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July 22th, 2015

Dear reviewers and Editor,

We thank all of you for your constructive comments that undoubtedly improved the quality of our manuscript entitled: ‘Area, elevation and mass changes of the two southernmost ice caps of the Canadian Arctic Archipelago between 1952 and 2014’.

Please, find enclosed a point-by-point response to all reviewer comments. Upon request, we can provide a track-change version of the revised manuscript.

We hope that all of our corrections now bring our paper to the required level of quality for publication in The Cryosphere.

Sincerely,

Charles Papasodoro, Étienne Berthier, Alain Royer,
Christian Zdanowicz and Alexandre Langlois

Main changes made on the paper

1. Dedication to Dr. Gunnar Østrem

First of all, we decided to dedicate this paper to Dr. Gunnar Østrem, a pioneer of the study of Canadian Arctic glaciers, especially Grinnell Ice Cap. We thus added this sentence in the acknowledgements section:

'This paper is dedicated to Dr. Gunnar Østrem (Ph.D. Stockholm Univ., 1965) a tireless pioneer in the study of mountain and Arctic glaciers across Canada, who surveyed Grinnell Ice Cap in the early 1990s.'

In addition to that, a brief mention of his works was added in the introduction section (i.e in the previous works paragraph):

'Under the supervision of veteran Norwegian glaciologist Dr. Gunnar Østrem, other measurements were conducted on GRIC in the early 90s by a scientific team from Bates College (Maine, U.S.A.) and the Nunavut Arctic College.'

2. Figures

Following the comments from the different reviewers, we decided to incorporate the panel D of the previous figure 5 within figure 6 since the comparison between historical and recent trends on Terra Nivea Ice Cap (TNIC) is a good complement to the previous figure 6 single graph. The figure 5 thus now only contains three panels and furthermore, panels B and C were swapped. The new figure 6 now contains two different graphs of elevation change per hypsometry.

Figure 7 was merged from 2 different graphs to a single one in order to improve the comparison of both ice caps. New colors were also used.

We added a new figure (now figure 8) that compares a same geomorphological feature on bedrock by three different technologies. This allows seeing the advantage of the Pléiades products for feature identification and thus, for GCP collection that can then be used for photogrammetric process of archive photographs. We added this figure because we thought we didn't insist enough on the important advances provided by the GCP collection from Pléiades products.

The previous figure 8 is now the figure 9 in the new revised manuscript. Following reviewer 1 advice, we decided to invert the Y axis of the panel A. We also combined Panels B and C for a better comparison. We finally removed the previous panel D for many reasons. First, the area changes were already presented in another figure; second, the comparison of areal changes with Way (2015) is already described in the discussion section. Third, we found it more optimal to make this figure only 'climate-related'. Thus, the new figure 9 only has 2 panels.

Those are the main changes on the figures; see the following point-by-point comments for further minor changes.

3. Density factor

Following reviewer 2 advice, we instead chose a density constant of 900 ± 17 kg m-3 for the recent period of mass balance estimation on Terra Nivea Ice Cap. A brief look at images of 2007 and 2014 has shown no significant firn area so this new density constant is more appropriate for that type of ice. The previous density constant was kept for the historical estimation since the firn areas were likely more significant during the 60 years of study.

5. Changes in the climatic factors

Many things were changed in the climatic factors section, following some of the recommendations of the two reviewers. First, we removed every mention of Arctic amplification. We instead investigated the contribution of the longer sea ice retreat (i.e. later freeze-up in autumn) as an additional cause of stronger mass loss, after the main cause which is the rising summer temperatures. We also improved the analysis of temperatures and positive degree-days of this region by using the Adjusted and Homogenized Canadian Historical Climate Data, instead of the raw data.

See the revised manuscript for details.

6. Grammar

One of the main comments from reviewer 2 was to improve the grammar of the paper. For this new revised version, we put the emphasis on the review of the grammar which was conducted accordingly. Once again, upon request, we can send a track-change version of the revised manuscript.

7. Order in the data section

In the data section, the ASTER DEM section was placed after the ICESat section since the latter data are mentioned in the ASTER section. Moreover, the CDED section was moved at 3.2 before the historical aerial photo section that is now 3.3. We found it more obvious this way because in the process, the photos of 1952 were chosen because the CDED was not optimal over Grinnell Ice Cap.

8. Abstract

The abstract has been improved by adding remarks about the climatic factors and the use of Pléiades for exploiting aerial photograph archives.

1st reviewer: R. Way

General remarks: This is an interesting study investigating glacier change for two glaciers on the Meta Incognita Peninsula of southern Baffin Island. I believe it provides new insight and uses a wide swath of available data sources which present a consistent picture of the evolution of Grinnell and Terra Nivea ice caps over the past half-century. Although the paper is certainly not brief, I believe that the lack of existing literature for these particular ice caps warrants the fuller discussion provided by the authors in the introductory material. Overall the science is well-implemented and the results are consistent other recent studies examining the eastern Canadian Arctic. The additional information provided by in situ surveys along portions of Grinnell Glacier represent a very important contribution in that the results have been validated using both field and remote sensing-based methods. The use of a Pléiades-derived DEM for ground control on photogrammetry-based DEMs is also a novel methodological approach and hopefully its widespread application for other, more topographically complex environments is investigated in more detail in future works. Overall, this contribution warrants publication although I believe some of the Figures could be presented in a clearer fashion.

P1668L23-25 Two of the references for the currently observed Arctic warming section refer to paleoclimatic reconstructions, which although very interesting, may not be necessarily the best studies to cite with respect to the Arctic being currently warming rapidly. I would suggest Comiso and Hall (2014) or Cowtan and Way (2014) as perhaps being more appropriate in these cases, particularly in that they show the regional distributions of warming trends over the recent periods from satellite and surface temperature records.

Kaufman et al. (2009) has been deleted since it is not focusing on the very recent warming (post-2000). We disagree for deleting Tingley and Huybers (2013) because it uses both recent observations and paleo-data and have the merit of putting recent observations (post-2000) in a long-term perspective. Comiso and Hall (2014) was added as it supplements the referenced warming studies with the satellite components.

Study Area Section: P1670-1671 (1) Lenaerts et al (2013) modelled the surface mass balance for the ice caps to be strongly negative – this point might be useful to add to this section. **(2)** The authors note several of the major results suggested by previous work in the area but not the the recent results of the complimentary study by Way (2015) mentioned at the end of the introduction. Given the overlap in some aspects of the areal change analysis it may be worthwhile to note the major findings of that study in the same brief manner that it was done for the other studies in the Study Area section. Notably that areal decline has accelerated, that ice thinning appears to be occurring and that recent increases summer melt intensity were linked to this decline. In my view, this is a nice tie-in to the current study because Way (2015) qualitatively suggested that ice cap thinning was ongoing (Nunatak exposure) but did not present a quantitative assessment like this study does.

(1) We decided not to add any mention from the study of Lenaerts et al. (2013) because the resolution of this study is 11 km, which we considered too coarse to compare with our results. Our ice caps (TNIC and GRIC) are represented by ~1 single pixel in their modelling effort.

(2) A comparison to Way's results is already done in the discussion section and we find it unnecessary to repeat it in the study area section. The Way (2015) paper is also already mentioned in the introduction.

Data Section: P1672L2-5 Although it is true that in some late-summer images there appears to be very little winter snow accumulated (even at high elevations), there is a high degree of volatility in summer snow cover for these ice caps. A casual glance at Landsat imagery over the past two decades reveals both years where there is very little late-season snow cover and where it is more widespread therefore it may not be safe to assume that the absence of winter snow cover is an annual occurrence.

Reviewer advice is true and reviewer 2 made a similar suggestion. Thus these sentences were removed: '*This is particularly true for the present images, since the ice caps were nearly winter snow free. Thereby, this is a good qualitative primary concern about the ice caps' fate (Pelto, 2010).'*

P1672L10-20 (1) Using aerial photography in high snow accumulation environments can be very difficult, particularly when using 1950s-era black and white photography from the Canadian Arctic. The authors note that the photos are much higher quality than the photos used to derive the CDED data in terms of scale but how accurate was the delineation between snow cover and ice for these particular images. **(2)** The manuscript currently has a number of figures so I will leave it up to the editor and the authors to determine if they feel this is a worthwhile suggestion but given the exceptional nature of the photographs and the lack of field photographs of the region, would it perhaps be useful to have a two-panel figure which shows a portion of the ice cap in the aerial photography from 1952 and then again in 2014 in the satellite imagery so that readers can cross-compare and also evaluate the quality of each image source.

(1) The two sentences that cover lines 16 to 21 were replaced by those sentences: *'These photos, exceptional in their quality of detail and texture, were used for the extraction of historical elevations on GRIC and were preferred to the CDED for this ice cap. In fact, the CDED covering GRIC contained major artefacts (i.e. much larger than for TNIC) in the large snow-covered accumulation zone where the texture was particularly weak on the 1958 photos and thus, was not suitable for historical elevations of this ice cap.'*

(2) We decided not to add a figure to compare the look of the ice cap on both the photos and the Pléiades image. Instead, since we focus on the approach of GCP collection on Pléiades products, we added a figure about this approach. See comments below and new Figure 8.

P1673L1-4 Was this comparison done for the region and if so what was the average elevation difference in this area?

The raw CDED was in the CGVD1928 altimetric reference, while all our other data were in ellipsoid reference (WGS84 or GRS80). So a direct elevation difference calculation CDED vs ICESat would include a bias related to the different altimetric systems. Instead of including our own analysis that would contain this error, we rather added the results from an analysis that was already conducted. However, we removed statistics from Beaulieu and Clavet (2009), that are from different parts of the whole Canadian Arctic, and replaced it in the revised manuscript by the analysis done by Gardner et al. (2012) for the 340 CDED covering Baffin Island (including our area) to show the good accuracy of the CDED. Thus, we added this modified sentence:

'The average elevation differences and their standard deviation (SD) were previously calculated off glacier for 340 CDED maps tiles covering Baffin Island and ICESat laser altimetry points and were reported to be 1.1 m and 5.1 m, respectively (Gardner et al., 2012).'

P1673L20-23 Was the vertical precision spatially or more importantly altitudinally influenced?

We verified the vertical precision as a function of elevation for the 57 available points and we found nothing significant. Note that 57 points is a very little sample for this kind of analysis. In any case, we already considered the errors to be 100% correlated so it is not a problem here. Given all of this, we did not modify anything regarding this in the revised manuscript.

Meteorological data: Is this data from the Adjusted and Homogenized Canadian Historical Climate Data or just the raw measurement from the online database. The reason I ask is because eastern Canada was impacted by the time of observation bias when the climatological day was redefined in 1961 and this appears to have had an observed impact on minimum temperatures for Iqaluit's station based on Vincent et al's (2009) analysis.

To ensure that no such bias was included in our analysis, the adjusted and homogenized Data of Iqaluit weather station (Vincent et al. 2002) were analyzed for this revised study instead of the raw data. These data have been bias-corrected and homogenized, in particular for consistency of minimum air temperatures used in our PDD calculations. We also refer to the latest analysis of Vincent et al. (2015) for Arctic-wide climate trends in Canada. These details were added to the text of the section 3.8, the resulting new PDD data can be seen in figure 9 (formerly 9), panel A, and the brief analysis of the impact of the PDD was also changed in section 6.3.

Methods: P1680L13-19 although I believe that the approach used for calculating uncertainties in the respective DEMs is undoubtedly valid, I would suggest that an additional caveat be added to the discussion given that the uncertainty in DEMs derived from aerial photography and ASTER would possibly have larger errors at high elevations for the ice caps relative to at the same elevations for unglaciated terrain because of the difficulty in pixel matching. This point would be mitigated if each of the DEMs were collected in years without substantial high elevation snow cover (like the Pléiades DEM) but that is unlikely to be the case for all the imagery used.

This is a good point. This said, the difficulty in pixel matching for higher altitudes of the ice caps occurred only for the 1952 DEM and the CDED (not for the ASTER DEM, see the comments below). At page 1680 (lines 16-19) we already mentioned the possible occurrence of errors related to artefacts and low coverage at higher altitudes and we took account of it in the conservative uncertainty approach of our two historical calculations. Thus, we think that this technical point is already enough addressed in the methodology part and should not be explicitly mentioned again in the results or the discussion parts.

P1681L5-8 The 3% uncertainty seems to be appropriate for the most recent imagery where a detailed evaluation could be made but I believe this estimate may be a little optimistic for the earlier aerial photography where it can be more difficult to interpret between late-season snow and ice. This perhaps should be noted.

We agree with the reviewer. This uncertainty has been conservatively increased to 5% for both the old margins of the Grinnell Ice Cap (1952) and the Terra Nivea Ice Cap (1958/59) to account for the slightly more difficult distinction between late-season snow and ice. This had no impact on historical mass balance uncertainties but it changed a little the uncertainty of area changes.

Results: P1681L19-26 These results should be also in Table 2.

Area changes results were not added in the table 2 since they are already provided in figure 2. See below what was modified on figure 2.

Discussion: P1684 Section 6.1 The Pléiades DEM being used for photogrammetric ground control is interesting and is a very worthwhile contribution. The authors may note that multiple Pléiades acquisitions subsequent DEMs over the same area could be useful to increase the confidence in its use for ground control.

Good point, this certainly goes into future works but we don't think it is worthwhile to add in this paper. Further, we actually think that it would be a better use of fundings and satellite resources to target Pléiades acquisitions over other ice caps in the Canadian Arctic than repeating the acquisitions over Grinnell and Terra Nivea ice caps with a time span of only 1 or 2 years.

P1685 Section 6.2 Although the mass change results are placed in the context of the surrounding ice caps and glaciers the area change results are not. It may be worthwhile to add a few brief comments on this to the discussion, for example there were area changes noted on the small glaciers to the northeast on Baffin (Paul and Kaab, 2005/Paul and Svoboda, 2010/Svoboda and Paul, 2010), on northern Baffin (Anderson et al. 2008) and the south in the Torngats (Brown et al. 2012/Way et al. 2014), not to mention many of the other nearby ice caps.

We haven't compared our area changes results to other ice caps located nearby, since it was already done in Way (2015). Nonetheless, we believe it is a good idea to mention that this comparison is already done in the paper of Way (2015). Here is the sentence that was added to the section 6.2:

'A comparison of the areal declines of GRIC and TNIC with those of other Baffin Island ice caps was already conducted in Way (2015) and is thus not presented here.'

P1687L13-20 (1) Although I can certainly see the importance of evaluating the sum of positive degree days there are limitations to its applicability in this case. A modest increase in the length of the melt season can be inferred from the results but its importance for the strongly negative elevation change rates is probably less than the importance of the melt season intensity. For instance, the meteorological analysis presented by Way (2015) suggests that although the melt season duration has modestly changed the melt intensity has substantially increased in the region. As a result, the ratio of warm season cumulative thawing degree days relative to cumulative freezing degree days has nearly doubled over the past decade (Way, 2015; Figure 5C). It would perhaps be more effective if the authors either referenced this finding or calculated melt-season thawing degree days which could be added to Figure 8 Panel A instead of the positive degree day sum. **(2)** I think it might also be worthwhile to mention that there is no particular trend in cold-season precipitation for the region (strengthening the result that mass changes are melt-driven).

(1) We believe that the Positive Degree-Days (PDD), which are widely and commonly used in glaciology, are appropriate as they encapsulate the cumulative effects of both the length and intensity (warmth) of the melt season. A difference between our PDD and the analysis of Way (2015) is the time interval: we used May-November whereas Way (2015) used April-Sept. Our May-November interval was chosen based on the fact that there are almost no PDD at all in April over the period of record, and (b) there are a few in November, particularly in recent years. According to all of this, we think it is out of scope to mention Way's results here.

(2) Different parts of the section 6.3 have been rewritten and we consider the mention of cold-season precipitation to be out of context of our analysis.

Conclusion: P1688L24-26 "…regional warming is linked to strong near-surface warming possibly caused by summer sea ice losses."

Following the rewritten section 6.3, this sentence was modified this way:
'…the ice cap wastage is linked to a strong near-surface regional warming and a lengthening of the melt season into the autumn that is possibly indirectly linked to the later freeze-up in Hudson Strait.'

Comments on Figures:
Figure 2: I believe that Panel C distracts from the focus of this figure, particularly given that there are so many outlines on the two images and also because it overlaps portions of both ice caps. A nearly duplicate figure to that of Panel C is also shown in Figure 8 Panel D. I would suggest that the areal changes provided in Figure 8 Panel D should remain (albeit in an altered form – see comments below) but that Figure 2 Panel C should be added to the Table 2 which could be slightly reconfigured to have both mass balance and area change.

We disagree with the reviewer on this. We believe that the complete figure 2 (with the three panels) makes it easier for readers to follow visually what happened on the maps as well as quantitatively on the graphs below. We however agree that the three different panels had to be slightly rearranged to enhance the readability of the figure. This was done.

Figure 4: The organization of the legend and line graph should be arranged in a more consistent manner as currently they appear to be disorganized.

The map legend and line graphs were rearranged.

Figure 5: (1) Captions b and c need to be rearranged. **(2)** From my perspective I do not believe that Figure 5 Panel A is needed or that it adds a particular amount of insight that could not be gleaned from Figure 5 Panel D currently. We can see from Figure 5B and Figure 5C that elevation changes have accelerated and Panel D emphasizes that**. (3)** Considering the uncertainties in ASTER DEMs I also find the area of elevation gain near the interior of the ice cap to be curious because upper elevations are more likely to be snow covered and therefore be more uncertain in the matching process of DEM generation. Do the icesat validation results for the ASTER DEM used by the authors suggest that accuracy is high at these upper elevations?

(1) Captions of maps B and C were slightly rearranged.

(2) We think that panel A is as valuable as panels B and C because it shows spatial variations of elevations changes for a different time interval. We believe that the three panels together well present historical and recent spatial trends. Following the comments from M. Pelto (see below), we instead decided to incorporate the panel D within the figure 6 because the comparison between historical and recent trends on TNIC is a good complement to the previous figure 6 single graph. The figure 5 thus now only contains three panels and furthermore, panels B and C were swapped.

(3) We understand that the elevation gain in the upper elevations mentioned by the reviewer is on the panel A, where we can see yellow (-0.25 to 0.25 m/yr) in the higher elevations. It might be curious but it is accurate. First, this class interval includes elevation gain and elevation lowering (i.e. -0.25 to 0.25 m/yr). After verification, this section of higher elevations on the ice cap is mainly characterised by little elevation lowering, rather than elevation gain (as we can also distinguished on the previous panel D that is now on figure 6).

Furthermore, looking to the 3B and 3N ASTER images that were used to automatically generate the DEM, the accumulation area is very weakly snow-covered. In fact, the images texture suggest a humid surface, rather than a snow surface, so the image matching was likely successful for the 2007 ASTER DEM (see the figures 1 and 2 below). We don't have any ICESat data from 2007 that covers the accumulation area to verify the accuracy of the DEM in those upper elevations. This said, patterns of d*H/dt* on panels B and C seems coherent and thus, suggest it is also the case for the panel A.

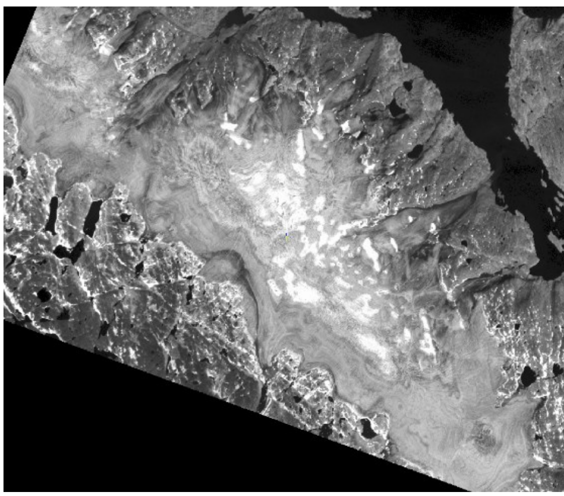

Figure 1. ASTER 3B

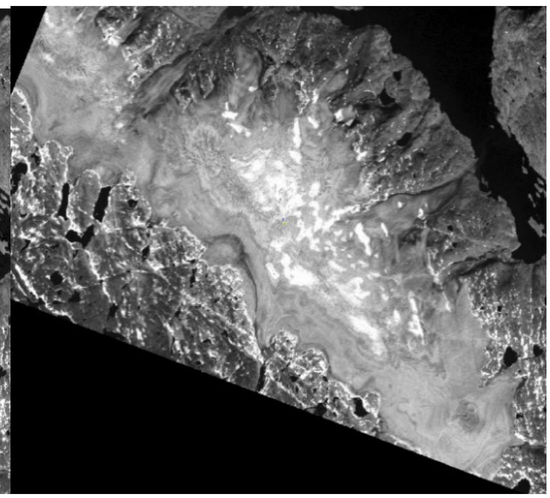

Figure 2. ASTER 3N

Figure 6: Top panel appears to have a portion of the graph cut off on the left.

Done. This figure now contains a second panel that was previously panel D of the figure 5.

Figure 8: I do not particularly like that Figure 8 Panel A has been inverted. I understand the point in that it is inverted to show a similar scale to the other three graphs but I believe that it is more intuitive to flip the scale to make sense (e.g. an increase in positive degree days).

We agree. The axis of this panel has been reversed back.

Figure 8 (1) Panels B and C show very similar results and perhaps should be combined – this would allow for panel A, B (combined) and C to be enlarged which is necessary for Panel D to be interpreted more easily**. (2)** As noted in the discussion of Figure 2, I believe that Panel D in Figure 8 duplicates the results to some degree from that earlier figure. I suggested that Figure 2 Panel C be removed and information be added to Table 2 but that Figure 8 D is retained. I do not necessarily believe that the dots from this study on Figure 8 D be connected by lines as that suggests that the results of this study and those of Way (2015) conflict whereas I believe they are very complimentary. These ice caps undoubtedly show large year to year variations in the amount of late-season snow cover which is why Way (2015) used multiple images for each average. Therefore it is not unexpected that deviations from the best fit lines would occur. The lines also suggest that substantial ice losses would have occurred between the 1950s and the mid-1970 results from Way (2015) whereas I believe that this is somewhat at odds with what is presented in that analysis. I suggest that the dots (all) are connected by a dotted line and that error bars are shown for the area estimates. The enlargement of the figure suggested above would facilitate this to be done and would enable a more useful figure.

(1) Done. Panels B and C were combined

(2) We finally decided to remove panel D of this figure for different reasons. First, it is true that this panel is very similar to the panel C from figure 2 so the added-value of this panel is weak. This is especially important for our paper since it already contains a great number of figures and that in response to the short comment by M. Pelto, another figure was added. Second, there are already many figures about glaciological changes and we find it reasonable that this figure could be only ‘climate-related’. Finally, the comparison of our results with other studies is also already done in the discussion section. Thus, the final figure 8 (newly figure 9) contains two panels: a first panel for Positive Degree-Days and second one for sea ice covered area in both Hudson and Davis Straits.

Short comment from M.S. Pelto

General comment

Papasodoro et al (2015) provide a valuable long term assessment of the changes over a 60 year period on the Grinnell Ice Cap and Terra Nivea Ice Cap. They utilize the recently launched Pleiades 1A and 1B satellites to supplement ICESat, ASTER and Historic aerial photograph based DEM's to assess changes in the glaciers. Two points that deserve more attention and would increase the value of the paper are: 1) Greater attention to the value that Pleiades imagery brought to the project. An additional figure may be needed of a small region to best illustrate this. The conclusion needs a mention of the added value. 2) The lack of a consistent accumulation zone without which a glacier cannot survive should be emphasized. Including reference to Landsat 8 imagery, even if not used in a figure. The thinning rate from 2004-2014 on the upper areas of both ice caps where the accumulation should be indicate that there is not an accumulation zone most years including superimposed ice.

Specific comment:

1670-22: Satellite images can show that this is true. Both 2012 and 2014 images (Fig. 1 and 2) illustrate the lack of retained snowcover. The imagery does not illustrate superimposed ice. However, superimposed ice cannot be retained if year after year there is now no snow or firn remaining. Further there can be no superimposed retained in an environment where ice thickness is being lost at more than a meter per year in the region where it would accumulate, and dynamic thinning is not capable of causing the change.

Visual observations at the summit of Grinnell Ice Cap in the early spring of 2003 and 2004 by one of the authors (CZ) showed that there was superimposed ice (SI) below the seasonal snow cover. It definitely was not firn, nor was it pure glacier ice. If SI was the main form of accumulation for an extended period of time (as probably was the case on this ice cap), it may take a while before it is entirely removed by successive high-melt years. In fact, there may not be any left at present. However, to avoid ambiguity, we decided to delete the sentence about superimposed ice from this paper.

On the following sentence, we added this:

'Hence the summit of the GRIC is probably close, or slightly below, the present-day equilibrium line altitude (ELA), making it highly susceptible to experience net mass losses (Pelto, 2010).'

1681-10: How much of the area change is due to expansion of nunatak/bedrock areas amidst the GIC? This is where the Pleiades imagery could be illustrated to best advantage. For a specific nunatak how accurately can the area be determined using Pleiades versus the Aug. 2014 Landsat 8 ore aerial photographs?

We agree that the use of Pléiades for GCP collecting is an important advance of our paper. We thus added a figure (figure 8) that compares the representation of the same geomorphological feature on ice-free terrain with different technologies, namely an aerial photography (August 1952), a Pléiades panchromatic band (3 August 2014) and a Landsat 8 panchromatic band (15 August 2014). Given our quite long paper, we decided not to insert any new figure or analysis of nunatak determination using Pléiades but only to focus on GCP collection. Also, Way (2015) already focused on nunataks of GRIC.

We added this sentence in the section 6.1 (discussion) for analyze of this figure:

'Furthermore, the very fine resolution of Pléiades can help to improve the accuracy of nunataks and/or whole ice caps delimitation, especially when compared to the frequently used Landsat images (Fig. 8).'

1682-16: Elevation change rate sharply decreased should be rephrased. The rate elevation loss greatly increased.

Given the negative values, we believe that the correct formulation would be 'decreased'. However, 'increased' is certainly more obvious for those kind of results. We thus used 'increased'.

1682-23: The change of -1.7 m in elevation at the highest of the icefield indicates the lack of an accumulation zone. This change in elevation would be useful to show in Figure 6. It is more important to show the increase in rate of ice loss in recent years compared to 1952-2014 than simply showing the long term trend. This comparison is shown in the small insets in Figure 4 and 5, but either need their own figure or be shown in Figure 6. Further as Pelto (2010) notes this is a clear indication that neither ice cap can survive current climate, let alone further warming.

We agree with this comment. We thus merged the panel D of figure 5 to the previous figure 6. Those two graphs go well together to compare historical and recent trends.

1683-22: You can use AAR to estimate mass balance based on ELA identification from Satellite imagery. Not suggesting you need to do this, but it is not accurate to say mass balance cannot be estimated.

Reviewer is partially right here because the ELA is above the maximum elevation of Grinnell Ice Cap now so the AAR method could not work for this particular ice cap. However, to avoid ambiguity, we removed this sentence.

1688-7: A key aspect of the paper is utilizing the Pleiades data, can you elaborate here on the advantages that were realized from these DEM's.

We added this sentence:

'This approach takes fully advantage of the highly precise Pléiades products and represents an important advance for eventually unlocking the vast archives of historical aerial photographs.'

We also decided to add this sentence to the paper abstract:

'On a methodological level, our study illustrates the strong potential of Pléiades satellite data to unlock the under-exploited archive of old aerial photographs.'

1684-13: The utility for Ground Control Point position determination is an important advance. At the same time that the nunatak expansion is illustrated, the GCP ability could be illustrated.

This was added, see the comments above.

1688-23: In the areas noted by the three references Kerguelen, Southeast Alaska and Patagonia there is considerable loss by tidewater glacier calving. Further all of these areas have substantial accumulation areas remaining. In the case of GIC and TNIC the snowline is rising above the glacier and there is no calving loss. The ice losses are not sustainable with current climate, since there is no accumulation zone. That is vastly different from the other regions in terms of impact and should be emphasized.

We agree with the reviewer. Thereby, here is the new version of this comparison:

‘The 2007-2014 mass balance on the TNIC is among the most negative multi-annual glacier-wide mass balances measured to date, comparable to other negative values observed in the southern mid-latitudes (e.g., Willis et al., 2012; Berthier et al., 2009) or in South-East Alaska (Trüssel et al., 2013). Given the absence of calving for TNIC, its high rate of mass loss can only be explained by negative surface mass balance due to an ELA that, for most years, is above the maximum ice cap altitude. Nonetheless, this similarity in rate of mass loss underlines the strong sensitivity of maritime low-elevation ice bodies to the currently observed climate change at mid-latitudes and in polar regions (Hock et al., 2009).’

Figure 7 Inset maps not needed if transects as on GIC are shown on previous diagram. Then the two glaciers can be combined in a single image for a more robust data set comparison.

We agree, the two glaciers were combined in a single graph with different colors, in order to better compare. We however decided to keep the two maps to help the reader. The final figure is more concise.

Figure 8 Panel D should show mass loss/ice thickness change as that is a more robust measure of the change. Panel A,B, and C show a transition to persistent negative values at the same time, does ice thickness rate show same?

For reasons mentioned above in the reviewer 1 advices and in the main comments, we decided to delete the panel D and to let the Figure 8 (newly figure 9) exclusively ‘climate-related’.

2nd reviewer – Alex S. Gardner

Overview and general comment

Papasodoro and coauthors present elevation and area changes for the two southern most ice caps on Baffin Island (Grinnell and Terra Nivea Ice Caps). They use exhaustive multitude of satellite and airborne datasets to reconstruct a sixty-two year record of glacier change. They show that both ice caps have experienced accelerated rates of elevation lowering and area loss in the most recent decades that they attribute to longer melt durations and loss of sea ice in Hudson Bay.

The authors have done a very good job reconstructing glacier changes for the Grinnell and Terra Nivea Ice Caps that confirms and compliments earlier works. Despite their small size, changes in these ice caps provide a good climate proxy for Southern Baffin Island. The authors also analyze elevations generated from Pleiades Satellite imagery and show that they provide good ground control for older imagery.

Overall the paper is in pretty good shape. My two main suggestions for improvement are for the authors to strengthen the attribution section and to carefully review grammar.

Major comments: 1. P1674L23 The RGI outlines for the Grinnell and Terra Nivea Ice Caps come from the CanVec dataset. . . check dates and source imagery provided in that dataset

Sentence was modified this way:

'For 1999, we used the ice cap contour from the Randolph Glacier Inventory (RGI 3.2; Pfeffer et al., 2014), which originates from the Canadian CanVec dataset for this region, itself derived from a September 1999 Landsat 7 image.'

2. P1680L7 I'm not sure that a density of 850 kg/m3 is appropriate for ice caps without significant firn area. I would recommend using a density closer to that of ice unless you can demonstrate that there are likely changes in the firn structure over the period of study.

This is a good point. Following the reviewer advice and after discussions with co-authors, we decided to keep the density constant of 850 ± 60 kg/m3 for the historical mass balance estimation on both ice caps, because we believe it is appropriate to assume that there has been significant firn on the ice caps during the last 60 years. However, we agree with the reviewer for the recent estimation since a brief look at both 2007 and 2014 images of TNIC (not shown here) is showing no significant firn area. Thereby, we instead chose the density constant of 900 ± 17 kg m-3 for this recent period. In the section 4.4.2 and 4.4.3 of the methodology, we modified the sentences this way in the revised manuscript:

'where ρ is the firn and/or ice density. For the historical mass balance of both ice caps, we used ρ = 850 kg m-3 (Huss, 2013), while we used ρ = 900 kg m-3 for the recent period on TNIC (2007-2014). The former value of ρ was chosen assuming that there was a firn zone on the ice cap during the last 6 decades, while a visual interpretation of our images (not shown here) suggests the absence of a significant firn zone after 2007. '

'Finally, an uncertainty of ± 60 kg m-3 (Huss, 2013) was assigned to the density factor when estimating the historical mass balance on both ice caps and of ± 17 kg m-3 (Gardner et al., 2012) for the recent estimation on TNIC.'

Furthermore, the new mass balance value was modified in all the paper.

3. P1686-1689 I would suggest removing any mention of Sea Ice and Arctic Amplification in the attribution section. The link between these two and rates of glacier change are complicated. Sea ice in Hudson Bay is likely responding to warmer spring temperatures but it is unclear how much changes in sea ice extent are in-turn modifying the summer temperatures over the ice caps. Glaciers are responding to warmer temperatures in summer, a period when we expect arctic amplification to be at a minimum. Gardner et al., 2007 and 2012 found that the vast majority of variance in the rate of glacier loss can be attributed to changes in lower-tropospheric temperature.

We consider that for glaciers of Meta Incognita Peninsula, which are located in such close proximity to Hudson Strait (unlike, for example Barnes or Penny Ice Caps), there are good grounds for considering a probable positive feedback associated with the accelerated retreat of sea ice cover, particularly in autumn. This is supported by PDD trends. The main forcing remains summer warming, of course. We have modified section 6 to be more explicit and specific about the possible indirect link to sea ice retreat.

Specific Comments: P1668 Remove acronyms from abstract

Done

P1668 recommend using a different acronym for the Grinnell Ice Cap as GIC is traditionally used for "Glaciers and Ice Caps".

Good point, we changed the GIC acronym to GRIC throughout the paper.

P1668L7 changes -> change Done

P1668L7 in-situ -> in situ Done

P1668L19 "the proximity" -> "the proximity to TNIC." Done

P1668 "In response to the currently observed warming in the Arctic" -> "In response to recent Arctic warming" Done

P1669L2 in 2009 -> during the 2007-2009 period Done

P1669 "An exception is a recent study (Way, 2015) which analyzed the changing rates of areal recession of both GIC and TNIC since the" -> "A recent study (Way, 2015) analyzed changing rates of glacier recession for the GIC and TNIC since the" Done

P1670L1 "data we used" -> "data used to determine glacier change." Done

P1670L2 delete "new" Done

P1670L5 delete "archive" Done

P1670L6 delete "possible" Done

P1670L7 delete "factors" Done

P1670L11 “rising at” -> rising Done

P1670L12 “extent near” -> extent to Done

P1670L15 “range to” -> “range as” Done

P1670L17 “cloudiness” -> “cloud” Done

P1670L20 “reached” -> “is” Done

P1670L22 “formed by in situ” -> “formed by” Done

P1670L23 “showed no firn”, “found no firn” Done

P1670L24 delete “net” Done

P1670L25 rate of surface lowering my melt -> amount of melt Done

P1670L26 very close -> close Done

P1671L13 DEM extraction on glacier -> glacier DEM extraction Done

P1671L20 especially accumulation area -> especially over the low contrast accumulation area Done

P1671L21 “and limit” -> and reduces the Done

P1672L2 delete “due to the more humid surface” Done

P1672L2 delete “This is particularly true for the present images, since the ice caps were nearly winter snow free. Thereby, this is a good qualitative primary concern about the ice caps’ fate (Pelto, 2010) Done

P1672L6 “These stereoscopic” -> Stereoscopic Done

P1672L7 “generation of recent DEMs -> DEM generation Done

P1672L8 “GIC.” -> GIC (Section 3.2). Done

P1672L9 “Archives aerial photos covering” -> “Historic aerial photos for” Done

P1672L11 delete “in this study,” Done

P1672L17 delete “mainly due to the late summer acquisition date.” Modified, see reviewer 1 comments

P1672L19 “elevation on the GIC and were thus -> GIC elevations and were” Idem

P1672L23 define “CDED” Done

P1673L3 An exhaustive validation of CDED for Baffin Island (340 map sheets) is provided in Gardner et al., 2012. They get a bias of +1.1 m and a std of 5.1m Added, see modifications from reviewer 1 suggestion.

P1673L7 “using the derived hillshade in order to exclude obvious false elevations” -> using a derived hillshade to exclude blunder in elevations This sentence was modified to :

Artefacts located in the accumulation areas of the TNIC CDED were manually identified and deleted using a shaded relief image derived from the DEM.

P1673L13 in order to calculate -> To calculate Done

P1673L16 “This DEM” -> “The DEM” Done

P1673L18 “with an horizontal” -> “with a” Done

P1673 did you do any filtering of the ICESat data for cloud returns? Did you apply the Gaussian

centroid bias? No filtering was conducted, only the obvious errors related to cloud were deleted. These error were easily to distinguish because they were of more than a few hundred meters.

P1673L6 were used for recent elevation change calculations -> used to estimate recent elevation changes Done

P1674L12 front of an outlet glacier -> front of one of the outlet glaciers Done

P1674L23 check original RGI source. . . CanVec Corrected, see in general comments

P1675L3 “we rather” -> we Done

P1675L9 August 2014 -> August of 2014 Done

P1675L10 is briefly -> is Done

P1675L12 “In order to” -> “To” Done

P1675L14 station for -> station for the period Done

P1675L24 but only allows reducing the vertical -> but can reduce the vertical Done

P1676L1 can be easily corrected on ice-free terrain with a good reference dataset -> can be corrected over ice-free terrain provided a good reference dataset is available Done

P1676L17 is thus expected -> is expected Done

P1676L24 The typical -> A typical Done

P1677L18 “Quantitatively, 66% of the GIC area was extracted with data gaps concentrated at the highest elevation in the texture-less accumulation areas” -> “66% of the GIC area was extracted with data gaps concentrated at the highest elevation in the texture-less accumulation areas” Done

P1677L22 DEM-based mass balance -> DEM based volume change Done

P1677L4 To evaluate the corrections constancy -> to evaluate the constancy of the corrections Done

P1677L5 DEMs -> DEM Done

P1677L10 delete “for such small zones.” Done

P1677L11 “Furthermore, the” -> “The” Done

P1677L11 DEM -> DEMs Done

P1677L12 subtracted -> differenced Done

P1677L14 These results prove -> these results confirm Done

P1679L1 delete “are useful to” Modified to ‘were used to’

P1679L4 delete “ using different DEMs in order” Done

P1679L5 “and mass balances” -> “from the DEMs” Done

P1679L7 delete “first” Done

P1679L7 delete “in order” Done

P1679L8 changes -> change Done

P1679L8 “, of elevation” -> “and elevation Done

P1679L9 “The no value pixels were assigned to the mean dH of the corresponding elevation band. Total volume change for an ice cap (dV) was then assessed by summing volume changes from all elevation bands (n) as follows:” -> “No value pixels were replaced with the mean dH of the corresponding elevation band. Total volume change for each ice cap (dV) was then determined by summing volume changes from all elevation bands (n) as follows:” Done in a slightly different way : *‘Pixels with missing data were replaced with the mean dH of the corresponding elevation band. Total volume change for each ice cap (dV) was then determined by summing volume changes from all elevation bands (n) as follows’*

P1679L18 “Sensibility” -> “Sensitivity” Done

P1680L7 a mean density of “850 kg/m3” is very low for an ice cap that has little to no firn area. I would recommend a value closer to that of pure ice. See our response to general comments

P1681L25 “has shrunk” = “shrank”, apply through entire document Done

P1682L6 delete “ “,a less pronounced rate when compared to” . . . start new Sentence Done

P1682L15 “(Fig. 5,uppser left map)” -> (Fig. 5a) Done

P1681L26 “with a reasonable” -> “with reasonable” Done

P1686/7 Lower tropospheric warming from advection of continual air masses in summer have been previously implicated in accelrated CAA glacier melt rates (Gardner et al, 2007 and Gardner et al., 2012) References added in the sentence.

P1686 Not sure if there is a clear link for changes in sea ice to be driving glacier melt rates. More thought need to given to link Arctic Amplification and Sea Ice to glacier changes. I would suggest that the authors revisit this section in its entirety.

This section was revisited and modified. All the mentions to arctic amplification were deleted. The sea ice impact explanation was limited to an additional contribution to stronger glacier melt, after the summer temperatures.

Figure 3 add “no data” to legend.

Adding no data colors to maps was too much altering the quality of maps. Instead, we add this mention in the description of the figure 3: *‘For this figure as well as for the next ones, no color (i.e. hillshade is visible) represent no data’*