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Interactive comment on "Glacier dynamics at Helheim and Kangerdlugssuaq glaciers, southeast Greenland, since the Little Ice Age" *by* S. A. Khan et al.

Anonymous Referee #2

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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.

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.

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?

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?

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?

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.

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?

o I think that the error estimate for the height change (sigma(LIA)) is wrongly defined. The quadrature approach would be suited to two DEMs independent in error. In this case, the sigma(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.

- 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?

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

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?

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.

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.

Interactive comment on The Cryosphere Discuss., 8, 1257, 2014.