

Interactive comment on “A constraint upon the basal water distribution and basal thermal state of the Greenland Ice Sheet from radar bed-echoes” by Thomas M. Jordan et al.

Anonymous Referee #2

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Summary: This paper uses a RES diagnostic the author's term "bed-echo reflectivity variability" for the long archive of RES observations over Greenland to get at the distribution of basal water. They then proceed to compare this to various prediction for the distribution of subglacial water. It is comprehensive and thorough, however I cant help thinking its being presented as a lot more sophisticated than it actually is. High passing radar data has been a (justifiable) refuge of radioglaciologists since the C-130 TUD days, and thats basically what seems to be happening here - just in slow time rather than fast time.

Major issues: Novelty: Bed echo variability has been long used for characterizing the

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basal interface (Neal, 1982; Peters et al., 2005; Carter et al., 2007), and the surface interface (Grima et al., 2014) - most of this literature is not mentioned from this context. For the most part, bed echo peak power variability has been used to indicate interface roughness. The authors here extend to very long length scales, and integrate in fast time over the echo to suppress roughness effects in an attempt to essentially map out subglacial water using an assumption of bimodal wet/dry distribution, to get around variability in attenuation that will inevitably bias absolute values.

Edge detection: The approach I feel is misnamed. From Figure 2 it seems clear that large scale changes dominate their analysis, and thus basically what the authors have is an edge detector. What they are finding is not so much variability as gradients. In order to get at the small scale variability indicated in figure 5b, they would have to high pass the data, which they are not doing. A multi-scale approach may be more productive to get at mixed media cases.

Statistics in dB space: I am concerned at the application of statistics in dB space, and think this needs to be better motivated. Due to the compression of the distribution of the echoes using the attenuation model, and the highly bimodal reflectivity of the bed, they 'get away with it' somewhat; however, I attach an jupyter notebook that attempts to illustrate the complexities of doing the variability statistics in dB using a synthetic fractal distribution (again, the hypothesized distribution will be more bimodal, but there will be a sensitivity long wave length errors in attenuation) and a bimodal distribution.

Calculation of sigma: It wasn't clear if you are taking the deviation in power of points separated by 5 km, or just taking the standard deviation of all points within the 5 km window.

Ice surface transmission losses: Surface losses due to roughness or near surface englacial water are not considered. I think for this paper they could be important, as they do not correlate with the predictions for whole ice sheet attenuation, and have the potential for sharp gradients. Surface and near surface losses should be addressed,

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maybe just by a demonstration that they are negligible.

Minor issues: Data traceability: The authors need to emphasize that these data are from the CSARP processor found on the KU website, and NOT the MDVR processed data found on the NSIDC website. The latter should not be used for quantitative analysis of the bed echo. NSIDC says this in their website, but a disclaimer here might help head off confusion.

Along track processing: There is very little detail on how azimuth processing has changed over time, and how this could effect the results. Also not clear: did the author do along track incoherent averaging as they did for earlier papers?

Power determination approach: The method for extracting aggregate power, and rejecting bad echoes, appears to have changed between Jordan et al 2016, 2017. In those works, a symmetrical window about the peak is taken, while in this work, a 10 dB threshold down from the peak power is used. A 10 dB SNR is used here, while in earlier work it appears to be a 17 dB threshold is used. The change needs to be better justified, and the opportunity taken to explain if there is any impact of the results of the earlier papers.

Figure 2: I suggest the authors reverse the order of intensity (ie have sigma solid and black, and have the power a lighter line). I also suggest adding the 6 dB threshold to the second row.

Figure 8c: I think that Martos's data does need to be considered from the POV of the input magnetic track lines, especially from this sort of comparison.

Please also note the supplement to this comment:

<https://www.the-cryosphere-discuss.net/tc-2018-53/tc-2018-53-RC2-supplement.zip>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-53>, 2018.