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The Cryosphere
Discussions

Interactive comment on "Changes in glacier facies zonation on Devon Ice Cap, Nunavut, detected from SAR imagery and field observations" *by* Tyler de Jong et al.

Anonymous Referee #1

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Overall this is a timely application of SAR data to examine proxies for the ice cap firn line and ELA. While it's worthwhile work, I have some issues with the interpretation. There is also a certain degree of sloppiness, especially with regard to the figures.

My intuition says, and I could be wrong, the that derived accumulation areas for the ice cap are far too small, especially given that there are also some large glaciers draining the ice cap in addition to loss of ice due to melt. Unless the SMB is really high in the top region, this ice cap would have to be lowering at a really high rate (I realize it is likely losing mass, but perhaps not this fast). To check my intuition, I did a quick check of the pior Devon literature. While perhaps a bit out of date, Figure 11 of Boon et al,

Arctic 2010 shows SMB as positive down to about to ∼1000 m, which is actually pretty consistent with the ELA being well below the bright transition seen in the figures. (I was really surprised not see this fairly comprehensive paper on Devon cited, especially given the overlap with authors on this manuscript). The original SMB data are from Mair et al and they show that the ELA is quite variable, ranging from ∼600 to 1200 meters (their Fig 8 and also a different rendering of the data in Fig. 11 of Boon et al). The northwest sector ELA computed by Mair et al is about 1200 m, which is consistent with the newer data in Figure 6, if there were increases of 100-300 m since their data were collected. But the fact that the earlier data has ELA for the other sectors several hundred meters lower, then the Figure 6 data in the current manuscript can hardly be used to calibrate the full backscatter map. (the Mair data should be weighted more heavily in the discussion rather than just the brief discussion).

I think there are some issues with mapping the radar backscatter to glacier facies. I believe the bright-to-dark transition is the separation between the percolation zone and the wet snow zone not the firn line, which means the actual ELA maybe several 100 meters lowers and more in line with GL2013 ELA. I would like to see a full-size figure of the ice cap with radar backscatter and the various zones identified (the dynamic range, the color table that is completely out of sync with the apparent backscatter values – see comment on figure 2, and the small size make it hard for readers to interpret the results). From figure 3, I would be more inclined to believe the transition between -2 and -3 dB marks the transition to bare ice, which I would normally think is the darker fringe around the ice cap at the lowest elevations (this region at the level I can distinguish from the tiny figures, is roughly consistent with the ablation zone of Mair et al). Though I am not entirely sure, because the very flat backscatter curve in this region is not consistent with the sharp transition visible in the imagery – geolocation issues?? Between the bare ice and the percolation zone, what is probably being seen is a broad range of super-imposed ice and wetsnow, with a difficult to interpret ELA in between. I would like to see a time series showing the evolution of the backscatter over the course of a year. There are times when the perc zone can retain moisture, and brighten up over the course of winter so that it would be classified one way in early winter and another in late winter. There is one example, but the 0.15 dB difference is perhaps 50 m or more according to Figure 3. I did a quick search of some Sentinel data. From the browse imagery, its looks like at least in 2017, the back scatter is really bright to much lower elevations (e.g. see ASF catalog S1B_IW_RAW__0SSH_20180320T122551_20180320T122624_010115_0125C8_6C27). There lots of Sentinel data, so it would be worth looking at the Sentinel times series to also see the timing and extent of melt.

Thresholds for classification seem rather arbitrary. Why exactly 0dB. Why is in the region where there is the least transition in backscatter, even though the figure indicates there should be a sharp transition – something isn't right. There needs to be some discussion of the sensitivity to incidence angle, which varies over the ice cap and can influence sigma nought by a few 10ths of dB. I would like to see a) what the range of incidence angle variation is, including slope effects, b) what the sensitivity of backscatter is to incidence angle – at least some reasonable assumptions based on the literature, and c) what the temporal variation in backscatter is through and annual cycle – i.e., are Sept images directly comparable with those from December or Feb.

The one thing that is fairly clear, is there is a bright transition from perc to wet snow (or saturation if you prefer) and its moving. I would note that the GPR seems to indicate some alternating ice and firn below the transition, which is more consistent with alternating years of full wet snow and percolation conditions. Pure glacial ice should be darker as it is more homogeneous. So, focus more on the radar observable, and be careful to note that it's only a proxy for the other quantities, and that movement in the radar line is significant.

In summary, it boils down to two options 1) the more recent data indicate that SMB has been drastically reduced from the 30 year ∼late 20th century average, with a consequent shift of the ELA by 100s of meters and a reduction in the accumulation areas from a majority (Fig 8. Mair et al) of the ice cap area to only a small fraction of the

area (∼25% - Fig. 4 this manuscript), 2) the interpretation of the data presented here is incorrect. While there probably has been some upslope migration of the ELA, from what I can see (and have seen looking at recent browse sentinel data), I suspect the interpretation is not completely correct and the thresholds used are not correct. And if option 1 is the case, then a much better presentation of the data is needed to make the case as well as better discussion of the changes, which if true, would be far more significant that the remote sensing aspects of the paper.

There are the makings of a good paper here. But the level of rigor needs to increased several notches in terms of a) producing a self-consistent set of figures with reasonable color bars and scales – it looks like ARCGIS was used but perhaps try python or matlab instead, b) providing more error and sensitivity analysis, and c) improving interpretation, being careful not to go further than is justified.

Here are several other line by line specific comments, some of which may echo the general comments above.

Figure 1 – zoom out to show and move legend to show full ice cap. Add a similar figure with radar backscatter. Figure 3- the transect shown on the inset seems not to agree with the plot – comparison with Figure 1 indicates the black line extends to lines lower than 1300 m – please check.

Figure 1 and Figure 2 – I could be wrong, but eyeball estimates make is seem like scale bars are not consistent. I certainly can't reconcile the distance covered by the profile in figure 3 (25 km) with the ∼40km distance covered by the profile in Figure 1 when using the scale bar.

Page 1, L17 – add word like "shift" to read "This shift coincided..." Page 1 L19 – use word like "change" "This change...." Page 1L23 – CAA is kind of a clunky acronym. Could you not just say "Canadian Arctic Archipelago, which henceforth we refer to as "the Archipelago" and substitute Archipelago for CAA. This is just a style thing, but the paper would read much better for it. Page2L38-51 – It would be good to reference

Fahnestock et al 1993 (Science), which was one of the first works to demonstrate SAR for glacier facies work. L55 – italicize “in situ” L58 – Suggest replace passive “have implications for” to “influence” L115 – Here again a reference to Fahnestock et al 1993 would be appropriate L117 hyphenate “small-crystal” Line 118 – in true try snow (no melt) at C-band you typically are seeing many meters, so not just the past summer. Line 123 – italicize “in situ” (again on line 126 – search and fix all). Line 134 – finish sentence with something like “since sigma nought is sensitive to incidence angle.” Line 137 – Please specify months rather than fall and winter (in places with saturated firn and firn aquifer, the backscatter can evolve over several months as water refreezes). Line 144 – 0.15 dB sounds pretty close given normal speckle statistics assuming source data have few looks – is this after the smoothing described blow. Line 148 – Can you say something more descriptive than a “a rigorous math model” Line 148-154 – Please specify whether sigma nought was determine with flat(curved) earth incidence angle, or instead with the local, topography dependent incidence angle. Line 151 – What was the point of oversampling the 150 m imagery to 12.5 m, then smoothing back down? To accurately get area between elevation bands? This is fine, just be clear why. L214 – Would be nice, albeit not required, to have a brief statement between 5.0 and 5.1

L220 -226 - I question whether any of this was truly dry-snow zone. I would also be careful about simply saying negative values indicate dry snow (unless it is very low acc like the Antarctic plateau, I would expect true dry snow to be -10dB or lower (e.g., Fig 2 of Joughin et al Jglac 2016). It’s really hard to tell what your sigma nought is (see figure 2 comment). I suspect even if the snow was dry these years, you are seeing melt layers from prior years. You bring this up later, but it’s not good practice to start off by defining something incorrectly, and then correcting the error below.

Figure 2 – Offhand, I don’t know what the noise floor of EnviSat, I doubt its better than -25 dB, so the color bar should not extent to -50. As a consequence of the current color bar, much of the detail in shading is lost. Please redo range of color bar. Please also check data are properly calibrated – even with 18-dB upper range – color bar is

saturating. The numbers on the plots look fine, but the perc zone is blowing out in the figures at 18dB. Also your own figure 3 seems to show a range of only about +1 to -3 dB, so why 68dB of range in the color bar.

Line 230 – “High SAR backscatter” quantify by adding “(x-y dB)”

Line 238 – Again I would be careful about identifying 0 dB as the transition to dry snow – without some clarification to indicate that it is not true dry snow (you are seeing some prior melt layers to get that brightness).

Line 248 replace “identified by having the” with “identified as those pixels (or regions) with the lowest..”

250: Quantify “low” with something like “($\sim$X dB)”

Line 253 – “Backscatter is low” again don’t use relative terms like this without quantifying add “(X-Y dB)”.

Line 259 – can you say what this unique value is and why its unique.

Line 257-265 – far from clear whether you accurately separating glacier ice from superimposed ice. Especially given the limited data to set the threshold, especially given there is some dependene of sigma nought on incidence angle. You might just be better lumping these two into one class.

Line 290 – Should introduce “pseudo dry snow earlier” and with some better definition of the term. Also “quasi dry snow” is probably better than “pseudo dry snow”, the latter more implies fake.

Line 319-329 – I would like to see some error analysis for the line based on 0-dB threshold. The ice cap is not that wide, but there is a range of incidence angles due to range and the slopes. Although the range is small, it could easily yield a few tenths of a dB variation in backscatter for ice of the same type – the variation depends on the target properties. Figure 3 indicates this range could alter the radar-derived ELA by +/-

50 meters or more. And exactly 0dB seems rather arbitrary to begin with.

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