Response to Reviewer #2

Radio-frequency probes of Antarctic ice birefringence at South Pole vs. East Antarctica; evidence for a changing ice fabric by Besson et al. Besson and coauthors studied propagation of radio waves reflected off the internal reflectors at a site at South Pole, as a function of polarization axis in the horizontal plane and oblique radio wave scattering. The authors performed various attempts using their own radar system using a kind of monopulse radar that use wide range of frequency spectrum. The authors' main points include (i) findings of several major internal reflectors at timing lower than 19 uS, (ii) a difference in birefringence between South Pole and Dome Fuji, (iii) orientation dependence of reflection amplitude at some reflectors, and (iv) "most precise determination of the ice thickness at South Pole". The title of the paper reflects the authors main claim ice fabric is different between South Pole and Dome Fuji. According to the authors' affiliations and according to their earlier publications, the authors' main research field include neutrino and neutrino detection. In the light of knowledge of glaciology and ice physics, frankly, I have several major concerns in the claims, description, ways of presentation in the TCD paper. I believe that my comments written below in this review is more or less common for the other glaciologists and ice physics researchers who know the related subject. One of my major concern is focus of this paper. As the title says, ice fabric is different between two sites with different dynamical condition. Such phenomena (difference) is very natural. In terms of science, the paper will be more or less interesting if the paper discuss concretely how is the dynamical condition and depositional condition at two sites and their impact to the COF formation. Present authors just tell to readers that the results are contrasted at two sites. This condition is unsatisfactory (or insufficient) for the readers to really understand what is going on in polycrystalline ice at these two sites. In the present manuscript, a variety of topics are included, such as radio wave scattering mechanisms, birefringence, attenuation, oblique propagation experiments, precise determination of ice thickness, and burial of radio sensors at the South Pole. However, to my view, such various topics does not necessarily mean that the paper contain sufficient (or rich) new results, applications or new theoretical developments of interest. For example, claims (i) (iv) above in the abstract is not particularly new. The authors detected that radio wave propagation depended weakly on the polarization plane of the wave.

Well, in short, we disagree. Although the reviewer is almost certainly much more well-versed than ourselves in this field, the reviewer's re-statement and re-interpretation of the main points of the paper draft, we feel, misses the essential content. We will use the reviewer's re-statements below for reference:

(i && iii) correct, however, this is an oversimplification. What the reviewer omits in her/his summary is that a) we observe, for the first time, a direct correlation with ice flow direction in the amplitude of the associated returns (the amplitude correlation observed at Dome Fuji was attributed to interference effects), and b) we additionally use that information to verify, for the first time radiometrically, the expected warming of the ice sheet with depth.

ii) again, we believe this is an oversimplification. Birefringence observed at Dome Fuji was claimed to be present in the firn and below. We contradict that result; our birefringence is only observed in the lower half of the ice sheet. The reviewer states that there is no reason why the ice sheet should be so similar between the two sites; this is a fair criticism, and we have de-emphasized this comparison in our revised draft. In fairness, however, we note that Bay *et al* have recently mapped the internal reflectors observed at Dome C directly to those observed at South Pole.

We also stress that birefringence at Dome Fuji was NOT directly observed; it was only inferred from an interference pattern which could have also been due to multipath surface effects, owing to the long integration time of that apparatus. This is the first direct observation of birefringence using a direct time delay measurement.

The reviewer requests more details about the ice mechanisms at South Pole which are responsible for the effects we observe herein. Absent a direct core, we do not have such a detailed model. A recently submitted proposal to drill a core to approximately 1500 meters at South Pole has just been approved; when available (2014?), we plan on using those core data to investigate the correlation between the molecular ice details and the radiometric data we have collected.

A first question then arose to me was meaning of the observation in science.

Response: With apologies, this first question is unclear to us. Perhaps the reviewer can elaborate on what is meant by: ''the meaning of the observation in science''? Taken at face value, it seems the reviewer is posing an ontological question regarding what the nature of ''observation'' is, and how various scientists interpret ''observation''. Is that what the reviewer is asking?

A usual crystal orientation fabric (COF) in the flowing part of the ice sheet is single pole because of vertical compression or simple shearing. With a single pole COF, it is natural that orientation dependence of the wave speed is weak.

We suspect the reviewer intended to make a more specific statement than the one above. If there is a single pole COF in the vertical, then, based on lab measurements, there should be a strong asymmetry in the wavespeed for vertical vs. horizontal propagation.

We understand that, what the reviewer intended to say was:

''With a single pole COF corresponding to vertical alignment of the c-axis, there should be only weak azimuthal dependence of the wave speed.''

Hopefully, the reviewer might correct us.

It seems to me that the authors show an example of such weak orientation dependence, that is, a natural consequence of COF in the sheet flow. In horizontal plane, orientation dependence of the wave speed appears when ice flows in convergent or divergent manner because of elliptic distribution of the single pole fabric. Earlier papers have discussed such conditions, often using real COF data. But present paper contains no discussions on the real COF or dynamical conditions at the South Pole.

The reviewer is correct that our results do match up with a model of an elliptical single pole fabric distribution. However, there is no direct measurement of COF at the South Pole, nor have there been any other measurements which explore the amplitude dependence of the azimuthal variation, as we have. Previous measurements based on interferometry have not had the resolution to deduce amplitude effects.

The dynamical conditions are, indeed, as the reviewer realize, tightly connected to either the convergence or divergence of the flow, as well as the ice history. We now comment on this explicitly in the text.

Present paper says that South Pole condition is different from earlier results of Dome Fuji, using an expression of "dramatic difference". Dome Fuji is always a kind of comparison target. But glaciology-based scientists who know ice fabric and ice dynamics will simply think that difference in COF is natural consequence of dynamical conditions between ice divide zones and flowing ice sheet. In addition, Dome Fuji is not any representative location in East Antarctica. It is just a site along the ice divide zone. Frankly, it seems inappropriate or unhealthy to see "South Pole vs. Dome Fuji". They are just two locations among various dynamical conditions.

Perhaps a gentler way of positing the question is whether the ice sheet is monolithic. We are aware of two studies which have compared ice properties at spatially disparate sites: a) Matsuoka et al have used 60-MHz and 190-MHz sounding equipment in both East Antarctica as well as West Antarctica. They observe clear similarities between the two sites:, b) Bay et al have recently mapped the internal reflectors observed at Dome C directly to those observed at South Pole.

In response to the reviewer's comments, as well as those from other reviewers, we have de-emphasized the comparison to East Antarctica.

Moreover, the authors suggested that ice thickness with errors of +-15 m was the most precise determination of ice thickness at the South Pole. I frankly wonder if it is worth mentioning in a paper.

In response to the comments of several reviewers, this measurement, and this section have been taken out of the current draft.

Nevertheless, we do feel that this measurement has scientific merit. In our opinion, there are two questions here: a) how well can relative changes in ice thickness be tracked with time, and b) what is the absolute ice thickness? The former question is clearly important in understanding climate change issues, e.g., and the question of mass balance. The second question is more difficult, and requires, in particular, firn-wavespeed corrections that can vary considerably from point-to-point. From an analysis of cross-over points, BEDMAP2 quote a depth resolution of 51.2 meters, albeit with large tails in the distribution. The question of precise absolute ice thickness is, as the reviewer implies, likely of little consequence to true scientists of glaciology. However, it does have two important practical consequences: i) since the bedrock is so close to the melting point, extrapolation of existing temperature profile data through 50 meters more than what is quoted by BEDMAP results in an ice sheet which may be lubricated from below, and thus more likely to glide over the base. On the other hand, a thinner ice sheet implies a lower basal temperature, and a more extreme strain profile near the bedrock, ii) similarly, as the reviewer is likely aware, since the effective volume for a neutrino telescope is cylindrically shaped, the neutrino sensitivity scales roughly as the candidate ice thickness itself. In light of recent results from the Auger Experiment, and the comcommitant uncertainties in the composition of ultra-high energy cosmic rays (and the implied uncertainty in the neutrino flux at Earth), it is important that such effects be precisely incorporated into astroparticle physics simulations such that the scientific rationales for new projects (ARA, ARIANNA, e.g.) be strongest.

I must point out another important point. The authors discussed oblique radio wave scat-

tering to see waveform of the scattered wave within the ice sheet. There are strong anisotropy of dielectric permittivity in polar firn (e.g., Lytle and Jezek, IEEE Trans. Geosci. Remote Sensing, 32, 290-295, 1994., Fujita et al., J. Geophys. Res, 114, 10.1029/2008JF001143, 2009.). This strong anisotropy does not have strong impact on the wave propagation along the vertical. However, for the oblique propagation, two components of the waves (along the vertical and along the horizontal) and their fluctuations within firn should have strong influence on the amplitude and phase of the waves detected at the horn antenna. Therefore, in a present condition that data and analysis in section 3 do not contain effects of this firn dielectric anisotropy, the discussions are unacceptable. Propagation of vertically polarized wave in firn need to be analyzed first. It is highly likely that present data and discussions are misleading.

This is difficult to argue with, except to say that the reviewer's claim is inconsistent with experimental measurement. The reviewer has a prejudice that there must be an observed anisotropy in the firn. This is directly contradicted by our measurements, and the reviewer therefore concludes that our data are incorrect. We believe our measurements are correct, and the reviewer's prejudice is wrong.

"Therefore, in a present condition that data and analysis in section 3 do not contain effects of this firn dielectric anisotropy, the discussions are unacceptable. Propagation of vertically polarized wave in firn need to be analyzed first. It is highly likely that present data and discussions are misleading."

Again, perhaps the reviewer has missed a basic point - our experiment probes the entire depth of the ice sheet, including the firn. Therefore, as our results show, there is no observed firn dielectric anisotropy, at least not for our geometry. This is the essential content of the first half of the paper.

In response to this and other reviewer's comments, the section on ''oblique scattering'' has, nevertheless, been taken out of the paper.

I provide many comments for some detailed points. Please consider them for future revision of the authors' paper.

We thank the reviewer for the considerable investment of her/his time, and hope that she/he finds the revision more acceptable.

#1, Please clarify real purpose (or focus) of this paper. In introduction, scattering mechanisms were first introduced. But this very special topic does not seem main purpose of this paper. If the authors hope to clarify an environment of the ice sheet at South Pole for their future neutrino detection, it seems much more understandable for readers to see such information.

Although we would be happy to add several paragraphs describing our neutrino detection experiment at South Pole, reading the reviewers' comments in full, we notice that point #22 below states simply that ''The paper is not focused.''. We will correspondingly defer to point #22 below and omit such information rather than risk additional 'de-focus'. Hopefully, interested readers will take the time to read the astrophysics references provided.

#2, L8-10, P4696 In the ice sheet, dynamical condition is highly variable. a distance 1400 km does not mean much, unless the authors really discuss dynamical conditions of the ice sheet. I hope to see why South Pole was compared with Dome Fuji and what is the scientific progress produced by the comparison.

As the reviewer may have guessed, the comparison with East Antarctica was made primarily because that was the most extensive data set available for comparison, at least of which we were aware.

#3, L19-22, P4696 In the ice sheet, horizontal shear stress exist everywhere except very special ice flow conditions such as ice divides or in the ice shelves. Indeed, there are really a variety of stress/strain configurations. Differential COF occurs based on them and based on heterogeneous softness (or flow low or stress/strain relations) of polycrystalline ice. It seems that the authors need to mention about deformational history and mechanical properties of ice. Otherwise, non-specialists reader will not understand what is told here. They may narrowly think that only strong shear strain can cause contrasts of COF.

As the reviewer is aware better than ourselves, in the present-epoch, horizontal shear stress does not exist at all depths at South Pole, and is, in fact, manifest only at depths below 2000 m. The AMANDA and ICECUBE neutrino telescope collaborations have now mapped this over the last 15 years or so. There are, of course, no such direct measurements of the strain history at previous epochs.

#4, L17-19, P4697 Please cite papers by specialists of COF. The authors' description is not necessarily wrong but there are more variety of changes in COF in the ice sheet.

We have considerably extended the list of references. Hopefully, this is to the reviewer's satisfaction.

#5, Table 1 Please indicate definition of the delta-epsilon here. Please tell the readers denominator for delta-epsilon (%) used in the table. In earlier papers, researchers have expressed dielectric anisotropy of ice using difference in relative dielectric permittivity (such as 0.037) between two components or difference in relative dielectric permittivity with reference to ice permittivity (1.1 %).

The text reads: ''With ϵ the dielectric constant, f the frequency of radar being employed, σ the conductivity of an acid layer and δ the difference in either ϵ or σ across some boundary, the magnitude of density (or acid) reflected power typically varies as the square of those differences, i.e., δ_ϵ^2 (or δ_σ^2/f^2)[?].'' It seems that the reviewer did not equate 'dielectric constant' with 'dielectric permittivity'; hopefully, by substituting the word 'permittivity' for 'constant', we have assuaged the reviewer's concerns.

Sometimes, refractive indices were used instead of dielectric permittivity. The authors way of expression of non of these. They used relative permittivity with reference to 1 (permittivity of vacuum).

We have tried to maintain consistency in the updated draft, and favored reference to the dielectric permittivity only.

Their way of expression is a kind of relative and relative anisotropy, which will confuse readers. I felt that the authors often use their own terminology or expressions. But sometimes such original way of expression causes risks of readers' misunderstanding and confusion. I must point that the delta-epsilon here is different from the authors definition at L6-8 in P4697.

Hopefully, the replacement of 'dielectric constant' with 'dielectric permittivity' addresses the reviewer's concerns.

I have further concern: please tell clearly to readers what "Lab ice" means in the table, polycrystalline ice or single crystal ice, natural ice from ice sheets or glacier or laboratory grown ice? What is the difference with the other ice? Present state of this table is not *understandable or beneficial for non-specialist readers. Readers cannot learn anything from the table.*

Yes, the reviewer is correct, that this was less informative than it might have been. We have now (hopefully) clarified that, for Fujita-1996, what was referred to as 'lab ice' was actually lab analysis of single crystals accumulated at Mendenhall Glacier, Alaska.

#6, P4697 last paragraph Please provide theoretical basis, perhaps in the supplementary information. It is not clear for readers if this section is the authors' original idea or citation. If it is citation, please provide information of reference paper.

This text is based on standard ''Rayleigh scattering'', with which the reviewer is presumably more familiar than ourselves. We name it as such now in the text, although feel it is perhaps not appropriate to reference the relevant papers, which are now almost 150 years old.

#7, L11, P4698 This cited number is wrong. Two wave speeds never differ by 3.3%.

We thank the reviewer for catching this error. The true wavespeed difference should be approximately half that; we were (obviously) erroneously interchanging ϵ with n.

#8, L15-17, P4698 Is this a key question of the authors? If so, more introduction of COF should be given. Readers will be confused by this kind of vague comments. Of course, there are various COF in the ice sheet depending on layering, deformational history and so on. And not everything was described systematically. But the authors' way of introduction will not lead readers anywhere. I do not understand what the authors are attempting to tell to readers here. No readers can understand what was explained by citation of Doake paper or Woodruff & Doake paper. Again, such situation makes me wonder what is the real purpose of this paper after all.

Here, we disagree with the reviewer that the references are not relevant. There is, we feel, a legitimate scientific question: does birefringence track ice flow? We find the answer to be affirmative, the Doake references do not.

#9, Figure 1, P4699 Please tell to non-specialists of antenna about acceptable VSWR for radar measurements. Please consider that there are not many readers who are familiar to VSWR.

The revised text 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...''

#10, Site Please tell to readers why the authors studied ice sheet at South Pole? Please tell to readers what is glaciological condition in terms of ice dynamics. Please tell to readers how thick is the ice sheet in introduction

To paraphrase Willie Sutton, we studied ice properties at the South Pole because that's where our radio sounding equipment is.

#11, L4-9, P4700 The authors explained to readers that their radar is different from ordinary ice radar sounders. I do not agree with the claim of the authors. (i) ns-scale pulse seems typically used in mono-pulse radars. In addition, ice sounders have used various pulse widths from nano seconds to micro seconds. In particular, using chirp and pulse compression

techniques, virtually very short pulses were realized.

Two responses: a) Prior to writing the paper, our literature search found no other experiments utilizing the 0.5-ns resolution hardware that we have employed. We would appreciate if the reviewer might supply a direct reference that contradicts our current statement, b) having 'very short pulses' is not the same as having precise distance-to-target resolution, as the reviewer realizes.

(ii) CW is not typically used in ice sounding radars. Only in some cases, they were used. (iii) We cannot say yet SAR techniques is typical ice sounding radar. Traditional pulse-modulated radar are the most often used. Overall, such wrong information only mislead non-specialist readers.

We surmise that the reviewer would rather see a description of a finite-duration ‘‘tone’’ rather than true ‘‘continuous wave’’. In fact, the previous bullet states: ‘‘We use a nanosecond-scale transmitted pulse, vs. ‘‘tone’’ signals of frequency ∼100--200 MHz, having duration of order microseconds. Doing so, in principle, improves our ability to resolve fine details of internal structure.’’

We have, in the bullet to which the refers, changed ‘‘continuous wave’’ to ‘‘monochromatic’’, which hopefully addresses the reviewer's concern.

#12, Figures 2, 3 and others Numbers in vertical axis are not for the profiles in the graphs. I guess that the authors were just lazy to express correctly. But such expressions are not acceptable in science. The readers, in particular, non-specialists will think that the axis is for all of profiles in the graph. In addition, alternate displacement by 100 ns confuse readers to check synchronicity of each reflection events. The authors claimed strong synchronicity of reflection events. But the readers have no way to check it. Moreover, the profiles in the figure looks very noisy even if we use zoom of figure in pdf. After all, readers cannot check anything about the authors' claim. In such a case, the authors should provide some figures to back up their claim, perhaps as supplementary materials.

Zooms are now included in the draft, and reproduced below, for 6 us, 9.6 us and 13.2 us reflectors.

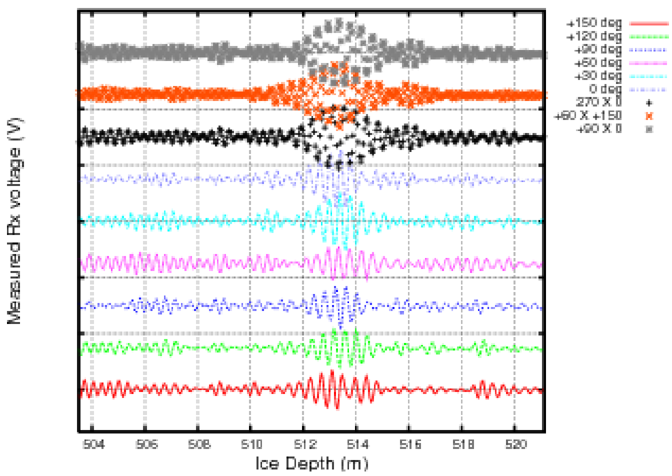

FIG. 1: Zoom of interval around 6 μs.

In the spirit of the reviewer's concerns that the paper is itself somewhat obtuse and obscure, we have also taken the liberty of translating

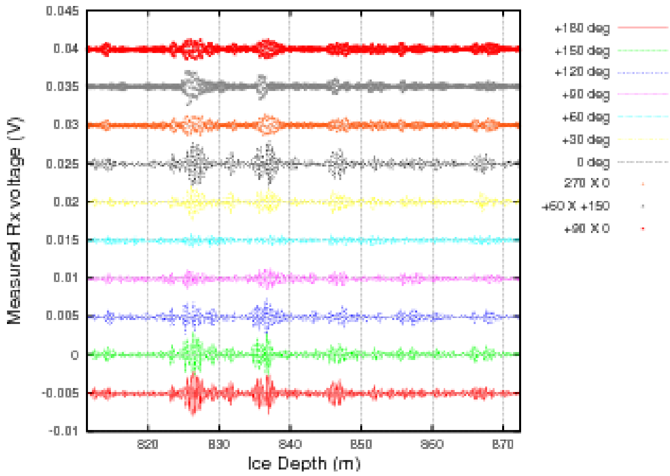

FIG. 2: Zoom of interval around 9.6 μs.

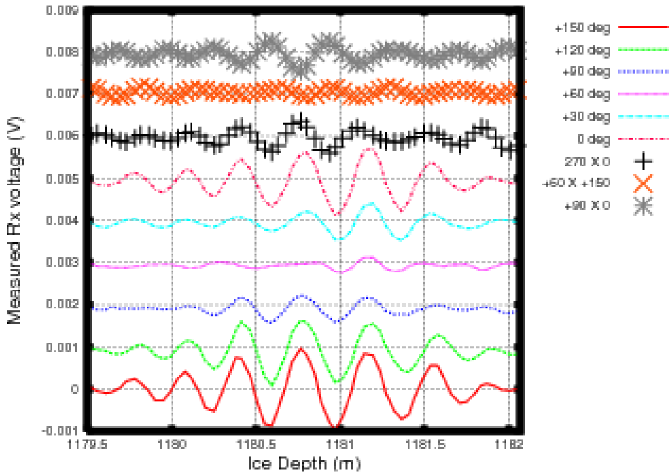

FIG. 3: Zoom of interval around 13.6 μs

the echo times in the original figures to echo depths in the zoomed figures, since correlations with actual ice cores will use the latter, rather than the former unit of measure.

Parenthetically, we might request that the reviewer abstain from characterizing the authors as ''**lazy**'', which we personally feel is both unnecessary and unprofessional, and not in the spirit of the constructive dialog which we hope this Journal seeks to maintain.

#13, L18 P4700 Comparison with "East Antarctica measurements" appeared suddenly here without proper explanations.

Again, as mentioned previously, this has now been considerably de-emphasized in the revised text.

#14, L5-8 P4701 "We note that the implied depth of the return at 13.9 s is consistent with layering identified using a laser dust logger in boreholes drilled for the IceCube experiment (Abassi et al., 2012)." This citation cannot be accepted by two reasons: (i) submitted paper and (ii) readers have no ways to check the claim or learn something.

Okay. Pending journal acceptance, we have removed the reference. If the

Abassi paper is accepted promptly, we will restore that reference.

#15, Section 2.5 Variation in return amplitude with azimuth is discussed. This topic has been discussed by Hargreaves (J. Glaciol., 21, 301-313, 1978.), Fujita et al. (J. Glaciol., 52, 407-424, 2006.) . They commonly demonstrated that 180 degrees periodicity means effect of anisotropic boundaries and not birefringence. Because the authors detected such data (180 degrees periodicity instead of 90 degrees periodicity), the earlier studies should be naturally introduced.

We thank the reviewer for her/his insightful comment, which is echoed by reviewer #5, and have included both the reference, as well as an extended discussion, in the revised text.

#16, Second paragraph in Section 2.5 Readers will be confused here. Do the authors claim that birefringence do exist in the radio wave from the bed but not from the shallower internal layers?

Yes.

Do the authors claim that 180 degrees periodicity from the bed are caused by birefringence?

Yes. We believe it is more than a claim, as we observe two distinct echoes within the same time-domain waveform, which is the signature feature of birefringence, and cannot be mocked by simple bed geometry effects or anisotropic boundaries.

Please define V(fast) and V(slow) that suddenly appeared here. Readers will be confused. In Line 26, do the authors cite Fig. 8 of Besson et al. (2010) or Fig. 8 of the present paper? The bed signal do not have echo time and voltage characteristic of birefringence because the signals have only 180 degrees periodicity. This point is very important. It seems that the signal feature is due to some bed features such as bed inclination, lineation of bed or something else. Please explain.

We disagree, and believe that the reviewer may be missing the physical significance of birefringence, as observed in the time domain. A signal propagating at 45 degrees with respect to the ordinary and extra-ordinary axes results in two signals received, which is exactly what we see. Again, the effect observed has nothing to do with the bed inclination, which, although it can indeed produce two distinct pulses, cannot produce two distinct pulses in the same waveform, as a function of rotation angle.

#17, Oblique radio wave scattering at Section 3 Firn birefringence which is very strong must be considered here (Lytle and Jezek, IEEE Trans. Geosci. Remote Sensing, 32, 290-295, 1994., Fujita et al., J. Geophys. Res, 114, 10.1029/2008JF001143, 2009.). Without this consideration, any further discussions are meaningless.

Again, there is no firn birefringence at South Pole.

Since this comment and the remaining comments pertain to the now-excised section 3 of the originally posted draft, we will, with the author's indulgence, not provide explicit responses for all them.

Readers will not understand where they should see in Figures 11 and 12. At line 6, page 4705, 50 ns is commented citing Besson et al. 2010. However, I could not understand here. Please better explain.

#18, Section 3.1, first paragraph No citations for the BEDMAP, CRESIS or BEDMAP-2. I did not understand what the authors are comparing and by what motivation. Compilation of BEDMAP is based on various kind of old data. In the past, precise positioning was in particular difficult because many of data are obtained before the age of GPS. The authors seems to try to show how old data set contains error and how precise measurement is needed.

But most of the problems comes from positioning and not from the wrong choice of wave speed, errors of timing detection, rise time or inaccurate firn corrections. It seems to me that it is not fair that the authors emphasize errors of the BEDMAP for their introduction to show their precise data of (+-15 m). Different problems are mixed. Such statements are not for wide readers.

#19, L20, P4705 BEDMAP is not only from airborne measurements. It is total compilation for many data sources of ice thickness measurement. BEDMAP was available since 2001 and not 2011.

#20, L24-25, P4705 I did not understand this sentence at all. "those", "monochromatic radio signals of order 10 us duration or coherently adding many echograms. I believe that few readers can understand.

#21, Section 3.1, second paragraph I did not understand here.

#22, Section 3.2 I understand that the authors installed their instruments in the ice sheet. But this section is one of examples that the authors are attempting various radio propagation measurements at South Pole Station. The paper is not focused.

#23, L14, P4707 in Conclusions The authors say that Fujita et al. (2006) showed correlation between ice flow and amplitude of internal reflections at Dome Fuji. This is not true. Fujita et al. have shown such examples at Mizuho which is very far from Dome Fuji. In addition, K. Matsuoka have written many examples of the anisotropic reflections (Matsuoka, K. et al. J. Geophys. Res., 108, doi:10.1029/2003JB002425, 2003.). It is not proper that Dome Fuji is particularly cited something like a target of comparison.

We thank the reviewer for pointing out the inexactness in our comparison, although we primarily used those data for reference only because it is the most extensive set available.

We have extended the discussion and also included additional references to Matsuoka's work.

#24, L28-22, P4707 in Conclusions Dome Fuji is not any representative of "East Antarctica". COF is highly variable depending on stress/strain configurations.

Again, we apologize for inexactness in our language.

#25, L10, P4708 in Conclusions The authors wrote "if the c-axis is exactly vertical". If the authors meant "if the c-axis distribution has a isotropic distribution around the vertical", the statement should be rewritten. Otherwise, some readers may feel that there is a single crystal or c-axes are perfectly along the vertical. Such conditions are unrealistic.

We thank the reviewer for her/his correction and have used the wording proposed by the reviewer.

#26, L10-19, P4708 in Conclusions I am very confused to read here. I cannot find any link between these statements and some section of main text in this paper. It seems to me that the authors sometimes emphasized lack of birefringence and sometimes emphasized presence of birefringence.

This is correct. We do not observe birefringence in the upper half of the ice sheet. We do observe birefringence in the lower half of the ice sheet. We believe both results have scientific merit.

#27, L20-25, P4708 in Conclusions I find little meaning for providing these statements to readers.

The conclusions have been significantly re-vamped, as well as abbreviated, in accordance with the reviewer's wishes. Hopefully, the reviewer feels the new text is an improvement.