Response to Reviewer #3

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.

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, Im having to guess, and the paper should leave no ambiguity.

We apologize for the confusion here. To hopefully help emphasize the point, we have elevated this observation to the abstract, which now closes with the sentence: ''Combined with other radio echo sounding data taken at South Pole, we conclude that observed birefringent asymmetries at that locale are generated entirely in the lower half of the ice sheet.''

Im 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.

As per reviewer #5's comments, as well, we have, indeed, excised this text in favor of reporting it separately elsewhere.

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.

Assuming this refers to section 3.2 of the posted manuscript, as mentioned above, that section has now been stricken.

Finally, given the title of the manuscript, Id 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.

Comparison is, we believe, complicated by the difference in the data acquisition systems used in the two cases. In our case, we observe signals directly in the time domain; for East Antarctica, birefringence is observed as a consequence of an interference effect. The data do, therefore, not simply overlay.

We note that the comparison to East Antarctica has been de-weighted in the new text.

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.

Yes, that mention has now been removed from the title.

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 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.

We understand that the reviewer is suggesting a logical organization with the following scheme: a) We set out to measure azimuthal variation of radio reflections, b) we observe variations, and c) we speculate on implications for such phenomena as birefringence. This would seem, indeed, to be a self-consistent and reasonable presentation outline. We have chosen (arguably somewhat arbitrarily) a different presentation strategy, which follows more closely our motivation for performing this study. Namely, given the observations of birefringence in the upper half of the East Antarctic ice sheet, we expected to observe a similar effect, albeit with a considerably different data acquisition system, at South Pole. We did not.

Given that no other reviewers suggested such a reorganization we prefer, with the indulgence of the reviewer, we prefer to retain the current approach.

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 dont 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).

We did, in fact, consider the reviewer's model of multiple reflecting layers, although it seemed to us it must be more complicated than that. The 20 ns duration of the observed reflections corresponds to approximately 2 meters of ice depth (round trip), which, using the canonical value of 10 cm/year accumulation at South Pole, implies events producing multiple layering within a time period of approximately 20 years. Possible, certainly, although it should also be noted that such a distended waveform should also have an asymmetric shape, assuming we are seeing an overlap between two very close reflections. Although it is true that the reflections are 'ragged', to us, the overall symmetry of the observed waveforms, to us, suggests reflection off a monolayer.

In deference to the reviewer, in any case, we now make this argument explicit in the text itself, so that the reader may at least consider the reviewer's argument.

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.

Reviewer #5 also commented on this, suggesting windowing on the observed reflections and carrying out a direct Fourier analysis. In fact, we performed such an analysis, and show a sample plot in the current text, which we believe supports the 1/f interpretation.

If the reviewer is interested, we also provide below a comparison of the frequency spectrum of the received signals, at various times, with the frequency spectrum of signal taken 'outside' the nominal signal interval. The general trends are, we believe, uniform.

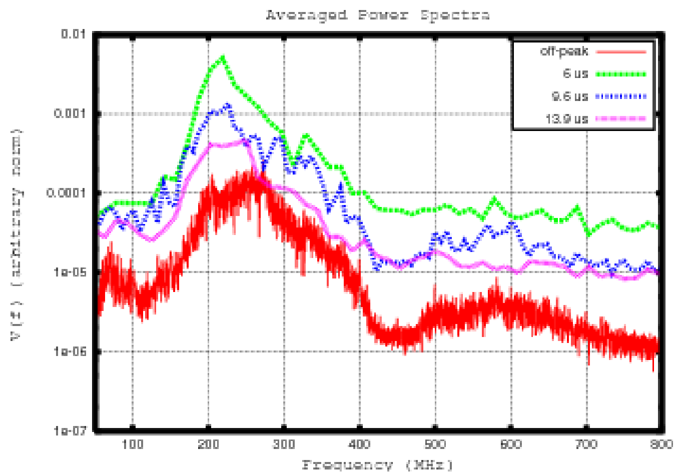

FIG. 1: Power spectra, isolated for indicated echo returns, compared to samples taken outside peak regions.

Im 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?

This was admittedly very 'soft' in the existing text. The fact that the reflections are 'sharp' in the time domain, and that COF re-alignment typically occurs over several meters argues for conducting reflections, actually argues for conductive scattering.

Section 2.4: Im 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?

Yes, perhaps this is a convention that is less familiar in the electrical engineering community, which perhaps includes the reviewer. Whereas skin depth is a term perhaps most often applied to metals, attenuation length is typically used to describe signal amplitude loss over macroscopic lengths (http://en.wikipedia.org/wiki/Attenuation_length, e.g.)

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 dont see why this analysis is relevant to the overall aim of the paper and would suggest that it is removed. If Ive 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.

Our purpose here was to cross-check how different the characteristics of the reflecting internal layers might be. If we had started with the assumption of equal reflectivity, and instead derived an attenuation length which was growing with depth, in contrast to what is known about the attenuation length (that it must shorten with temperature, which, in this case is a proxy for depth), we would have concluded that the reflectivity of the deeper layers is likely greater than the reflectivity of the shallower layers.

We have tried to rationalize this, as per above, with the following text:

''The observed echo amplitudes shown in the Figure are largely determined by three factors: the intrinsic reflectivity of each layer, the diminution of signal strength with distance, and attenuation of the signal due to continuous ice absorption.

We can test the assumption that the scattering mechanisms for all scattering layers are the same by investigating whether the amplitude dependence of the observed reflections is consistent with the known warming of ice with depth, and the reduction in ice attenuation length, at radio frequencies, with increasing temperature...''

Hopefully, this is an improvement...

Section 2.5: Its 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?

Our interpretation (hopefully more cogently made above) is that all the reflectors are of the conductivity-type.

We will sidestep addressing questions related to Section 3 below, since, as stated earlier, it has been dropped from the paper.

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 authors 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, Im 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 its 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).

As mentioned previously, what was formerly section 3 has now been removed.

Conclusions: I think its ok to compare observations from two different sites (i.e., Southpole and East Antarctica), but there should also be an explanation of why these differences occur (although I dont 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: Im 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?*

Yes, the tagline is, basically: in East Antarctica, birefringence is observed in the upper half of the ice sheet. At South Pole, we exclude birefringent effects in the upper half of the ice sheet, and only observe this in the lower half of the ice sheet.

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.

Yes, hopefully the revised text is, indeed, 'sharper'.

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

Thank you; an oversight on our part.

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

Hmmm. Although we appreciate the reviewers' comment, we're not sure that 'observations' can be 'well-established'. Perhaps the revised text: ''Although several positive observations of birefringence in the ice sheet have now been made (Table ??)'' is acceptable to the reviewer?

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

We apologize if this was unclear; we had (admittedly, incorrectly) assumed that this terminology would be familiar to the Cryosphere's readership.

The revised text now reads: ''These antennas have good transmission characteristics, from 60 MHz up to 1300 MHz, as indicated by their Voltage Standing Wave Ratio (VSWR) specifications. The VSWR represents a measure of the fraction of signal delivered at the input port of an antenna which is broadcast into the environment, with a value of 1.0 representing 100% power transmission efficiency, and a value of 3.0 corresponding to 75% power transmission efficiency.''

P4699 L24 isnt GigaSamples s-1 the same as GHz? Its just that GHz is a more elegant unit.

The reviewer is correct, although digital oscilloscopes of the type used in our experiment are typically described in terms of GigaSamples s^{-1}, to make explicitly clear that this specifically refers to a rate at which some analog signal is sampled. The admittedly more compact ''GHz'', perhaps, can refer to any event with a frequency of 1 billion times per second.

P4700 L7 Im not sure you spell out the CW acronym.

It does, indeed, appear that we had omitted this. Our error.

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

Now explicitly included, thank you.

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

The ''bulk'' velocity of 169 m/microsecond directly translates into an implied index-of-refraction of 1.774 (hopefully we understood the reviewer's comment correctly here.)

P4702 L26 It seems that you maybe shouldnt include the 17 microsecond reflection the azimuthal analysis, since only 4 orientations are deemed to be noise-free.

We believe this is a judgment call, and prefer to leave it in at this point.

P4709 L9 Im 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?

Unfortunately, not quite yet. As such, it has now been removed from the text.

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 theres some quite important material that is referenced from it.

Hmmm. Perhaps we're not understanding, but Figure 8 from the Astroparticle Physics paper indicates the same lack of birefringence in the upper half of the ice sheet that we demonstrate in this paper; the advantage of including that Figure is therefore not fully clear to us. Nevertheless, we now include the online link (http://xxx.lanl.gov/pdf/1005.4589), which is freely accessible, for persons who may be interested.

P4707 Im 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 Ive forgotten what the lead in to the list was by the time I get to the next paragraph.

Yes, this has been considerably amended. The new ''conclusions:'' text reads:

''We have observed azimuthal correlations of echo returns with ice flow direction at South Pole, which we summarize as follows:

1. Although previous probes of the ice sheet in East Antarctica[?] were reported as birefringence in the upper half of the ice sheet, we observe birefringent effects exclusively in the lower half of the ice sheet. Both experiments observe correlations of birefringence with the ice flow direction.

2. In the ''standard'' picture, if the c-axis is exactly vertical, and the wavespeed asymmetry is different only for propagation parallel (i.e., along z) vs. perpendicular (horizontal) to the crystal stacking axis (i.e., $\hat{c}$), then the wavespeed is uniform for all directions in the horizontal plane and there is no expected birefringence as a function of azimuthal orientation. However, our results imply an asymmetry for azimuthal propagation along vs. perpendicular to the ice flow direction, in contrast to the laboratory measurements for single crystals, which would have implied azimuthal symmetry. Our results *are* consistent with a vertical girdle average orientation at depths greater than $\sim$1200 m, although ice core analysis indicates that the ice should be increasingly uniaxial at these depths.''

We hope the reviewer feels this is an improvement.

Figures 2, 3, 4, 5, 6, 11, 13 the y-axis in these plots is inappropriate. Im 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 ?

In addition to following the reviewer's suggestion, and removing the values of voltages indicated in the vertical scale, we have also tried to address this with an additional caveat in the caption: ‘‘Ensemble of echo amplitudes observed as a function of azimuthal orientation, for both co-polarized and cross-polarized broadcast signals, for echo times between 5 μs and 14 μs. A DC offset has been added vertically to successive voltage traces for visual clarity; alternate waveforms are similarly also offset by $\pm$100 ns along horizontal. Each division vertically corresponds to 10 mV.’’

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).

We have amended the figure, as well as the captions, as per the reviewer’s wishes.

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.

As mentioned above, this figure, as part of section 3, has now been removed.