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Interactive comment on "Temperature and strain controls on ice deformation mechanisms: insights from the microstructures of samples deformed to progressively higher strains at −10, −20 and −30 °C" *by* Sheng Fan et al.

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This paper essentially presents a nice set of experimental data on polycrystalline ice specimens (synthetic specimens) deformed at constant strain-rate under uniaxial compression, at -10°C, -20°C and -30 °C. Besides mechanical tests, the authors provide a detailed analysis of the microstructure of ice grains and its evolution, largely based on EBSD performed in a dedicated scanning electron microscope. Grain size, grain morphology, spatial grain distribution, intragranular misorientation, statistical grain orientation (crystallographic texture) are investigated with respect to temperature and strain

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(up to 20%). The authors want to put the focus on the effect of grain size on the rheology, as state in the last line of the abstract that can be considered as a summary of the findings of this study : "Grain size reduction, which can be observed in all deformed samples, is most likely to cause weakening (enhancement) and should be considered to have a significant control on the rheology of natural ice flow".

I suggest rejecting this paper for the following reasons :

** 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. 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 $\sim$0.03. There is no data in this paper relating rheology with grain size, although grain size effect is presented as a major conclusion. 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.

** Along the same line, the sentence (p12 line 2) “gbs is kinematically required for all

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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 ?

** 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, . . .).

**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 defor-

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mation 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 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.

** 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 ?

Others remarks :

** 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 ($\sim$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.

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

** p1 line 16 : "displacement rate" instead of "displacement"

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

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

** 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.

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

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

** 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 ($\sim$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.

** p 14 line17: why should there be an "acceleration of grain rotation rate due to intracrystalline basal glide" since the overall strain-rate (prescribed) is constant ?

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** 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.

** 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.

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