

General comments

This study contributes a useful longer-term perspective on recently observed rapid thinning and dynamic changes on two major East Greenland glaciers. This new perspective comes from comparing the height of Little Ice Age moraines/trimlines with more recent measurements of surface height, in particular a continuous photogrammetric digital elevation model from a 1981 survey which is used as a reference for preceding and subsequent change. The most notable novel finding from this comparison is that the surface height of Helheim glacier was similar around 1850 and in 1981, while Kangerdlugssuaq's surface was 250 m lower in 1981 than 1850. The authors use this distinction to assert that trough geometry is the dominant control on glacier behaviour on longer time scales which, they claim, contrasts with a belief that climate and ocean forcing not only dominate on short timescales but over the longer term too.

Authors: Thank you very much for reviewing the manuscript. In the light of the insightful responses, we have made changes which have improved the original manuscript. A detailed response to your comments addressing ALL of the identified problem is given below.

Weaknesses in this study are as follows:

- Although this study extends the height record back to the LIA, it also presents height, flow and marginal change since the 1980s, and associated changes in climate and ocean forcing, as if they were new results. Helheim and Kangerdlugssuaq have been extensively studied over this more recent period and several published papers describe dynamic, frontal and mass balance changes and their relationship to external (climate and ocean) forcing – some of these papers were in fact published by authors on this study.

Bevan et al. (2012) published a detailed reconstruction of flow rates and frontal positions, and their link to ocean and atmospheric forcing, for the same glaciers since the 1980s. Bjørk et al (2012) published frontal changes with their response to ocean and atmospheric forcing back to 1910/12 for the same glaciers, and have a temperature reconstruction back to 1840. They also seem to have created the 1981 DEM used in this study as a reference height. Andresen et al. (2011) published a calving history, linked to ocean and atmospheric forcing, back to 1890.

This raises the question of what is really new in this study?

- The main novel finding is the measure of height change from the LIA trimlines/moraines, but this finding could do with more description and analysis.

Authors: We agree with the reviewer that flow rates and frontal changes have been published by Bevan et al. (2012) and Bjørk et al (2012), respectively. The latter study did construct a DEM but it was used to create ortho-photos used for geo-referencing historical imagery. So, the 1981 DEMs presented here is new and give an important picture of elevation changes from 1981-2012. Thickening on HG during 1981-1998 as reported here is new, also the fact that dynamically induced thinning started in 1998, and not ~2003 is a new and important finding.

o The authors assert an LIA date for these features based on citation of a short paper by Lowell (2000) but this paper appears not to describe the LIA glacier extent in East Greenland. Can we be sure that these extents date from the LIA? Could they even come from more recent glacial advances?

Authors: We fully agree with the reviewer. The question was also raised by the other reviewer.

Most literature states the retreat began around 1850, but we cannot say for sure as there are no historical records that place the frontal position exactly at the LIA max position available for these glaciers. We are however so lucky to have photographic evidence from the early 1930s, and we can in these pictures clearly see that retreat from LIA max has started. For Kangerdlugssuaq the ice was close to its LIA position in 1932 but had retreated substantially by 1933. By 1932 Helheim had retreated from its LIA position indicating change prior to 1930s.

To accommodate this we have changed the wording, and now state that the exposed trimlines and moraines terminated prior to early 1930s and in the discussion section indicated the above mentioned periods more explicitly. Whether they are from 1850 or perhaps earlier is less important for this study. The purpose of using trimlines is to compare short-term ice surface elevation change with long-term elevation change. By short we mean last ~2 decades, by long term fluctuations we mean more than 80 years.

o The LIA end-date of 1850 is given by Lowell apparently as the approximate end of this period that is broadly applicable over a large area. Could it not be substantially different in this region? Can it be sharply defined? What other evidence is there?

Authors: We fully agree with the reviewer. See above response. We have changed the wording.

o How did the authors decide which single point along the trimlines/moraines to use as a height reference? Why not multiple points? How precise is the definition of the edge of the trimline/moraine?

Authors: We agree with the reviewer. Elevation change for a single point is actually very accurate ($\sigma=1\text{m}$). However, we have decided to list elevation change range in the MS text, rather than just the elevation change closest to the "sample point". Thus, for KG we state that the elevation change between LIA and 1981 is 230-265 m (see also fig 1).

o When comparing the LIA height measured at the glacier lateral margin to modern height measurements in the glacier centre, the authors make the assumption that the shape of the glacier surface cross-profile is the same on the early and later dates. Has this assumption been tested? Couldn't the cross-profile shape vary in relation to the distance of the profile from the glacier margin? This could introduce substantial bias to the height change measurements.

Authors: We fully agree with the reviewer that this may cause a bias, thus an uncertainty of 1 m could be underestimated. Consequently, we believe it is safer to list a range of 230-265 for KG, rather than the number for a single point. Additionally, Howat et al (2007, science) figures 1a and 2a suggest the shape does not change much.

o When looking at the recent (post 1980s) height changes, why did the authors not use the ASTER DEMs for the same glaciers and within the same time period that I think the lead author has previously published?

Authors: The ASTER DEMs were provided by Gordon Hamilton and Leigh Stearns and not processed by the lead author. However, we do not think including ASTER DEMs (in this case) will make any significant difference to the overall scope of the MS.

o I think that the error estimate for the height change ($\sigma(\text{LIA})$) is wrongly defined. The quadrature approach would be suited to two DEMs independent in error. In this case, the $\sigma(\text{DEM1981})$ term presumably is an absolute uncertainty in height, containing systematic and random error. What is needed in this case seems to be the relative error in the DEM1981, which would not include the systematic errors

present in sigma(DEM1981). There should also be some consideration of the uncertainty in defining the height of the trimline/moraine, and how representative that point-measurement is.

Authors: We have updated this section and now provide more details, by including a calculation of how the relative uncertainty for the dh(LIA-1981) data points is calculated. Furthermore, as listed above, we provide thinning range (230-265 for KG) for the frontal portion of KG rather than a single number.

- In the SMB reconstruction based on Box (2013), how well resolved are the glacier tongues? The model gridding is 5 km which is similar to the glacier width, and the glacier tongues are deeply incised into the higher ground all around. Can the model represent well enough the SMB at these low altitudes? If it is applicable to these sites, is the 0.45 m uncertainty for the SMB also applicable in these areas of high snowfall, high melt and strong gradients, or is it intended to represent the broader ice sheet?

Authors: In response to this comment, we have inserted the following new text:

"Compared to typical regional climate model output (see e.g., Vernon et al. 2012), the 5 km resolution facilitates resolving sharp spatial gradients in the ablation area and where terrain induces complex spatial structure in accumulation rates. Along the ice margin, the uncertainty will be larger than 0.45 m because: 1.) the melt (and often accumulation) mass fluxes are largest; 2.) a 5 km grid does not well resolve glacier tongues; and 3.) due to the fact that grid cells are some mixture of land, ice, and sea.

- How did the authors identify the glacier frontal positions at the LIA?

Authors: We have used trimlines to identify the LIA maximum marginal position of the two glaciers. The trimlines are formed during advance and the transition from vegetated to non-vegetated surface mark the maximum position of the glacier front at a certain time – here LIA. The approach used to identifying the LIA maximum position is different for the two glaciers.

By investigating the first historical aerial photographs from the glacier, it is clear that HG has experienced an advance post LIA-max which had overridden the LIA-max-position. Using the historical imagery it is however possible to map the LIA maximum position in those images. The trimline is very clear, and using the mapping methods described in Bjørk et al 2012, it is possible to map the end of the trim-line with very high accuracy (<100m). These images are the only evidence available that place the LIA maximum position in front of the 1932 position. There has been no evidence on the entire coast of Greenland that there was an advance between the timing of the LIA-maximum and the early 1930s.

The story is different with KG, in the early 1930s the marginal position of KG was very close to the LIA maximum position, and during the course of a single year from 1932-1933 the advanced floating tongue collapsed and the front retreated more than 7 kilometers (Wager et al 1937, Spender 1933), marking the onset of the KG's 20th Century thinning. This maximum position very close to the outer trimlines was mapped by Spender and the resulting collapse photographed from land by Wager et al. From historical aerial photographs recorded by the British Arctic Air Route Expedition, it is confirmed that KG indeed was at the advanced position in 1931.

Additional references:

Wager, L., Deer, W., Wager, H & Manley, G. (1937): The Geographical Journal. Vol 90, no 5

Spender, M. (1933): Mapmaking during the Expedition. Meddelelser om Grønland. Vol 104, no 2

The analysis and conclusions make some assertions that require more evidence. Principal in this is that, while on the short-term the glacier behaviour is well coupled to ocean/atmos forcing, over the longer term the differing behaviour of these two glaciers (250 m net thinning of Kangerdlugssuaq, no net change of Helheim up to 1981) implies a decoupling from forcing and a dominant role for other (mostly trough geometry) factors.

The assumption here is that the forcing on the two glaciers was the same over the LIA-1981 period. This is not demonstrated. Given the long-term records of forcing that are available to the authors (and have been published by some of them before), why did they not attempt to explain the contrast in long-term (century-scale) glacier behaviour in terms of the forcing, rather than stopping at 1978?

Authors: we agree with the reviewer and now show all available ocean and air temperatures (see fig 11). We show sub-surface water temperature during 1950-2012 (no data is available prior to 1950). We show Tasiilaq air temperature during 1895-2012 and Aputiteeq temperature during 1958-2012.

Furthermore, no analysis (or even a detailed description) of the influence of other factors is made and yet it is claimed that they dominate the mass change. The complex behaviour of fjord glaciers has long been known. It is well established that such glaciers do not respond simply to climate/ocean forcing because of the flotation feedbacks that depend strongly on fjord depth and shape. The more sophisticated glacier models include or attempt to include these factors. Consequently, given the results presented, I don't think that this study should claim to demonstrate that glacier response is more complex "than hypothesized", or that these results as they stand undermine the use of detailed decade-scale glacier observations to tune predictive models of glacier behaviour.

Authors: We agree that this paper does not give a detailed description of the influence of other factors. Instead we briefly mention potential factors that could be dominating the long-term mass change. The factors are described in details by e.g. Jamieson et al., 2012 and Enderlin et al., 2013. We have changed the wording and state that other factor "may" be more domination, or are "likely" dominating. However, lead author is working on a study that will go into more details and describe the observed differences between KG and HG using numerical ice flow model.

This study does make a novel and valuable contribution in quantifying glacier change over more than a century, using some very good data from the photogrammetric DEM and orthophotos. Analysis of this is (inevitably) limited by the fact that the LIA-1981 change is a single change measurement through time - no time series is available that would allow a more comprehensive study of the link to forcing.

I recommend a tighter focus in this manuscript on this novel long-term result, with a more thorough analysis of the LIA surface (and any other intermediate relict surface that may be detectable), a consideration of the longer term climate/ocean reconstructions and more detail on the non-climate/ocean factors that could explain the observed discrepancy in height change.

Authors: As requested by the editor, we have included all available long-term climate data. Furthermore, the main focus of this study is elevation change during LIA and 1981 AND 1981 to late 90s. Especially, 1981-1998 elevation changes have not yet been published, and are very interesting as they reveal dynamically induced thickening, which in principle should be captured by numerical ice flow models.

Thanks a lot for reviewing the manuscript.

*Best regards
S A Khan*