The Cryosphere Discuss., 9, C2794–C2807, 2016
www.the-cryosphere-discuss.net/9/C2794/2016/
© Author(s) 2016. This work is distributed under
the Creative Commons Attribute 3.0 License.

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "Annual Greenland accumulation rates (2009–2012) from airborne Snow Radar" *by* L. S. Koenig et al.

Anonymous Referee #1

Received and published: 16 January 2016

Overview

The authors present a comprehensive data set of snow accumulation rates over the Greenland Ice Sheet from newly acquired Operation IceBridge snow radar data collected between 2009 and 2012. Specifically, the authors have developed a semi-automated picker for layer selection, which was run over several thousand kilometers of snow radar data. By assuming a common seasonal time (July 1) for each internal horizon, accumulation rates are measured in combinations with a hybrid measured-modelled density profile, which is crucial for conversion from thickness to actual mass deposited. After comparison with several in situ measurements of density, it was determined that the regional climate model density data were appropriate at depths over 1 meter, and above a meter the average density was used. The measured accumulation

rates were then used to evaluate the regional climate model MAR's ability to reproduce accumulation rates for each year (2009-2012). The data set is of key importance for model evaluations because typically the long-term average accumulation rate is used as a marker. Instead, the authors are able to use data over several 1000s of km over a few years for evaluation, which is a monumental effort.

At the same time, the work makes several assumptions that are not adequately justified in the work, requiring some additional explanations. The assumption of a July 1 date of for creation of the internal horizons is not adequately explained, and this choice will widely change accumulation for the most recent year of accumulation. Furthermore, there is a lack of description of the evaluation of the MAR density data from both a time and space perspective, which is crucial in Greenland where melt occurs at different times, reducing the precision of the final results. Finally, while this reviewer sees the value of the work, the discussion is nearly the shortest section of the paper, which has the potential to be the most informative section for the audience. Therefore, further analysis and interpretation of the data set would make the paper much more valuable: specifically, the authors should discuss the details that appear to be missing in the original manuscript including (1) a validation of the assumption of the July 1 layer age as melt is can continue into August, (2) conveying more detail into the MAR density model evaluation especially considering that assuming a constant density in the upper 1m can create a strong spatial bias in the final accumulation measurements which can impact the model evaluation, and (3) a more appropriate crossover analysis comparing range bins, and (4) an improvement to the uncertainty analysis and its description since it does not appear to aptly combine all data uncertainties. Each of these, along with other comments, is described in more detail below.

Specific Comments

What is the justification for comparing the radar-derived and in situ measurements of accumulation, which are only within 5 km of one another? It is well known that the accumulation rate can vary spatially over short distances. Consider using the radar-

TCD
9, C2794–C2807, 2016

derived accumulation rates to determine the accumulation correlation length scale to justify their use of 5 km.

Density: The largest source of uncertainty comes from the density profiles, but the actual description of the model evaluation and the data used is lacking. Several issues that must be addressed are listed below.

The density model evaluation needs to be described further because it is crucial to the final results. There should be a discussion of what is exactly used in the comparison from both the measurement and model side. On the measurement side, most of the density profiles were likely taken in the summer. On the model side, it is less clear what was compared to those measurements: annual averages? Summer averages? This detail is important because firn compaction is highly seasonal with highest compaction rates in the summer. Therefore, further discussion and clarification of the density measurement to model comparison is warranted. The best comparison would be average spring/summer density profiles, unless there is justification otherwise.

It is also of importance that the radar data are typically collected in the mid to late spring. A constant density above 1m depth is used, which is based off of the measurements. Do the measurements coincide temporally with this window of time, and if so, it should be stated.

When using the density data from MAR to actually calculate accumulation rates, are the April 30 average profiles used? Annual averages? Spring averages? Also, are they averages over the time period of the measurements (2009-2012)? If annual averages are used, the authors must justify that the difference in annual and spring profiles are negligible. At depth, this is not an issue, but closer to the surface, it can be substantial. The description of the densities used needs much more attention.

Finally, the use of a constant density (0.338 g cm-3) requires more discussion as well because there is the potential to spatially bias the radar-derived accumulation rates. There is no reason to assume a constant density in the upper 1m of the ice sheet

TCD
9, C2794–C2807, 2016

as a whole because the surface climate varies so widely. A recent paper by Kuipers-Munneke et al. (2015) in The Cryosphere found a relationship between surface temperature and density (Equation 2). If you assume temperatures vary from about -15C to -40C in the dry snow zone, surface density might vary between 410 kg mˆ-3 and 287 kg mˆ-3. It is possible that in the upper 1m, you could have densities between 300 and 400 kg mˆ-3, which is about a 30% difference. Because the densification rates depend on surface climate (both temperature and accumulation), using a constant density will likely spatially bias the radar-derived accumulation significantly as any error in density directly translates into an accumulation error. Therefore, the spatial bias must be addressed especially considering majority of the data shown are from the first radar layer. Because of the spatial bias, the model evaluation from the uppermost layer (i.e., Figure 9) should be questioned.

Accumulation rates and uncertainties

Differences in the radar-derived depths depending on the dielectric model used are outlined, which is a valid discussion. A description of the error in depths due to uncertainty in the density profile used, however, is not addressed, which could be largely more significant than the uncertainty due to the dielectric model used. The errors generated from uncertainty in the density profile accumulate with depth and should be discussed.

In order to appropriately measure the accumulation rate, the age of each layer must be known. Here, the layers are assumed annual in nature. Perhaps of greater importance is determining the time of creation of the radar horizons to which the authors place a date of July 1st ($\pm$ 1 month). Where melt occurs, the strongest reflection should occur between the ice layer formed the previous summer and the low density snow sitting on top of it. The choice of July 1st needs more justification, as the paper cited (Nghiem et al., 2005) shows that between 1999 and 2003, the date of the final melt event varied between June 14 and August 31. Furthermore, we know that in more recent years (especially, 2012), melt continued into late August. Hence, more justification is needed

TCD
9, C2794–C2807, 2016

especially since the Nghiem citation states: “Results reveal large interannual variability in the first- and the last-melt date. . .,” and the paper further states that the timing and duration of melt “vary spatially.” For the dry snow zone, the argument is equally deficient as the timing of depth hoar formation from a single site (GISP2 Summit; Alley et al., 1990) that was found to occur in late June to early July. Others have found low density faceting that was created in the wintertime as well (Steffen et al., 1999). Because of the importance of the choice of July 1st, especially for measuring accumulation from the 1st layer, I believe further discussion is warranted. Because the timing could vary spatially, another spatial bias in the measurements could be introduced.

Also, it is not clear whether the age of the first layer in each echogram is assumed equal to a full year. If you assume a surface date of April 30, and a horizon date of July 1, you only have 10 months. Therefore, the first layer does not represent a full year and is only 10 months. (Later, there is discussion of this, but it remains unclear as to whether the calculations were based on 10 months or a full year).

The paragraph (P 6706, L3-11) on total uncertainty is lacking important details. A few specifics: the assumed error for the age is 8%, but for measurements using the 1st layer, that error should be larger; if the July 1st date is correct, ± 1 month is 10% (1 out of 10 months). L4-6 states that “Equation (1) is written to show the relationship between the density profile. . .” but never says what the relationship is. Between the density profile and what? The following sentence beginning with “The derivative of Eq. (1). . .” should state the derivative of Eq. 1 with respect to density as it is not explicitly stated. Were the accumulation rate errors done in the same fashion with age (i.e., take the derivative of equation 1 with respect to age)?

Perhaps of greatest concern is that the maximum accumulation uncertainty is stated as 11%, which cannot possibly be the case if the assumed error in density is 12% and age is 8% (or specifically 8.3%). The equation for the accumulation rate can be simplified to b = (depth * density) / age where the errors from depth, density, and age will accumulate to provide an accumulation rate error. The depth error largely depends

TCD
9, C2794–C2807, 2016

CC BY

on density as well as the picking procedure, but for this example let's just say it is zero. That leaves us with errors from density and age. If Δ's represent the errors, the accumulation error as a percentage of b is then Δb/b = sqrt((Δdensity/density)ˆ2 + (Δage/age)ˆ2) → sqrt(0.12ˆ2+ 0.083ˆ2),which is equal to ∼15%, indicating that the minimum error in the accumulation rate is 15% (and that is without including the error in depths). Or looking at it from a different perspective, the assumed error in picking is ±3 bins (∼8 cm), which is equivalent to approximately 0.027 m w.e. (assuming 0.338 g cmˆ-3). Figure 4 shows accumulation rate measurements of less than 0.18 m w.e., which suggests the errors from picking alone are 0.027/0.18 = 15%, a value larger than the stated 11% maximum. Therefore, either the uncertainty calculation description is not complete or the calculation was not done properly and needs to be redone.

Picking procedure

The picking procedure in 4.3.2 & 4.3.3 is not easy to follow. Why not include a graphic? It would speak volumes more, especially considering how visual of a process this is.

Also, why smooth the layer picks? Can we see an image before and after smoothing to see the benefit? The spatial/along-track resolution of the smoothed picks should be discussed as well since the smoothing has effectively removed some of the higher frequency variability, resulting in coarser data resolution.

Results

For each section of the results, there should be explicit text about what the accumulation rates represent. This detail is important because the snow radar can image many years' worth of accumulation. I believe in section 5.1, only the top layer from each of the years is discussed, but it should be more clear. Perhaps move P6708, L17-19 up to the beginning of the section so we are aware of the time coverage.

I would be hesitant to state that they can see annual variations in the accumulation rate without some description of their confidence that the age of creation of the radar-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

detected layer is the same each year (this was discussed above). The year-to-year variations could be due to differences in that date. Therefore, please ensure (and state) the differences are outside of the error bounds.

It appears that discussion of the subannual nature of the top layer is described in the results as it is an important distinction. I imagine use of the top layer is done in order to maximize spatial coverage; however, there is a lot working against the use of the top layer. First, it simply does not cover a full year, and because it is less than a year, the resulting uncertainty is larger. Second, the density in the upper 1 m is assumed constant and does not vary in space. While in some regions the top layer will extend beyond 1 meter depth, the constant value is still incorporated into every measurement from the top layer. Therefore, I would suggest using the 2nd layer even though it would decrease your coverage.

It is not clear once again what is being compared between the measurements and model data. If the top layer is used, why not compare accumulation rates from MAR covering the same time interval as the measurements (July1 – April30)? The comparison would then be appropriate. Please be very clear as to the time intervals being compared.

Crossover Analysis

I assume that the crossover analysis was done for each year only, using the top layer. Because of the nature of the analysis, it is less useful to compare the resulting accumulation rate at crossover points, and instead the picked range bins for the two flyovers should be compared. At crossover points, the same age and density profiles are used, so why compare final accumulation rate values? The more informative comparison would be looking at the picking error because that is all a crossover analysis of a single year is showing. Therefore, the values in Table 1 and in Figures 7 & 8 should be in range bins, not in absolute accumulation rates because the range bins are the only variable that differs. This result would also allow you to use your picks to assign a

TCD
9, C2794–C2807, 2016

picking error rather than just stating an error of 3 bins. As a result, the final sentence of section 5.1 should only describe the ability of the picking scheme not of the method as a whole because a crossover analysis of this nature is only testing picking ability.

The crossover analysis issue brings up a more interesting evaluation that could be done to evaluate the method as a whole (rather than the ability of the picker). Why not do a crossover analysis between all years to ensure consistency? If the 2010 horizon is detected in 2012 data, why not compare the 2010 accumulation rate from the 2011 and 2012 radar data? Such a crossover analysis would help you evaluate how well your hybrid measurement/model density profiles represent reality. There is a substantial portion of the ice sheet where the radar can see more than one layer, so the analysis should not be difficult. Taking it a step further, because IceBridge has repeat surveys, your results can be compared at the transect level (rather than at a simple crossover point).

Comparison with modelled accumulation

A general comment: Stating that the comparison between the measured and modelled accumulation shows the "errors" in the model results is slightly bold. There are many assumptions that go into the measurements, and the errors are quite large (I believe, at least 15%). The language in the section should be careful to note the measurement deficiencies and how they might manifest in the comparison. A huge issue is the constant density, which will introduce a spatial bias in the accumulation rate measurements. As a result, the comparison with these biased measurements (if the measurements are assumed correct) might lead readers to think there a regional biases in the MAR modelled accumulation rates.

Also, it is not clear if the comparison uses MAR accumulation between July 1 and April 30 or if it is May 1 to April 30. If it is the former, it would be difficult to say very much about the ability of the model at all.

Comparison with in situ data

TCD

9, C2794–C2807, 2016

The comparison with the NEEM records begins to show some of that internal consistency (comparing accumulation rates between two years of collection). It would be useful to see the radargrams for these comparisons, especially since the shallower layers from 2011 were not detected while they were in 2012 (Figure 11 top).

Discussion

The section as a whole is very short, which is disappointing because it has the potential to be the most interesting. Additional paragraphs on the temporal history of accumulation would be most welcomed, especially considering there are substantial regions of coverage with >5 layers detected. The paper begins with the importance of the snow accumulation as it is a dominant control on Greenland's mass change, but the importance of the results from such a perspective was not included. The paper as of now is more of a description of creation of a data set, which is then compared to modelled values. There should be additional detail into what this means for Greenland's mass balance and its recent history. The fact that over two decades of measurements exist, as stated in the Conclusions, but are not further analyzed is missed.

Technical Corrections

P: 6699 L20: remove "of ice" as it is implied L23: remove "being governed by" and "being dominated by" as it is redundant and awkward

P: 6700 L3: "here after" should be "hereafter" L6: add "in number" after "limited" to clarify L11: comma after "(Benson, 1962)" L27: replace "and map" with "the lateral persistence of"

P: 6701 L8: use of "to" after "penetrate" is redundant; consider removing "to" or rephrasing L10: comma after "frequency-modulated" L11: remove the comma after "radars" L25: comma after "preserved" and remove the commas around ", therefore," or consider a semicolon after "preserved" and remove "and"

P: 6702 L16: comma after "Frequency-Modulated" ; also, I am not sure why

TCD
9, C2794–C2807, 2016

"Frequency-Modulated Continuous Wave" is capitalized here and not on P6701, L10-11, so please be consistent. I suggest not capitalizing it. L17: change "when preserved" to "where preserved" Section 3.1: somewhere in this section there should be a description of the differences in the radar system for the different years, including its range precision.

P: 6703 L3-4: remove "reanalysis" add "global atmospheric reanalysis" after "ERA-Interim" L12: change "accumulation-rate" to "accumulation rates" as the former suggests you are using accumulation rate profiles from MAR, which seems awkwardly phrased L20: Why only to 15 m? Is it due to the fact that no layers below 15 m are used? If so, please state it. L20: "1500 measurements" is misleading and really does not inform the reader of the value of the data set for comparison. I would prefer listing the number of sites, with a description of the range of measurements at each site. Something along the lines of "which contains measurements at ## sites, and at each site the number of measurements ranges in number between XX and XX and maximum depths of XX and XX." L23: change "measured" to "in situ" L26: change "additionally" to "additional" L27: The phrase "which includes additional cores to the SUMup dataset" is redundant because it was already made clear by the "additional" in the prior line.

P6704 L5: The second half of the sentence is oddly phrased, please reword beginning at "we require. . ." L18-19: The sentence beginning with "We note. . ." needs to be appropriately cited as this is not common knowledge.

P6705 L1-2: in the sources of error for derivation of radar depth, why is the actual density profile used not included in the list? The error from uncertainty in density is likely larger than based on the dielectric model used. L2-6: The description of the dielectric model evaluation is confusing, please clarify. Perhaps, begin with a statement explaining that you are evaluating X, Y, Z dielectric models because that only became apparent at the end. Eq1: Why is a dependent on x? The age of a layer should not be dependent on location as the layers are assumed isochronous. The equation might need further clarification because variables should be dependent on x, but also on

TCD
9, C2794–C2807, 2016

depth (or on the layer number). I suggest stating the equation is for a given horizon to eliminate the additional complexity. L13: The phrase "... is cumulated snow/firn density at depth..." is confusing. I suggest adding "average" after "cumulated" because otherwise it sounds as if the densities are just added together. L16: The same issue arises here as with the previous comment. The use of "cumulative" suggests adding together all the densities below that depth, which in an integrative sense would produce a cumulative mass (kg m-2). Perhaps, reword or add "average" again.

P6707 L2-3: If vertical traces are tossed out if it appears the surface is not properly picked, how is the stacking procedure done? If a few traces in a row are tossed out, you would not want to average the now spatially separated traces. L7-8: Why not stack a different number of traces to end up with similar along-track spacing for all years? L13: change "in" to "from" L24-26: please rephrase the sentence beginning with "Layer indices are..." because I find it difficult to understand what is meant by the "partial overlap that can exist between layers." A graphic of the procedure is really necessary.

P6708 L14: Insert "the" before "accumulation rate"

P6709 L3-9: Consider moving to the picking section as it seems more appropriate. L16-18: It is not clear which cluster in the crossover analysis show rates off by a factor of two, so perhaps circling it on Figure 8 would make it easier.

P6709 L24-26: Consider applying a threshold number of radar measurements for comparison with the MAR grid cell to eliminate comparisons that are likely not as representative.

P6710 L7-9: The larger differences are associated with areas of higher accumulation. A more informative comparison would be as a percentage. Otherwise, the details in the low accumulation areas are lost. L17-20: The strong statement of "These values are not well correlated...emphasizing that further improvements in accumulation-rate modeling are needed..." should be reworded because the measurements are not without fault, so putting the blame on the model is risky. L27: consider changing "closely

TCD
9, C2794–C2807, 2016

located" to "nearly co-located"

P6711 L20: consider removing "the" before "large portions" L20-23: Again, this is a very strong statement. It should be changed to state that while these are useful for model evaluation, we must still consider the assumptions that go into the radar-derived measurements. Such a statement would give way for a discussion of the new data needed to reduce those uncertainties.

P6712 L9: consider changing "resolves" to "will resolve" L13: the phrase "constantly varying flightlines" is unclear as to what is varying, please reword

Table 1 Please state in the caption what time interval is used from MAR (July1-April30 or July1-May31). Consider adding a column of the mean accumulation from the crossover points for each year.

General figure comments Please change the color intervals used in Figures 4 & 5 to be more meaningful: e.g., 0.2-0.3, 0.6-0.7. The values are non-traditional, making it difficult to quickly interpret the patterns. The black background does not add to the meaning, and is a little ink heavy.

Figure 1 Is there overlap between the density measurements (red) and ice core accumulation measurement in blue?

Figure 2 Please change depths to positive numbers since a depth is positive moving downward. The caption should be very descriptive as to what the differences existing in the timing of the measurements and what model timing is used. This relates to the statements in the beginning on explaining the details of the comparison. For instance, if the average April 30 density profile from MAR is used, please state it. Please do something similar for the measurements as well.

Figure 3 Please change the Distance values along the x-axis to more appropriate intervals (26, 78, etc. are odd values). An inset map of these transects would be beneficial. They could even be added to Figure 1.

TCD
9, C2794–C2807, 2016

CC BY

Figure 4 Please state that only the accumulation rates from the top layer is plotted for each year in the caption.

Figure 5 Same as with Figure 4, state the time intervals represented here (May1 – April30?). Consider overlaying the radar-derived measurements for comparison.

Figure 6 The intervals in the legend should be changed to not have overlap: 1, 2-3, 4-6, etc.

Figure 7 These values should be plotted as percentages rather than absolute values because the crossovers in regions of low accumulation are lost. Also, as described above, the crossover analysis as done here is only a measure of the ability of the picker, so the maps shown here would be better off showing the differences in range bin picks, not in total accumulation. Please be sure to use appropriate intervals for the color bar, if the mean crossover difference was 0.03 m w.e., then majority of them would fall into the first interval.

Figure 8 Similar to Figure 7, this plot should be comparing the picked range bin rather than accumulation rate.

Figure 9 The color bar should be a gradient between two colors, reaching white in the middle in order to appropriately show regions where the model is less than or greater than the measurements. There are too many colors here, making interpretation difficult. Also, be careful with the value intervals making sure the center interval straddles zero evenly (e.g., -0.05-0.05). This way people can easily see the transition between more/less accumulation difference. A histogram of the differences would be a useful addition that can be inlaid onto each map.

Figure 10 There are a few interesting features here that could be further discussed in the paper. For instance, the 2011 (blue) dots appear to have a linear feature at 0.75x and at 1.5x suggesting the picker detected the 2nd layer rather than the 1st. All the previous plots were broken down by year, it might be useful to do the same (4 plots) to

TCD

9, C2794–C2807, 2016

see the details of each as the values <0.5 m w.e. get lost. I would suggest showing the best fit line to the data as well to ease interpretation.

Figure 11 It would be useful to have the echograms from each year shown as well, so the reader can see the differences in the data between years. It would also lend insight into whether the very large accumulation from the radar in 1995 is due to the picker missing a layer, which is especially interesting because the 2011 data end in 1996.

Interactive comment on The Cryosphere Discuss., 9, 6697, 2015.