Summary & Major Comments:

The authors use remotely-sensed observations of glacier surface elevation and terminus position to construct a time series of glacier change for two of Greenland's largest and best studied outlet glaciers. The presented record builds on previous publications by placing the recent rapid changes in a historical context: digital elevation models (DEMs) constructed from stereo photographs collected in 1981 are used to assess elevation changes that occurred at the end of the last century and moraines and trim lines are used to infer changes in glacier size between the 1800s and the end of the last century. The onset of similar recent changes in dynamic behavior at the two glaciers is related to regional increases in offshore subsurface water temperatures and surface air temperatures. Differences in long-term response to environmental forcing are attributed to geometric differences between glacier basins. While placing recent dynamic changes in a longer-term context is incredibly important for improving the current understanding of tidewater glacier sensitivity to climate forcing, I few a few major concerns that must be addressed prior to publication.

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.

1) First and foremost, I do not think the authors provide enough justification for using exposed trimlines and moraines to estimate past glacier thickness and extent. The reference that the authors cite in support of their assumption that the exposed trimlines and moraines are from the LIA, terminating in ~1850, does not provide any information on the age of these features. I am particularly skeptical of the use of these features to reconstruct glacier LIA metrics because the authors also site sources stating that the Kangerdlugssuaq marginal position was advanced relative to present in the early 1930s. Without direct observational evidence, which does not exist, or radiocarbon dates for these features, I do not think that the authors can confidently state that these features are from the LIA. Prior to publication, other evidence must be provided in support of the stated age for these features or the text must be revised to reflect that these features were likely formed some time before the 1930s but the exact date cannot be determined. Also, these features should be pointed-out in one of the figures so that the reader can easily assess the historical glacier extent.

Authors:
First and foremost, we do not feel that we have to justify using moraines and trimlines to map past glacial extent and elevation. This is an approach which has been widely used for decades and serves as the basis of much of the palaeo- glaciological knowledge. For reference, see the works of Gifford Miller (Canadian Arctic), Anker Weidick (Greenland), Johannes Oerlemans (the Alps), Olav Nesje (Scandinavia), Niel Glasser (Patagonia) and many others.

What is up for debate is the precise timing of the little ice age in Greenland. And here the reviewer raises an important question, as the precise timing of the retreat of the neo-glacial maximum position is unknown. 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 LIAmax has started. These trimlines must be LIA trimlines as no advances have ever been observed between LIAmax and 1930.

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 mention 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.*

2) Similarly, the uncertainty in the glacier surface elevation that is reconstructed from the lateral remains is likely much larger than estimated in the manuscript. The current uncertainty estimate only accounts for DEM uncertainty but several other factors must be considered that could cause the moraine elevation to change between the time of deposition and the present measurements. The two major factors to consider are moraine slumps/slides and isostatic adjustment. At the time of deposition, lateral moraines are often underlain by glacier ice. This ice often persists after the margin retreats and later melts, causing the moraine position to subside relative to its depositional position. The isostatic adjustment that occurred since moraine deposition may also substantially influence the measured moraine elevation. Uncertainty due to both the listed sources should be estimated from other peer-reviewed studies and incorporated into the elevation uncertainty estimate presented here. Also, it is worth noting that most of the other references to elevation change between the 1981 DEMs and the lidar observations occur near the central glacier flowline but the geomorphic features can only be used to infer elevation change along the margins. Although your illustration (Fig. 6) provides an idealized example of uniform cross-flow thickness change, it is not necessarily true that the marginal change in thickness is the same as the change near the centerline. This should be noted in the text.

Authors: We are fully aware of above mentioned factors.

The reviewer mentions that lateral moraines are often ice cored, and post depositional melting might influence our results. During the mapping process we have taken this into account and have mapped the highest elevations where ice has scoured the bedrock or deposited lateral moraine. Any post depositional ice core melt will not affect these elevations. This has now been made clearer in the text. The elevation change between the maximum extent during the LIA and 1981 is obtained using stereo photogrammetric imagery from 1981. This means that 3D mapping of geomorphic features related to the LIA and the 1981 surface elevation is mapped simultaneously. When considering elevation changes over 100 years (or since LIA) using lateral moraines and trimlines, then there is no need to take PGR or elastic uplift into account. Both the ice surface and bed experience the same uplift. In other words, since we measure ice surface relative to bed, the effect of isostatic adjustment is eliminated.

Furthermore, in relation to PGR please consider the following peer-reviewed studies (Khan et al., 2007, GRL; Wahr et al., 2013, JGR; Bevis et al., 2012, PNAS; Nielsen et al 2013, JGR). They all suggest (using either modelled or observed data) uplift of ~1 cm/yr. Over the last two decades uplift in southeast is 10-15 cm. This is a very small signal compared to the observed thinning of more than 100 meter. Note, in the manuscript we do not provide millimetre, centimetre or decimetre accuracy.

Anyway, we can easily model elastic and viscoelastic uplift, but again the signal is very small. As we see it, it is absolutely not a major issue as indicated by the reviewer.

3) Please provide more support for the use of the 315 m-depth subsurface ocean shelf temperatures in your analysis. Also, you state that the glaciers themselves are situated along the continental shelf, which is not accurate. You should revise this section so that is it clear to the reader that these glaciers are located within long, narrow fjords (provide details) and that hydrographic observations of water masses within these fjords indicate that the same (but slightly modified) water masses present on the shelf are also found within the fjord. The Straneo et al. 2012 paper, whose full citation is included at the end of this review, describes the hydrographic surveys that have been conducted on the shelf and in the fjords.

Authors: We agree that the fjords where HG and KG are situated (e.g. the Sermilik fjord) are deeper than 300 m, likely 800-1200 m (see fig 1 of Andresen et al, 2011, DOI: 10.1038/NGEO1349). Here we use a global subsurface temperature grid with resolution of 1x1 degree. The data does not provide a temperature estimate right at the glacier front. The nearest grid point is located right outside the Sermilik fjord where water depth is limited to few hundred meters.
Water depth of 315 m-depth is selected in order to have temperature close to the ocean bottom. However, whether we use 250 m or 350 m does not significantly change figure 7.

4) In your discussion of how you separate the sources of thinning (dynamic versus SMB change), please provide a more specific description of how you remove the SMB component. Do you take your thickness change at each point and subtract SMB from the nearest grid cell? Do you use SMB anomalies in this calculation or raw SMB data? If you use anomalies, over what time period is the reference SMB calculated? For your plots in Fig. 7, from what location on the glacier were the speed and elevation time series extracted? The flight lines shown in Figs. 4 & 5 do not perfectly overlap so I am assuming you have to perform some sort of interpolation in order to construct the Fig. 7 time series. It looks like you have a 'Sample point' for each glacier noted in Figs 8 & 9 but you do not mention them in the text. Do you simply use the observations closest to these points and assume thickness change is uniform across-flow? Please clarify in the text.

Authors: We now provide more details on how SMB is removed. The SMB reference period is 1961-1990. We use bilinear interpolation to estimate SMB at each observed point. Note the SMB model is delivered on a 5 km grid and the signal is small (5-10 meter) compared to the dynamic signal of more than 100 m. Furthermore, SMB does not change much over small distances (of few km).

Additionally, we have decided to add an extra paragraph to the SMB section. In the new paragraph we assess the BOX SMB model. We now also predict elevation changes due to SMB fluctuations using the RACMO2 model. The two models seem to agree very well (see new fig 10).

The flight lines in Figs. 4 and 5 do not perfectly overlap. We take topography into account using the 25x25 m grid DEM from 1981. The max distance between the "sample point" and the observed points is 200 m. This is now stated in the text. Finally, the extraction points for Fig. 7 are identified in the caption as being marked on Fig. 4a.

Minor Line-by-line Comments:
p. 1259, line 25 through p. 1260, line 3: Mass loss from these glaciers was investigated in detail in Howat et al. (GRL, 2011) and a broader assessment of the discharge contributions from these glaciers can be found in Enderlin et al. (GRL, 2014) (reference information at the end of the review).
Authors: Thanks for the references.

p. 1260, line 9: The numerical modeling study by Enderlin et al. (2013) did not model the GrIS's 21st century contributions to sea level rise. Please remove.
Authors: done.

p. 1261, line 18: Is the speed uncertainty estimate derived in your analysis or is this an estimate quoted from other sources? Please provide an explanation for how the uncertainty was estimated if it was calculated as part of this study. Otherwise, please include appropriate references.
Authors: Speed uncertainty estimate is from Bevan et al. (2012). This is now stated.

p. 1263, lines 12-14: The Helheim terminus position re-advanced after 2005, as shown in Fig. 8, so it is unlikely that thinning rates decreased after 2006 because the terminus retreated into a deeper portion of the bed. A more likely explanation was proposed in Howat et al. (Science, 2007): the terminus regrounded on a shoal in 2006, increasing resistive stress and reducing thinning rates.
Authors: changed accordingly.

p. 1264, lines 12-20: Presumably you are proposing that warmer ocean and air temperatures have an additive effect on dynamic change because warmer air temperatures lead to more melt and high subglacial discharge. The increase in subglacial discharge and the warmer ocean temperatures cause submarine melt rates increase markedly, leading to dynamic instability. Although this is a possible explanation, and timing of atmospheric and oceanic warming correlate with the onset of dynamic acceleration and thinning, the observed correlation cannot be used to infer causation. Please revise the wording accordingly. Additionally, please elaborate on the explanation and try to improve its integration within the text. As is, you suddenly jump from talking about the sensitivity of Helheim glacier to external forcing to submarine melting, with no transition explaining that you are now talking about a potential triggering mechanism for dynamic change.
Authors: we have changed the wording. We agree that the observed correlation cannot be used to infer causation. We have made it clear that our explanation is a "potential explanation".

p. 1265, line 11: I suggest also referencing the companion paper to the Enderlin et al., 2013 citation. The companion paper, which was also published in The Cryosphere (see reference information below), also found that small differences in basal sliding or the effective viscosity of the glacier can lead to large differences in glacier behavior. Like the cited paper, this companion study explores a range of parameters for idealized glacier geometries but the results also suggest differences in basal sliding and viscosity between glaciers influence dynamic behavior.
Authors: Thanks for the reference.

Technical Corrections:
p. 1261, line 24: Change "HG and KG is" to "HG and KG are".
Authors: done

p. 1262, line 9: Remove the sentence "Data is available from DMI (Boas and Wang, 2011)." because this information is already provided in the first sentence of the paragraph.
Authors: changed accordingly

Suggested References:

Enderlin, E.M., Howat, I.M., and Vieli, A. (2013) The sensitivity of flowline models of tidewater glaciers to parameter uncertainty. Cryo., 7, 1579-1590, doi: 10.5194/tc-7-1579-2013.

Enderlin, E.M., Howat, I.M., Jeong, S., Noh, M.-J., van Angelen, J. H., and van den Broeke, M.R. (2014) An improved mass budget for the Greenland ice sheet. Geophys. Res. Lett., 866-872, doi: 10.1002/2013GL059010.

Howat, I.M., Joughin, I., and Scambos, T. (2007) Rapid changes in ice discharge from Greenland outlet glaciers. Science, 315m 1559-1561, doi:10.1126/science.1138478.

Howat, I.M., Ahn, Y., Joughin, I., van den Broeke, M.R., Lenaerts, J.T.M., and Smith, B. (2011) Mass balance of Greenland's three largest outlet glaciers, 2000-2010. Geophys. Res. Lett., 38, L12501, doi:10.1029/2011GL047565.

Straneo, F., Sutherland, D.A., Holland, D., Gladish, C., Hamilton, G.S., Johnson, H.J., Rignot, E., Xu, Y., and Koppes, M. (2012) Characteristics of ocean waters reaching Greenland's glaciers. A. Glaciol., 53, 60, doi:10.3189/2012AoG60A059

Thanks a lot for reviewing the manuscript.

Best regards
S A Khan