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

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.

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

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

#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.

#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.

#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.

#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.

#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 %). 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). 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.

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.

#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.

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

#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.

#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.

#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

#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.
(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.

#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.

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

#14, L5-8 P4701
"We note that the implied depth of the return at 13.9 μs 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.

#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.

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

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.

#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.
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.

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

#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.

#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.

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