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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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[General comments]

Eisen et al. delineate radar reflection which can't be replicated with a DEP-based synthetic radargram. Since this modeling accounts only conductivity-based reflection and permittivity-based reflection can be caused only by ice fabric at that depth (over 2000 m), they conclude that that reflection is caused by an alternation of ice fabrics. Thin section analysis of the ice core found that ice-fabric patterns are alternated from vertical girdle to single pole at that depth. This manuscript can constitute an important contribution to The Cryosphere, but some problems mentioned below must be resolved before the publication. This manuscript describes a quite important point in glaciology. Thus, I would like to encourage authors to make more efforts to improve this manuscript. If requested, I am happy to give a review for a revised version of this manuscript.

A major weakness of this paper is a lack of discussion about azimuths of the radar polarization plane. The authors attribute the reflection at 2200 m to a rapid change in the ice fabric. The most right panel of Figure 3 indicates that two eigenvalues are rapidly changed but the other one remains unchanged. It means that the fabric-based reflection can occur only when the radar polarization has a smaller azimuthal angle with the plane that includes the eigenvector showing the rapid change. Because the ice core was retrieved from an ice ridge, c axes are likely aligned in the plane parallel to that ridge. Is the radar polarization parallel to the flight azimuth? Otherwise, this fabric alternation can't make the reflection at all. Eisen's previous paper (2006 in JGlac) showed that they have radar data with various polarization azimuths within 1 km from the drilling site (Figure 2 in their JGlac paper). It provides an opportunity to test a hypothesis that the radar polarization right to the ridge does not detect the reflection. If this hypothesis is correct, it yields strong evidence that the reflection at 2200 m is caused by the anisotropic fabric found in thin section analysis (Figure 4). I would like to suggest the authors to include examination of the radar data with various radar polarizations that were collected nearby the ice-coring site. Otherwise, the title of this manuscript overstates their current findings.

Also, it is shame for me that extensive modeling efforts are not fully acknowledged in the context of this paper. The model outputs can be used 1) to discuss differences in echograms obtained with two pulse widths, 2) to determine possible effects of numerous minor conductivity variations, and 3) to estimate the radio-echo waveform caused by a gradual (but still rapid) change in COF. The latter two allow the authors to delineate COF-based reflections quite nicely rather than making qualitative discussions given in p. 6-7.

[Specific comments]

Sequential number is given through all specific comments.

[Title]

1. Title is, with my opinion, overstated their finding. More data analysis or title modification is necessary (see above).

[Introduction]

2. Together with impacts on ice rheology, lateral variations of the ice fabric can also be used to examine past ice-sheet topography.

3. p. 2 line 23 Harrison (1973) proposed a hypothesis that COF is a major cause of reflection, but at that moment dielectric anisotropy was not measured. Fujita et al. (1993, annals) first reported anisotropy in permittivity at a radar frequency so that Harrison (1973) and Fujita (1993) can be referred together here.

4. p. 3 line 5 state that frequency and temperature dependence of the anisotropy is insignificant and give a possible uncertain range in the anisotropy (rather than referring a single value of 0.035). Experimental errors provided with laboratory measurements and a range of results given at several frequencies/temperatures allow the authors to pin down a range of the anisotropy. It yields to estimate a range of reflectivity caused by the fabric (p. 8).

5. p. 3 line 13 Matsuoka et al. (2003) made multi-polarimetric measurements, but Matsuoka et al. (2004) attributed a sudden disappearance of a high-scattering zone to a azimuthal variations of the radar-polarization plane. Thus, the statement (with such experiments...) is not correct.

6. p. 3 line 21-22 Fujita et al. (2006) examined the dual-frequency radar data in terms of the ice-core fabric. The statement starting with unfortunately does not acknowledge this previous study.

[Data and methods]

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Interactive Comment

7. p. 4 line 12-13 it is unclear for me whether the radar receiver stacks the data coherently (stacked a given number of waveforms and then detected) or incoherently (detected first and then stacked). What is the log compression? There are several inessential statements about the radar system in the context of this paper; it is good to drop off such sentences. It is, instead, important to show a lateral distance that one datum for the further analysis shown in Figure 3 represents for. With my reading, it can be 1300 m (6.5 m x 200 waveforms) or even 13 km (1300 m x 10 traces), but probably it is 650 m (6.5 m x 10 traces).

8. p. 5 line 8 how is the DECOMP results transferred to 150 MHz? What kinds of frequency dispersion are modeled here?

9. p. 5 line 13 why is the time shift of +0.1 usec made? Hilbert transform makes 90o phase shift, and the 0.1-usec shift is equivalent to a half period of 20-MHz radio wave, not 150 MHz. For consistency, use either 0.1 usec or 100 nsec only. Why is the Gaussian smoothing necessary?

10. p. 5 lines 16- is azimuth of each thin section controlled? Otherwise, presentation in Figure 3 includes an implicit assumption that the eigenvectors of the ice fabrics do not alter along the ice core. Also, thin sections collected parallel or perpendicular to the ice-core axis are, in general, neither vertical nor horizontal. This is because the core axis is not always vertical. I think that, if data are unavailable, it is reasonable to assume that eigenvectors have consistent azimuths through the ice core and ignore tilt of the core axis. However, these must be explicitly explained and possible effects of these assumptions must be discussed in the paper. Also, it is not necessary to distinguish whether thin sections are sampled parallel or perpendicular to the core axis (they can be converted straightforwardly).

[Results and discussion]

11. p. 6 line 7-8 "echo-free zone" refers a zone closest to the bedrock which has a sharp decrease of the echo intensity just above it (Fujita et al., 1999; Matsuoka et al.,

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2003). A zone that gives no signal return due to insufficient radar performance is not the echo-free zone. Radar profiles shown in the most left panel in Figure 3 do not show such a sharp decrease of the echo (missing abscissa scale does not allow me to evaluate this point clearly however). And, this statement is out of the main track of this paper. Also, see #25 below.

12. p. 7 line 14-16 authors argue that coherent interference of multiple minor peaks in the conductivity profile can't constitute a significant echo found with the 600-nsec radar data. However, this discussion is quite weak. This is the point that synthetic radar data can clarify. Is it possible to simulate radar echoes from many of minor peaks? It allows the authors to estimate a possible maximum echo intensity caused by these minor peaks so that the cause of significant reflection at 2030 m can be identified more straightforwardly. See the second general comment above.

13. p.7 does the modeling give relative echo intensities of several major reflections that match well with the observations?

14. p.7 line ˜15 it is quite useful if Figure 3 includes synthetic radar data both for 60-nsec and 600-nsec pulse widths.

15. p. 7 line 21 air hydrates have permittivity of ˜2.8 which is much smaller than that of the ice (˜3.2). However, because volume density of air hydrates is quite small, bulk permittivity of the ice and air-hydrate mixture is almost identical with that of the ice. More discussions can be found in Section 5.1 in Matsuoka et al. 2004, J. Glaciol., 50, 382-388.

16. p. 7 bottom line - p.8 lines 1-3. Fabric eigenvalues/vectors measured with horizontal and vertical sections can be converted either way.

17. p. 7 lines 5-10 move some sentences here to Section 2.4 This information is much more important than ice temperature described in that section!

18. p. 7 line 15 why is this approximation (delta ey $>>$ delta ex) necessary? State

clearly that the reflection coefficient (or Fresnel reflectivity) of -64.5 dB is calculated for two-layering strata (rather than for multiple layering that fits with the measured fabric variations). Also, give a short description how -68.8 dB was derived or appropriate references. If this is something that can be done only with this extensive modeling (rather than simple Fresnel estimates), mention it clearly.

19. -64.5 dB was derived for the radar polarization that includes a rapid change in the eigenvalue, right? However, azimuths of the eigenvectors are not known for the fixed space (i.e. north and east), so it is not straightforward to compare this estimate and the radar data. Show reflectivity for the other radar polarization and clarify an azimthal pattern of the reflectivity for an orthogonal coordinate defined by the two (nearly horizontal) eigenvectors. Otherwise, it is not appropriate to say that fabric gives 12 dB larger reflection than conductivity (if the polarization does not align the eigenvectors, it gives reflection smaller than the estimate of -64.5 dB).

20. p. 9 line 10-15 authors attribute lateral variations in the echo intensity as a difference in the pulse length. However, birefringence can also cause such a variation, if the fabric varies over this lateral distance range. My reading of Fig. 2 is that both radargrams show smaller echo intensity in the right side of these panels (distance $>$ 2 km). It is possible to estimate phase differences between two principal wave components (following Fujita et al., 2006). If the phase difference is close to pi/2 near this depth range, it is likely that some small perturbations in the COF cause birefringence that is laterally variable.

21. p. 9 line 17 it is necessary to show bed topography together with the radargrams in Figure 2, if internal layering will be interpreted in terms of the bed topography.

[Conclusions]

22. p. 9 line 23-24 it is incorrect. Fujita et al. (2006) directly compare radar data at the drilling site and COF from the ice core.

23. p. 9 bottom - p.10 top it is unclear for me whether authors argue that COF reflector can be isochrones or not. I can't see an in-depth discussion on this point in other sections of this manuscript.

[Figure 1]

24. Figure 1 needs to include surface (and bed) topography.

[Figure 2]

25. Figure 2 right panel (60-nsec radar data) shows a relatively sharp decrease of the echo intensity at about 2100 m. However, Figure 3 left panel indicates that depth variations in the echo intensity there vary more gradual. Please double check the data used for these two figures.

26. Use an identical color scale for the two panels in Fig. 2, show the color scale used for these panels, and give a radar system uncertainty so that differences in terms of pulse widths can be apparent.

[Figure 3]

27. The residuals (difference between the observation and modeling) can be used as a quantitative proxy of permittivity-based reflection. The 600-nsec-pulse-width radar data show a significant reflection at about 1840 m. This reflection magnitude is similar to the reflection identified as the fabric-based reflection by the authors. What is the cause of this reflection at 1840 m?

28. Fabric from horizontal and vertical thin sections can be shown together in a single panel.

29. It is unclear for me why 0.5 m is applied to identify significant reflections in the synthetic data caused by a sharp conductivity variation. Is this identification sensitive to the choice of depth range (0.5 m)? Anyway, I can't see a clear reason why this pick up was made. Give scale for observed and modeled echograms.

[Figure 4]

30. It is more appropriate to show Schmidt nets at depths close to 2030 m. There are three data points at the beginning, middle and ending of the depth range of a sharp COF variation (most right panel in Fig. 3).

[Technical corrections]

1. p. 3 line 5 remove "i" just before e'_perp.

2. p. 3 line 17 it is unclear what "at the same sites" says.

3. p. 4 line 26 does permittivity in DECOMP refer complex permittivity? A single word permittivity can refer both real part of permittivity (as used at p.4 line 19 for instance) and complex permittivity, so it gets me confused.

4. p. 3 line 19-21 unclear.

5. p. 4 line 4 "2891.7 m" is the elevation of the ice sheet surface measured above the sea level?

6. p. 4 line 8-9 "performance figure" means a dynamic range? If so, "dynamic range" is more appropriate here.

7. p. 4 line 19-21 "a mean at 2100 m" is not clear.

8. p. 7 line 19 epsilon prime here is referred as a complex number, although it was defined as a real part of the complex permittivity (p. 3 line 5).

9. p. 7 bottom line add space between sentences.

10. p. 9 line 3 the longer pulse width returns less number of layers, but it allows us to peer into greater depths. If you refer this phenomenon as noise, it is not a noise issue.

11. It is not necessary to show DEP permittivity.

12. p.12 line 8 and line 10. G. "de". Q. Robin, not G. D. Q. Robin.

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