Review of Revised MS “Crystallographic analysis ice on Rhonegletscher, Swiss Alps” by Hellman et al.

General

The paper has been considerably improved in this revised version, with the referees’ comments largely addressed. It is good see some further details on the numerical modeling. It would be useful to add a brief discussion on the implications of using a model based on isotropic ice viscosity for natural ice with anisotropic fabric and thus anisotropic ice viscosity. The significance of this is implicit in the text in lines 206, 207.

There are still a few issues of interpretation of the COF in relation to the states of stress and strain, as I discuss below. But, with the uncertainties involved in knowledge of the states of stress, strain rate and strain in this glacier, there is room for a range of interpretations. Nonetheless, I do basically agree with the conclusions

Specific points keyed to line numbers in the text

Line 1. The centroid does not align with the modeled maximum compressive stress for the deepest sample. Perhaps this exception should be noted in the abstract, and the adverb “approximately” should be placed before “align.”

Line 100. τ is the second invariant of the stress tensor, not τ^{n-1}

Fig. 5. The labels λ_1, λ_2 and λ_3 should also be added to each of this projections, not just the first one.

Line 166. I really don’t like the term “fissure” for these features, since it implies an opening, which is not there.

Line 216. τ_{xz} is the shear stress component, it does not represent simple shear unless the only deformation component in this reference frame is one of base parallel shear strain γ_{xz}.

Table 3. The borehole data on strain rate do not provide much useful information. Clearly the errors are large compared with the signal over the limited time between survey measurements. On a short time frame velocities can vary significantly (as the authors note), and this may explain why the model strain rates are poorly constrained by strain rates derived from the boreholes (especially the shear strain rates). The shear strain appears to be most strongly developed in the deepest ice, with less of a gradient in the top part of the profile than the model predicts. This may be due to the fabric related anisotropy and its effect on the flow law.

Line 235. Kinematics – the pattern of movement - is indirectly related to stresses, but in this case there is a close association. The word “causes” here may not be the best one, since a direct casual relationship is not established. “is associated with” might be better.

Fig. 7. The principal stress directions should be labeled σ_1, σ_2 σ_3 on these plots.

Line 240. Since this is a new paragraph, it’s not clear which observation this refers to. If it’s the relationship between fabric and σ_1 at 79m, this is not what Budd and Jacka (1989) show for multi maxima fabrics. The centroid of the fabric is vertical in both Budd and Jacka’s examples and the ice at 79m, but σ_1 is vertical in Budd and Jacka, but at a strongly inclined angle to vertical for the ice at 79m.

Line 250. The ice c-axes tend to become oriented such that the basal planes are aligned for easy glide (have high Schmid factors), which may not be in the ice flow direction. It depends of course on $\sigma_{1,}$ which in this case lies in the vertical plane following the flow. In the ablation zone, σ_1 would be close to vertical near the surface and the relationship between flow direction and crystals oriented for easy glide would be different.

Line 254. Again, this is not the “simple shear” component, just the shear component.

Line 260-263. This is not strictly simple shear, but it does approach simple shear towards the base. You might say “dominated by the shear component, which approaches simple shear.” The last sentence of this paragraph is good.

Line 266-268. I may disagree here with what you appear to be saying. I believe the single maximum pattern seen in the deep parts of the Antarctic and Greenland ice cores is related to subhorizontal shear strain (close to simple shear) dominating the flow, as first suggested by Gow and Williamson. That is, they are related to shear strain, with the σ_1 direction inclined at some angle to the shear plane by an amount related to the degree of anisotropy of the fabric. I suspect the multimaxima fabrics with clusters arranged about a vertical line in these deep cores have a similar relationship to shear strain and stress that is rather like the situation in your deepest sample. Perhaps this is not in disagreement with you.

Fig. 8. A very nice addition!

Line 296. Not everyone thought that the multiple maxima were artifacts of limited sample size and sampling single grains several times, although this is something Monz et al. suggest may account for many of the early measured multimaxima fabrics. Kamb, in particular, was very careful to overcome this problem by the way he took samples.

I’m not sure what you mean here by “method-immanent.”

Line 299-300. “High strain rate” is a relative term. Strain rates in valley glaciers are typically orders of magnitude smaller than those in most experiments. The key thing that appears to control whether or not multimaxima fabrics develop is temperature. Typical multimaxima fabrics are restricted to “warm” ice, above about -10^{o}. As for strain rate, they appear to form under a wide range of strain rates. Russell Head and Budd (1979), for example, considered they might develop in nearly stagnant ice.

Line 305. Kamb did not produce typical multimaxima fabrics in his 1972 experiments. They were either double maxima, in simple shear, or a small circle girdle in shear plus compression. Also, Kamb noted that fabric development was mostly related to strain and only weakly to stress. Thus, Kamb may not be the best citation here to support your argument.

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