The Cryosphere Discuss., 1, S5–S8, 2007
www.the-cryosphere-discuss.net/1/S5/2007/

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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Received and published: 3 July 2007

Review of Eisen et al., The Cryosphere v. 1 by R.W. Jacobel, St. Olaf College Department of Physics jacobel@stolaf.edu

GENERAL COMMENTS:

This is a well-done study and a clearly-written and succinct manuscript that demonstrates the detection of an internal ice reflector arising from changes in crystal orientation fabric (COF) as distinguished from other layers nearby which are caused by conductivity changes. It's strength derives from the combination of data from the two-frequency RES experiment in the vicinity of the DML ice core where fabrics have been measured (though coarsely, at 50 m depth intervals) together with DEP measurements

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which allow the authors to model the reflected waveform based on changes in conductivity. The principal result of the work, being able to identify changes in COF (albeit with the help of a nearby ice core), has implications for understanding ice sheet history, at least in a qualitative sense. It also has the potential for providing constraints to ice sheet models, although the utility of this information in a quantitative application likely needs to await a future generation of models. I recommend publication with minor changes/additions and add that I think this manuscript makes a strong contribution as one of the first papers in the new journal Cryosphere.

SCIENTIFIC ISSUES/ QUESTIONS:

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? Do the authors see this frequently in RES profiles, or is there something unique about this particular location? 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 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; what's 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.

In the discussion of previous work (p. 3, lines 8-23), the question of birefringence

TCD
1, S5–S8, 2007

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..." It's 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 don't 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.

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 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.

MINOR POINTS ABOUT SPECIFIC TEXT:

P. 2 line 9 observations allow us to

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

p. 3 line 24 Moreover we...

p. 4 line 9 bursts

p. 4 line 14 In the subsequent analysis, we ...

TCD
1, S5–S8, 2007

p. 6 line 6 pulse widths

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

p. 7 line 10 We therefore exclude the possibility that ...

p. 7 line 16 these reflections do not seem narrow, especially compared to conductivity 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.

p. 9 line 6 drop also

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.

Interactive comment on The Cryosphere Discuss., 1, 1, 2007.