

Interactive comment on “Long-term contributions of Baffin and Bylot Island Glaciers to sea level rise: an integrated approach using airborne and satellite laser altimetry, stereoscopic imagery and satellite gravimetry” by A. S. Gardner et al.

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Dear Dr. Pelto,

Thank you for taking the time to read and comment on our manuscript. We've responded to all of your concerns/suggestions below.

Sincerely, Alex Gardner and coauthors.

>1566-12: Citing a single 20 year old reference on an issue that has not been resolved is >not sufficient to say that the Barnes Ice Cap played a central role 116 k years ago
C1226

in ice >sheet development. Restate as .."it may have played a central role" or : : ."it played a >role"

Changed. Thank you for the clarification.

>1579-12: There is no commentary here on the fact that BIC has superimposed ice.
>Superimposed ice has a higher density than snow or firn and may have a role here.
In >March 2011 a field campaign on BIC found only superimposed ice accumulation at the >summit of the ice sheet (Dupont et al, 2012). I understand the data from this study does >not answer the question, it just must be mentioned.

We account for uncertainties in the bulk density of ice and uncertainties due to changes in mean firn density. As you have pointed out, the Barnes Ice Cap has very little firn so we only include the uncertainty of the bulk density of glacier ice. For glacier areas > 1400 m we apply additional uncertainty for unknown changes in the density structure. To make this clearer we've removed "This approach assumes a constant rate of compaction and internal accumulation over the past 50 years" to "This approach assumes a constant firn density over the past 50 years".

>1584-16: Compare to the results from (Dupont et al, 2012), who note that "for the BIC >elevation change data from the ICESat altimeter confirmed the thinning of the BIC at a >mean rate of -0.75 m/yr for the 2003-2009."

We believe you may have overlooked the fact that Dupont et al. simply took the dh/dt estimates from the published supplementary materials of Gardner et al., 2011. They did not conduct any original analysis of the ICESat data.

>1586-13: Changes in melt season length has been identified as a key as well (Dupont et >al, 2012). "the melt season lengthened by 33% from 65.6 _ 6 days at the beginning of >the period (1979-1987) to 87.1 _ 7.8 days towards the end (2002-2010). The interannual >variations of the number of melt days were in agreement with those derived from active >microwave backscatter data from the QuikSCAT scatterometer for

the overlapping >2000-2009 period.”

Changes in melt season length of the single SMMR and SSM/I pixels analyzed by Dupont et al, 2012 provide some indication of a lengthened melt season for the Barnes Ice Cap but are greatly limited in explaining changes in rates of mass loss as melt duration does not easily translate to melt intensity. Dupont et al, 2012 find no correlation between temperature (Reanalysis, station and MODIS LST) and melt duration. We find a very strong correlation between the rate of mass change and mean summer temperature (reanalysis and station). This suggests that melt duration is a very poor indicator of mass change for this ice cap. We now include the statement:

“...and recent analysis of passive microwave data indicate an increase in the number of melt days over the Barnes Ice Cap between 1979-2010 (Dupont et al., 2012)”

Interactive comment on The Cryosphere Discuss., 6, 1563, 2012.