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The Cryosphere
Discussions

Interactive comment on “Direct evidence for radar reflector originating from changes in crystal-orientation fabric” *by* O. Eisen et al.

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One big issue in the review by Robert Jacobel is the extent of the large picture and context of this work. We will try to incorporate related information in the revision, without producing a lengthy manuscript. In the following we respond to this and other issues separately, copying the referee’s comments for easier reference in bold font and setting our response in normal font.

1 Scientific issues / questions

RJ: In looking at the bigger picture of identifying particular RES layers and their origins, it would be worthwhile for readers to see the occurrence of this layer a slightly larger context and in particular to understand how it was identified. Is it the only one without a corresponding peak in conductivity in the whole ice thickness? Was this a serendipitous identification?

The work of Eisen et al. (2006) was started in 2003, when DEP was available to a depth of 1564 m, and in 2004 another 1000 m of DEP data were available to yield a depth of 2565 m (about 30 μs two-way traveltime). COF data was not available at that time. The idea was to reproduce the reflections and locate their origin. The depth range they discussed down to 2200 m depth (about 26 μs two-way traveltime below the surface) already includes the depth section discussed here. The question is answered by Fig.6 in Eisen et al. (2006): Apart from the rough structure of the radargrams in the upper 10 μs related to slight variations in density, and issues of reflection shapes in intermediate depth, the only obvious location where an observed reflector is not reproduced is at 24.1 μs, the one we discuss here. In larger depth we enter the EFZ. As only short pulse data were considered because of their higher quality in terms of resolution and the possibilty to model them, it was suggested by Eisen et al. (2006) that the missing peak is related to shortcomings in the ice-core data in terms of representativeness, as discussed by Wolff and others (2005). Nevertheless, it was stated that the necessary filtering of permittivity removes any signals stemming from COF in the synthetic results. Moreover, the peak seemed relatively small compared to the ones at other depths. With the retrieval of the last 200 m of the ice column in season 2005/06 (no drilling was performed in 2004/05) the full core was available and measurements of COF started also in higher resolution (albeit still coarse) at larger depth. We then again turned to the lowermost region of the ice column, checking other physical parameters, as described here. As now also COF was available, it was evident that DEP-permittivity measurements were too noisy to reproduce the slight variations caused by changes in COF. The discovery is serendipitous in the sense that depths where COF measurements were made in 10 m distances were chosen independently of RES results, but where interesing things in the COF happened. As it became evident that COF influences the short pulse data, we also checked the long-pulse data, as presented here.

RJ: Do the authors see this frequently in RES profiles, or is there something unique about this particular location?
Do you mean that we can often explain reflector origin by changes in COF? If you take the numbers of deep ice cores available to date, then take those with high-quality COF data, and then take those where high-quality DEP is available, you find that not that many places are available where a comparable study could have been carried out. We did not investigate other sites yet, but efforts are under way to do so at EPICA Dome C, Dome Fuji, Berkner Island, and NGRIP. The only special thing about the EPICA DML (EDML) site is that is located near an transient ice divide. But it is not equally distinguished as a true ice divide or a dome with zero flow (maybe it is more interesting just for that reason). We do not expect that COF-reflectors of this type will be observe as often as reflectors from conductivity, which are not limited to certain stress histories or regimes.

RJ: There is also a more specific question that relates to its identification and uniqueness: The last two panels of Figure 3 show at least two additional abrupt changes in the eigenvalues of the crystal orientation tensor. These do not appear to correspond with peaks in the RES record. The absence of strong reflectors at these depths deserves some comment.
The two other changes in COF (1840 and 1970 m) could be real, but they could also be attributed to system noise. Looking at the profile of eigenvalues over the whole depths it seems that a variability of about 0.1 (in λ_i) could occur at all depths. However, as indirectly suggested by Kenichi Matsuoka, the reflector in the long pulse at 1840 m could be caused by that change in COF. Following his suggestions, we think about joining both (vertical and horizontal) panels of COF and dropping the lines in the revised paper. This will make the level of uncertainty more evident.

RJ: The authors point out, p. 8 and 9, that the COF-reflector is quasi parallel to the other internal layers (isochrones based on conductivity changes). This raises a larger question which is certainly glaciologically relevant that the authors should address, if only briefly.

Why do changes in COF appear to follow isochrones; whats the mechanism that produces them? It seems hard to imagine large changes in stress that occur during deposition or even as the firn seals off at a particular depth. Changes in the stress regime after the ice is formed would not seem to recognize isochronal surfaces.

(See also our comment to K. Matsuoka's comment number 9.) In ice sheets, deformation horizons (e.g. the increase of stress with depth) occur more or less parallel to the bed (depending on the ice flow velocity and wavelengths of the bed undulations). That the observed horizon is parallel to isochrones results from the fact that those are parallel to the bed (bed topography will be included in the revision). A possible reason for weaking the ice might be dust, as it is also responsible for the differences in glacial and interglacial crystal size and ice rheology. But we have no evidence yet that this mechanism plays a significant role here. We hope for future ice-flow modelling efforts to answer this question.

RJ: In the discussion of previous work (p. 3, lines 8-23), the question of birefringence and polarization studies is emphasized and the authors state that, "...multi-polarization experiments are required to resolve ambiguities arising from anisotropic reflection and wave propagation in a birefringent medium..." Its not clear to me how the issue of birefringence is relevant to this study since the conclusion is that the echo in question arises from a rheological boundary where fabric changes. I dont see any ambiguities. Without polarization studies, the issue of birefringence can not be addressed in any case, so why is it the theme of this paragraph on previous and related work? It seems the same papers could be cited without the focus on birefringence.

The topic of reflections from COF, anisotropic scattering, and birefringence has been dealt with in several studies by the NIPR (Shuji Fujita, Kenichi Matsuoka, and others) in the last decade. As their approach, so to say, comes from the other side than ours (analysis of polarimetric radar yields physical properties), consideration of birefringence is vital to provide unambiguous results. We thought about a shorter introduction in the beginning, but then reconsidered to give a broader picture of the issue of COF reflections developed in the past. The criticism expressed in the review by Kenichi Matsuoka basically confirms this. However, we agree with your point of view that it is not essential for the main issue of our work and could drop this part, if K.

Matsuoka as the other reviewer and the editor agree.

RJ: However, it is interesting that in one of the references cited there, Fujita et al. [2006] show that Dome Fuji (another dome site, on the face of it, presumably like the DML drill site) is dominated by scattering in a birefringent medium with isotropic boundaries. This would seem to imply that the DML site, unlike Dome Fuji, has seen some changes in the strain regime and the authors would perhaps like to comment.
The flow at Dome Fuji is a classical divide flow. As the flow velocity is very small, stresses are small, too. At EDML, we have flank flow (0.7 m/a) with an additional small component of divergence (see Wesche et al., JGlac., 2007, in press). As evident from the drilling, the ice at EDML is not frozen to the bed, but flow modelling suggests that it is frozen upstream. So the ice at larger depth at EDML has and probably still does experience some shear. We will incorporate this in the revision.

RJ: The final sentence in this section, line 22-23 about previous studies not comparing COF data directly with RES, also leaves me puzzled because Fujita et al. [2006] do in fact compare COF data directly with their radar results. This claim also arises in the conclusion, p. 9, lines 23-24. The authors need to clarify this point. In my view the methods used here do not need to be unique or "first-time" to justify publication.
Our formulation seems to have caused some misunderstanding, as the same point of criticism was raised by Kenichi Matsuoka (his number 22.). We apologize for that. Fujita et al. (2006) deal with radar point measurements and discuss phase shifts and anisotropic reflections, but do not compare profiles of COF with radar reflection horizons. We think that the new part in this paper is the comparison of RES reflections horizons with profiles of COF. Will be clarified in the revision.

2 Minor points about specific text:

RJ: P. 2 line 9 observations allow us to
ok

RJ: P. 2 line 18 drop "from the surface" as it seems to indicate (incorrectly) that the fabric changes are occurring near the surface.
ok

RJ: p. 3 line 24 Moreover we...
ok

RJ: p. 4 line 9 bursts
ok

RJ: p. 4 line 14 In the subsequent analysis, we ...
ok

RJ: p. 6 line 6 pulse widths
ok

RJ: p. 6 line 10 As would be expected, fewer layers ...
ok

RJ: p. 7 line 10 We therefore exclude the possibility that ...
ok

RJ: p. 7 line 16 these reflections do not seem narrow, especially compared to conductivity
changed to "produce the dominant reflections being one pulse widths long."

RJ: reflections p. 8 line 12. Something is wrong with the double equality in the formula. The symbol may be converted improperly in my .pdf version.
$\delta(\varepsilon'_x, \varepsilon'_y, \varepsilon'_z)$: small delta (for variation) times vector ε' with components (x, y, z)
$= \Delta\varepsilon'\,(0.6, 21.1, -20.5)\,10^{-2}$: anisotropy $\Delta\varepsilon' = 0.035$ times vector of observed changes in

eigenvalues
$= (0.2, 7.4, -7.2)10^{-3}$: resulting vector of variation in permittivity.

RJ: p. 9 line 6 drop also
ok

RJ: p. 9 line 7 "...downstream (right) side ..." Designation (left/ right) helps the forgetful reader who does not remember the sense of the flow in the figure from the text description.
ok