

Changes in glacier facies zonation on Devon Ice Cap, Nunavut, detected from SAR imagery and field observations by Tyler de Jong, Luke Copland & David Burgess
The Cryosphere, manuscript # tc-2018-250

We thank the referees for their useful comments, and provide our responses to both sets of comments in red below

Anonymous Referee #1

Overall this is a timely application of SAR data to examine proxies for the ice cap firm 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, that the 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 prior Devon literature. While perhaps a bit out of date, Figure 11 of Boon et al, Arctic 2010 shows SMB as positive down to about 1000 m, which is actually pretty consistent with the ELA being well below the bright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 to 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).

We agree that our derived accumulation area of the ice cap is quite small, particularly in the last years of our study (particularly 2010 and 2011). However, this is symptomatic of an ice cap that is undergoing rapid changes in response to strongly negative mass balance conditions in the recent past. Our field measurements are consistent with our remote-sensing derived calculations, and indeed are used to validate them. For example, the net annual mass balance recorded at the stake at 1317 m (Fig. 7b) shows that the MB there was positive in 2004/5, but strongly negative after that (close to -0.40 m w.e. a^{-1} in 2008 and 2009). So a direct comparison with MB values determined from older studies such as Mair et al. (2005) cannot be made, as these pre-date the period of our study. We didn't refer to Boon et al. (2010) as this is a review paper, so we rather referred to the original papers that Boon spoke about. However, we agree that it's still a useful review and so will include reference to it in the revised manuscript.

In terms of the comment as to whether the ice cap is undergoing rapid surface lowering, this is a question that is outside the scope of this paper, but we're currently looking at as part of another MSc project. Also note that the accumulation area of Devon Ice Cap has been rapidly densifying recently (Bezeau et al., 2013), which reduces the impact of negative MB on surface lowering rate in the short term.

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 wet snow, 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 per 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.

We will add a new full-size figure of the ice cap with radar backscatter and the various zones clearly identified. Our facies zones are validated against our field measurements, so we are confident of their locations, but we will provide additional comparison to field data in the new figure and updated text. As mentioned in the previous reply, Mair et al. (2005) reconstructed the ELA based on an older period (1963-2000) than the period of our study (2005-2011), so there is no reason to believe that their ablation zone should be comparable to ours in a rapidly warming climate. The CAA underwent a dramatic change in climate between 2000 and 2005, as described in detail in the abstract of Sharp et al. (2011): “*Relative to 2000–2004, strong summer warming since 2005 (1.1 to 1.6°C at 700 hPa) has increased summer mean ice surface temperatures and melt season length on the major ice caps in this region by 0.8 to 2.2°C and 4.7 to 11.9 d respectively. 30–48% of the total mass lost from 4 monitored glaciers since 1963 has occurred since 2005. The mean rate of mass loss from these 4 glaciers between 2005 and 2009 ($-493 \text{ kg m}^{-2} \text{ a}^{-1}$) was nearly 5 times greater than the 1963–2004 average. In 2007 and 2008, it was 7 times greater ($-698 \text{ kg m}^{-2} \text{ a}^{-1}$).*” This reinforces the point that it’s unrealistic to make comparisons between facies zones derived from different periods.

I did a quick search of some Sentinel data. From the browse imagery, it 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.

From our field measurements across the CAA (including Devon Ice Cap) in 2017, it is clear that this year was anomalously cold, with positive mass balances and low ELAs common across many glaciers and ice caps. This was a common feature for the Arctic in 2017, as stated in Sharp et al. (2018): “*Five of the 18 annual net balances reported for 2016–17 were more negative than the 1985–2015 mean, and 13 were more positive. The mix of positive and negative anomalies in 2016–17 contrasts with the tendency for predominantly negative mass balance anomalies over*

the past decade.” It’s therefore unrealistic to compare our 2005-2011 results with those from a different period, when we know that there is large interannual variability between years. Sharp, M., Wouters, B., Wolken, G., Andreassen, L., Burgess, D., Copland, L., Kohler, J., O’Neel, S., Pelto, M., Thomson, L. and Thorsteinsson, T. 2018. Glaciers and ice caps outside Greenland. In: Blunden, J. and Arndt, D.S. (eds.). State of the Climate in 2017. Bulletin of the American Meteorological Society, 99(8), S156-S161

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.

We will add a section in the revised paper to address the question of sensitivity of the backscatter to incidence angle. The threshold values weren’t chosen arbitrarily, but are rather based on our field mass balance and GPR measurements, but we will provide additional support for our choice of 0dB threshold. Unfortunately there is limited availability of Envisat scenes for our period of interest, so it’s unlikely that we can look at variations through an annual cycle, but we will do the best possible with the data available.

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 than the remote sensing aspects of the paper.

These comments hit the nail on the head! Option 1 is what’s happening to Devon Ice Cap: there has been a dramatic reduction in SMB (and rise in ELA) of the ice cap over our study period compared to the 20th century average. This is one of the findings of our paper, and is clear in both our field (e.g., Fig. 5, 6, 7) and remote sensing (e.g., Fig. 2, 4) data. In addition to the rapid changes described by Sharp et al. (2011) and Sharp et al. (2018) already referred to in the replies above, other examples of papers that discuss the rapid recent changes in SMB of glaciers in the CAA, including Devon Ice Cap, are:

- Cook, A. et al. In press. Atmospheric forcing of rapid marine-terminating glacier retreat in the Canadian Arctic Archipelago. *Science Advances*. *Average marine-terminating glacier retreat rates have been >3 times greater since 2000 than before it.*
- Gardner, A.S. et al. 2011. Sharply increased mass loss from glaciers and ice caps in the Canadian Arctic Archipelago. *Nature* 473, 357–360. *Rates of ice mass loss from the QEI were three times higher in 2007-9 than 2004-6.*
- Box, J.E. 2018. Global sea-level contribution from Arctic land ice: 1971–2017. *Environmental Research Letters*, 13, 125012. *Ice mass loss from the Canadian Arctic occurred at a rate of 0.055 mm yr⁻¹ sea level equivalent between 1986-2005, which quadrupled to a rate of 0.213 mm yr⁻¹ between 2005-2015.*
- Noël, B. et al. 2018. Six decades of glacial mass loss in the Canadian Arctic Archipelago. *J. Geophys. Res. Earth*, 123, 1430–1449. *Surface mass balance for the QEI was an average of -80 mm w.e. yr⁻¹ from 1958-1995, and -237 mm w.e. yr⁻¹ from 1996-2015*
- Thomson L.I. et al. 2017. Comparison. of geodetic and glaciological mass budgets for White Glacier, Axel Heiberg Island, Canada. *J. Glaciol.*, 63(237), 55–66. *There has been a ~200 m rise in the ELA at White Glacier since 1999.*

We will include additional references such as these in the manuscript to describe these changes, and additional discussion to highlight how unusual they have been in the historic record and to place our results in wider context.

There are the makings of a good paper here. But the level of rigor needs to be 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.

We appreciate the critical feedback, and will improve the figures, sensitivity analysis and interpretation in the final paper.

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.

OK – will do

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.

The line in figure 3 is the complete straight line from Dev1H to the edge of the ice cap. It's a different depiction than what is shown in Figure 1.

Figure 1 and Figure 2 – I could be wrong, but eyeball estimates make it 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.

Thanks for catching this. It seems that the scale bar in Fig. 1 got carried over from a previous version, so is incorrect. We'll correct all scale bars for the final version.

Page 1, L17 – add word like “shift” to read “This shift coincided. . .”

Page 1 L19 – use word like “change” “This change. . .”

Will modify as suggested

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.

The CAA acronym is used widely in other papers, including almost all those referenced in the above replies, so we would prefer to keep it

Page 2L38-51 – 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.

OK – we’ll include this reference

L55 – italicize “in situ”

OK

L58 – Suggest replace passive “have implications for” to “influence”

OK – will do

L115 – Here again a reference to Fahnestock et al 1993 would be appropriate

OK – will add it

L117 hyphenate “small-crystal”

OK

Line 118 – in true try snow (no melt) at C-band you typically are seeing many meters, so not just the past summer.

Agreed – we’ll modify the wording to reflect this

Line 123 – italicize “in situ” (again on line 126 – search and fix all).

OK – will do

Line 134 – finish sentence with something like “since sigma nought is sensitive to incidence angle.”

OK – will change as suggested

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).

Will do

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.

We’ll check on this

Line 148 – Can you say something more descriptive than a “a rigorous math model”

This comes from technical documentation for the PCI software that we used; we’ll dig into this more to see if we can provide something more descriptive

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.

Sigma nought was determined using the topography dependent incidence angle (we used a DEM for this). We’ll specify this in the updated text.

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.

Yes, this is correct – we used it in an attempt to delineate the transition of the facies zones more precisely

L214 – Would be nice, albeit not required, to have a brief statement between 5.0 and 5.1

We’ll add something if it fits amongst the other text

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.

Yes, it’s possible that we’re seeing to some deeper layers with melt, although this would still result in a relatively low backscatter compared to surrounding regions. Our interpretation of the dry snow zone is based on the temperature record at station Dev1H at 1781 m (near the ice cap summit), which showed little to no melt during the years when it was present. However, we agree that it’s difficult to know that this zone was completely dry, which is why we refer to it as the ‘pseudo dry snow zone’ in our results. We’ll updated the definition of this zone in the paper to make sure that it’s consistent throughout.

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.

We scaled the colorbar in Fig. 2 to account for the full range of returns present in the ENVISAT images, including surrounding sea ice and open water. However, we now appreciate that this isn’t necessary, so we’ll adjust the colorbar to the range of backscatter values for the ice cap, which will hopefully provide finer detail than at present.

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

OK

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).

Agreed: we introduced the term ‘pseudo dry snow zone’ later in the paper (L298) to account for this, but will now introduce it here

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

OK – will do

250: Quantify “low” with something like “(~X dB)”

OK

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

OK

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

Yes, we’ll add this

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 dependence of sigma nought on incidence angle. You might just be better lumping these two into one class.

Our separation between these zones is based on in situ SMB data, which has a high degree of reliability. However, we’ll add details for the threshold used, and assess its sensitivity to differences in incidence angle.

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.`

OK – we’ll introduce the term earlier (as already mentioned in replies to comments above), and are happy to change it to ‘quasi dry snow zone’

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.

The choice of the 0dB value is based on comparison with our field measurements; we were also a little surprised at first that it was this exact value, but that’s what best matches our observations. We’ll assess the potential influence of differences in incidence angles and slopes on the derived zones and how this might change their calculated extents.

Anonymous Referee #2

The authors use Envisat Advanced Synthetic Aperture Radar (ASAR) imagery for the period 2004-2011 to outline and study the evolution of glacier facies zones over Devon Ice Cap, northern Canadian Arctic Archipelago (CAA). Glacier facies zonation is validated using a combination of observational data including in situ surface mass balance (SMB), ice cores, ground-penetrating radar (GPR) transects and climatic measurements at automatic weather stations. The technique proves to successfully map the evolution of glacier ice, saturation/percolation and dry snow zone in time (2004-2011). The study highlights a clear inland migration of glacier ice and saturation/percolation facies at the expense of dry snow zone, threatening the sustainability of Devon Ice Cap in the future when increased meltwater runoff at all elevations lead to enhanced mass loss.

This is a sound, well documented study describing a remote sensing technique that successfully map changes in glacier facies zonation over a small ice cap. The paper is of great interest, well written, with clear figures illustrating the methods and main results of the study. Comparison to the results of a climate model providing spatially continuous SMB maps over Devon Ice Cap, more detailed discussion of the SMB response to changes in glacier facies, and some clarifications at places would make the study more robust and appealing for a broader audience. Including model outputs would increase the scientific impact of the paper, combining all available instruments/data (in situ, remote sensing and model) and extending the discussion on the ice cap response to facies changes to 2017. In brief, I deem that the current manuscript requires minor revisions before acceptance for publication in *The Cryosphere*. Hereunder, the authors can find my remarks listed as Substantial, Point and Stylistic comments, which should be addressed before acceptance.

Substantial comments

1. The authors should better describe the sign convention (positive/negative) of the backscatter intensity measurements (σ^0 in dB) from Envisat ASAR. In addition, a Table listing typical σ^0 values (minimum/maximum thresholds) for the different glacier facies zones (glacier ice, saturation/percolation, pseudo dry snow) would facilitate the interpretation of e.g. Fig. 2. This extra Table should be discussed in Section 5.1 i-iii.

We'll add a table showing typical relative backscatter values for each zone. We also feel that Figure 4 already really helps with the interpretation of Figure 2.

2. Mass balance is often used when referring to “surface mass balance” (SMB) i.e. the difference between mass gain from precipitation and mass loss from sublimation and meltwater runoff. This is confusing because “mass balance” quantifies the difference between SMB and solid ice discharge from calving glaciers. To clarify this, the authors could reformulate L33 as follows: “Surface mass balance (SMB), i.e. the difference between accumulation from precipitation and ablation from sublimation and meltwater runoff, is estimated to have accounted for 48% [...]”. Then “SMB” should be used instead of “mass balance” across the manuscript. At L36, the authors could refer to Noël et al. (2018) to estimate present-day SMB (1958-2015) for the northern CAA, highlighting that mass loss from ice dynamics is negligible compared to surface processes.

OK – will do. Thanks for the suggestion.

3. The discussion/conclusions (Sections 6/7) could elaborate on the impact of upward migration of glacier facies zones (bare ice and percolation zone) on the contemporary/projected mass loss of Devon Ice Cap, and the CAA ice masses in general. In line with the current study, Noël et al. (2018) show a progressive saturation of the remaining firn pore space of CAA ice caps caused by intensified melt following the recent Arctic warming. Reduction of the firn zone extent and snow buffer capacity has recently accelerated the mass loss of these ice caps through increased meltwater runoff. The authors should mention these corroborating results in their discussion/conclusions. For the long term perspectives, the authors should refer to model climate projections suggesting drastic reduction in firn retention capacity of CAA ice caps within the next century (Lenaerts et al., 2013). These publications demonstrate that upward migration of glacier facies do have an immediate impact on the ice cap mass balance as opposed to conclusions drawn at L479---480. This statement should be reformulated in the revised manuscript (see also point comment L479-480).

Will revise our discussion to include Noel et al (2018) and Lenaerts et al. (2013). Our findings are in line with these papers, but we'll be more explicit about it.

4. In Figs. 4-6, the authors should strongly consider comparing their observational data to a climate model e.g. SMB product at 1 km statistically downscaled from RACMO2.3 that is freely available without conditions (Noël et al., 2018). Including model data would not only make the comparison more robust and comprehensive but also increase the scientific impact of the paper, as it would combine all instruments/data currently available: in situ, model, and remote sensing. This could also enrich the discussion on the SMB response to recent migration of glacier facies, as well as extend trends in ELA migration to 2017 (see also Substantial comment #3). In Fig. 4a, contours of annual ELA (SMB = 0; e.g. in yellow) could be outlined and compared to the ELA derived from remote sensing. In Figs. 5-6, evolution of modeled ELA could be compared to in situ and remote sensing estimates.

This is a good idea. We'll look at the Noel et al. (2018) downscaled RACMO2.3 outputs and see how they compare to our results.

Point comments

L10: “surface mass balance” instead of “mass balance”. This holds for the whole manuscript.

Will do

L29-30: The authors should better use “±” instead of “+/--”.

OK

L32: For consistency, the $493 \text{ kg m}^{-2} \text{ a}^{-1}$ estimate could be integrated over northern CAA ice masses to obtain a value in Gt a^{-1} (listed in brackets), comparable to the value mentioned at L29.

OK – will do. We'll also add reference to this new paper, which provides updated numbers: Box, J.E. 2018. Global sea-level contribution from Arctic land ice: 1971–2017. *Environmental Research Letters*, 13, 125012.

L32-36: Here the authors could mention mass loss estimates from Noël et al. (2018). Their Table 1 clearly shows that decreasing SMB, governed by increased meltwater runoff, is

the main driver of recent mass loss over solid ice discharge (see also Substantial comment #2).
OK

L56: “[...] air temperature and in situ SMB data.”
OK

L80: “NW” is not defined in the main text. While it obviously means “northwest”, the authors should either write: a) “northwest sector (NW)” or b) “northwest sector”. If a) is chosen, the northwest transect/sector should be referred to as “NW” across the rest of the manuscript e.g. L158, 261. The same holds for “northeast (NE)” e.g. L401.
OK – we’ll change throughout

L84-85: This sentence is confusing; I understand that 58% of the mass loss between 1963-2009 occurred after 2000. Could the authors clarify and reformulate?
Yes, 58% of the loss occurred after 2000. We’ll reword this to make it clearer

L92-93: The authors certainly mean “runoff of meltwater” instead of “surface melting”.
Correct, will change

L102-103: I suggest: “Here, meltwater percolates into the firn and further refreezes below the last summer surface, resulting in internal accumulation.”
OK, will put a variation of this

L104-105: I suggest: “In the percolation zone, meltwater percolates within the annual surface layer where it is retained and refrozen.”
OK – will change as suggested

L107: “[...], where (almost) no melt occurs.”
OK

L110: Here a brief description of the meaning of dB unit and sign convention would be instructive; some explanation arises at L151 which appears to be too late (see also Substantial comment #2).
OK, will provide explanation

L124: “SMB” instead of “surface mass balance”.
OK

L154: The filter was used to remove noise in the σ^0 field? Please clarify.
Correct, will clarify.

L170: Is “ringing” a jargon word? Does it mean “resonance”? Please clarify and/or reformulate.
Ringling is a common GPR term that describes repeated returns that originate from reflections from layers with a strong dielectric contrast (e.g., air/snow interface). We’ll modify the wording to include a definition similar to this.

L173: “Dev1H (550 kg m⁻³; Fig. 1)”. The authors should also add a sentence mentioning the location of ice cores in the caption of Fig. 1.

OK, will include reference to Figure 1

Section 5.1: Here the authors should include a Table listing typical σ^0 values for each glacier facies, and refer to this Table in subsections i--iii. L225: “[...] negative values ($\sigma^0 < 0$). [...]”. L245: “This boundary coincided with $\sigma^0 = 0$ in Envisat.”. L250: Here a Table listing typical σ^0 values would be appreciated as “ σ^0 values are low” is rather subjective and could mean either a) negative values or b) smaller σ^0 positive values than observed for the percolation/saturation zone. L261: To reproduce the author’s technique, the “ELA σ^0 value” is required and should be listed in the additional Table.

OK, will include table and refer to it, and provide specific values in the text where suggested

L217: The authors should refer to Fig. 2 instead of Fig. 4, since Fig. 3 has not been discussed yet. It’s Figure 4 that delineates the facies zones, so we believe that it’s the most appropriate to refer to here. We’ll double-check that all of the figures are referred to in the correct sequence throughout the paper.

L260 and L332-333: Why did the authors estimate a unique σ^0 value based on local in situ measurements instead of spatially varying σ^0 values derived from e.g. ELA (SMB = 0) from a gridded climate model? How would this affect the outlining of glacier facies? Is it reasonable to assume that the σ^0 thresholds are constant in space, or that the SMB transect, on which the σ^0 is based, is representative of conditions over the whole ice cap? Please clarify.

The superimposed ice zone and glacier ice zone is indistinguishable using ASAR alone. We therefore believe that in situ measurements provide a better indication of the superimposed ice/glacier ice boundary than a gridded climate model, but will verify our results with those derived from RACMO2.3, particularly for locations away from our transects.

L263-265: Could the authors point to this in Fig. 2?

We’re going to add a new high resolution figure that shows backscatter variability for Devon Ice Cap (see replies to comments for reviewer 1), so will include reference to that here.

L284: Add a reference to Fig. 3c as: “0dB σ^0 value (Fig. 3c)”.

OK

L286: “(Fig. 3a)” instead of “(Fig. 3c)”.

OK

L322: Fig. 5 is described before Fig. 4. The authors should swap Fig. 4 and Fig. 5. L332, 333 and 337: “(Fig. 4)”. L346, 359, 372, 385, 387, 400 and 468: “(Fig. 5 [...]”. Fig 4 is first listed on L217 ; Fig 5 on 322

Fig 4 is first listed on L217, and Fig 5 on L322, so we believe that the sequence is correct. However, we’ll check the sequence of all figures, and update as necessary

L356-359: For comparison, the authors should consider showing changes in ELA elevation from e.g. RACMO2.3 at 1 km in Fig. 5, and compare the model-derived ELA migration rate with in situ/remote sensing estimates.

OK, we'll try this

L373: Add "(Fig. 5b)" after "Devon Ice Cap"; remove "(Fig. 4b)" at L374.

Will add Fig 4b to that line and remove it on L374

L382: Add "(Fig. 5)" after "pseudo dry snow zone".

We don't believe that it's necessary in this case

L395: Could the authors provide a p-value for these regressions?

OK (a: p-value = 0.0051 ; b: p-value = 2.4×10^{-6})

L412: What do "its" and "there" refer to in this sentence? Please clarify and reformulate.

Its and there = superimposed ice / glacier ice zone. Will reformulate.

L439-448: Here the authors could elaborate on the recent and projected response of changes in glacier facies on Devon Ice Cap SMB as mentioned in Substantive comment #3.

Will do, and will include Lenearts reference and Noel references.

L466-469: This sentence is somewhat insubstantial and does not address whether, after the study period 2004-2011, the whole of Devon Ice Cap more frequently experiences surface melt (with a peak in the extreme year of 2012). The author's statement could be verified using melt and runoff fields from a gridded climate model for the period 2004-2017. While I understand it may be beyond the scope of this study, this question is worth being addressed with more robustness than the current statement. Therefore, I strongly encourage the authors to extend their discussion/conclusions on the SMB response to glacier facies changes after the Envisat period using climate model data (2004-2017; see also Substantive comment #3). Could try to include a projection based on a gridded model, but like you may it is beyond the scope of this paper.

We agree that this would be interesting, but is beyond the scope of this paper. Our data and results are focused on the period 2004-2011, so we don't feel comfortable making projections beyond this without significantly more verification.

L479-480: The sentence: "While these changes do not have an immediate impact on the mass balance of the ice cap" is not supported by Noël et al. (2018). See for instance Fig. 7(c-f) showing how runoff has increased after the mid-1990s due to drastic reduction in retention capacity of the fast retreat of the firm zone for CAA ice caps, including Devon Ice Cap. The authors should, in my opinion, revise their conclusions and reformulate accordingly.

Will include Noel reference and reformulate that section.

L489--491: To support increased melt after the mid-1990s, the authors should also refer to the recent work of Noël et al. (2018) (see their Fig. 7).

Will do

Stylistic comments

L101: Maybe “persisting throughout summer and being”.

L158: “(red line in Fig. 1)”

L184: “ice lenses” instead of “ice layers”.

L189: “[...] of 57 poles (blue dots in Fig. 1) [...]”.

L193: “[...] ~400 m a.s.l. (green dots in Fig. 1).”.

OK to all these

L199: “[...] net balance between year n-1 [...]”.

We believe this may make the wording more confusing, so prefer to leave it as-is

L203: “[...] from positive (accumulation) to negative (ablation) is identified [...]”.

OK

L284, 306 and 321: “ $\sigma^0 = 0$ line” instead of “0 dB σ^0 value”.

We prefer to leave this the way it is, as we believe that it’s clearer

L383: Replace “with σ^0 values < 0 dB” by “($\sigma^0 < 0$)”.

L390: “negative” instead of “< 0dB”.

L391: “positive” instead of “> 0dB”.

L394: “appears”.

L466-469: Remove “which” and “say lie fairly close together” and write “(Braithwaite et al., 1994)”.

OK to all these

Figures

Fig. 1: “surface mass balance” instead of “mass balance”. An additional statement on the location of ice cores should be inserted in the caption. The authors should include longitude/latitude on the map, as well as a second box in the inset to locate the sector of Devon Ice Cap depicted in the main figure.

OK

Fig. 3: “red cross” instead of “+” in the caption; I suggest “ σ^0 (sigma nought)” in the caption.

OK

Figs. 4-5: Swap Fig. 4 and Fig. 5. In new Fig. 4, replace “mass balance” by “SMB”.

Won’t swap (see comments above), but will replace with SMB

Figs. 4-6: See also Substantial comment #3 for including comparison to model data.

OK

Figs. 7b and 8b: For consistency with numbers mentioned in the main text e.g. L364, the authors should use “m w.e.” as units on the y-axis.

OK, will do