Author comments: "An ice-sheet wide framework for englacial attenuation and basal reflection from ice penetrating radar data"

T.M. Jordan, et al., *The Cryosphere*

Review by: Mike Wolovick

We thank the reviewer for their exceptionally detailed feedback to our manuscript. They have well understood the methods developed, and have given many helpful suggestions regarding how we can improve the overall clarity of our presentation. We provide detailed feedback to their comments in blue italicised text. The summaries of our revisions which address the major comments are highlighted in bold text.

Summary:

This paper describes a semi-empirical method for estimating attenuation losses and bed reflectivity in radar data from continental ice sheets. The method uses a prior thermal model to estimate the spatial gradient in attenuation. Based on this spatial gradient, the method selects local regions that are expected to have broadly similar attenuation rates based on a segmentation approximation. Within each region, the thermal model is used to correct the observed bed-returned power for the local differences in attenuation relative to the mean for that region. The corrected bed- return power data are then fit with a least-squares linear best-fit representing the mean attenuation rate for that region. The residual to the fit represents the basal reflectivity.

The authors apply the method to the CReSIS radar dataset collected over the Greenland Ice Sheet for Operation IceBridge. They find that the method converges where ice thickness is variable and attenuation rates are high in the south and east of Greenland. In the north and west, as well as in the ice sheet interior, the method does not converge. They find a tradeoff between spatial coverage and precision, such that the area where the method is considered to have converged increases if one is willing to accept higher uncertainty. The output bed reflectivity estimates show a reduced spread consistent with the range of plausible subglacial materials. The authors also demonstrate that the estimated attenuation rates can be used to check the temperature bias in the original input models.

This manuscript is clearly relevant for *The Cryosphere*. The method developed by the authors represents an important advance in the integration of ice-penetrating radar data with ice sheet models. The authors demonstrate both how models can be used to guide the interpretation of radar data, and how the radar data can in turn be used to diagnose biases in those models. I have two major concerns, one relating to the inability of the model to converge in the ice sheet interior, and the other relating to the segmentation approximation. The first concern is actually an opportunity for the authors to explain the scope of their results. I believe that they have been too conservative, and that in fact they can constrain reflectivity in the interior of Greenland even if they cannot also constrain attenuation. The second concern could be addressed by showing how the results respond to alternate means of choosing local sample regions.

Major Comments:

<u>Ice Sheet Interior</u>

The main weakness of the method developed by the authors is that it does not work in interior regions of Greenland where variations in ice thickness and attenuation rate are both low. In other words, the method doesn't work where the problem is easy! With little variability in ice thickness and relatively constant thermal structure, one could get good estimates of the basal reflectivity anomaly in interior Greenland by using no attenuation correction at all.

The reason why the authors' method does not converge in the interior of Greenland is one that the authors themselves identified: the variability in ice thickness is too low. Quantitatively, we can say that in order to get a good correlation between bed returned power and ice thickness, the following condition must hold: $2B\Delta h > \Delta R$, where B is the regional average attenuation rate, Δh is the standard deviation of ice thickness, and ΔR is the standard deviation of bed reflectivity. When the variability in ice thickness is below this threshold, a local linear best-fit cannot constrain attenuation rate.

However, when the variability in ice thickness is low, the total attenuation losses should also be roughly constant. Attenuation therefore becomes less important, and it should be possible to estimate basal reflectivity anomalies even if attenuation cannot be constrained. Local variations in attenuation rate may still produce variability in total attenuation losses; however, the authors are already using a numerical model to estimate local variations in attenuation rate. The authors make a good case that the local gradient in attenuation rate from the models is more reliable than the mean value, so they can simply continue using the model to correct for local variations in attenuation rate. When the variability in ice thickness is low, the authors' method will be unable to constrain the regional mean attenuation rate, but it should still do a good job of estimating basal reflectivity, and the authors could present those reflectivity results in the interior of Greenland.

In order to capture regions where it is possible to constrain reflectivity but not attenuation, the authors could introduce an alternate quality control check as a substitute for the $r^2[P_C]$ check they introduce in Section 2.7. When the variability in ice thickness is low compared to the variability in basal reflectivity ($2B\Delta h < \Delta R$), the authors could check the standard deviation of bed reflectivity instead of $r^2[P_C]$. The authors use the standard deviation of reflectivity as a check on the validity of their results anyway (Section 3.2), so it makes sense to formally add this metric to the quality control step. If the standard deviation of bed reflectivity is reasonable for subglacial materials, then the alternate quality control is passed and the authors can present results for reflectivity in that region.

The reviewer has made the case that it is possible to constrain relative reflection (reflection anomalies) in regions where it is not possible to constrain attenuation. They have correctly implied that our quality control measures, described in Sect. 2.7, are designed with attenuation solution accuracy in mind, and therefore fail to identify some regions where relative reflection can be constrained (primarily the northern interior). Additionally, they have suggested that we could reformulate our algorithm using the standard deviation of relative reflection as a quality control measure.

We appreciate that we should have been more explicit about the findings of previous studies, Oswald and Goginenni (2008, 2012), that have already addressed exactly the problem that the reviewer raises: constraining relative reflection in the interior of Greenland. Specifically, Oswald and Goginenni (2008, 2012) demonstrated that, assuming a simple (approximately constant) attenuation rate model, relative reflection values for the interior have a decibel range that is near-invariant with ice thickness, with an approximate bimodal distribution that they associate with wet and dry beds.

As described in the conclusions of our paper, in future work we aim to use a similar approach when producing a gridded reflection data product for the interior, (however probably in conjunction with a forward Arrhenius attenuation model, rather than the specific attenuation model in Oswald and Goginenni (2008, 2012)). We therefore believe that to focus to on a reformulation of the problem (i.e. `quality control in terms of the distribution of relative reflection') would detract from the important step forward that we have made in our paper: that we have developed a robust, automated, method of inferring full ice column attenuation values toward the margins of *ice-sheets (i.e. where the assumption made regarding the attenuation model in Oswald and Goginenni (2008, 2012) breaks down, and where their method cannot be applied). Additionally, whilst our study deals with relative reflection, an ongoing goal is to incorporate radar system performance ([S] in equation (8)), and therefore constrain absolute basal reflection rather than basal reflection anomalies. This approach would require englacial attenuation to be known (subject to an estimated uncertainty bound), which is consistent with the approach we have taken in our paper.*

We envisage the following problems using the standard deviation of reflection as an automated control measure:

(i) It makes the underlying assumption that the distribution for basal reflection is unimodal. Whilst this appears to be is the case for the coverage region in SE Greenland (Fig. 11), it is not required as an a priori assumption of our existing method. As mentioned above, Oswald and Goginenni (2008, 2012) demonstrated that the distribution of basal reflection values for the interior of Greenland is approximately bimodal.

(ii) Automatically selecting regions on based upon a minimising the spread of the distribution in basal reflection values is not necessarily desirable. Subject to this control measure, the algorithm would preferentially select regions that are homogenous, and fail to select sharp transition regions.

In summary, we thank the reviewer for adding some true clarity to the problem which we address and we suggest the following changes to the manuscript:

- ***(i)*** ***We will make it clear in the introduction that Oswald and Goginenni (2008, 2012) concluded that relative reflection/reflection anomalies can be constrained in the interior of Greenland where attenuation rate variation is low. However, due to both the higher spatial variation and higher absolute values in attenuation rate (as predicted by Arrhenius models), the same is not true toward the margins.***
- ***(ii)*** ***We will state explicitly that the algorithm quality control measures, equations (13) and (14), are specifically designed with attenuation rate/loss accuracy in mind, (rather than constraining the distribution of relative reflection). Given the valuable second use of radar attenuation to constrain temperature, we believe that this scientific problem requires a full investigation as outlined in our paper.***
- ***(iii)*** ***In view of point (ii) we will revise the title of the manuscript title to 'An ice-sheet wide framework for englaical attenuation from ice penetrating radar data'. The introduction/abstract will now better focus on the `dual role' for an IPR-derived attenuation solution (i.e. constraining basal reflection and temperature).***
- ***(iv)*** ***Finally, as part of our feedback to the other reviewer's comments, we will present a reflection map for the interior of Greenland using a forward Arrhenius model. This backs up the conclusion in Oswald and Goginenni (2008, 2012) that when attenuation variation is low, the reflection distribution is well constrained and near thickness-invariant.***

<u>Segmentation Approximation</u>

The segmentation approximation seems needlessly complex. The purpose of the segmentation approximation (Section 2.5) is to define a local region in which attenuation rate is roughly constant. Why not simply define an oval-shaped region where the RMS variability in attenuation rate is less than some threshold? Or why not simply define an irregular contiguous region containing all grid cells where the difference in attenuation rate is less than the threshold? With an ellipse, the unknowns at this step of the problem would be reduced to three: the orientation of the elipse, the length of the major axis, and the length of the minor axis. With an irregular shape, no a priori assumptions about the nature of the ice sheet temperature field need to be made at all. Using an ellipse instead of the segmentation approximation would eliminate the sharp corners created along the segment boundaries in Figure 5f.

In addition, an ellipse or an irregular shape would drastically simplify Section 2.5. Nothing that the authors have presented indicates that the segmentation approximation is a particularly good representation of the ice sheet thermal structure. The segmentation approximation has no physical basis in ice dynamics or temperature that could justify the use of such a complex model. The only virtue of the segmentation approximation seems to be that it is capable of elongating perpendicular to the gradient of attenuation rate, but an ellipse or an irregular shape could do that too. In addition, the segmentation approximation is only capable of elongating at 45º angles, but an ellipse or irregular shape could elongate at any angle.

I do not believe that the awkwardness of the segmentation approximation invalidates the later results of this paper. It is likely that the authors would have achieved similar results with any reasonable method for selecting a local sample region based on the input thermal models. However, the unnecessary complexity of the segmentation approximation makes the paper harder to follow, has no realistic basis in ice sheet physics, and potentially

The supplemental material (Sections S1 and S2) explores the sensitivity of the sample regions produced by the segmentation approximation to the temperature model input (S1) and to the choice of RMS tolerance (S2). However, neither section addresses the sensitivity of later results to the segmentation approximation itself. I would like to see an exploration of how the results are affected by completely different means of choosing a local sample region. What happens if the segment boundaries are shifted by 22.5º (half a segment)? What happens if six segments are used instead of eight? What if an elliptical, a circular, or an irregular sample region is used? The authors need not address every single possible method of determining local sample regions, but I would like to see some exploration of the effects of using a segmentation approximation to choose local sample regions.

The reason why the segmentation approximation uses the ` compass directions' to define anisotropy is analogous to why finite difference methods for differential operators do: it is the most computationally practical method for a gridded data structure. Conceptually, the `local difference' terms, <B∞(x,y))>-<B∞ (x_0,y_0)>, are analogous to the numerator of a finite difference derivative. (As an aside, we use local differences rather than the finite derivatives, due the derivative being much nosier and having sharp discontinuities present. Additionally, our use of `8 compass directions' captures greater information than the `4 compass directions' that would occur when using a standard horizontal gradient operator.) Ultimately, the geometric parameters of either and an oval or ellipse would also be conditioned by the horizontal gradient/difference terms, and therefore would also be subject to similar limitations (i.e. being conditioned by 4 or 8 axes) and have similar artifacts present.

What we perhaps did not make as clear as we should have done in the manuscript, is how the `complexity' of the method arises due the more obvious approaches (such as those suggested by the reviewer and experimented with by ourselves during the method development) having practical difficulty in their implementation. Below we deal with specific points made by the reviewer.

Re: Why not simply define an oval-shaped region where the RMS variability in attenuation rate is less than some threshold?

As mentioned in the manuscript we did originally experiment with a more simple RMS measure of window tolerance for the segments line 220). We concluded that this approach produces sharp discontinuities in the spatial dependence of the target window dimensions (i.e. the `window radi' in Fig. 6), and therefore very sharp *discontinuities in radar-inferred attenuation rate/IPR data that is sampled. Hence our use of the integrated RMS tolerance measure*

Re: why not simply define an irregular contiguous region containing all grid cells where the difference in attenuation rate is less than the threshold?

Whilst this is a simple question to ask, this is a substantially more computationally demanding approach than our current method. At our chosen resolution each grid cell (~10^6 in total) would have an associated `sample region mask' to be defined (~10^4-10^5 cells in total). Additionally, a specific algorithm would have to be developed to define the contiguous region. We do, however, agree that this approach provides the best estimate of the thermal/attenuation structure of the GrIS and this has been added to Section 2.5.

Re*:* What happens if the segment boundaries are shifted by 22.5º (half a segment)? What happens if six segments are used instead of eight?

Again, these suggestions are significantly more complex to implement than our existing method for a local difference measure on a rectangular grid.

Re*:* What if an elliptical, a circular, or an irregular sample region is used?

See above comments regarding elliptical and irregular shape regions. We originally investigated circular (isotropic) regions, where the radius of the circle is a function of the magnitude of the horizontal gradient in the depth-averaged attenuation rate. However, all other things being equal, this resulted in more pronounced systematic biases for cross-over measurements of accuracy (different temperature fields and field seasons).

In summary, given that the reviewer does not believe that `the segmentation invalidates the later results of the paper' we suggest that it not truly a major concern/comment, and that their suggestions for how one could potentially reformulate procedure are best placed in the context of future modifications. We suggest the following revisions:

> ***(i) Clearly stating the segmentation approximation is just one possible representation of the anisotropy of the estimated <B∞ (x,y)> field, list the other possibilities that could be considered, and state that an irregular contiguous region is the most desirable, but computationally expensive, approach.***
>
> ***(ii) A rewrite of Section 2.5 where the conceptual arguments are made more explicit, whilst moving the more technical details/equations regarding the segmentation approximation to a new appendix (Appendix B). This is in correspondence with our response to the other reviewer.***
>
> ***(ii) State that the `8 compass directions' used in the segment approach, arises through analogy with numerical schemes for finite difference derivatives, (and that the local differences that we use are much smoother and more tractable than simple application of a finite derivative operator.)***

Minor Comments:

Line 4: "...which is an exponential function of temperature."

Attenuation is an Arrhenius function of temperature, not an exponential function.

We have changed `exponential function of temperature' to `Arrhenius function of temperature'.

Line 5 (and elsewhere): "stationarity"

I'm not sure I agree with the authors' use of the term “stationary”. Typically, “stationarity” refers to a time series whose statistical properties are constant over time, and that concept could be generalized to spatial data whose statistical properties are constant over space. However, the authors use “stationarity in the attenuation rate” to mean a constant attenuation rate. A constant is stationary, but not all stationary data are constant. In this case, the authors could be both easier to understand and more accurate by saying “constant” when they mean constant.

The reviewer is correct and, with regards to the attenuation rate field, we exclusively use `stationary' to mean `constant'. This a very sensible suggestion we have changed all usage of stationary to constant.

Lines 16-31: Several places in this paragraph could benefit from adding additional (often older)

references.

ice thickness: add [*Bailey et al.*, 1964; *Evans and Robin*, 1966; *Robin et al.*, 1969; *Jankowski and*

Drewry, 1981]

basal material properties: add [*Oswald and Robin*, 1973; *Peters et al.*, 2005]

internal layer structure: add [*Robin et al.*, 1969; *Conway et al.*, 1999, 2002; *Vaughan et al.*, 1999;

Fahnestock et al., 2001; *Dahl-Jensen et al.*, 2003; *Ng and Conway*, 2004; *Tikku e* new data products for bed elevation and ice thickness: add [*Morlighem et al.*, 2014] Additional uses of radar data that could be added to this paragraph:

ice rheology: [*Raymond*, 1983; *Hindmarsh et al.*, 2011; *Kingslake et al.*, 2014]

grounding line dynamics: [*Conway et al.*, 1999; *Catania et al.*, 2006; *Christianson et al.*, 2013]

basal melting or freezing: [*Fahnestock et al.*, 2001; *Catania et al.*, 2010; *Bell et al.*, 2011]

ice dynamic changes: [*Conway et al.*, 2002; *Bingham et al.*, 2015]

We deliberately added ‘e.g.’ when listing references in this introductory section, to represent that our reference list was non-exhaustive. However, as a major motivation for our work is to develop new data products, we have added Morlighem et al. (2014). Additionally, as basal melting or freezing is potentially very relevant to our work on basal reflection we have added [Fahnestock et al., 2001; Catania et al., 2010; Bell et al., 2011].

Line 33:

add [*Oswald and Robin*, 1973]

Done.

Line 38: “Arrhenius models where the attenuation rate is an exponential function of **inverse** temperature”

Done.

Line 45: “...make the implicit assumption...”

In some papers, the assumption is explicit. Maybe just say “...make the assumption...”

Done.

Line 45: “locally stationary”

See my comment above. “Locally stationary” is an oxymoron; stationarity implies that statistical properties are globally constant. Use “locally constant” here instead.

Done.

Line 53: “A central feature of our algorithm is the use of a prior Arrhenius model estimation of the attenuation rate as an initial condition.”

The use of the phrase “initial condition” is incorrect here. Initial conditions apply to models that predict the evolution of some variable over time. A better term would be “first guess”, “initial guess”, or “initial estimate”.

We agree, the term `initial condition’ is normally used in the context of a dynamical system and we do not want to confuse the reader here. We have replaced ‘initial condition’ with `initial estimate’.

Lines 54-55: “Conceptually, the initial condition is used to estimate regions where the assumption of stationarity is valid within some specified tolerance.”

Removing “stationarity” can clean up this sentence: “The initial estimate is used to determine regions where attenuation rate is approximately constant to within some specified tolerance.”

Done – good suggestion.

Line 158: “...exponential dependence upon **inverse** temperature

Done

Line 160, 164 (and possibly elsewhere):

Be careful about MacGregor et al., [2015a] versus MacGregor et al., [2015b]. I'm pretty sure you mean the second one in this context.

Done. This mistake was also raised by the other reviewer.

Lines 165-172:

Somewhere in here would be a good place to indicate that brackets <X> indicate the depth-averaged value of X.

Done. We agree that it is very important to make this clear

Lines 173-181:

For completeness, it would be a good idea to state how big of an effect you expect to see from climate transitions in Greenland. The MacGregor et al., [2012] reference refers to East Antarctica, where climate transitions have both a smaller signal in ice chemistry and are more closely spaced in depth than in Greenland. In Greenland, the stratigraphic chemistry changes are dominated by the Holocene- LGM transition, which occurs at wildly different depths in southern and northern Greenland [*MacGregor et al.*, 2015]. The difference in depth of the Holocene-LGM transition with respect to the warm ice near the bed might be expected to produce a large difference in attenuation rate between northern Greenland and southern Greenland.

This is an interesting point. We have now incorporated this discussion into Sect. 3.5 where the Arrhenius model is used to determine temperature bias. We specifically note that: (i) the depth-averaging approximation using GRIP core values generally underestimates attenuation loss relative to using layer stratigraphy, (ii) this underestimation is greater in South and West Greenland where there is a greater proportion of Holocene ice.

Lines 190-191:

Simplify this sentence to: “For the majority of the IPR data coverage region, GISM has lower temperature and therefore lower attenuation rate than SICOPOLIS (Fig. 4c).

Done – we agree this is clearer.

Line 215

There is an extra parenthesis inside the square root sign.

Done.

Lines 216 and 217

The averaging brackets <> are in the wrong place. As written, the square root cancels the square and the expression reduces to the absolute value of the difference. The brackets should go outside of the squared difference. The expression inside the square root sign should be: $<(B_\infty(x,y)-B_\infty(x_0,y_0))^2>$ (and likewise for the second expression). The alternate possibility is that the averaging brackets represent column-averages, not real averages. In that case, the expressions reduce to the absolute value of the difference.

*The second interpretation is correct and the averaging brackets represent column/depth averages, and they are therefore **not** in the wrong place. This should be clear, since throughout the paper <> corresponds to column/depth rather than statistical averages.*

Yes we agree, the expression does reduce to an absolute difference. However, due to the later development of our integrated tolerance measure (where the square root is taken outside of the integral), we prefer the equivalent squared/square root notation rather than the modulus.

Lines 208-217

This paragraph is very confusing. It sounds like the authors assume only radial dependence within each segment, but later, the sample region boundaries (Figure 5f) clearly show a dependence on angle even within each segment. This is because the region boundaries are interpolated from the central radius vector for each segment along a circular arc, in order to produce continuous (but not differentiable) window boundaries. It would be helpful to state somewhere in this paragraph that the ultimate goal is to produce a variable radial length of the target window by interpolating with respect to angle.

See our response to the major comment on the segmentation approximation

Equations 6 and 7:

Move the constant terms outside of the integral and simplify them to $2/R^2_n$.

Agreed. This is better practice.

As written, the RMS is also a function of θ_n.

We agree that the RMS measure is also a function of theta. However, theta is fixed for each integral, where Rn is the `target variable' to be solved for.

Lines 237-239:

See my major comment above. If the gradient of ice thickness with distance is small, it should be easy to estimate bed reflectivity, because the mean attenuation rate has little effect on total attenuation. Variations in total attenuation only arise from variations in the attenuation rate, and the method the authors develop relies on an a priori model to correct for local variations in depth-averaged attenuation anyway.

See our earlier response to the reviewer's first major comment.

Lines 259-261:

Most readers probably know this already, but it still might be helpful to explicitly state that crevasse scattering is most likely to cause problems for radar analysis of basal conditions in fast-flowing regions near the ice sheet margin.

Good suggestion. We have added this point.

Lines 279-282:

Simplify this statement. This step corrects for the difference in attenuation rate between the measurement point and the central point.

We respectfully disagree with the reviewer on this point as line 279 describes exactly what equation (11) represents.

Lines 283-287:

Why are thinner ice columns warmer, and thicker ice columns colder? On the one hand, conductive cooling should tend to produce warmer conditions in thicker ice. Most people reading this paper would probably assume that thick=warm and thin=cold. On the other hand, the Peclet number ($Pe=ah/\kappa$) indicates that thicker ice columns should have a greater dominance of advection over diffusion, and therefore the cooling effect of surface accumulation should be greater in thick ice. In addition, ice must flow faster (with a higher driving stress) in thin regions, producing more shear heating. Finally, low surface elevations tend to have a higher surface temperature because of the atmospheric lapse rate, but this effect should not influence local temperature differences due to bed topography. Which one of these mechanisms is responsible for producing the thick=cold, thin=warm association?

As discussed in Section 3.5 of our paper, and Macgregor et al. (2015b), the depth-averaged attenuation rate and the depth-averaged temperature are proxy variables for each other, and it is in this sense we use the terms *`warm' and `cold'. An estimate for the spatial variation in the depth-averaged attenuation rate over the Greenland ice sheet is shown in Fig. 4(b). It is clear that, as a first approximation, the depth-averaged attenuation rate is proportional to ice thickness (e.g. Bamber et al. (2013), Fig. 3), and it is lower in the interior of the ice sheet where the ice is thickest. This suggests that surface temperature (and its dependence upon elevation), is the dominant `mechanism' that governs the spatial distribution of depth-averaged attenuation rate. This supports our general 'thick=cold, thin=warm' association. Finally, it is clear that this association holds over the spatial scale of our sample regions, (refer to Fig. 6 for the window vector plot).*

State the two criteria in words at the beginning of this section. Something like, "As a quality control check, we are looking for regions where (1) the correlation between ice thickness and bed-returned power is good, and (2) the correlation between ice thickness and bed reflectivity is poor."

This is a sensible suggestion that improves the clarity of the section. We would, however, argue that point (2) should be stated as `the correlation between ice thickness and bed reflectivity is poor relative to the correlation between ice thickness and bed-returned power'. As an aside, we did initially experiment with using the thickness correlation for [R∞] (the Arrhenius model estimate of the relative basal reflection coefficient), as a quality control measure, but we found the correlation ratio (14) to be more robust (in the sense that we have greater coverage for given solution accuracy).

Lines 349-350:

Why were the field seasons processed independently, if Lines 81-83 stated that power measurements from different field seasons could be combined?

This was done as we wanted to test if our attenuation algorithm was repeatable for different field campaign data/flight tracks (as described in lines 453-456) and the Supplementary Material. We have now stated this explicitly.

Section 3.2:

State the definition of "convergence" up front. The reader has to wait until the last paragraph (Line 415) to learn that convergence means "a normally distributed difference centered on zero".

We have changed `convergence' to `convergence (defined here as a normally distributed difference centered on zero)'.

Lines 413-421:

The first sentence of this paragraph should state that the algorithm converges in southern and eastern Greenland, but not northern or western Greenland. The reader should not have to flip back and forth to the figures to determine where the basin numbers are located spatially.

Done.

Line 437: "A gridded map of the basal reflection coefficient...is shown in Fig. 11a." Figure 11a does not look like a "gridded map". I realize that technically it is gridded at 1km cell size, but for all intents and purposes Figure 11a shows reflection coefficients along-track.

We have replace gridded map with ` map for relative reflection along flight tracks'.

Lines 504-508:

It would be appropriate to mention here that the relationship between attenuation rate and temperature is highly nonlinear, so the difference in depth-averaged attenuation rate does not transfer neatly to a difference in depth-averaged temperature. $\mathrm{mean}(x^2) \neq \mathrm{mean}(x)^2$.

We agree, the complicating effects of non-linearity should be emphasised here. We have replaced `non-unique' with ` non-linear and therefore non-unique'

Section 4: Conclusions

This section should have a paragraph commenting on and interpreting the reflectivity results. From Figure 11, it appears that high reflectivity is concentrated in the approach to fast-flowing outlet glaciers. This is consistent with distributed hydrological networks or with saturated subglacial till, either of which would promote faster sliding.

This suggestion was also made by our other reviewer and we have expanded Fig. 10 and Fig. 11 and Sect 3.3 to have more geophysical interpretation. In particular, we now compare the reflection map to a velocity map.

Line 542-543: "We suggest that the converged radar algorithm attenuation solution is preferable to using a forward Arrhenius temperature model to calculate basal reflection coefficients."

Strengthen and clarify this conclusion: "We find that our data-based attenuation algorithm is superior to an attenuation correction calculated purely from an a priori temperature model."

We agree; this is a conclusion that we draw. Our evidence for this is Section 3.2, where we show that the converged radar-inferred solution significantly reduces the thickness correlated bias for the depth-averaegd attenuation rate. However, as our algorithm has incomplete coverage (whereas the Arrhenius model solution has complete coverage), we have now restated the conclusion as:

'The converged radar algorithm attenuation solution provides a means of assessing the bias of forward Arrhenius temperature models. Where temperature fields are poorly constrained, and where the algorithm has good coverage, we suggest that it is preferable to using a prior Arrhenius model. This is due…'

Lines 546-548: "Notably, we demonstrated that even a small constant bias in the attenuation rate across a region; (this could be either with respect to a "true" value or another modelled value), leads to a thickness correlated bias in attenuation loss and therefore the basal reflection coefficients."

This sentence is awkward. Rephrase as: "We demonstrated that even a small regional bias in attenuation rate leads to thickness-correlated errors in attenuation losses and therefore the basal reflection coefficients. These thickness-correlated errors persist regardless of whether the regional bias is with respect to the 'true' value or to another modelled value."

Done – we thank the reviewer for making this point clearer.

Lines 562-564:

Is interpolation of bed reflectivity onto a regular grid even desirable, given that subglacial hydrology and geomorphology are likely to vary at scales much smaller than the grid spacing?

We agree that bed reflectivity will have sub-grid variability. However, the same is also true for ice thickness, and gridded basal topography data products are of widespread utility for ice-sheet modelling. Our hope is that a `coarse grained' bed-reflectivity data product (and the relationship to basal traction/basal sliding) could help to define the lower-boundary condition for ice-sheet models (see line 29 in the introduction).

Lines 566-569: "Due to this lower spatial variability, (and despite the caveats in the paragraph above), these regions [ice sheet interiors] could potentially have their basal reflection values derived by using forward Arrhenius temperature model for the attenuation."

See my major comments above. When ice thickness has little variability, errors in the regional mean attenuation rate have little effect. Only the spatial gradients in attenuation rate matter, and as the authors point out earlier in the conclusion, the models do a better job representing these than they do at representing the mean value. The authors should have been able to take advantage of this fact to produce reflectivity estimates in the ice sheet interior.

See major comments and the results of a forward Arrhenius model in the revised Appendix A.

Figures:

In general, the figures need better subplot titles and labeling. Symbols without words are inappropriate for subplot or axes labels because symbols are hard for readers to understand without flipping back and forth to the places where those symbols are defined. The subplot titles should express their meaning in words, and the corresponding symbols can be given in the caption if necessary. Many of the figures also need to be larger to permit more detail and wordier labels. Units should be placed on colorbar labels, not in subplot titles.

For most of the figures, I've given my suggestions for more descriptive titles and labels. The authors need not follow these specific suggestions, but all of the subplot titles should use descriptive words rather than symbols.

We again thank the reviewer for their detailed feedback, and have made the majority of the suggested changes. In particular:

- *Moving units above the color bars*
- *Using more informative labeling.*

<u>Figure1</u>

Subplot titles:

a) Flight Tracks

b) Drainage Basins

Put the numbers for the drainage basins in (b) arrayed around the coast of Greenland, rather than all together in the key. That way it is easier to tell at a glance which number refers to which basin. Also, it might be a good idea to circle or otherwise highlight the four basins in which the algorithm converges.

Figure 3

Subplot titles:

Y-axis label: What does “linear units” mean, other than “not decibels”? Either convert to actual units of power (W or Wm^{-2}), express as a fraction of the transmit power, or normalize so that the peak in each plot is 1. Normalization may be the best option, so that the quality control check (decays to 2% of peak power) can be easily visualized.

State where the two examples were taken from in the caption.

Units have been changed to `relative received power W’ (which follows the description in the CreSIS L1B data product.

Figure 4

Subplot titles:

a) Arrhenius Model for Attenuation Rate
b) Attenuation Rate from GISM
c) Difference between GISM and SICOPOLIS

The y-axis label in plot (a) should say the words “Attenuation Rate”. The colorbars should be labeled with their units (dB/km).

It might be appropriate to include a map for SICOPOLIS itself, in addition to the difference map.

Figure 5

subplots:
a) Model Estimated Attenuation Rate
b) Segments
c) Segment Approximation of Model Estimate
d) Difference from Central Value
e) Segment Approximation of Difference
f) Window Boundaries
The units should be given next to the colorbars, not in the subplot titles. The colorbars should have a larger font size as well.

The units should be given next to the colorbars, not in the subplot titles. The colorbars should have a larger font size as well.

Don't the square root and square cancel in plot (d)? Isn't that plot just showing the absolute value of the difference? The same comment that I made about lines 216 and 217 applies to the expressions in the caption. Either the averaging brackets are in the wrong place, or the expressions reduce to the absolute value of the difference.

See previous comment regarding lines 216/217

Figure 6

Subplots:

a) Vector R1

b) Vector R2
c) Vector R3
d)Vector R4

Colorbar label should be "Length (km)"

<u>Figure7</u>

Plot title: "Attenuation Difference Correction"

It is more accurate to refer to the process shown in this figure as the attenuation difference correction, rather than the attenuation correction. The step only corrects for the difference in attenuation rate between the data location and the central point.

<u>Figure 8</u>

Subplots:

a) Correlation Between Power and Ice Thickness
b) Correlation Between Reflectivity and Ice Thickness
c) Correlation Ratio
d) Coverage
e) Attenuation Rate
f) Attenuation Loss
Put units (dB, dB/km) in the colorbar labels.
Put plots a-c on the same color scale.

Note in caption whether high values or low values are good in a-c. In (a) and (c), high values indicate the algorithm converged, but in (b) low values indicate convergence.

Changed as suggested.

<u>Figure 9</u>

Subplots:

a) Difference between Model Inputs

b) Difference between Algorithm Outputs

c) Attenuation Rate Difference Distribution

d) Attenuation Rate Difference vs Ice Thickness

e) Attenuation Loss Difference Distribution

f) Attenuation Loss Difference vs Ice Thickness

Add units to the colorbar.

Label important outlet glaciers in either (a) or (b). Helheim Glacier is in view here, for example.

See revised section 3.3 and Figure 10 and 11 in the other reviewers' comments.

Note in the caption what the reader should be looking for in terms of convergence: in plots (c) and (e), a normally distribution about zero indicates convergence, while in (d) and (f), a lack of systematic ice thickness dependence indicates convergence.

Done.

<u>Figure10</u>

Plot title: Attenuation Rate

Label colorbar with the units.

As in Figure 9, label important outlet glaciers, including Helheim. Also put an inset box showing the area of detail in Figure 11.

We have labeled Helheim and Apuseeq glaciers (corresponding to the inset region for the reflectivity map).

<u>Figure11</u>

Subplots:

a) fine as is

b) Reflectivity Distribution

c) Reflectivity vs Ice Thickness

Put units on the colorbar label. Label the outlet glacier(s) in the lower right of (a). Note in the caption that a range of approximately 20 dB in (b) is right for plausible subglacial materials, and that a lack of systematic ice thickness dependence in (c) indicates algorithm convergence.

<u>Figure 12</u>

Subplots:

a) Attenuation Rate Difference Distribution b) Attenuation Loss Difference Distribution c) fine as is.

Note in the caption that green is a subset of red, which is a subset of blue.

Done.

<u>Figure 13</u>

Subplots:

a) Difference Between Prior Model and Radar Estimate (GISM)

b) Difference Between Prior Model and Radar Estimate (SICOPOLIS)

c) Comparison Between Models and Dye3 Ice Core

Add units to the colorbar labels.

This figure has now been substantially revised to incorporate two conductivity modelsfollowing the other reviewer's comments.

Supplemental Material:

Line 33: "...the sample regions will contain individual ice columns..." Replace "individual ice columns" with "grid cells".

Ice column is our preferred term, as the linear regression procedure applies to each (along-track averaged) measurement

Lines 43-45: "If this is rescaled by ice thickness for a sample region in the interior of the ice sheet (mean ice thickness ~2500m) this results in a desired attenuation rate accuracy ~1 $dBkm^{-1}$." This explanation is very simple and should go in the main text.

Agreed. This has been moved to main text.

Lines 69-70:

Mention that basins 3, 4, 5, and 6 are all located in south and east Greenland.

Done.

Figures:

Same comments as for the figures in the main text. All of these figures need better subplot titles and units labelled on the colorbars.

See our response to the main article figures.

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