Comments on "Crystallographic preferred orientations of ice deformed in direct-shear experiments at low temperatures" by Chao Qi et al.

These comments are made following two initial reviews and submission of the revised MS.

The MS is well written and well organized, and I congratulate the authors on a fine piece of original research. This is an important contribution to the experimental work on ice rheology, with a focus on the mechanisms that accommodate deformation and the crystallographic fabric that results from the operation of those mechanisms, and thus on the rheology that depends on both mechanisms and fabric.

The experimental procedure is well explained, the results clearly presented, and the interpretation and discussion well founded, leading to questions that should be addressed in future work. I have a few specific comments, which may or may not be addressed before final publication.

p. 1, line 2 I don't understand what is meant by "equivalent to an extrusion of 150%" There is no extrusion in these experiments (except by the limited bulging, but that is minor). This figure of 150% relates to the "equivalent strain" shown on Fig. 3, but this is also confusing to me. It requires a definition. For another possible measure of strain intensity, a simple shear strain of 2.6 - the maximum in these experiments - corresponds to a maximum stretch of 2.94 or an extension of 1.94.

p. 4, line 16   It seems to me the statement here should be the other way round. That is, if gamma measured and gamma calculated are very close in value, it implies that epsilon axial is very small, and thus bulging is slight. If bulging is calculated first, how is it done?

p. 6, line 8-9   "There <u>are</u> almost no subgrain…."

p. 6, line 13-15. I would interpret flattening strain to mean shortening normal to the shear plane, not axial shortening, and that this would be determined by the difference between ho and h1 - although I see from Table 1 that the measurement of h1 is not precise. The "axial strain" as calculated here, as I understand it, would only be a true axial strain if the deformation were coaxial, which it is not.

p. 9, line 11   "There <u>is</u> a range of grain ….."

p. 9, line 16-18   The stress drop following peak stress (Fig. 3) is rapid, yet the possible reasons for this given here - weakening due to grain size reduction and thus increase in the contribution of grain size sensitive deformation mechanisms, and geometric softening due to the development of CPO are both likely to be gradual. So why so rapid a drop?

p. 10, line 3-4 The flattening that occurs in the samples is not just accommodated by extension normal to the shear direction but also by extension parallel to the shear direction. So it's not clear why there should just be preferred elongation in the direction normal to the shear direction. Why not a broadening in all directions?

Figs. 5-7. The fields of view shown may be deceptive, but the average grain size and number of grains counted are hard to reconcile with the images. In particular, why only 144 grains in the lowest strain sample at -5 and 3000+ grains for the highest strain sample at -30 for a grain size that is on average larger?

Figs. 5-7. In principle the plane of section here - the shear plane - is a plane of no strain, so one would expect to find little or no SPO. It would be interesting to see sections perpendicular to the shear plane and containing the shear direction, where one would expect to see a more pronounced SPO, and where it might be possible to get more information about the recrystallization mechanisms.

Also, to the extent that there is SPO it should be in the shear direction, yet in the image for the highest strain at -30, the direction of max elongation appears to be at an angle to the shear direction. Is this just an illusion?