Dear Tom,

Thank you for your work to improve the original manuscript. Now I have received reviews from Joe MacGregor and Mike Wolovick who provided insightful reviews to an earlier version of your manuscript. They both suggested accepting your manuscript only with technical corrections. Below, please find my own comments to your revised manuscript. I request minor revision at this stage. I expect no major disputes on the issues brought by the reviewers and by myself, so I will not send the revised one to the reviewers, but review it by myself.

Thanks for choosing The Cryosphere to publish your valuable work.

Best regards,

Kenny Matsuoka

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General issues.

G1: The manuscript is well written but I am afraid that many people may confuse differences in for example <B hat> and <B>. Please add such symbol to all labels of the figures, when appropriate (e.g. Figure 4's ordinate, labels for Fig. 5a and 5b, label of Fig 6a, Fig. 9, Fig. 10).

G2: GISM and SICOPOLIS models do not predict ice thickness accurately. I assume that the authors ignore the difference between observed and predicted ice thicknesses, and used observed ice thickness and model-predicted vertical profile of the temperature for relative depth (i.e. fraction of the local ice thickness). Is this understanding correct? Anyway, please add a paragraph to explain this point, and if possible to present the difference of GISM-, SICOPOLIS-modeled ice thickness to the observed ice thickness.

Line 30-35: I also recommend including pioneer work of radar to measure ice thickness. Fretwell's and Bamber's work are cited lines below in the context of new bed DEMs of Greenland and Antarctica. I cannot clearly see the difference between work done for basal material properties and for basal melting or freezing. At lines 42-43, basal melting is considered as a part of basal material properties. I think that everyone has different opinions which work is most significant for these sub disciplines in radioglaciology, but I would suggest considering to cite following work as well.

For internal layer structure, I suggest Fujita et al. (1999), which is away more significant than my own work in 2010 that you cited.

Fujita, S., Maeno, H., Uratsuka, S., Furukawa, T., Mae, S., Fujii, Y., & Watanabe, O. (1999). Nature of radio echo layering in the Antarctic ice sheet detected by a two-frequency experiment. *Journal of Geophysical Research-Solid Earth, 104*(B6), 13013-13024.

Also, Bentley et al. (1998) and Peters et al. (2005) made milestones.

Bentley, C. R., Lord, N., & Liu, C. (1998). Radar reflections reveal a wet bed beneath stagnant Ice Stream C and a frozen bed beneath ridge BC, West Antarctica. *Journal of Glaciology, 44*(146), 149-156.

Peters, M. E., Blankenship, D. D., & Morse, D. L. (2005). Analysis techniques for coherent airborne radar sounding: Application to West Antarctic ice streams. *Journal of Geophysical Research-Solid Earth, 110*(B6), doi:10.1029/2004JB003222. doi:10.1029/2004jb003222|issn 0148-0227

Line 50: Peters 2005 should be Peters et al. (2005). Correct the reference list as well.

Line 88: remove the end parenthesis ")".

Line 113: missing figure number. It should be Figure 2.

Line 133: "surface roughness" → "bed roughness"

Line 193: "electrical conductivity" → "dielectric conductivity"

Line 199: Define M. In the current form, M = micro mol/L. It is probably better to define M = mol/L so that CH+ = 0.8 micro M.

L205: [L hat] is defined as the total loss, but it is two-way attenuation. Total loss sounds like that it includes surface transmission loss, volume scattering due to crevasses etc as well.

L212: "electrical" → "dielectric" (you said "dielectric properties" at line 209).

L217: "electrical" → "dielectric"

L218: "radar system frequency" → "radio-wave frequency (or radar frequency)"

Line 225: I assume that the authors calculated depth series of (in-situ) attenuation rates using depth series of ice temperatures predicted by the two models, integrated the in-situ attenuation rates over the full ice column and then divided it by the ice thickness. Please briefly explain this process around this line. I often see that people first calculate depth-averaged temperature to estimate the depth-averaged attenuation rate, which is wrong due to the Arrhenius relationship between them.

Line 228: I think all other depth-normalized values are in the unit of per kilometers, not per meters.

Line 233: I cannot see this point clearly in Figures 1a and 5c. Because data density is highly variable over the GrIS, majority of the data and majority of the data covered region are quite different.

Line 250: Matsuoka (2011, GRL) demonstrated that even if everything is equal but only ice thickness varies, the depth-averaged attenuation can vary. In other words, even if the sampling region is small enough to avoid any variable SMB, geothermal flux, or such, the empirical method to estimate the attenuation rate from the depth variations of the returned power is inherently not robust (see Figure 3b and Figure 4 in Matsuoka, 2011). I accept the approach the authors took but this point should be mentioned here to clarify the limitation of the proposed method. Depth-averaged attenuation derived in this way is hardly consistent with the attenuation rates estimated with temperature models (Fig. 3b in Matsuoka, 2011).

Line 281: "slowing varying" → "slowly varying"?

Line 285: "in the supplementary material (Figure S2)"

Line 295: Matsuoka et al. (2012b) analyzed depth dependence of the returned power but it is to demonstrate how the classical analysis is not robust. So, it is not appropriate to cite Matsuoka et al. (2012b) in this context.

Line 298-299: [S] is not defined. The current Equation (6) includes [S] so it does include the instrumental factors (I assume that [S] represents instrumental factors, such as transmission power).

Line 308: “and if d[S]/dh = 0” When d[R]/dh is large, it is usually caused by tilted bed.

Line 314: [S] is not well constrained in many cases, so usually only spatial variations of [R], not the absolute value of [R], is discussed.

Line 317: It is probably helpful to cite Matsuoka (2011).

Line 328: Figure 8 shows that corrections are typically more than zero for thinner ice, whereas the corrections are less than zero for thicker ice. However, in theory, thinner ice is colder (not warmer) and then the attenuation rate is predicted smaller than the thicker ice (Fig. 3b of Matsuoka, 2011). So, I don’t know whether this depth dependent features are really from the ice temperature or from a combination of many factors. Can you demonstrate how this depth dependence is generally vaid over the GrIS?

Line354: Equation (6) is defined as [Pc] = [R] - [L] + [S], so the Equation (10) should be [R] = [L] + [Pc] - [S] = 2<B>h + [P^c] - [S].

Line 357: Please define [R hat] clearly so that the difference between [R] and [R hat] will be clearer. My understanding is that [R] = 2<B>h + [P^c] – [S], so only one difference between [R] and [R hat] is whether Arrhenius-model-based or Radar-inferred attenuation rates are used. Is this correct?

Line 423: what do you want to say with “radar-inferred attenuation rate/loss”? Is it rate or total loss? And loss could include for example volume scattering from crevasses. I think that it is better to say “attenuation rate” and “two-way attenuation”.

Line 457: sigma is used to define the dielectric conductivity. I don’t really see a need to define mean and standard deviation here using symbols.

Line 464: “wet” → “thawed”?

Line 485: <B> is already defined with Equation 8, so please use a different symbol, such as <B mean>. (“mean” can be a bar over “B”).

Line 502: “region region”

Line 503: “near-continuous”

Line 507: “the frequency distribution” → “the probably distribution (or probability function”? “Frequency” is confusing with this context.

Line 578: please rewrite “For our temperature fied-conditioned, bed –returned power, method this is not…”

Line 591: “The difference between Arrhenius-model and Radar-inferred attenuation rates averaged over the ice thickness”?

Line 594: “electrical” → “dielectric”

Line 596: I assume that Figure 13e shows modeled temperature at fractions of the modeled ice thickness in terms of observed ice thickness. E.g. modeled ice temperature 10% of the model-predicted ice thickness below the surface is shown here as the modeled temperature 10% of the observed ice thickness below the surface. Please see general comments G2.

Line 614: please add a reference for the acidity argument.

Line 640: “Attenuation rate/loss”. See my comment above.

Line 695: “impurities” → “constituents”

Line 709: “electrical” → “dielectric”

Line 730: “non-specular, volume scattering”?

Table 1: Table 1 says that [R hat] is defined with Equation (12) but it is defined with Equation (10), not (12). [R] is said that it is defined with Equation (8), but I cannot see an equation that defines [R].

Figure 3: Change the ordinate so that the radar returned power is shown in the logarithm (dB) scale. All other figures show radar data in the decibel scale. Also consider using the depth instead of depth index for the abscissa.

Figure 4: "Arrhenius model **M07** in MacGregor et al. (2007)”

Figure 8: please add the lengths of the targeted window.

Figure 9: [L] is defined total loss in the main text so it is inconsistent (but I proposed to call it “two-way attenuation”, not “loss”).

Figure 12: (here and elsewhere) [L] should be called more consistently. “Attneuation” and “loss” are used in interchangeable manner. I recommend to call [L] as two-way attenuation.

Figure 14: Bold (a) and (d) in the caption.

Supplemental document (SD) Line 7: “Reproducibility”?