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Interactive comment on "Layer disturbances and the radio-echo free zone in ice sheets" *by* R. Drews et al.

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General comments

The authors compared echo free zone (EFZ) of the radio echo sounding (RES) data with the physical data of a very deep EDML ice core. It is certainly a valuable opportunity to examine directly conditions of deep part of the ice sheet in a flank flow regime. Using a real ice core sample is an advantage and originality of this work. The paper will be very useful to better understand the nature of the echo free zone and also dynamical conditions in the deepest several hundred meters of the ice sheet. However, it seems that many items and discussions are premature in the manuscript. Discussions and presentations have a large room to be improved. In this review I suggest several points for improvement of the paper. A few examples are as follows. (i) Basic definition of the echo free zone: the authors argued a signal drop of only $\sim$3 dB at a depth was a significantly abrupt feature of the EFZ. But this number is small and it seems that our community has not defined any very clear criterion for the EFZ. It is sometimes uneasy to distinguish the EFZ from gradual signal drop into a level lower than the detection limit. Below in the specific comment #15, I suggest an idea to improve this point. (ii) Presentation can be much improved for a better understanding of readers. For example, giving an area map should help readers a lot to understand Figure 1 and dynamical condition near the EDML site. In addition, Figures 1 and 3 are too small to observe features echo free zone and ice core data. Another example is scale of time and depth in Figure 3. When readers compare the RES data with the ice core data, clear indication of depth scale will help a lot. (iii) I suggest that the discussions in the section 3 should be improved. I understand that the authors suggest geometry due to layer disturbance is the primary mechanism of the EFZ. However, discussions for conditions of stratification of the electrical conductivity and crystal orientation fabric are quite insufficient. The author did not examine possible magnitude of the reflection coefficients although ice core data occupy major part of this paper. Are there still boundaries of dielectric properties within the EFZ but did coherent echo disappear simply due to layer geometry? Or, are the boundaries of the dielectric properties already very weak in the EFZ because of the physical conditions? Depending on an answer, a role of the layer disturbance is either primary or secondary of the EFZ. Another possibility is that the disappearance of the dielectric contrasts and the layer disturbance are superposed to create the EFZ.

I suggest a major revision. The radar data and the ice core are excellent. Basic discussions are sound. Thus I believe that the paper will contribute a lot for our understanding if it is appropriately improved. I hope to encourage it.

Specific comments

#1. Page 308 and Line 6 in Abstract.

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Interactive Comment

It seems that “microstructure” is not an appropriate word. The authors used a profile of the dielectric properties, ice fabric and visual stratigraphy measured using a line scanner. I imagine that the authors meant the visual stratigraphy as a tool to see “microstructure”. But microstructure means very small features such as micro-inclusions, bubbles and grain boundaries in ice. These were not discussed in the paper. The authors discussed stratigraphy which has a scale much larger than these. I suggest that the authors describe as follows. “We compare echoes around the EPICA drill site in Dronning Maud Land, Antarctica, with the dielectric properties, crystal orientation fabrics and optical stratigraphy of the EPICA-DML ice core.”

#2. The latter half of the abstract includes major part of results and implications. I felt that in the abstract and in the main text, little is discussed on the role of DEP conductivities and crystal orientation fabrics (COF) at depths between $\sim$2100 m and $\sim$2400 m. Readers will be interested if there are stratified contrast of the DEP conductivities and the COF or not. Layer disturbance is one of important points. But because the authors used detailed ice core data, please examine possible reflection coefficients for a condition without the layer disturbance.

#3. Area map

Because a topic of this paper is closely related to ice dynamics, a map around EDML site, such as Figure 1 in Oerter et al. (2000) (Oerter et al., Ann. Glaciol. 30, 27-34), will help readers. Readers will understand more about glacial dynamics if two radar profiles (022150 and 033137) are indicated on such a map of the ice sheet topography. It seems that the profile 033137 has a special situation in the ice sheet. The profile is toward the mountains that locate as a barrier of the ice flow. If it is the case, readers should be informed of it.

#4. Page 308 and line 19

I suggest that a number ($\sim$>500 m) should be removed because the upper depths of the significant COF-based reflections are variable from one place to another.

#5. Figure 1

Please improve Figure 1. (i) Two panels are too small. Readers cannot observe detailed features of echo. Even echo free structure is unclear in the present state of presentation. The size of each panel can be twice of the present size. (ii) Please add depth scale. Showing only time scale does not help readers' understanding. (iii) The authors can give gray scale in a narrower range, for example from -82 dBm to -92 dBm, to show echo zone and echo free zone very clearly. (iv) In the figure caption, please tell to readers explicitly that the internal layers are expressed using power (dBm) with an explanation of "dBm". (v) It seems to me that the present 3D expression is not very useful. If the area map (see #3) is given appropriately, plain 2D panels seem better to see.

#6. Page 309, line 4

"direct evidence" can be "direct evidence from ice cores".

#7. Page 309 and line 20

Again, "microstructure" does not seem very good word (see #1).

#8. Page 310, lines 14-18

In terms of the ice core data, readability of the present manuscript is not good. In the present manuscript, the authors could easily show a figure like Figure 3 in Eisen et al. (2007). All of necessary data are in the authors' hand. In addition, even if readers see this reference paper, data are shown only to depths of 2200 m. Data from deeper side seems to be unavailable in any publication. For readers, it is disappointing that the results are given only by these four lines of sentence. Moreover, no information is given how these DEP peaks can potentially cause reflections if there is no disturbance of layers. Major focus of this paper is a depth range of 2100-2400 m. Please show readers necessary data clearly.

#9. Page 310, lines 19-25

Here again presentation for the ice core data needs to be much improved. The author could show profiles of eigenvalues in a depth range of 1800-2400 m. How much is the variability of the eigenvalues in each direction? How much power reflection coefficient that the COF can potentially cause? How much is the variability of tilt angles for the principal axes? The authors could show such in formation easily. It seems to me that present manuscript did not give anything new in terms of COF and its possible effect to the radio echoes.

#10. Page 311, lines 6-12

Here again, presentation for the ice core data is insufficient. Ideally, I hope to see more examples of the line scanning data, for example, every 100 m or something like that. The authors could quantify the smoothness, degree of parallel, dip angles and variability of dip angles. The authors could show such data even if it is preliminary and rough analysis. The authors described most of items with several lines of sentences in a vague manner. Please just show graphs indicating how features of strata evolve with increasing depth. It is the best way to make readers understand. By the way, dip (∼15 degrees) seems important because the center of the reflected wave will not come back to the radar. There seems no discussion on this point in the manuscript.

#11. Page 311, lines 13-16

Presentation for the crystal grain has the same problem. Please just show us a graph, which tells us very clearly conditions of the ice sheet. With the present manuscript, readers need to make a kind of graph by their own, to digest what is going on in the ice sheet as a function of depth. Indeed, I needed to do it to understand. Even after making such a graph they are insufficient to estimate something (such as power reflection coefficient) quantitatively.

#12. Page 311, lines 20-23

Specification of the radar system is very important. Missing information is specification of antennas, in particular, type, gain and beam-width in E-plane and in H-plane. They are important related to the dip angles of the layers. I believe that the specification of the radar system can be much better presented using a table. Considering the antenna beam-width and the dip angle 10-15 degrees, please estimate how much dB can signal level drop compared with the layers with 0 dip angles.

#13. ID numbers of profiles and traces

Eisen et al. (2007) gave trace “4205” for the profile 022150. It is unclear to me what the difference between the trace “4005” and the trace “4205” is. Are they both representative of the EDML coring site? Why did the authors choose the different trace?

#14. Page 312, line 6 and after

The authors gave a statement “Small signals sometimes appear within the EFZ but these are laterally usually not continuous”. Is this authors’ observational fact? If it is the case, please demonstrate it in the improved Figure 1.

#15. Page 312, line 10 and after

It seems a bit difficult to claim “At EDML, the EFZ is less pronounced but still evident” because signal drop is small at the trace No. 4005. At line 13, the authors claim that the signal drop is ~4dB. But this number seems exaggeration for ~3 dB or less in reality. It does not mean much to discuss how much dB step is sufficient to claim as an evident feature of EFZ. If we do not clarify this point, readers may feel unclear for the definition of the EFZ.

I propose the authors to give a new graph showing observed power reflection coefficient (PRC) and theoretical detection limit (e.g., Figure 2 in Millar (1981) Nature, 292 (30), P. 442). The authors have sufficient ice core data to estimate the theoretical detection limit and the observed PRC. Then, the authors can demonstrate that minimum level of PRC that is really absent within the EFZ. For example the authors may claim that “PRC is less than -85 dB. It is much less than the PRC, -75 dB at depth of 2000 m". An alternative (but similar) idea is to add a theoretical curve in Figure 3 showing a profile of the echo from -70, -80 and -90 dB of the internal reflections. Then readers can see how small real signals are in comparison to the expected (theoretical) signal level. The theoretical curve is simply for a flat undisturbed surface and for a condition without birefringence effect. Lack of the significant reflectors and the possible layer disturbance are discussions after that.

#16. Page 312, lines 13-15

Please demonstrate features of the "weak, frayed, discontinuous, and partly incoherent signal" as real data. The authors can do it easily using a zoom and proper gray scale for Figure 1.

#17. Page 312, line 15

I felt hard to find what 2375 m and 2385 m meant. It required some time to understand 2375 m was mentioned twice in the manuscript and the 2385 m is first appearance here. I believe that lack of proper graphs or diagrams (see comments #8, #9, #10 and #11) is causing such poor readability.

#18. General comment for the section 3 (and for Page 312, lines 18-19)

Please examine three conditions: (i) existence (or absence) of dielectric contrast, (ii) existence (or absence) of undisturbed reflecting surfaces to cause detectable signal, and (iii) minimum signal level detectable by the radar at each depth. The authors discussed almost only (ii). Missing in this manuscript is discussions for (i) and (iii).

#19. First paragraph of the section 3

I did not understand what the authors meant "the long pulse has not reached its detection limit" (Page 312, bottom line). It seems that such a discussion can be done only after examinations commented at #15. In case of the Figure 3c, the deepest reflection peak at $\sim$24 micro-second in the 600 ns profile is larger than the noise level only by $\sim$3 dB or less. If we rely on the logic of the authors, we need to discuss such a very small number as the EFZ. It seems to me a simultaneous disappearance does not mean much.

#20. Page 313, lines 5-10

Can the authors trace the onset of the EFZ in the improved Figure 1 or in a separate figure? Such a demonstration will help to understand what is described here.

#21. Page 313, lines 10-14

Isotherm hypothesis has two aspects. One point is attenuation, which the authors discussed in the manuscript. Another point is a kind of iso- "mechanical properties" because ice viscosity is a function of temperature. To my knowledge there has been no such a discussion in publication. But to a discussion of EFZ in Fujita et al. (1999) JGR paper, Dr. Paul Duval in LGGE France gave a comment about that. An assumption is that the layers tend to be disturbed when ice temperature reaches close to melting point ($\sim$-10 degrees C) where softness of ice reaches some critical point. Indeed, many of physical properties of ice (for example, mechanical properties and diffusion coefficient) have transition of activation energy at $\sim$10 degrees C. I believe that we should still keep in mind this possibility.

#22. Page 313, line 19 – page 314, line 6

Please suggest readers possible reflection coefficient at depth below $\sim$2200 m compared with shallower reflections. Are expected reflections well below detection limit? How about a relation between conductivity boundaries and the reflections 2375 m? If the 2375 m is a boundary between glacial and interglacial ice, there may be a step of the electrical conductivity. I ask these questions because I hope to know about existence (or not) of the reflection boundaries (that is, contrasts of dielectric properties). If the reflection boundaries are absent, then the layer disturbance can be interpreted just an additional (secondary) cause of the EFZ. If the reflection boundaries are still significant, the layer disturbance seems to be an essential cause of the EFZ breaking the coherency of the reflected wave. Present manuscript did not make these points clear. Rather, present state of argument seems qualitative.

#23. Page 315, line 1

“A change in a borehole geometry at 2385 m depth”. This description is a sudden appearance in the paper. Citation?

#24. Page 315, lines 5-6

“It seems to be stronger developed in the flank-flow regions than the regions closer to the dome position.” At the summit of Dome F, I found only gradual decrease of the radar signals into a level of detection limit. Abrupt drop appear only when we are away from the dome.

#25. Page 315, lines 26 -

When the authors say “EFZ is an excellent indicator of internal stratigraphy”, it “EFZ is an excellent indicator of internal stratigraphy”, it is more correct to say “EFZ is an excellent indicator of disturbance for internal stratigraphy”.

Technical corrections

Reference paper Eisen and others (2006) needs to be corrected. It is Vol. 52, No. 177.

Interactive comment on The Cryosphere Discuss., 3, 307, 2009.