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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.

O. Eisen et al.

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The review provided by Kenichi Matsuoka is very thorough and fundamental. The referee asks us for further in-depth analyses. In the following we provide a point-by-point response, explaining where we agree with further analyses and where we think that they go beyond the scope of the paper. We copy the referee's sequential comments for easier reference in bold font. Our response is set in normal font.

1 General comments

KM: 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 cant make the reflection at all.

The ice core is unfortunately retrieved without knowledge of the azimuth of the core (see also below). The orientation of the core's xy-plane is therefore ambiguous. The ridge is only weekly defined, and the presence of a splitting ice divide few km upstream of the core causes a flow field which cannot be easily compared to classical ice ridges or divides (Wesche et al., in press, J.Glac., 2007). Therefore the c-axes do not necessarily have to lie in the xz-plane containing the ice divide, although it is very likely. The transmitting and receiving dipoles are oriented along flight direction (Nixdorf et al., 1999). Thus the transmitted em-wave is also polarized in flight direction, or, in the present case, parallel to the ice divide. If the plain containing the c-axes is indeed parallel to the divide, then c-axes and polarization are in the same plane. Because of the birefringent properties of ice there will always be a phase shift in the propagating electromagnetic wave, and therefore a change of the polarization of the em-wave. This implies that the polarization plane at the surface is not necessarily the same as the one at 2 km depth.

KM: 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.
The reflector properties of that profile will be investigated and results incorporated in the revised manuscript.

KM: 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.
The forward modelling is just a tool here. We could indeed perform the same analysis without using the synthetic trace at all, by directly comparing RES data witht the conductivty and COF profiles. We agree that the forward model could be used to that end, which is also possible with the matrix-model presented by Fujita et al., 2006, of which the referee is a coauthor. Investigating all possible experiments that could be performed with the forward model is not the topic of this paper. As discussed in Eisen et al., J.Glac., 2006, we don't know the wavelet exactely because of the rectification of the received signal. We just know its envelop. Experiments with varying pulse length will therefore yield purely synthetic results. The missing knowledge about the shape of the 600 ns wavelet at large depth, the necessary restriction to 1D because of the large spatial comain (3 km in 2 cm resolution), and the missing knowledge about the variation of physical properties within the first Fresnel zone (several meters, compared to the core diameter of 10cm) covered by the long wavelet make it difficult to transfer synthetic results for long wavelets to measured data. Nevertheless, we take this comment as a stimulus to carry out those experiments.

2 Specific comments, p.13ff

KM: 1. Title is, with my opinion, overstated their finding. More data analysis or title modification is necessary (see above).
That depends on the analysis of the azimuth-dependence of the profile, see above.

KM: 2. Together with impacts on ice rheology, lateral variations of the ice fabric can also be used to examine past ice-sheet topography.
Please explain further what you mean.

We consider the ice rheology is a consequence of the dynamic behaviour rather than its dominating cause.

KM: 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.
Albeit correct, that sentence describes the "historic" suggestions more than 30 years ago. In the effort to keep the list of references concise, the work of Fujita (1993) is implicitly included in the Fujita et al. (2000) reference, without depriciating their achievments.

KM: 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).
Do you mean we implicitly state the insignificance of frequency and temperature-dependence? Aiming for conciseness we say that the 0.35 are an approximate value and refer for further details to Fujita et al. (2000), where f- and T-dependences are discussed. No measurements of anisotropy are available at the frequency used in this study (150 MHz). Our estimate is based

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on an extrapolation provided by Fujita et al. (2000). We don't find it useful to pretend the knowledge of an error bar at our frequency despite the lack of measurements which could pin down that error bar.

KM: 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.

Although Matsuoka et al. (2004) did not carry out experiments with varying angles betwee TX and RX, they base their findings also on the multi-polarization experiments by Matsuoka et al. (2003). Nevertheless we will concretize the statement.

KM: 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.

We apologize if this statement has been understood in this way. We did not mean by "Unfortunately" to depreciate the study. In the context of this discussion we would find it helpful if a COF-profile would have been provided in Fujita et al. (2006). Will be adjusted.

KM: 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.

That is subjective. Just providing a reference to the system description (Nixdorf et al., 1999) would require the more interested reader to pick up that paper. Again, we tried to be concise by limiting the system description to the minimum, but find this bit still necessary. The log-amplification refers to the characteristic curve of the amplifier. In contrast to linear amplification, the log-amp. does not amplifie all signal levels by the same factor, but with a reduced factor for large amplitudes. This yields a basically a compression of the dynamic range. The systems receives the data from a single shot (i.e. detects it), rectifies and logarithmically amplifies it, and performs A-D-conversion. 200 of these single shots are then stacked and stored. As the stack acts on the rectified signals, de-stacking does not occur. This stack of 200 traces occurs every 6.5 meters, so every 6.5 m a trace is stored on tape.

KM: 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).
Yes, in this analysis we then stack another 10 traces (each consisting of the 200-fold stack), yielding 65 m trace distance (the trace spacing in Figures 2 and 3). (a typo in the referee report, 6.5 m x 10 traces = 65 m).

KM: 8. p. 5 line 8 how is the DECOMP results transferred to 150 MHz? What kinds of frequency dispersion are modeled here?
Inverse application of DECOMP separates the mixing of the real and imaginary part of the dielectric constant at DEP frequencies. Forward application is then carried out at 150 MHz (see Eisen et al., 2006, for details). No dispersion is considered for conductivity, as for the given frequency and temperature we are still be low the ε'' minimum shown in Fig. 4 of Fujita et al. (2000). A possible effect of the Debye relaxation on permittivity is taken into account by the calibration of traveltime presented by Eisen et al. (2006).

KM: 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?
All is necessary because of the missing knowledge about the truely transmitted wavelet. It has nothing to do with the phase shift of the Hilbert transform. See Eisen et al. (2006) for details.

KM: 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).

It is techncally not yet possible to record the absolute core azimuth in situ, so it is not available for the thin sections. It was tried to log a continuous orientation of the different core sections after retrieval, relative to the first piece of core. However, brittle and broken cores inhibit this orientation to be reliable over the whole depth. Yes, we assume that the small deviation of the core axis from vertical (about 2-3 degrees at most) are neglegible. The accuracy of the fabric analyzer is on the same order of magnitude. Given the width of the distribution of c-axes of some tens of degrees, this assumption is indeed satisfied. Despite the convertability we prefer to present the vertical and horizontal section separately, as at shallower depth (outside the range considered here) a conversion artefact occured, related to the immatureness of automatic fabric analyzer systems.

KM: 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., 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.

The EFZ was observed already in the 50s and 60s, so we use the definition of Robin and Evans that the EFZ is the zone above the bedrock where continuous internal echos cannot be observed anymore, but which is not caused by shortcomings of the system. What is visible in Fig. 2 is indeed the EFZ, not something resulting from insufficient radar performance. This is obvious from the lateral variation in depth of this zone, which basically follows bedrock topography.

We have profiles at other locations with thinner ice where the EFZ occurs as shallow as 1000-1500 m (at 80-90% ice thickness). A published figure is available in Nixdorf et al. (Ann. Glac., 29, 1999, Fig.5). Not mentioning the EFZ here would mean to leave the reader without information why the radargram has almost no signals in the lowermost part.

KM: 12. p. 7 line 14-16 authors argue that coherent interference of multiple minor peaks in the conductivity profile cant 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.
See our general comment above.

KM: 13. p.7 does the modeling give relative echo intensities of several major reflections that match well with the observations?
Yes, see Fig.5 in Eisen et al., 2006. We have very good matching from conductivity in the depth range below 1000 m, with only few exceptions.

KM: 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.
Not feasible, see comment on forward modeling above.

KM: 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.
Right, and as the DC is almost identical to that of ice we refer to it as a "constant" permittivity profile.

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

We nevertheless want to show and discuss them both separately, see above. A horizontal section (across core) is simply not the same as a vertical section (along core), although they can be converted into each other.

KM: 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!
But the discussion of missing data in the DEP profile in section 2.4 has nothing to do with the fabric studies. Or do you mean 2.2 (DEP data)?

KM: 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.
It is not necessary, but justified and convenient here, because we can directly use the value for $\delta\varepsilon'_y$ to calculate the reflection coefficient. But by referring to Paren (1981) it is obvious that we mean a simple boundary or interface. The -68.8 dB follow from using the COF data measured at these two depth, instead of using the values measured at 2025 and 2045 m. We will try to rephrase and clarify this paragraph.

KM: 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).
Yes, that is correct. -64.5 dB is an upper bound. As you mentioned, if the plane of polarization and the plane containing the c-axes are not aligned, we wouldn't see such a large effect, almost

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none in the worst case.

KM: 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.
We don't say that the lateral variations are caused by the different pulse length, only their different appearance at the drill site. We suppose that the reason for the disappearance of the reflector is more complicated that changes in phase differences. However, to move beyond speculation a closer analysis requires further data across this location, also perpendicular to the divide, In this work we demonstrate the origin of an observed continuous internal reflector. Investigating the lateral properties of that reflector is way more difficult and beyond the scope of this paper.

KM: 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.
Ok.

KM: 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.
Do you refer to figure 10 in Fujita et al. (2006, 52, 178, JGlac.)? If not, please provide the exact location. Our statement that "This finding goes beyond previous analysis, which related RES signals to COF, but did not provide a direct comparison of nearby in-situ data of COF" refers to the direct comparison of COF with observed continuous internal reflections. Fujita et al. (2006) deal with radar point measurements. To our understanding, the only place where they perform a direct comparison of radar and COF data from the ice core is figure 10. However, that figure treats anisotropy and phase shift, but does not compare the reflectivity with changes in COF.

KM: 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.
We simply don't know yet! Can you answer the question whether fast changes in COF an isochronous feature in each ice core? From the current understanding COF results from ice dynamics, and is therefore rather unrelated to isochronous behaviour. Even if the COF reflector seems to follow an isochronous distribution, it does not necessarily have to be one. See the discussion of the relation between streamlines and isochrones by Hindmarsh et al. (JGR, 111, F02018, doi:10.1029/2005JF000309, 2006). For an exact analysis you need two ice cores, connected by a COF-reflector, for cross correlation of independent age-depth scales.

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

KM: 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.
Fig. 2 has a linear color scale white-blue-black. Fig. 3 instead displays the data on a log-scale, as indicated on the axis.

KM: 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.
The color scale is optimized for each pulse width to make the internal layers visible. We cannot use the same colorscale for both pulse widths without loosing clarity. The differences in amplitude are evident from Fig.3, the raw data.

KM: 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?
We don't know. It could be conductivity, it could be the small change in COF at this depths, which you might be aiming at. Maybe interference effects, as there is no peak in the 60 ns data. Reasons for unfeasability of 600-ns forward modelling are given above.

KM:

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

KM: 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 cant see a clear reason why this pick up was made. Give scale for observed and modeled echograms.
The majority of internal reflectors were identified by Eisen et al. (2006) to stem from conductivity peaks about 0.5 m wide, see their Fig.6. That's why we use this threshhold. We use a log-scale for all radargrams, which are then shifted to allow accomodation of all traces in the panels in case of the RES data.

KM: 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).
Do you mean to replace the diagramm from 1755 m with one from 2025 m?

KM: 1. p. 3 line 5 remove "i" just before e_perp.
Thanks.

KM: 2. p. 3 line 17 it is unclear what "at the same sites" says.
Where some of the polarization studies discussed in Matsuoka et al. (2003, 2004) were carried out.

KM: 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.
We have the same problem at times because there is no definition. Even conductivity can be treated complex. In DECOMP it indeed refers to a complex permittivity, equivalently used to dielectric constant. Will be rephrased.

KM: 4. p. 3 line 19-21 unclear.
Will be rephrased.

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

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

KM: 7. p. 4 line 19-21 "a mean at 2100 m" is not clear.
That is a standard geophysical term, referring to one of the three velocities used in geophysical processing: interval velocity, root-mean-square velocity, or mean velocity. Mean velocity refers to the mean velocity resulting from the traveltime over whole depth range (in our case 0–2100 m).

KM: 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).
No, we mean the real part. As we excluded conductivity as origin, we also exclude the imaginary part, and are thus left with investigating the real part ε'. Otherwise we could write "the real part of ε".

KM: 9. p. 7 bottom line add space between sentences.
Ok.

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KM: 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.
By noise we don't mean thermal noise. We could also use the term clutter, or speckle, but these are also noise. The structure seen in the 600 ns-pulse in Fig. 2 is less clear than the 60 ns pulse, with less coherence of reflection amplitudes along a single reflector, because of the lower resolution. How would you term the point-like features seen between 20 and 24 us, or the strong variation of amplitude along the conductivity reflector around 25.8 us?

KM: 11. It is not necessary to show DEP permittivity.
Yes, it is, because other readers might ask if a change in COF appears as a change in DEP permittivity.

KM: 12. p.12 line 8 and line 10. G. "de". Q. Robin, not G. D. Q. Robin.
Ok. Bibtex problem.

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