The Cryosphere Discuss.,
doi:10.5194/tc-2016-168-RC2, 2016

The Cryosphere
Discussions
Open Access
EGU

TCD

Interactive comment

Interactive comment on "Mapping snow depth within a tundra ecosystem using multiscale observations and Bayesian methods" *by* Haruko M. Wainwright et al.

Anonymous Referee #2

Received and published: 16 November 2016

The authors explored a number of methods for estimating end of season snow depth distribution in an Arctic ice-wedge polygon tundra environment: in situ snow depth probe measurements, ground-penetrating-radar (GPR), and Photogrammetric Detection and Ranging (PhoDAR) technique with an unmanned aerial system (UAS). The GPR and PhoDAR methods were compared and evaluated using the in situ snow depths, and relationships to topography using a 0.5 m high resolution DEM were explored. Results indicate that complex microtopography strongly controls the distribution of snow in an ice-wedge polygon environment. The topographic control of snow distribution in a tundra environment is not a novel result, however, the GPR, PhoDAR and use of a wavelet transform on a high resolution DEM in a tundra environment to de-

Printer-friendly version

Discussion paper

termine these relationships are unique. A Bayesian geostatistical estimation method was utilized to integrate the local snow depth measurements (GPR and in situ), with the high resolution DEM data (LiDAR) to produce a snow depth product for the entire extent of the DEM study area, taking into account correlations with topography. Independent evaluation of the Bayesian estimated snow depths showed a high level of accuracy.

The use of GPR, PhoDAR and Bayesian approaches are novel methods for estimating snow depth in a tundra environment. All three methods produce very good RMSE values compared to in situ snow depth probe measurements and show great promise for application in future campaigns/projects. The manuscript is generally well written, but often lacks details concerning steps taken in processing/analysing the data and/or quantitative statistics to back-up the author's statements. The content of the paper is a useful addition to the literature, providing background on new methods for mapping snow distribution in a tundra environment, and as such is relevant for publication in The Cryosphere. However, the lack of detail or clarity on steps taken in the analysis would make it very difficult for other researchers/scientists to replicate or modify the methods presented. Therefore, I recommend publication of this manuscript following the major revisions suggested in this review:

General Comments:

The authors compare many different methods for measuring and estimating snow depth in a tundra environment over several years. An impressive amount of data collection/processing was involved in preparing this manuscript. However, due to the volume of data and the variety of different measuring methods used over several years at different sites/plots there is often confusion about which in situ measurements are being used to predict/validate the various methods compared for estimating snow depths. - More detail should be provided to describe which in situ snow depth measurements are being discussed. Please be specific about which snow depths, from which years, from which sites, etc.

The authors describe the controlling influence of the microtopography on snow depth distribution and highlight the variability that is present at the meter-scale. However, there is minimal discussion concerning the errors/uncertainty associated with each of the GPS positioning information for all of the various data being compared (DEM LiDAR, GPS Magna Probe Snow Depths, Non-GPS tile-probe snow depths, Non-GPS GPR snow depths, and the interpolated GPR depths, and the georeferenced UAS PhoDAR derived snow depth maps). -More detail needs to be provided on both the horizontal and vertical positional uncertainty of all datasets used in this analysis. A discussion needs to be added to the manuscript on how GPS positional errors, or lack of GPS information (tile-probe, GPR measurements), were taken into account and how these errors can affect the agreement (RMSE) between measured/estimated snow depth products in an environment where meter-scale microtopography controls the snow distribution.

The authors use an extensive survey of Ground Penetrating Radar (GPR) measurements to produce interpolated snow maps for several study plots (A,B,C,D) within the full study domain. The interpolated snow maps produce realistic patterns of snow depth with high accuracy (RMSE of 2.9 cm OR 5.4 cm non-filtered data). The authors note that careful attention must be made regarding the estimation of radar velocity and associated errors due to positioning to be able to produce accurate snow depth maps, but provide very little detail on the correction and technical processing of the data used in this analysis. They acknowledge that snow density affects radar velocity, but assume a consistent speed through the snowpack. -More details should be provided on the GPR system and associated post-processing including: Near field effects: The literature suggest that GPR does not collect good data near the surface down to a depth of about 1.5 times the centre wavelength - 500 MHz wavelength = 60cm x 1.5 = 90cm snow depth! - What about echoes from snow layering – ice crusts/depth hoar layers? - What methods/software are used to pick the top/bottom of the snowpack - Discuss post-processing for: changing topography? – need to warp the data in the vertical direction so that reflection horizons are not distorted? forward velocity variations? –

need to dynamically stretch or compress in the horizontal direction after data collection to correct for any significant variation in forward velocity – Typically corrected via attached wheel odometer, however wheel odometers have issues over uneven snow and ice features - Provide more detail on how the GPR data were linked to in situ snow depth probes for validation. Both datasets did not have GPS positional information. What methods were used?

The authors describe a unique method of using the Photogrammetric Detection and Ranging (PhoDAR) technique with an unmanned aerial system (UAS) to derive high resolution snow depths in a tundra environment. However, the authors needs to be more consistent in the terminology they use when describing these data: UAS-based snow depths, UAS-based PhoDAR, UAS data, UAS derived snow depth, UAS and PhoDAR, UAS methods, UAS approaches, UAS are all used interchangeably. The authors should use the term PhoDAR, rather than UAS when describing these data. UAS does not describe the specific technique used to derive the estimates, and is also used to describe a UAS-based LiDAR acquisition technology. Please choose a terminology and make changes throughout the text.

One of the main results of this study confirms that changes in complex surface topography influence snow distribution in a tundra environment. They identify regions with complex topography (ice-wedge polygons), as areas with high topographic variability using two different methods: 1) calculated elevation difference (range in elevation) using a 0.5 m DEM, in a 1 m radius surrounding snow depth measurements (in situ and GPR). 2) A wavelet transform method is applied to the 0.5 m DEM. The output high-pass image from this transform produces what the authors call 'microtopographic elevation'. The wavelet transform method for identifying topographic variability in a tundra environment is a novel approach that to my knowledge has not been done before. However, very little detail is provided by the authors on what exactly the microtopographic elevation units represent. Both positive and negative numbers are listed on the y-axis of Figure 5c. There are also no comparisons between these two measures of topographic variability for identifying complex terrain or discussion on the use of two distinct methods. - Can the authors provide a direct comparison of these two measures of topographic variability? Can they provide justification for using two different methods? Should only one method be used for consistency?

Specific Comments:

Section 1: Introduction

Line 76-77 – Please check the definition of microtopography. It is a term used to describe the study of surface features on a very small scale. Therefore, the soil freeze/thaw processes do not lead to microtopography. The authors need to qualify this term with an adjective such as complex. Please check all references to microtopography and macrotopography throughout the paper to ensure proper use of the term with appropriate adjectives.

Line 83-86 – Fragmented sentence – Please put this sentence in context, i.e. related to investigating controls of snow on ecosystem properties.

Line 86-89 – Awkward/vague sentence – please rephrase

Line 91-92 – The authors should differentiate between alpine and Arctic tundra, as both LiDAR and GPR have been used in alpine environments. Additional references should also be added, as much work has been published on characterizing Arctic tundra snow. Suggestion: 'Snow depth characterization in Arctic tundra environments have traditionally been observed using snow depth probes (Benson and Sturm, 1993; Hirashima et al., 2004; Derksen et al., 2009; Rees et al., 2014; Dvornikov et al., 2015), or modeled using terrain and vegetation information (Sturm and Wagner, 2010; Liston et al., 1998; Pomeroy et al., 1997).'

Additional References: - Sturm, M., and A. M. Wagner (2010), Using repeated patterns in snow distribution modeling: An Arctic example, Water Resour. Res., 46, W12549, doi:10.1029/2010WR009434.

- Hirashima, H., Y. Kodama, N. Sato, T. Ohata, H. Yabuki, and A. Georgiadi, 2004: Nonuniform Distribution of Tundra Snow Cover in Eastern Siberia. J. Hydrometeor., 5, 373–389, doi: 10.1175/1525-7541(2004)005<0373:NDOTSC>2.0.CO;2.

- Derksen, C., A. Silis, M. Sturm, J. Holmgren, G. Liston, H. Huntington, and D. Solie, 2009: Northwest Territories and Nunavut Snow Characteristics from a Subarctic Traverse: Implications for Passive Microwave Remote Sensing. J. Hydrometeor., 10, 448–463, doi: 10.1175/2008JHM1074.1.

- Rees, A., English, M., Derksen, C., Toose, P. and Silis, A. (2014), Observations of late winter Canadian tundra snow cover properties. Hydrol. Process., 28: 3962–3977. doi:10.1002/hyp.9931

- Liston, G.E., and M. Sturm. 1998. A snow-transport model for complex terrain. Journal of Glaciology. 44: pp 498-516.

- POMEROY, J. W., MARSH, P. and GRAY, D. M. (1997), Application of a distributed blowing snow model to the Arctic. Hydrol. Process., 11: 1451–1464. doi:10.1002/(SICI)1099-1085(199709)11:11<1451::AID-HYP449>3.0.CO;2-Q

Line98 – Spelling error

Line 100 – Replace ‘at site or’ with ‘local and’

Line 98-103- These references are for LiDAR-based studies only. Can you provide references for the structure-from-motion algorithm and use of PhoDAR for snow depth estimation?

Line 104 – Reword – Suggestion: ’While there is potential for providing detailed information about local-scale snow variability using LiDAR and PhoDAR snow depth estimates, these techniques have not been extensively tested in ice-wedge-polygonal tundra environments.’

Line 108 – Please state ‘increased snow depth uncertainty’

Printer-friendly version

Discussion paper

Line 109 – If you are going to use the term point probe to refer to all ground-based snow depth probe measurements – be consistent throughout the text.

Line 108-117 - The authors discuss snow depth uncertainties associated with indirect measurement techniques. This is a good paragraph to acknowledge and provide details on the uncertainties associated with snow depth probe measurements (all types of probes), especially in a tundra environment due to over-probing into the organic subsurface.

Additional Reference: Berezovskaya S, Kane DL. 2007. Measuring snow water equivalent for hydrological applications: part 1, accuracy of observations. In: Proceedings of the 16th International Northern Research Basins Symposium and Workshop. Petrozavodsk, Russia http://resources.krc.karelia.ru/krc/doc/publ2007/SYMPOSIUM_029-35.pdf

Line 121 – insert comma after multiscale, - This correction should be repeated at Line 134 – check entire paper.

Line 127-128 – The term 'integration methods' is vague. Distributed blowing snow models could be considered an integration method. – Reword Suggestion: 'To our knowledge, Bayesian methods have not been developed...'

Line 130 – Change 'UAS' to 'PhoDAR'. UAS can also be used to record LiDAR snow depths.

Line 132 – are objectives 2 and 3 the same?

Line 135 – Change 'the LiDAR domain' to 'a LiDAR DEM covering an ice-wedge polygonal tundra landscape'

Section 2: Data and Site Descriptions

Line 146 – Can you provide a reason/explanation for defining a study site of 750m x 700m? Extent of DEM?

Line 148 – Insert NGEE-Arctic acronym

Line 156 – Vague sentence. Current references (Sturm and Dvornikov) imply that these works confirm that no such plants exist within this study site – Suggestion: There are currently no tall shrubs or woody plants established within the study site, therefore complex topography is most likely to control the snow depth distribution within the study domain (provide references that complex topography can control snow depth).

Line 171-172 – Change 'providing' to ', and were used to produce a high-resolution. . .' Additional LiDAR DEM details are provided in Hubbard et al., 2013 – horizontal accuracy 0.3 m and vertical accuracy 0.15 m should be stated in this paper, as this relates to aligning/matching the various geospatial data used in this paper

Line 173 – Change 'resolved' to 'resolves'

Line 175-176 – How many RTK DGPS measurements were used to evaluate the LiDAR DEM? Were they obtained across the whole study domain? What is the positional accuracy of the RTK DGPS (vertical and horizontal)? Was the LiDAR horizontal accuracy 0.3m stated in Hubbard et al., 2013 considered when computing the RMSE between the RTK DGPS and LiDAR DEM? The RMSE of 6.0cm is within the 0.15m vertical accuracy of the LiDAR DEM – which is good!

Line 180-181 – To avoid confusion with the transects measured within the study plots (A,B,C,D)– please refer to the three long transects that traverse the study domain as the '500 m transects'. Please make these changes throughout the paper. It is stated that snow depths were measured along transects, but no details on which probes were used, and which sampling methods were employed (intervals) and the total number of depths recorded along these lines are provided.

Line 185 – The term accuracy should be changed to precision. Also a reference should be provided. What about the horizontal positional accuracy of the GPS snow probe measurements (references)?

Line 186-187 – Which transects? Even the 500 m transects? Fine Grid? Coarse Grid? GPR transects? All of them? How did you 'correct point measurements'? A simple XY shift for each individual transects? Was it a single shift for all depths across the study domain? What are the horizontal uncertainties associated with the 'correction'?

Line 189-190 – Please change the term accuracy to precision of 0.01m.

Line 191-192 – The 'fine-grid' total snow depths were reported as single dataset = ∼7200. For consistency, report the 'coarse-grid' snow depths as a grand total as well, rather than a per/grid number. I also did some rough calculations for estimating the total coarse-grid measurements: Five ∼160m survey transects at 5m intervals only equals ∼ 160 snow depths per grid. The authors state 380 snow depths were sampled per grid. If at equal intervals the distance would be ∼2.1 m between depths? Was the spacing of the tile probe snow depths measured by survey tape as hinted at by Line 200, or only roughly estimated? Please provide further details on the accuracy of the GPS positioning of the tile probe/GPR measurements to provide context on how they were linked to the LiDAR DEM.

Line 194-195 – See comment for Line 180-181 – Please refer to the three transects as the '500 m transects'.

Line 197 – How were they co-located? GPS? RTK DGPS? Were the unique ID numbers associated with particular GPR measurements noted and related to snow depth measurements? Please provide more details, and a measure of uncertainty associated with this co-locating.

Line 199-200 – What does 'exact location of each measurement was within 1 m' mean? Were the GPR survey lines conducted along survey measuring tape laid on the ground? If so, how were the measuring tapes fixed to the ground? Did they bend/blow in the wind creating potential positional uncertainty? Were the start and end coordinates of the GPR transects marked with RTK DGPS? Does the GPR data have GPS coordinates linked to each measurement?

Line 200-202 – It should be noted that the use of GPR in Hubbard et al., 2013 was not for snow depth extraction. Please change text to 'a detailed explanation of the use and processing of GPR data for the purposes of identifying the active layer depth at the end of the growing season at this study site, are provided in Hubbard et al., 2013.' Are the Hubbard et al., 2013 thaw-season GPR measurements for active layer thickness even relevant to the GPR measurements obtained in the frozen winter-period for snow depth estimates? The GPR measurements are conducted over two very different media. Hubbard et al 2013 describe a different GPR system entirely with two different antennas (450 MHz and 900 MHz vs the 500 MHz antenna used in this study). Can the authors please provide specific details on the software used and the steps taken to maximize signal-to-noise ratio, and correct for change in topography, change in speed of data collection (comment on the utility of the odometer in a sastrugi environment). In-addition, can the authors comment on the presence of any snow layering (ice crusts/wind crusts) that may or may not have caused echoing and how this affected the ability to identify the air/snow and snow/ground interfaces? Can the authors also comment on how sensitive the GPR data was to the thick organic layer and mosses, lichens and vascular plants that can make it difficult to ascertain where exactly the snow/ground interface actually begins? What/where exactly is the GPR return signal bouncing off?

Line 202-206 – The pre-processing routine is explained. Can the authors explain for steps 1 and 2 how this was done? What automated algorithms were used? Was there human-visual intervention/training involved in identifying these two interfaces?

Line 206-207 – Please justify how the use of GPR for identifying the tundra active-layer thickness at the end of September (thawed) in Hubbard et al (2013), provides context for the use of GPR for snow depth estimation in the same tundra location in May (frozen).

Line 209 – It is not clear whether the UAS and PhoDAR were measured over all four plot sites (A-D), or the three long 500 transects? Or entirely new transects in the same study area.

Line 211-214 – What was the resolution of the PhoDAR elevation/snow height products?

Line 218-220 – How many control points? What distribution? How did you assess this using the RTK DGPS? What kind of horizontal and vertical accuracy did you achieve with the PhoDAR products? How different were these accuracies from year to year for the snow-free ground measurements (were the same snow-free ground areas surveyed in both 2013 and 2014? Are there any comparisons between estimated PhoDAR elevation of the same area between 2013 and 2014? What were the vertical accuracies any different for the estimation of the snow surface elevation compared to the estimation of the snow-free ground?

Line 222-223 – Were the snow probe locations recorded at the RTK DGPS control points? If not, what GPS positional information was associated with these 183 snow depths, and what horizontal positional uncertainty did they have?

Section 3: Methodology

Line 229-230 – snow density can be variable in both a vertical and horizontal direction (Proksch et al., 2015)

Additional References: Proksch, M., Löwe, H. and Schneebeli, M. (2015), Density, specific surface area, and correlation length of snow measured by high-resolution penetrometry. J. Geophys. Res. Earth Surf., 120: 346–362. doi: 10.1002/2014JF003266.

Line 241-242 – How did you link the DEM elevation data to the tile probe snow depth measurements?

Line 251 – Change ‘GPS elevation measurements’ to ‘RTK DGPS control point elevation measurements’

Line 253-254 – change ‘co-location’ (implies horizontal positional accuracy), to vertical agreement.

Line 255-256 – All snow depth measurements? Or just RTK adjusted magnaprobe measurements? Do you use the three same schemes for exploring co-location of the two snow depth estimates? Do you use the average UAS snow depth within 0.5m radius in the vicinity of the snow depth probe (as described with the elevation datasets at Line 417-418)?

Line 262 – remove term 'recently'

Line 266-267 – Do all four images produced in the wavelet transform provide topographic information? What information do the two images not used in this analysis produce? What are the different 'scales' for the wavelet transforms, and how are they determined?

Line 270-271 – Which high-pass image is used: horizontal or vertical (refer to Line 266-267)

Line 278-279 – Please be specific on what the dependency was relating to the DEM resolution? Decreases with increasing resolution?

Line 297 – -change to – 'interpolating the sparse in situ snow depth measurements.'

Line 334-335 – Change 'accuracy' to 'precision'. How would the inclusion of a larger error associated with each probe measurement affect the results? AS mentioned in my comment for Line 108, there are common over-estimation errors associated with snow probe measurements in a tundra environment that could be added on to any potential error associated with the precision of the depth probing instrument.

Line 362 – Please define MCMC before using in a sentence (Line 360).

Section 4: Results

Line 369-372 – These two sentences contradict one another. First sentence states no systematic dependency. Second sentence states radar velocity depends on snow depth and topographic variability.

Line 372-373 – Please provide more quantitative interpretation of Figure 2 variability. Based on the Figure 5 boxplots, the median snow depth for the site is somewhere between 50 cm and 45 cm = ∼47cm. If you insert a vertical line in figure 2a to highlight the median snow depth for all sites, areas of shallow vs deep snow are better highlighted – Could also insert vertical lines at +/- 1 STDEV from the median to really highlight shallow vs deep snow. With these lines inserted It becomes more obvious that higher radar velocity is more frequent in deeper snow areas (provide average and STDEV), and low radar velocity is more common in shallow snow (provide average and STDEV). Overall variability is higher in shallow snow vs deep (compare STDEV), but this may be a function of the frequency of occurrence – i.e. smaller n in deep snow areas results in lower variability. The addition of a vertical line at the study domain average elevation difference could also help highlight the characteristics of radar velocity in complex vs smooth terrain. Please also provide more quantitative interpretation of Figure 2b, with average and STDEV velocities for complex vs smooth terrain. Why was 0.05 m elevation difference chosen as a cut-off to estimate the average radar velocity for smooth terrain?

Line 378-379 – Can you provide the correlation values for both all depths, and only those depths in regions of low topographic variability?

Line 377-382 – In section 2 the authors indicate that snow depths were located within 1m of GPR measurements. GPR measurements were recorded every 10cm. Were the GPR snow depths averaged in a radius surrounding each snow depth probe measurement? Was the closest snow depth/GPR measurement taken? if so – how was that determined using non-georeferenced tile-probe depths? Can the authors please provide more details on how the GPR vs probe snow depths were analyzed?

Line 399-400 – They are not true mirror images of each other. Please re-phrase.

Line 407 – Insert 'snow depth' in-front of 'estimates'

Line 408-409 – Reword. Suggestion: 'The high resolution GPR snow depth estimates are useful for determining if macrotopographic features can influence the distribution of snow depths across each study plot.'

Line 416 – Be specific about which transects. 'In the region of the 500 m long transects, the UAS. . .

Line 416-417 – Change '. . .was first compared with GPS data in Table 2,' to '. . .was first compared with the RTK DGPS elevation control points measured in July 2013 and 2014. The RMSE values from this comparison are listed in Table 2.' – Please add the total number of control points measured for these comparisons to Table 2. The authors indicate that taking the average UAS derived elevation in the vicinity of each RTK DGPS elevation measurement provides the lowest RMSE, however all values listed in Table 2 are within 0.69 cm! Therefore, these results show that it doesn't matter what method is used to co-locate the RTK DGPS and the UAS derived DSM as these are less than the precision of all available datasets.

Line 418 – The value for the RMSE=6.0 cm does not match any of the values presented in Table 2. Can you provide justification on why you separate the results in Table 2 by year of survey if you are trying to compare the three different schemes for assessing the vertical agreement between the RTK DGPS and PhoDAR elevations? If you have multiple years of PhoDAR elevation measurements of the same terrain, can you provide a comparison between the two snow-free elevation products from PhoDAR alone?

Line 421-422 – It is unclear whether the same three schemes tested and presented in Table 2 are also used to co-locate the UAS derived snow depth estimates with the 183 in situ snow probe measurements. Should the same methods be used? i.e. average UAS snow depth within 1m of the snow depth probe?

Line 422-424 – Did you use the same cut-off of elevation difference 0.05 m? How did you decide on the threshold between large/small topographic variability?

Line 426 – Can you include the location of the snow depths used to assess the UAS product in Figure 4b?

Line 429 – Replace 'Figure 3' with 'Not shown' – The scale of Figures 3 and 4 do not illustrate the added detail found in the UAS data.

Line 430 – Do you mean to say 'the minimal effect of macrotopography?'

Line 446-447 – What do the units in 'microtopographic elevation' from the wavelet transform represent? How does it differ from elevation difference calculated for a 1 m radius of all probe/GPR measurements using the LiDAR DEM used to infer topographic variability in Figures 2b and 4c? Please justify why the authors use two different measures of topographic variability? Why is a scale of 32m chosen?

Line 450 – Change to - In contrast, Plot B has the largest variability in both microtopography and snow depth.

Line 467-469 – Can you provide a possible explanation why there was no correlation of snow depth with curvature in this study vs previous studies? Is this a significant result?

Line 475-477 – Suggest removing the line ' Such a large correlation length is consistent with the field observation that the snow surface is smooth across the site.' This sentence is very subjective. Do you have any measure of smoothness or photographs perhaps? – Replace with something like: The estimation of a snow surface height (elevation + snow depth), effectively removes the influence of microtopography resulting in much larger correlation lengths (250 m).

Line 483 – add comma after 'Equation (1),'

Line 489 – Change to – 'The Bayesian estimated mean snow-depth field over the full study domain (Or NGEE-Arctic study site)...'

Line 492-493 – Change to – 'The snow depth does not have any large-scale trends over the full study domain, which is different from the LiDAR DEM in Figure 1b, but consistent with the interpolated GPR snow depths depicted in Figure 3 (right column), and the measured UAS snow depth measurements depicted in Figure 4b.

Line 494-496 – I don't understand how exactly Figure 7b was produced? Please rephrase 'the mean field based on the kriging-based interpolation of the snow surface elevation.' What snow depth inputs were used to produce Figure 7b? And what method of kriging was applied (there are several), and why? It is unclear how the work of Diggle and Ribeiro, 2007 relates to Figure 7b.

Line 496-497 – The two figures looks very similar – however I have difficulty identifying which regions are considered 'central', and what type of measurements are considered 'many'? Can the authors also provide a more quantitative assessment on how similar? Perhaps produce a third figure of the snow depth difference between the two rasters providing a quantitative comparison/statistics?

Line 499 – change to – 'The estimated standard deviation of the Bayesian derived snow depths. . .'

Line 509 – Please provide more details on how the independent snow depths used in the validation were 'randomly selected', and how many were kept independent for validation purposes vs those used in the production of the Bayesian and Interpolated snow depth maps.

Line 514-515 – Suggest changing to – 'The RMSE for the Bayesian method of estimating snow depth including the topographic correlation is 6.0 cm, while the RMSE for the interpolated snow surface is 8.8 cm.' (I think a more representative/descriptive word should be used to describe this 'interpolated' snow depth data – i.e. what input data was used and what type of interpolation – then update all references to this data throughout the text)

Section5: Discussion

Line 520 – This RMSE value is for only the smooth terrain (limited to areas of low topographic variability). Please update stats for all tundra terrain.

Line 525-529 – Firstly, thick snow is very limited in this type of tundra environment, especially in areas with low topographic variability (where GPR estimates are more reliable). The small sample size for these types of GPR measurements is the most likely reason for the observed smaller range in radar velocity variability. Secondly, - I agree. Thirdly, I feel the authors are contradicting the Second result concerning the influence of topography, by stating that there are no trends in radar velocity related to topographic position. See my comments for Line 372-373. I believe there is a trend for radar velocity related to snow depth.

Line 530 – This methodology was never fully explained

Line 532 – see comment for Line 229.

Line 553 – Change 'option' to 'alternative'

Line 569-577 – Why was a hundred-meter scale used? Please support the statements in this paragraph with quantitative anlaysis (average and STDEV values). Be consistent with the use of the average snow depth vs median snow depth throughout paper.

Line 585 – replace 'the improved' with 'an improved'

Line 596 – The authors never quantify whether the improvement is significant. Visually in Figure 8, one can see an improvement in lowering the STDEV when the topographic correlation is taken into account.

Line 603-605 – Reword – Suggestion: 'It would be possible to quantify the seasonal changes in the topography-snow correlations by designing a full season ground-based measurement campaign and acquisition of remote sensing snow depth measurements (by PhoDAR or LiDAR), that monitored the same site over several years to account for inter-annual variability.'

Line 613 – Change to 'ice-wedge' (check text throughout paper for consistency)

Line 617 – Reword – Suggestion: 'The PhoDAR derived snow depth estimates have great potential for accurately characterizing snow depth over large regions, with an RMSE of () relative to the in situ snow depth measurements. The GPR snow depth estimates were slightly more accurate, with an RMSE of () relative to the in situ snow depth measurements, but required considerable more effort to obtain, and require complex post-processing to minimize errors associated with radar positioning and changing snow density.

Line 620-621 - Please provide the quantitative results: correlation values and correlation lengths

Line 622 – smooth snow surface is subjective (are there photos perhaps?) I thought the results showed that most of the snow depth distribution did not follow macrotopography (with the exception of the DTLB)

Line 630 – Change 'extensible' to 'applicable for estimating both spatial and temporal variability of snow depth at other sites, and in other landscapes (prairie),'

Figures:

Line 860 – Figure 1 caption – The acronym NGEE is not explained in the text until the Acknowledgement sections. Be consistent on whether to use the description 'NGEE-Arctic site' as the name for the full study domain throughout the text/figures. It would also be helpful to always refer to the long transects that traverse the study domain, as the '∼500 m transects' to differentiate them from the transects surveyed at each individual plot – Please update all references to these '∼500 m transects' throughout the paper. It would also be useful to use the same color ramp and scale for all snow depth maps and elevation maps (Figure 1, 3, 4, 7), with preferably different color ramps for snow depth vs elevation.

Line 872 – Figure 3 – It would be helpful to delineate the extent of the 'fine grid' sample box with a black outline in all three figures for each plot to compare polygon features.

It would also be useful to use the same color ramp and scale for all snow depth maps and elevation maps (Figure 1, 3, 4, 7), with preferably different color ramps for snow depth vs elevation.

Line 878 – Figure 4 – Can the authors overlay symbols of snow depth measurement location used to assess/validate the UAS product on the snow depth map in Figure 4b? It would also be useful to use the same color ramp and scale for all snow depth maps and elevation maps (Figure 1, 3, 4, 7), with preferably different color ramps for snow depth vs elevation. Figure 4c – state the elevation difference threshold used to define areas of low variability (red circles).

Line 884 – Figure 5 - boxplot 1 – change y-axis title from ‘snow thickness’ to ‘snow depth’ for consistency.

Line 889 – Figure 6 – describe in the caption what the dashed vertical lines represent and why only some colors are repsented.

Line 894 – Figure 7 caption – Use ‘NGEE-Arctic site’ or ‘full study domain’ when describing the spatial extent of these figures. It would also be useful to use the same color ramp and scale for all snow depth maps and elevation maps (Figure 1, 3, 4, 7), with preferably different color ramps for snow depth vs elevation.

Line 900 – Figure 8 - Matching the color ramp scales would be helpful for comparing the STDEV results between the two methods of snow depth estimation.

Line 906 – Figure 9 - Use the same symbol size and line width for each plot.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-168, 2016.