1 and M2. This suggest bulging as the dominant mechanism for nucleation at the conditions of our experiments, as it tends to create grains with a closer orientation to the parent grain. We rephrase this section to make our point clearer.

Added text : (pp.17 L.15) “This suggest bulging as the dominant mechanism for nucleation at the conditions of our experiments (prior to annealing), as it tends to create grains with a closer orientation to the parent grains. Furthermore we report an offset angle between the M1 and M2 submaxima of 17.4âŮę greater than what would be expected for a perfect symmetry to the finite extension direction. Other experiments, which didn’t undergo annealing show a difference in angle of only 1-3âŮę with the perfect symetry. In the TGI0.71 annealed sample, M2 is much closer to M1, as would be expected for a higher finite shear strain. This could be interpreted as a sign of preferential growth of bulging nucleii with orientation closer to M1 than the bulk CPO of the sample before annealing ”

19. Bulges cut off by rotation of a subgrain boundary was first suggested (described from see through experiments) by Janos Urai (I think). You should reference (Urai et al., 1986).

Response: We included this reference in the discussion (pp.17 L. 33) of the revised version of the manuscript.

20. Spontaneous (random) nucleation? I have a problem with this - it is a bit of magic with no physically realistic explanation.

Response: Spontaneous random nucleation was first hypothesized by Duval et al. (2012), who proposed that the energy for nucleation is provided by internal stress field associated with dislocations pile-ups. CPO data corroborating the existence of this process as a secondary nucleation mechanism, the dominant one being the association of bulging and subgrain rotation, were presented by Chauve et al. (2017).

21. The conditions of your experiments are not close to those in cold glaciers and ice streams (page 18, line 5). Your slowest transient strain rate is 2.7E-7s-1 which corresponds to a 100m thick shear zone having a velocity difference across it of 850m/yr. The tertiary strain rate in your high strain experiment corresponds to $\sim$2700m/yr difference across a 100m shear zone. I'm not so familiar with temperate glaciers but such shear rates do not exist in polar ice sheet systems eg (Bons et al., 2018; Rignot et al., 2011). Even fast ice stream shear margins max out below 1E-9s-1 (Bindschadler et al., 1996; Jackson, 1999; Jackson and Kamb, 1997). The strain rate has a significant effect on the microstructure and the CPO (Hirth and Tullis, 1992; Qi et al., 2017; Tullis, 1972): increasing strain rate has a comparable effect to decreasing temperature. It is not possible to do an experiment to significant strain at natural conditions. Instead experiments need to provide scaling relationships that allow us to predict the effects of T, strain rate (stress) etc on rheology and CPO/microstructure (with the complication that there are feebacks where CPO/microstructure affect the rheology).

Response: We thank the reviewer for this useful comment and changed the sentence in the revised version of the manuscript as:

Changed text (pp.19. L.28) "The experiments, performed at high temperature, up to shear strains of 2, favored dynamic recrystallization observed in natural conditions with slower strain rates such as cold glaciers, ice streams, and some deep ice core areas."

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2018-213/tc-2018-213-AC2-supplement.zip

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-213, 2018.