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Interactive comment on "Electron backscatter diffraction (EBSD) based determination of crystallographic preferred orientation (CPO) in warm, coarse-grained ice: a case study, Storglaciären, Sweden" *by* Morgan E. Monz et al.

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The article presents EBSD data on the crystallographic preferred orientations of ice in a shear zone composing the lateral boundary of a warm mountain glacier, where deformation took place at temperatures > -10°C. The main conclusion of the article is that under natural low strain rate conditions simple shear under high homologous temperature conditions, which favor dynamic recrystallization by grain boundary migration, produces ice CPO evolutions similar to those observed in simple shear experiments at high homologous temperature conditions. A secondary conclusion is that the multi-

maxima CPO patterns observed in previous samplings of natural high-temperature shear zones in ice were an artifact due to analysis of a too small number of grains and oversampling of some grains with very irregular shapes. Both conclusions seem undoubtedly correct, but not really new.

The main challenge in collecting such data was the extremely coarse grain sizes of ice in such conditions (typically > 20mm, but attaining >90mm, as the authors describe). To circumvent this problem, the authors analyzed composite sections constructed from serial sectioning of blocks 15x15x30 cm blocks. The technique is presented as new, but use of multiple coherently-oriented samples to analyze a representative volume of coarse-grained materials is a rather traditional (and effective) solution for this problem. Given the fact that the authors still observe multiple maxima CPO patterns for all analyzed composites, one may question if the technique proposed is really effective (the spacing used for the sectioning is probably still smaller than the maximum dimension of the grains). One may therefore question why should one prefer this method to the even more traditional one (at least in geology) of collecting oriented samples in a series of profiles normal to the shear zone trend and then add up the data for samples with similar positions across the shear zone. This second approach would allow to: (1) spread the sampling a much larger volume, (2) preserve the relation between CPO and microstructure, which is essential for discussing the role of deformation and recrystallization processes on the evolution of the CPO, and (3) collect data for variable finite strains (which is missing here and would have been extremely useful to discuss some features, such as the deviation of the [0001] maxima relatively to the normal to the shear plane along the plane normal to the shear direction or how the CPO evolves with finite strain).

In conclusion, neither the results nor the technique are completely new. If the article is to be published (I do not know the journal well enough to make a recommendation), it has to be revised to present in a more objective way its actual contribution: new data on the evolution of CPO of ice in natural shear zones, which confirm the current knowledge on the subject: simple shear under high homologous temperature produces a CPO characterized by concentration of [0001] axes normal to the shear plane. Moreover, the discussion should be reinforced and present a comparison of the observations with all available experimental data in simple shear (why focus the comparison on a single set of experiments?) discussing in a more straightforward way the similarities and discrepancies between the different datasets. The rather 'surprising' observations of: (1) lack of a maximum of <a>-axes parallel to the flow direction and (2) the deviation of the [0001] maxima relatively to the normal to the shear plane along the plane normal to the shear direction - should be discussed in a more effective way. The present discussion, although long, does not propose any clear explanation for neither of the two observations.

Additional points: The statements presenting the relation between microphysical processes and CPO evolution in the abstract, introduction, discussion, and conclusion lack precision and give the (false, in my point of view) impression that CPO evolution is mainly controlled by recrystallization (cf. lines 15 & 58-60) or that dynamic recrystallization may completely reset the CPO (cf. lines 30-32 & 434-436). As I see CPO is produced by dislocation glide and recrystallization modifies it, by creating new orientations (most often only dispersion around the orientation of the parent grains) and selectively consuming others when grain growth is effective as it is the case here. The first process certainly buffers the increase in the CPO intensity, but not fully resets the CPO. The second may significantly change the CPO when grain growth is orientation-dependent.

Which are the arguments which justify that low strain rates should enhance dynamic recrystallization and grain growth (l. 434)? I would rather propose the opposite as the forces associated with dislocation density gradients should be smaller at low strain rates.

Referencing is often loose and there are many places where pertinent references are missing. For instance, l. 61, Wenk and Christie (1991) is not the best reference in

a phrase dealing with CPO-induced mechanical anisotropy when there are a large number of studies that investigated precisely this effect (cf. review by Gagliardini et al. 2009 and references therein).

The aims of the article should also be redefined. Those stated in l.79-82 were probably the initial aims of the study, but given the results, they cannot be the aims of the article.

The authors indicate that 8 areas were sampled and that at least two composite sections were made for each of the eight samples. However, in the map only 4 sampling sites are located and data is shown for only 3 samples. Why? Where are the data for SG6-B, which seems from its location in the map to sample a lower strain domain? Data for domains with variable finite strains may help explaining the two unusual features in the observations: (1) the lack of a maximum of <a>-axes parallel to the flow direction and (2) the deviation of the [0001] maxima relatively to the normal to the shear plane along the plane normal to the shear direction.

In l. 294, it is indicated that EBSD work is performed on 40mm x 60mm sections. However, all EBSD maps presented in the article are much smaller (25 x 25 mm on average in Fig. 6a and 3.5 x <3 mm in Fig. 7a). Why use a reduced analysis area in a study where the size of the mapped area is critical?

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