The Cryosphere Discuss.,
doi:10.5194/tc-2016-8-RC1, 2016

The Cryosphere Discussions

Interactive comment on "An ice-sheet wide framework for englacial attenuation and basal reflection from ice penetrating radar data" *by* T. M. Jordan et al.

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Received and published: 11 February 2016

Review of "An ice-sheet wide framework for englacial attenuation and basal reflection from ice penetrating radar data" by T.M. Jordan et al.

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Summary

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-

derived results 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.

Major comments

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.

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.

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.

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.

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.

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

(11): 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 high-frequency limit, but that’s somewhat obscure and won’t be obvious to most readers. I recommend to stick with nomenclature from earlier studies.

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

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

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

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.

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.

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

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.

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

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.

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.

Figure 7: Show best-fit lines also.

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.

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

Figure 11: Is the quantity shown in panel a 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.

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

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

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