

Interactive comment on “Radio-frequency probes of Antarctic ice birefringence at South Pole vs. East Antarctica; evidence for a changing ice fabric” by D. Besson et al.

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Received and published: 2 January 2013

GENERAL COMMENTS

Besson et al present a detailed analysis of a radio-wave dataset, and investigate it for azimuthal dependencies in amplitude and velocity. Whilst the data show some evidence of azimuthal variation, I find it very difficult to assess the significance of these observations because the paper is not well-structured. There is almost an ‘imbalance’ between the number of figures and the discussion that is allocated to each – as such, I am left with the impression that there are a lot of observations here but I find the interpretation difficult to extract.

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This could, in part, be a structural issue (for example, I find it distracting that the map of the acquisition set-up appears half-way through the results rather than in a more introductory passage), but I also notice that the focus of the paper seems to shift and that the results at some points appear to contradict each other. With regards the focus, the main part of the introduction focuses on three mechanisms of producing englacial reflectors within an ice sheet, when it seems (given the title of the paper) that a discussion of the origins and magnitude of azimuthal effects on velocity and amplitude would be more appropriate. With regards the apparent contradiction, the authors exclude birefringence on Page 4700 (and in the abstract) but then invoke it to explain a series of observations on Page 4705. If these are separate effects then much more explanation of how they arise is required. I guess you mean that birefringence is present in the lower part of the ice sheet, but not in the upper? The problem is, I’m having to guess, and the paper should leave no ambiguity.

I’m also not sure that all the material in the paper is relevant; for example, the comparison of CRESIS vs. BEDMAP depth estimates does not sit comfortably within the scope of the paper. Furthermore, the outlook for further work is also not relevant to the subject. Removing these paragraphs would instantly sharpen the focus of the paper; either this, or their relevance must be stated for the work that is performed here.

Finally, given the title of the manuscript, I’d have expected a comparison of data from two different sources but only one is shown, and is compared descriptively to experiences on the East Antarctic. Obviously, a comparison between any two data sources can be made, but I think here that the East Antarctic reference should be removed from the title of the paper.

SPECIFIC COMMENTS

Introduction: As stated in the general comments, I think that the introduction could be usefully revised to focus specifically on the azimuthal dependence of radar-relevant properties. At the moment, it sets the paper up for an analysis of different ways of

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generating reflections, rather than different ways of introducing birefringence (and their glaciological significance). I also think that the description of the MAPO site (currently contained in Figure 9 and introduced in Section 3) could be moved to the introduction, or a 'site description' section included thereafter. Such a section would also be the place to explain the wider relevance of the particular study, and potentially what was anticipated prior to this acquisition.

Section 2.3 (and Figures 4-6): The authors compare the time domain characteristics of the wavelets identified in earlier figures, and comment on their frequency content. I don't agree that the shallowest wavelet has a greater proportion of lower frequencies – it just looks to be a more complex, ringier pulse (potentially reflected from a series of closely-spaced layers). Nonetheless, it strikes me that the more effective way to analyse the frequency content of a series of wavelets is by plotting their respective amplitude spectra. Not only is this the more appropriate analysis to perform, it would also remove the need to apply low/high pass filtering either side of the 500 MHz cut-off (incidentally, how was this cut-off chosen, and how was the filter designed?), which itself could have distorted the waveforms.

I'm not sure, however, what conclusion the others are deriving from this analysis. Is the suggestion that the reflectors are of COF type, given the absence of any frequency-dependant effects? If this is so, why is a frequency-dependant effect invoked to explain the enrichment of low-frequencies in the shallowest reflection?

Section 2.4: I'm not familiar with the terminology 'attenuation length'; is it something like the skin-depth, where the distance for amplitude to drop by some amount is calculated? If so, how certain are you that reflection coefficients are uniform at each boundary? The reflectors are certainly of different character, since the authors note a) frequency differences between the pulses, and b) some reflectors are actually a series of boundaries (e.g., for the 9.6 microsecond event). Regardless, the main conclusion that is drawn from this analysis only confirms that attenuation length decreases with depth, as expected from increasing temperatures. I therefore don't see why this anal-

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ysis is relevant to the overall aim of the paper and would suggest that it is removed. If I've missed a more 'meaty' conclusion, that relates to the three factors mentioned at the start of this section, then it should be more clearly stated.

Section 2.5: It's interesting to note that there is no amplitude variation with azimuth for the shallowest and deepest reflections observed. Does this suggest that these boundaries correspond to something else, other than a change in COF?

Section 3: As stated previously, the description of the site seems like some quite introductory material that is presented after the discussion of results, and therefore is in the wrong place in the paper. However, the rest of this paragraph would benefit from some extended discussion since it is here where the apparent ambiguity about the presence (or otherwise) of birefringence occurs – yet this analysis occupies just 6 lines of text. Perhaps there is an over-reliance on the lead author's previous publication; either way, despite it being present in the literature, I think it is worth explaining the significance of the 50 ns lag time here.

Section 3.1: Although this analysis is interesting, I'm not sure what relevance it has to the aims of this paper, and it seems that it just adds length to the paper to the detriment of its focus. The authors should consider removing this from the paper, or clarifying why this is relevant to the aims stipulated in the introduction. In any case, the +/- 15 m depth accuracy that is recorded is, in any case, comparable to accuracies observed by other authors using CRESIS data (e.g., +/-10 m, in comparison to ice core data, from Oswald and Gogineni, 'Recovery of subglacial water extent from Greenland radar survey data', *Journal of Glaciology*, 54 (184), 94-106).

Section 3.2: While it's good to see an outlook from this work, I think that this section also 'muddies the waters' a little and distracts from the overall aim of this paper. I would also consider removing this paragraph (or potentially abbreviating it, and adding it as a note in introductory comments).

Conclusions: I think it's ok to compare observations from two different sites (i.e., South

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pole and East Antarctica), but there should also be an explanation of why these differences occur (although I don't think that the concluding section is the place to do this). It could also be inappropriate to make general comments about the state of the ice sheet (e.g., P4707 L20-22) from two sets of observations that show different results.

P4708 L10-19: I'm a little confused here, as the statements in this part of the conclusion seem to contradict other parts of the text which have stated that there is no birefringence. Does the conclusion instead refer to the observations made in Section 3?

I think the concluding remarks as a whole would benefit from sharpening up, but this may happen when the necessary changes are made to the focus of the introduction.

TECHINCAL CORRECTIONS

P4697 L8 – the index should be inside the parenthesis, or another pair of parentheses are required to enclose the expression.

P4698 L15 – it is not the birefringence that is well-established in the ice sheet, it is 'observations of birefringence'.

P4699 L7 – 'reasonably good transmission' – reasonable compared to what? What is the significance of VSWR?

P4699 L24 – isn't 'GigaSamples s-1' the same as 'GHz'? It's just that GHz is a more elegant unit.

P4700 L7 – I'm not sure you spell out the "CW" acronym.

P4702 L11 – I think it is worth including the Friis equation here.

P4702 L21 vs. P4706 L11 – how does the velocity you use compare with the index of refraction?

P4702 L26 – It seems that you maybe shouldn't include the 17 microsecond reflection

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in the azimuthal analysis, since only 4 orientations are deemed to be noise-free.

P4709 L9 – I'm not sure that the Abassi reference is acceptable here, given that it is only in the submission stage. Can this be updated to accepted yet?

P4703 L26 – it might also be worth including Fig 8 from the previous paper here, for comparison. It strikes me that many glaciologists might not have access to this journal, but there's some quite important material that is referenced from it.

P4707 – I'm not sure that the bullet-pointed list assists with the clarity of the concluding comments, partly because each entry in the list is quite long, and I've forgotten what the 'lead in' to the list was by the time I get to the next paragraph.

Figures 2, 3, 4, 5, 6, 11, 13 – the y-axis in these plots is inappropriate. I'm aware that you just want to show the magnitude of each event, but labelling the axes in this way is confusing. Maybe remove the axis labels altogether, and just have the equivalent of a 'scale bar' ?

Figure 7 – Greek letter mu should appear in the legend, rather than 'u'. Explain the arrow at ~ 150 degrees in the figure caption, rather than in the figure itself (as with Figure 8).

Figure 17 – This is a polar plot, but it is not plotted in a polar display. Please fix this as I find the plot very difficult to interpret in its current state.

Interactive comment on The Cryosphere Discuss., 6, 4695, 2012.

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