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Discussions

EGU

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

M. Montagnat (Referee)

maurine.montagnat@univ-grenoble-alpes.fr

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Dear authors.

I enjoyed reading your work, and I find it very complementary to some recent work that we did in Grenoble, and that my colleague Baptiste Journaux presented at AGU last year (publication is about to come...). In this work, we performed torsion test on ice polycrystals at high T, and followed the microstructure and texture evolution by EBSD. I will therefore mention it sometimes. Please consider my remarks below sometime as open questions, and sometimes as a participation to help improving some of your analyses or interpretations. The link between dynamic recrystallization mechanisms and

texture development remains an open question (even in the metallurgical community), and I do not pretend possessing the keys to its full understanding... Maurine

About the experimental procedure and results:

- What could be the impact of Piso on the recrystallization mechanisms? For instance, could it slow down the texture evolution, and the disappearance of the M2 pole? There exist very few work about it, I could just find the paper of Jones and Chew (J. Phys. Chem, 1983) that show the variation of the minimum creep rate with hydrostatic pressure... not sure it helps!

- M index is never used (or did I miss something?)...

- Could you expect any post-dynamic evolution of texture and grain size, especially in the case of PIL135 and PIL144? Therefore how caution should you be to use those results at the same level as the one from other experiments?

- You mention very often the elongation of the c-axis clustering. This elongation is not observed by Bouchez and Duval 1982 (later referred to as BD82), neither in natural ice from Hudleston (1977 or 2015 review paper), and not even in the deeper part of ice cores where shear is strong (as for instance bottom of Talos Dome ice core, Montagnat et al. 2012). We do not observe it neither in our recent torsion exp, going to gamma=2 (Journaux et al. AGU2017)... Therefore couldn't it result from the specific geometry of your test? It could be link to an extension component, cf Kamb 1972? The same remarks hold for the specific anisotropy of a-axis orientations on which I would be very careful before exploiting it too far in the conclusion...

- I am not sure to understand the interest of pole figures made by taking one point per grain. First because it does not include information about grain size, that is otherwise directly integrated, and second because it could induce a bias in case of a non normal distribution (and it is the case I think, regarding your microstructure). Then, since intragranular misorientations are strong, it can induce another bias depending on the

selected point inside the grain... I understand it in order to compare to "old" texture measurements that were done manually, but since the info is included into the "full" texture, not sure it is really interesting...

- in paragraph 2.5, about grain size measurement. How does it take into account the grain shape anisotropy? Such a method seems to be well adapted for a microstructure that evolves in staying "self-similar", but it is not the case at all, since the shape anisotropy (and the distribution of grain size) evolves with strain. Maybe you could just mention such a limitation? In the same idea, there is no mention of the fact that you are using a 2D technique to observe and evaluate grains that have a true 3D structures (with anisotropy). We all do that, of course, but the impact is different when a microstructure is relatively equiaxed, and remain so, or not. In particular, by doing so, we are totally unable to distinguish a small new grain from a cut piece of a highly serrated large grain... It therefore makes it complicated to distinguish nuclei (cf l. 13 of p.8). Again, you are using a "mean grain size", in the case of a non normal distribution, this mean has a weak meaning. Wouldn't a median + quartile representation be more adapted, in order, for instance to follow the evolution of the grain size distribution with strain?

- You refer to SGB, that you assume to be numerous and to evolve with strain. Honestly, this is hard to evaluate from the only figures 5 to 7, since there is no quantitative analyses of it. Relatively to some observations that I have done, I am even surprise not to see more of them, and I would be enable to say that there is a clear evolution with time. Couldn't you estimate, for instance, the Kernel Average Misorientation, and its evolution with time? (cf l.16 p 13 for instance).

About the discussion part:

- part 4.3 About the φ angle, well, I have quite a lot to say (sorry...). First, does it really make sense to compare an evaluation of this angle performed based on very different materials, from experiments with very different conditions? In particular, if I want to

separate the two population of M1 and M2 orientation, I aim to take into account not only the visible angle between the 2 poles, but also the number of orientations on each pole (some kind of "weighted angle"). Why? Because in some cases there remain so few orientations in the M2 pole, that the separation is doubtful (and strongly dependent on the measurement technique). For instance, my interpretation of the gamma = 2 texture in BD82 is that there is no more M2 (or too few to be considered, and this was also the interpretation of BD82). Therefore, in this case, the phi angle has not meaning. I would do the same interpretation on your figure 2d at -20 and -30°C. Then, from my point of view, figure 8 is highly confusing. (a) you compare experiments made in drastically different conditions. For instance Burg et al. worked on thin plates of ice, with very few grains, and strong boundary conditions effects are expected. Kamb added some axial compression at different levels, Hudleston samples are from natural fault... (b) the weakness of the model used by Llorens et al. is also hidden in the figure. This model, by construction (because it requires non basal prismatic and pyramidal slip system to accommodate the deformation) is unable to stabilise the M1 pole to the vertical, as it is observed naturally or in the laboratory. Although the angle can be small between this pole and the vertical, it persists and the main reason is a non adapted representation of the mechanisms that are accommodating basal slip. Even when adding some representation of subgrain mechanisms can't they correct this bias. But when plotting only the phi angle, this problem is hidden, and the interpretation can be biased... Well, I am not sure that this phi angle evolution is so necessary to the interpretation of your results, and maybe this one would be clearer without trying to fit all other existing data???

- Part 4.5 Here, again is evoked the increase of subgrain boundary and lattice rotation effect without it being really quantified... It is therefore not so straighforward to assess an evolution from GBM to lattice rotation dominating process during recrystallization.

- Part 4.6 This paragraph is, to my point of view, giving a quite simplistic explanation for CPO development. It has been shown in ice and other materials, for quite some

time now, that stress and strain field heterogeneities prevent from making a clear distinction between grains "well oriented" and grain "badly oriented" (see e.g. Grennerat et al. 2012, Piazolo et al. 2015 for ice). Such a clear separation is holding when dealing with mean-field modeling approachs that are considering grain as an inclusion in a homogeneous equivalent medium. Full-field modeling approaches such as the one of Llorens et al. does reproduce the complexity. Furthermore, other mechanisms such as twinning (not in ice) and kink-banding (in ice) can be invoked to accommodate basal slip without the requirement of glide in non-favorable slip systems. And internal distortion does not always need non-basal dislocations to be formed... (tilt bands in ice are made of edge basal dislocations). And internal distortion is not a proxy of the dislocation density! It is only a proxy of the geometrically necessary dislocations, which are not necessarily correlated with the full dislocation density... Maybe some quantitative observation of a relation like subgrain density = f(schmid factor) could help? But we have tried, and we find no relation, such as the results of Grennerat et al. (2012).

- Part 4.7 seems highly speculative to me. Mainly because this could very likely result from this specific experimental set-up on which a compression or tension component may add to the shear deformation (see for comparison results of Duval 1981, JOG vol 27 who shows the effect of adding some compression on a shear experiment. Although they do not measure a-axis orientation, their resulting c-axis orientations could explain partly your a-axis distributions). Just to let you know, we do not observe this specific distribution in our recent torsion tests on ice (Journaux et al. AGU 2017). On more point on this paragraph, related to the references given for c-axis clusters observe in nature: it seems to me more fair to cite pioneer works when they exist (at least some) than to always refer to the review work or the more recent work.

- Conclusion:

From the remark about paragraph 4.7, I would suggest not to mention the point 4 of the conclusion, since it has not been shown that the observed results are not related to the specific experimental conditions used here. To turn this observation into a generic

tool to interpret natural texture appears to me a little too fast...

Similar remarks would hold also for the point 5, and the mentioning of the elongation of c-axis cluster that has not been observed in previous work (for instance Bouchez and Duval 1982).

About point 6, please refer to my comment on subgrain observations that are very weak in this paper. In order to be able to provide some info about subgrain size, one would need a measurement of this size, or at least a proxy, and it is not given in this work.

Point 7, a similar remark holds here too since, in order to link what the author calls "high Schmid-factor grains" to GBM, one would need to show that there exist a relation between the Schmid factor and the grain size, or grain shape for instance (if grain size or shape is taken as a proxy of GBM). And this is not provided here. And the last sentence appears speculative too, since, to be able to discriminate the nucleation mechanisms, one would need to be able to observe nuclei! But, because of the 2D observation tool used here, one cannot distinguish small new grains from cut part of a serrated grain boundary. On top of that, nucleation by bulging could very well occur, but one would not observe it by only looking at 2D microstructure at the end of the test. Therefore, the only qualitative observation of subgrain boundaries is not enough to discriminate the nucleation mechanism. So this could be your interpretation, or assumption, but to my point of view this is not shown by the results presented here.

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