Review: “An ice-sheet wide framework for englacial attenuation and basal reflection from ice penetrating radar data” . Second Review.

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

Review by: Mike Wolovick

Summary of Changes:

The authors have changed the emphasis of the paper to focus on englacial attenuation in addition to basal reflectivity. They have improved the clarity of the figures and made other minor changes.

Response to Major Comments:

In my previous review, I had two major comments. The first was that the authors could extend the scope of their results to include basal reflectivity anomalies in the ice sheet interior. I suggested that, because the interior of the Greenland Ice Sheet has little variability in attenuation loss, the authors could constrain basal reflectivity anomalies even if they could not also constrain attenuation rate. Rather than expand the scope of their results, the authors refocused the emphasis of the paper onto constraining attenuation rate, and therefore diagnosing the biases of ice temperature models. This response is acceptable, if unsatisfying. There are few independent constraints on ice sheet thermal structure, other than sparsely distributed ice cores. The addition of full-depth temperature constraints from widely distributed radar data greatly expands the area over which we can constrain ice sheet thermal structure. As such, the advance presented by the authors is worth publishing on it's own, even if I would have liked to see them present reflectivity results in the ice sheet interior and compare those results with previous studies that did not use prior thermal models.

The second major comment I made was to express concern about the segmentation approximation the authors used to select a local sample area. I felt that the segmentation approximation was arbitrary and overly complex, and I suggested that the authors explore the sensitivity of their method to other means of selecting local sample areas, although I also stated that it was likely that any reasonable method of selecting a local sample area would produce similar results. In response, the authors included language stating that an irregular contiguous region would be preferable to a segmentation approximation, but declined to implement such a method because of

computational constraints.

I am inclined to allow the authors' manuscript through on this round, but only because I attempted to write a script that computed irregular contiguous sample regions myself. I found that the algorithm itself is extremely simple; however, the difficulty arises because the resulting irregular regions are *too* irregular, to the point that they would probably produce unreliable results if implemented in the authors' method. The key bit of code that actually computes the irregular region (lines 76-100 in the attached script) is only 13 lines of Matlab code (excluding comments and without using ellipses to break up one long line of code into multiple lines). If you have access to the image processing toolbox, "imfill" does the same task in a single command. However, computation time is about 5 seconds per grid cell, which is unacceptable when there are $\sim 10^6$ active grid cells. In addition, the resulting sample regions often have highly unusual shapes and tend to be elongated parallel to the coast. If I did not impose a maximum area, some of the sample regions would form rings around the whole ice sheet margin, because attenuation rate tends to be higher around the edges of the ice sheet. Obviously, it is not glaciologically reasonable to include opposite sides of the ice sheet in the same sample region. I still do not believe the the segmentation approximation is the optimal solution to this problem, but given the difficulties I encountered when attempting to implement an alternative, I now think that the authors' method is better than I originally gave them credit for.

Minor Comments:

Line 18:
I still think that these two references should be replaced by older ones. I realize that the "e.g." is meant to imply additional uncited references, but radioglaciology did not start measuring ice thickness during the Obama administration. This place in the introduction is where you should give the audience a sense of the broader historical context of your work. Bailey et al., [1964] and Evans and Robin [1966] are more appropriate here. Besides, Fretwell et al., [2013] and Bamber et al., [2013] are referenced later in the paragraph.

Line 25:
The reference to Morlighem et al., [2014] is still incomplete.

Line 47:
dB should be dB/km

Lines 312-313:

I am glad that you took my suggestion to state the quality control criteria at the beginning of this section. However, this sentence is still very unclear. It can be clarified be using words in addition to symbols: say "(i) a strong correlation between bed-returned power and ice thickness ($d[P^C]/dh$) and (ii) a weak correlation of reflectivity and ice thickness ($d[R]/dh$) relative to the correlation between power and ice thickness ($d[P^C]/dh$)." Using only symbols makes this sentence extremely opaque.

Line 395: "(defined here as...)"

Clarify the wording in the parentheses by saying "(convergence is defined here as...)".

Lines 454-458:

I am glad you have added geophysical interpretation to your results section.

Line 610:

I'm not sure I agree that the roughest topography in Antarctica is found around the margins. The Siegert et al. paper was published before the Gamburtsev Mountains were surveyed in detail, for example.

Figures:

I appreciate the improved titles and labeling on all of the figures. I would have liked it if Helheim was labeled in addition to Apuseeq, as a much higher percentage of the audience will have heard of Helheim.

Supplement Lines 35-36:

These lines still have "stationary" instead of "constant" (although I'm glad you made the change in the main text).

Supplement Line 76: "Greenlan"