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: Joseph Macgregor

We thank the reviewer for their very constructive and thoughtful comments. They have well understood both the motivation for our study and the methods developed. In order to address the reviewers' specific concern (about the choice of conductivity/attenuation model) we have completed a further investigation, and we describe this along with our proposed changes. We agree with the vast majority of the other suggestions and we provide detailed feedback to each point below, with our comments italicised in blue text.

This manuscript describes a new method for inferring full-thickness depth-averaged attenuation rates from radar data. It leverages existing techniques and reasonable assumptions regarding the nature of attenuation variability to better understand the Greenland Ice Sheet's attenuation structure. The authors then compare their radar-inferred values in Southeastern Greenland to two existing models to infer the spatial variation of their apparent temperature biases, which has inherent value for ice-flow modeling. The potential for this application to larger regions is considered.

The manuscript is novel, well structured, argued, illustrated and written. In those regards, I find little to fault. There are a few places in the method section where greater clarity is needed, and I think the nomenclature can be further simplified. I have a key specific concern regarding the attenuation model used, discussed below. While I wonder about the broader applicability of the method, given the breadth of data already available to the authors, the study as it stands is reasonably complete in terms of introducing and testing the method, and the well-considered Supplementary Material addresses some of these concerns. Repeatedly in the manuscript, where this is a question as to origin and motivation for approach, the authors present an effective justification and place it in context of their overarching goal. As it stands, the methodology presented is a clear advance over earlier techniques. It's not yet immediately applicable to all radar sounding data, but the authors do a superlative job of establishing present strengths and weaknesses. The comparison of temperature biases at DYE-3 and that inferred from the radar is particularly compelling.

The manuscript is clearly appropriate to the readership of The Cryosphere. The authors make a reasonable effort to extend the appeal of the manuscript beyond the relatively small audience interested in radar sounding analysis methods, by addressing the specific ice-sheet property (temperature) that can be constrained

---

Section 2.4: Following the nomenclature of MacGregor et al. (2015b), the authors select model M07 as their radar-attenuation model of choice. While I recognize that model as having a longer track record, MacGregor et al. (2015b) effectively deprecated that model in favor of a frequency-corrected version of the Wolff et al. (1997) model (W97-corrected). While reasonable people may disagree over the physical significance of the applied correction to W97, based on Figure 6 of MacGregor et al. (2015b) it is indisputable the W97-corrected model better relates radar-inferred and borehole depth-averaged temperatures than model M07 for the Greenland Ice Sheet. Hence, when relating the spatial variation in depth-averaged radar-attenuation rate to temperature, regardless of chemistry, the W97-corrected model ought to be used.

We agree that, in view of the results in Macgregor et al. (2015b), further investigation regarding the choice of conductivity/attenuation model is desirable to complete our study. Based upon our new results, which considers the W97-corrected model alongside the M07 model, we suggest the following changes to the manuscript.

(i) Revised Appendix A (choice of conductivity model).

Following the nomenclature of Macgregor et al. (2015b), we now outline the background to the W97 model (Wolf et al. 1997), and its frequency corrected form: W97-corrected. To the best of our knowledge, the W97-corrected model has not been used to calculate full ice column losses and basal reflection values. Subsequently, we propose a test for the validity of the different conductivity models, based upon the distribution for the Arrhenius model/estimated basal reflection values as a function of ice thickness. Strictly, this decibel range need not be thickness-invariant, but a strong positive/negative correlation would indicate a significant over/underestimate for the conductivity at the frequency of the IPR measurements.

We consider 4 conductivity models: (a) W97 (uncorrected), (b) M07, (c) W97-corrected (with empirical correction parameter=1.7), (d) W97-corrected (with empirical correction parameter=2.6). Model (c) is consistent with the frequency dependence observed in Paden et al. (2005), and model (d) is the best fit value from Macgregor et al. (2015b). Northern Greenland (basin 1 in Fig. 1b) is chosen to illustrate our results as the predicted spatial variation in attenuation rate is relatively low in this region, and therefore the effects of changing the conductivity model can be better isolated. The Arrhenius model estimates for the basal reflection coefficient as a function of ice thickness are shown below (Fig. A) using the GISM temperature field.

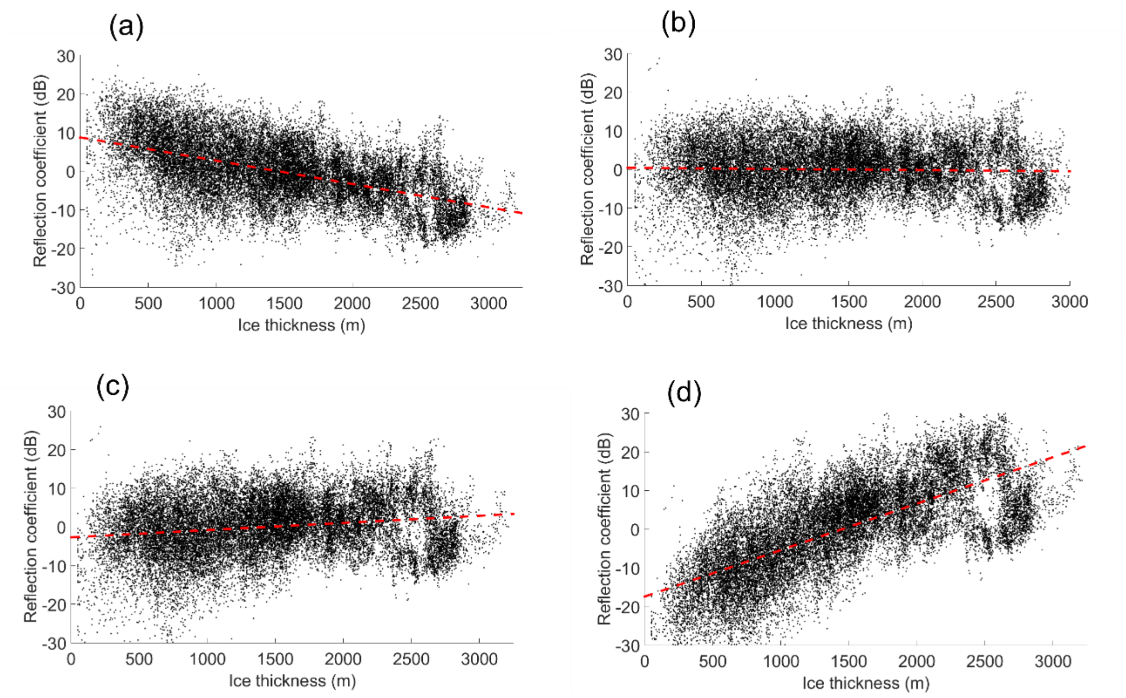

Fig. A. Arrhenius model estimate for basal reflection coefficient versus ice thickness for four different conductivity models. (a) W97 (gradient=-6.03 dB/km, r^2=0.29), (b) M07 (gradient =-0.29 dB/km, r^2=0.0009), (c) W97-corrected (1.7) (gradient=1.86 dB/km1, r^2=0.03), (d) W97-corrected (2.6) (gradient=12.02 dB/km, r^2=0.49). The negative and positive correlations in (a) and (d) are interpreted as significant underestimates/overestimates for the conductivity at the IPR frequency, whereas the near thickness-invariance in (b) and (c) are interpreted as good estimates.

Assuming a linear trend, the (uncorrected) W97 model has a negative correlation between modelled reflection and ice thickness. This is consistent with the conclusion in Macgregor et al. (2015b) that it significantly underestimates the conductivity/attenuation rate. Both the M07 model and W97-corrected (correction parameter=1.7), are near to being thickness invariant. We conclude that either of these models serve as suitable estimate for the attenuation rate/conductivity model used in our algorithm. W97-corrected (correction parameter=2.6) has a strong positive correlation, from which we infer that it represents a significant overestimate of the attenuation rate. Repeat analysis for other regions of the GrIS and using the SICOPOLIS temperature field confirm these conclusions, with the M07 model closest to overall thickness invariance.

(ii) Revised Sect. 3.5 (Temperature bias of ice sheet models)

We have now used the W97-corrected (correction parameter=1.7) model alongside the M07 model, when evaluating temperature bias. This enables us to highlight the sensitivity of the evaluation of temperature bias to the choice of conductivity model.

Our central conclusion, that our radar-inferred values are consistent with the model temperature bias at DYE 3, also holds for the W97-corrected(1.7) model alongside the M07 model. In recognition that temperature bias evaluation may be the aspect of our study that is of widest interest, we also now include an attenuation rate bias plot for the entirety of our converged region (basins 3,4,5,6), rather than just SE Greenland.

Additionally, following the investigation in Macgregor et al. 2015b, we also discuss how the inclusion of layer stratigraphy in the Arrhenius model could alter the result.

Sections 2.5 (including Figure 5) and the latter half of 2.6 were the newest and unsurprisingly the hardest concepts to grasp. I got the gist and am comfortable with the approach (the consideration of anisotropy in the horizontal gradient is an important advance), but I recommend additional review of the text in this section for accessibility.

We agree that Sect. 2.5 and Sect. 2.6 represent the two central method developments proposed in the paper. We propose that the following changes will improve their accessibility:

(i) We will make it clearer in Sect. 2.2 (overview of algorithm), that Sect, 2.5 and Sect. 2.6 represent the major original contributions in our paper. We will also state explicitly what problems they address (see also later comments about Figure 2).

(ii) We will move the more technical material/equations in Sect. 2.5 to an appendix (Appendix B). In the main text we now focus upon the key conceptual steps that enable us to constrain the anisotropic sample region.

(iii) We will highlight in Section 2.6 that equation (11) (which acts to standardise power for local attenuation variation within the sample region), is our major modification to the basic-bed returned power method. Additionally, we will make it clear that, if there is a thermomechanical model available, this correction could also be used in future regional studies (i.e. the method proposed in Sect. 2.6 could be applied in a regional study even if the sample region windowing methods in Sect. 2.5 are not applied).

Section 2.8: The gridding method used is never discussed. Based on the distortions present in maps in Figure 6 onwards, I can reasonably guess that a bilinear or natural neighbor gridding was used. While the gridding method is not a critical element of this study, I strongly recommend that ordinary kriging be applied instead. As it stands, a sub-standard gridding perhaps unintentionally diminishes from the results presented. Regardless, clarify the method used.

The gridding in our paper, is a simple cell average (1 km resolution) and this has now been stated explicitly in Sect. 2.8. This is done primarily for the convenience of plotting raster data.

Just to be clear, no gridding (other than the along-track averaging) is initially applied to the power measurements when performing the thickness versus power linear regression (this maximises the available data density). However, the `gridding' of the radar-inferred depth-averaged attenuation rate arises naturally from the scan resolution of the central point moving target window (set to 1 km). These points have also been emphasised in Sect. 2.8.

13-4, 345-6, Section 3.5: These statements and this sub-section seem to imply that this is the first time that the temperature bias of a major ice-sheet model has been evaluated using radar-attenuation estimates, when in fact MacGregor et al. (2015b) did exactly that with the steady-state ISSM instance from Seroussi et al. (2013), albeit not for the entire ice column. Reword

We have rewritten Sect. 3.5, to better acknowledge this aspect of the prior study, and how it provides much of the groundwork for our own study. Seroussi et al. (2013) has also been added to the references.

Minor comments
21: The use of the term "noticeable" here is somewhat odd. "substantial" would be better. The improved coverage is the result of the efforts of innumerable individuals over many years and is intentional.

Changed as suggested.

40: Clarify whether linear or dB units are meant when referring to the range of variability of attenuation rate.

'range' has been replaced with 'decibel range (~5-40 dB).

How about defining and using Delta B_infty instead? What is _infty meant to represent, anyway? It seems to originate on 167. I assume it references the highfrequency limit, but that's somewhat obscure and won't be obvious to most readers. I recommend to stick with nomenclature from earlier studies.

We suggest that the use of Delta B_infty here may potentially lead to more confusion as we also consider other `differences' in attenuation rate for the temperature field biases later in the paper.

We agree that the infinity notation is a bit obscure, and potentially confusing. In the context of our paper, the key point is that we are dealing with a prior/estimated quantity (not that the variable is a consequence of the high frequency limit). In order to distinguish the prior value from the radar inferred value with have now used a `^' notation for the prior/estimated value. This parallels the use of prior notation Bayesian statistics.

(12): What is the value of [S]? If dS/dh is assumed to be negligible, then it is irrelevant to dR_infty dh, right?

That is correct: dS/dh is considered negligible and (under this assumption) is therefore irrelevant to dR/d_infty. In order not to introduce unnecessary notation we have removed [S] from equations (8), (9) and (12), but mentioned explicitly that our method is `based upon the assumption that the variation in radar system performance with respect to ice thickness (within a field season) is considered negligible.'

(14): Come up with a more meaningful subscript title than "_{ratio}".

After much consideration we cannot think of a clearer label. We have reinforced that strong relative correlation in dP/dh with respect dR/dh (which follows as a requirement for an accurate/unbiased solution of the radar equation) is represented by this `correlation ratio'

3. Results and discussion: It would be better to separate out results from discussion, as the current structure as labelled is uncommon. However, I find the text quite readable in this section so perhaps this is a non-issue.

In an earlier version of the manuscript, we did originally separate the Results and Discussion sections. However, we decided that an ongoing discussion of the results was the best way of describing our new algorithm.

503-4: Here I think "As was proposed by MacGregor et al. (2015b)," can be dropped, as the following statement is generally accepted regardless of that earlier study.

Changed as suggested.

555: I don't understand what the qualifier "final" adds here. It implies that the future products that will be generated by this method will not need to be improved, which is a bit strong.

We agree. ` Final' has been removed.

Appendix A: I don't mind this appendix but it closely hews to earlier studies. For brevity, it could be dropped.

We have now edited the existing content, and extended the content of Appendix A to discuss the different conductivity models (see earlier our response).

580: Better to cite MacGregor et al. (2012) instead of MacGregor et al. (2007) here as the former provided the correct form of the equation shown.

Done.

585: M not uM for chemical impurity units, following the dimensions given for the other parameters.

We agree. For brevity, (and as we have now expanded Appendix A to focus upon the conductivity models), we now reference the summary table for the dielectric parameters in Macgregor et al. (2015b).

Figures

For at least one Greenland-wide map, perhaps Figure 4c, it would be helpful to have a box representing the focused SE study area of Figure 9 onwards

An outline map with box has been added to Fig. 9 where SE Greenland is introduced as our example region.

Figure 2. Use a legend rather than the caption to explain the coloring. I find this diagram helpful but would like it to be expanded, although I'm not exactly sure how. Perhaps including related figure numbers (thanks for including section numbers) and, more ambiguously, the question/challenge each algorithm step addresses.

Colour coded boxes have now been added along with references to figures. The accompanying text that explains the steps of the algorithm (Sect 2.2), has also been made explicit to show the questions that we are addressing.

Figure 7: Show best-fit lines also.

Good suggestion. This adds clarity to the argument about the systematic underestimation in attenuation rate that is predicted to occur without the correction.

Figure 9: This figure's panel titles and legends are good examples of where the nomenclature needs some simplifying. Table 1 helps but is not quite enough.

We have now used variable descriptions in words rather than symbols for the figure titles (and listed the symbols in the caption).

Figures 10 and 11 could be combined into a 2x2 panel. Either way, identify the subregion for Figure 11 in Figure 10.

We agree. Figure 10 and 11 have been combined, identifying the sub-region.

Figure 11: Is the quantity shown in panel actually reflectivity, as is in more common usage? A grayscale or some other color illustrating ice velocity beneath the bed values would be nice (e.g., Figure 1 of Jacobel et al., 2010, The Cryosphere).
Grammar, etc.

Strictly, the quantity is the reflectivity based upon the depth-integrated power, as described in lines 258-259 of our manuscript, and referred to as P_adjusted in Oswald and Gogineni (2008). This quantity represents the sum of specular and diffuse reflection/scattering components, and is therefore predicted to have a narrower dB range for reflectivity than the peak power definition. In order to make this explicit we have changed: `basal reflection coefficient' to `basal reflection coefficient using depth-integrated power' in the Fig. 11 caption. We have also added an extra sentence to Section 3.3 describing that the integrated measure is predicted to reduce the overall dB range.

We have now underlain the reflectivity map with balance velocity. We observe regions of faster flow where basal reflection values are higher.

Regularly and particularly in the introduction, MacGregor et al. (2015a) is cited where I believe MacGregor et al. (2015b) is meant to be cited.

Apologies; these citations have now been corrected.

2: ice sheet 6: englacial 34: subglacial 90: missing parentheses around citations 55: tolerance 281: a hypothetical ice column 441: 63% 544: is present 547: a 'true' Table 1 title: principal

All suggested changes have been made. 'ice-sheet' is now change to 'ice sheet' throughout the article.