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Interactive comment on "Active Layer Thickness Estimation from X-Band SAR Backscatter Intensity" *by* Barbara Widhalm et al.

Anonymous Referee #1

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Widhalm et at. [2016] relate x-band backscatter to in situ measurements of Active Layer Thickness (ALT). They found the backscatter correlates well with ALT for ALT greater than about 70 cm, but not so much for smaller values of ALT. Essentially, the X-band backscatter measures the vegetation characteristics that control or influence ALT, such as vegetation height. The idea is new, interesting, and shows potential for mapping ALT over larger areas. However, the paper will require major revisions before it is ready for publication. I have several major comments: 1) The authors need to incorporate more physical interpretations of volumetric scattering and vegetation characteristics as they relate to ALT. The authors emphasize the technique, but if they want to capture the imagination of this journal's readership, they must expand the physical interpretation. We know vegetation influences ALT based on probing data and plant type, but these backscatter measurements offer direct, pixel-by-pixel measurements of the vegetation

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physical characteristics associated with ALT variability. How does this effort relate to efforts to use backscatter to measure biomass? What physical characteristics drive the backscatter strength: vegetation height, total biomass, or leaf density? How do these characteristics relate to the thermodynamics that drive ALT? This is the true science advancement of this paper. 2) The authors must include the exact mathematical form of the curve fits, the regression coefficients, and associated uncertainty. Others cannot reproduce these results without the form and coefficients. The uncertainty is absolutely essential to interpreting the results. 3) The authors should include a map of ALT and uncertainty for the entire X-band scene, not just the CALM sites. This is what the readers and I really want to see. Such maps are more useful and interesting to the readers than the curve fit plots. I recognize the technique does not work as well for ALT < 70 cm, but this is fine as long as the uncertainty reflects this. 4) Drop all functions except the linear curve fit. Figures 5 and 6 clearly indicate no statistical difference between the three functions within the data range. Using three curve fits adds volume, but not value to the paper. I know of no theoretical basis for choosing any one of these functions over the others, so I recommend the authors stick with the simplest form: the linear curve fit. 5) Drop the comparison to NDVI. The backscatter technique does appear more robust than the NDVI technique, but I am not sure how much value this adds to the paper. The NDVI method is only one of several methods identified by the authors, and definitely not the best, so I do not see why the authors focus on this particular technique. Besides, the stated goal was to evaluate backscatter, not compare with the NDVI method. 6) Rewrite the manuscript using the active voice. Readers find the passive voice difficult to follow. Minor Comments: P1, L3, Table 1; P2, L16; P2, L23-4: The techniques listed in Table 1 are not 'limited' to ALT < 70 cm. The wording implies the techniques do not work for deeper active layers, which is not true. For example, Schaefer et al. [2015] and Gangodagamage et al. [2014] estimated similar ALT because that is the actual ALT around Barrow, AK. Other studies using similar techniques measured ALT > 70 cm in different areas, such as Liu et al. [2012] around Prudhoe Bay and Pastick et al. [2013] around Yukon Flats. P1, L16: The authors need

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more recent and comprehensive references for carbon emissions, such as Schaefer et al., [2011] or Schuur et al. [2013]. P1, L4-5: Although this may be true for Yamal, it is not true in general. All the papers in Table 1 as well as others not included show spatial patterns of ALT. P2, L6-26: The authors need to rewrite this section with a more logical flow of ideas. The authors need to clearly separate modeling techniques to estimate ALT from the upscaling techniques based on probing. They start out with the Stefan model and Kudryavtsev models, but suddenly switch to Gangodagamage et al. [2014], who used a probing data to scale up lidar data, a technique that is remarkably similar to what the authors describe with backscatter. Then they switch to InSAR, then to soil moisture, and back to upscaling probing data. P2, L31-4: The authors need to explain why they think backscatter relates to ALT. The authors state their hypothesis, but do not explain why backscatter should relate to ALT. What have people done in the past relative to backscatter and biomass in permafrost regions? How does biomass relate to ALT? How is x-band an advantage? Why not use C-band or L-band? P3, Fig1: This figure should outline the X-band scenes used in the study. P4, Tab1: The authors should identify the general area of the studies, such as Barrow for Gangodagamage et al. [2014]. The text should explain that all these studies except Schaefer et al. [2015] used regressions of ALT against a remotely sensed characteristic to scale up probing data. The exception is Schaefer et al. [2015], who used InSAR to measure seasonal subsidence and did not use probing regressions. P6, L5; P8, Fig2: The authors need to explain this figure, which I did not understand. One sentence is not enough. How did the authors use this data? P6, L26-31: The authors should delete this paragraph. TanDEM-X data can be used for InSAR, but that is not what the authors did. The authors essentially created regressions of ALT vs backscatter, not InSAR. P7, L23-4: Why would a relationship between NDVI and ALT indicate a relationship between backscatter and ALT? P7, L27 to P8, L2: The authors need to explain the technical terms associated with SAR that the broader audience of the Cryosphere will not understand. They need to define: Range Doppler correction, radiometric normalization, sigma-zero (the primary parameter), speckle, and near neighborhood. The average

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permafrost scientists will have no clue what these terms mean. P8, L6: What ALT values? From the CALM sites? Later the authors use the same ALT to 'validate' the regression, which seems circular. P8, L4-5: The authors need to explain how and why they choose these classes. P8, L7-8: Stick to the linear function, as I state above. Show the exact form here. P8, L20: The authors need to explain figures 3, 4, and 5. One sentence each does not suffice. P9, L7-8: The authors must explicitly define 'coefficient of determination in the methods section, exactly how they calculated it, and what it means. P8, L8: The authors need to estimate the ALT uncertainty as a function of backscatter. RMSE is OK, but we really need an uncertainty estimate. P11, L7-8: The authors need to explain why September would differ from August in terms of backscatter. I strongly suspect that leaves have fallen off the plants and the ground surface has started to freeze, altering the backscattering characteristics. P11, L14-5: Please explain the 'restrictions of the used approach.' P11, L19-20: The moisture content will definitely contribute to backscatter, but the authors need to explain how. The authors should identify the expected penetration depths for dry and wet tundra. P 13, L9-11: The backscatter technique performs better than the NDVI technique, but this does not support or refute the initial hypothesis that you can use backscatter to estimate ALT. I suggest deleting this. P13, L12-4: I agree that you can scale this technique to larger areas and suggest you add a map of ALT for the entire x-band scene. This is what the readers really want to see.

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