Response to the Interactive comment on

"Recent dynamic changes on Fleming Glacier after the disintegration of Wordie Ice Shelf, Antarctic Peninsula"

by Peter Friedl et al.

Anonymous Referee #3
Received and published: 16 November 2017

We would like to thank the reviewer his helpful comments on our manuscript. All comments have been considered and a list of responses and changes in the manuscript is given below. Responses are written in bold face type and changes in the manuscript are written in *blue*.

The topic of this paper, Fleming Glacier, is extremely interesting given the significant changes reported by previous publications, so I was very keen to read these new results. The authors present new ice velocity, elevation change and grounding line position data acquired from a range of airborne and satellite based instruments. The ice velocity results are nice, and I believe constitute the most complete time series of velocity measurements over Fleming glacier which provides new insight into the timing of ice speedup on this sector.

However, after reading the manuscript there are a number of major flaws with the methods employed to derive surface elevation change and to measure the grounding line retreat. As it stands, the problem with the techniques make it highly likely that both the magnitude of the elevation change signal, and the grounding line retreat, may not be correctly reported in this paper. For example, cross calibrating the DEM elevations over sea ice, which varies annually and can range in thickness from 0-5m, is extremely unsatisfactory. Moreover, even if this correction is accepted, the authors estimate that known X-band penetration bias can account for ~50% of the dh/dt signal across the basin. Even if the estimate of elevation change is accepted, the associated error measurements do not reflect spatial variability in the data quality, and are unrealistically small given the spread of the raw data. For example, it is stated that the CAMS-ATM dh/dt data has an error of 0.2m/yr in regions where the point measurements at the same location range from -0.5 to -7 m/yr. These errors must therefore be revised. Regarding the estimate of grounding line retreat, the technique is unproven and is un-validated in this paper. Even if the authors demonstrate that the technique can be trusted, as it stands the results presented in this paper are contradictory because regions measured to be grounded are located within the new floating ice shelf area.

On top of these technical issues, I have a few more minor concerns about the use of scientific terminology throughout the paper, and the overly simplistic nature of the analysis and discussion sections. The specifics of these and other concerns are documented in detail below. My criticisms of the methods and results presented in the paper are major, and will be time consuming and require a significant effort to properly address. The implications of these concerns is that I believe the magnitude and spatial pattern of the elevation change and grounding line retreat data may be incorrectly reported in this paper. This is significant as I do not have confidence in two of the three core datasets presented, therefore, it is my recommendation that this paper is not suitable for publication in its current form.

Specific Edits
P1 L21 – Edit 'far upstream of the glacier', using this as a location is ambiguous, better use a fixed reference location, like the calving front in 'x' year.
The sentence has been deleted during rewriting of the abstract.

P1 L26 – Edit paper to quantify 'much larger ice masses'.
It is difficult to exactly predict how much ice will get lost in the future due to the response of the glacier to further grounding line retreat from observational data. However, if the grounding line retreats up to the deep retrograde trough ~3-4 km upstream of its current position, it is likely that the glacier gets into an unstable configuration where rapid grounding line retreat and more mass loss than today can be expected (see answer P11 L11). The quantification of the expected mass loss, however, needs the involvement of ice sheet modelling, which is out of the scope of this paper. Nevertheless, we have changed the sentence into:
Hence, this endangers upstream ice masses, which can significantly increase the contribution of Fleming Glacier to sea level rise in the future.

P2 L4 – Edit missing to 'be'.
We have changed this accordingly.

P2 L7 – Edit ice 'shelf' tributaries.
We have changed this accordingly.

P2 L30 – Edit 'explain' to 'investigate'. I'd argue at this point the authors haven't demonstrated they can explain the observed signal.
We have changed this accordingly.

P3 L23 – Poor wording, edit out 'got' both times.
We have changed the sentence into:
During the following years (2010–2015) the fronts of the glaciers in Wordie Bay remained quite stable, except at the Prospect system where the once interconnected floating ice tongues of the three glaciers disconnected and some floating ice was lost.

P4 L2 – Edit 'dynamic' and check use throughout paper. Previous publications (e.g. Rignot et al 2005) have demonstrated that Fleming exhibited dynamic imbalance during their study period, but as the authors state this paper spans a longer time period, and at this point it hasn't been proven that Fleming is dynamically imbalanced for the full duration of their study period.
We agree with the reviewer, that at this point it is not clear, whether the dynamic changes we observe are consequences of the disintegration of the ice shelf or a result of other processes. However, at least we investigate the dynamic changes of the glacier in the time period after the disintegration of the ice shelf. We hence have changed the sentence into:
We used a broad remote sensing data set in order to investigate the changes in ice dynamics at Fleming Glacier between 1994 and 2016 after the disintegration of Wordie Ice Shelf.
We have also checked the use of the word "dynamic" throughout the paper and have changed it where necessary.

P5 L6 – More informative to state what % of each velocity image is removed during the filtering process as this should be an indicator of the quality of the tracking output. The filtering should remove 100% of the unreasonable results, otherwise its not a very good filter!
We apologize for this inaccuracy. The filter removes more than 99 % of erroneous vectors and removes very few false negatives (Burgess et al., 2012). We have changed this in the text accordingly. The filter has been successfully applied in several other peer reviewed studies (e.g. Rankl et al., 2014; Rankl et al., 2016; Seehaus et al., 2015; Seehaus et al., 2016). However, the proportion of velocity vectors removed from each velocity image by the filter is not only depended on the quality of the tracking but also on the coverage of the scene. For example, more erroneous tracking results are likely to be found in areas where no marked features can be used for tracking (e.g. in the interior of the peninsula). If a scene covers more of such areas, the amount of removed velocity vectors will be higher than in other scenes which only cover the glacier tongue. Hence we have calculated the proportion of removed velocity vectors for an area over Fleming Glacier, which is covered by all scenes and which is relevant for our velocity measurements. The area for which the calculations have been done has been added to Fig. S1.

Additionally Tab. S3 has been updated with a column containing the proportion of removed measurements.

P5 L16 – 0.2 m is the accuracy of the original point measurements; the authors are using the dataset after re-gridding it so state the accuracy of this dataset instead or as well as the accuracy of the raw data. The accuracy is also different for different sensors, so the authors should provide statistics for each dataset.

We thank the reviewer for this important comment. We have now cited additional references to state the accuracies of the different sensors used in this study:

We derived ice thinning rates on Fleming Glacier for 2004–2008 and 2011–2014 by comparing ellipsoid heights of the PIB (ATM, 2004), the Centro de Estudios Científicos Airborne Mapping System (CAMS, 2008) and the OIB (ATM, 2011, 2014) airborne LiDAR datasets. The vertical accuracy of the ATM elevation data is estimated to be better than 0.1 m (Krabill et al., 2002; Martin et al., 2012). For the CAMS data, vertical accuracy is 0.2 m (Wendt et al. 2010).

P6 L2/3 – This is correction extremely unsatisfactory. Sea ice is a complex parameter, and is certainly not a stable/constant reference surface for precise cross calibration of elevation measurements. In the Antarctic, sea ice can range from 0 to 5 m thickness with very large spatial and temporal variability, snow depth on sea ice is not routinely measured but can account for half the thickness retrieval, and ocean height varies with tides, atmospheric pressure etc. When deriving the correction, the authors have not attempted to account for interannual variability in sea ice thickness so this must be addressed before any confidence can be had in the elevation change measurements. This is critical because the range of thickness variability is the same order of magnitude as the dh/dt signal calculated from the DEM differencing. The authors must revise the manuscript to characterise the temporal variability of the sea ice over which they are cross calibrating the DEM's, and to rule out any influence from this factor on the end elevation change. If this effect can be proven to be negligible, the authors should also state the size of the correction, and which DEM was adjusted, in the manuscript. Having said all this, I suggest the authors dont cross calibrate the DEM's over sea ice at all as it hugely reduces the confidence that I believe we can have in these results.

We thank the reviewer for raising this important issue. According to the previous suggestions of reviewer #1 we have changed the vertical registration procedure in such a way that we now use a subset of the TanDEM-X global DEM at 12 m resolution as an absolute height reference. Before differencing, both TSX/TDX digital elevation models were vertically adjusted to the TanDEM-X global DEM according to their median offsets measured over stable ground. This improved the vertical registration and resulted in a more realistic penetration depth bias.

The maximum difference between ATM and uncorrected TDX/TSX elevation change rates was 1.25 m/a, corresponding to an absolute 3.75 m difference for the 3 year time span. However, differences in elevation change were only measured in the lower areas of the glacier tongue, where surface melt can occur. In the upper areas (above 600 m altitude) the difference between ATM and TSX/TDX elevation change rates was close to 0 m. Here the medium was likely completely frozen on both dates of TDX acquisition, so that the penetration bias cancelled out. A comparison of backscatter values showed that in areas below 600 m altitude the backscatter of the 2014 acquisition was lower than the backscatter of the 2011 acquisition. In the upper areas above 600 m, however, the backscatter values were similar.

We have updated the TSX/TDX elevation change rates in the text and all figures that contain TSX/TDX elevation change data (i.e. Fig. 4, Fig. 5 b, Fig. S4 a-c and Fig. S6.1-4).

We have also added a Figure S4 d) showing the differences in backscatter between both acquisition dates.

Furthermore we have updated Fig. S1 with the new areas over stable ground used for vertical adjustment and error estimation.

Finally we have largely changed the corresponding section in the text:

Before differencing, the TSX/TDX-DEMs must be vertically referenced. For this purpose the median vertical offset between the DEMs and the TanDEM-X global DEM was measured over stable areas (i.e. tops of nunataks and rock outcrops, which were not affected by image distortions) at altitudes between 150 m and 1000 m (Fig. S1), before both DEMs were adjusted accordingly.

After subtracting the vertically registered DEMs, the elevation differences were converted into yearly elevation change rates. We assessed the accuracy of the vertical registration over another set of stable areas at altitudes between 150 m and 1300 m (Fig. S1). The absolute median value of the extracted change rates was 0.37 m a^{-1} which primarily accounts for errors related to the vertical registration.

…

The comparison between elevation change rates obtained from the 2011–2014 OIB ATM flights and the 2011–2014 TSX/TDX data after the vertical registration of the DEMs showed a maximum overestimation of ice thinning of 1.25 m a^{-1} for the TSX/TDX measurements (Fig. S4 a, b). However, the general trend of the elevation change rates fits well to those calculated from the LiDAR data and significant differences in elevation change were only measured in the lower areas of the glacier tongue. In the upper areas (above 600 m altitude) the difference between ATM and TSX/TDX elevation change rates was close to 0 m. Here the snow volume was likely completely frozen on both dates of acquisition, so that the penetration bias cancelled out. A backscatter comparison showed lower values in 2014 than in 2011 in areas below 600 m altitude, whereas the backscatter in the upper areas above 600 m altitude was similar for both dates (Fig. S4 d).

P6 L20 – The authors calculation that up to 2 m/a of the thinning rate can be attributed to TSX DEM penetration bias. This is a huge error which accounts for ~50% of the dhdt signal present across the majority of the basin. The ICESat and ATM tracks that this error was calculated from have extremely limited coverage, and don't pass through the region with the highest thinning rates, therefore its possible that this number might even be an underestimate. For example, other studies have shown that the penetration bias in DEM's derived from TSX/TDM data over snow covered terrain can be as large as 4m (e.g. Dehecq et al 2016), which is the same magnitude as the dh/dt signal presented in figure 4. The large size of the known errors relative to the size of the signal, combined with the limited data that has been used to characterise the error makes it very difficult to have confidence in the thinning rates presented here. Other auxiliary datasets such as atmospheric temperature data, or SAR backscatter images might also be used to characterise the onset and spatial pattern of melt in the study area. The authors description of how they have accounted for this source of error is cursory given its size relative to the dh/dt signal in the study area. As surface melt and therefore penetration is known to have large spatial variability, I recommend that the authors revise their approach to account for this spatial variation across the basin, as a polynomial fit derived from a single track of airborne data will definitely not capture the magnitude or pattern of this effect across the study area.
Same answer as for P6 L2/3. Furthermore we understand the reviewer`s concers on our approach of applying a penetration depth correction derived from a single track of altimeter data to the entire study area. However, we have to deal with the data we have in hand. The tongue of the Airy-Rotz-Seller-Fleming system (which is our main study area) has only a width of ~20 km. We therefore think that it is acceptable to assume that melt conditions and the penetration depth bias is similar for this area and just altitude depending.

P6 L27 – Assuming an error of 0.2m/a just because its one order of magnitude higher than the direct inter-comparison with the ATM data the dh/dt was calibrated against isn't satisfactory. Errors are spatially variable, so the authors should revise their approach.
Due to the relatively small size of the study area the spatial variability of penetration bias in our study area should be little too (see previous answer). Hence, we think that assuming an error of 0.2 m/a due to penetration bias variability is already a conservative estimate.

P6 L33 – Why use a 35 m buffer if the ICESat footprint is known to be 70 m? I recommend the authors use the same footprint size as the aim is to do the most direct comparison possible.
We apologize for the misleading phrasing. We used a buffer with a radius of 35 m, which is equal to the 70 m diameter of the ICESat footprint. We have changed the sentence into:
To take into account the 70 m footprint of the GLAS instrument, we applied a buffer with a radius of 35 m and calculated the median from the extracted values at each point.

P7 L15 – Cite a reference for the source of the firn density correction variable.
We have added a reference to Griggs and Bamber (2011).

P7 L25 – As far as I'm aware, this method of detecting grounding line position has not been proven in peer reviewed literature. Although the logic behind it is reasonable, (i.e. if the ice is in hydrostatic equilibrium it must be floating), factors such as the spatial resolution and error on each input dataset will severely limit the sensitivity of the technique for detecting grounding line position, let alone change in grounding line position. To be convinced that the technique works, I recommend grounding line retreat from this method is evaluated against known retreat rates, in the Amundsen sea for example. If suitable data isn't available to validate this technique in another area, then alternatively a proven technique can be employed to evaluate the hydrostatic technique in this study area. For example, ERS-2 SAR data with a 3-day temporal baseline was acquired in this area in 2011, so if coherent, this should be used to produce a grounding line estimate from the proven quadruple difference interferometry technique (Rignot et al, 1998). At a minimum the authors must state the error on their estimate of grounding line position from hydrostatic height anomaly, and it follows that if the uncertainty on the measurement is greater than the change in position assumed, then the method is not viable.
We understand the reviewer`s concerns. Of course DInSAR would have been the preferred method for grounding line detection. We tried to form interferograms from ERS-1 (1994) and ERS-2 (2011) data with a temporal baseline of 3 days as well as from Seninel-1 data (2016) with a temporal baseline of 6 days. However, due to the high speed of the glacier none of the datasets maintained enough coherence over the relevant areas. Besides strong temporal decorrelation, the ERS-2 data of 2011 suffers from large and unstable Doppler centroid frequencies due to the failure of the gyroscope in 2001, which additionally hampers coherence (Miranda et al., 2003). Hence, we moved to the alternative approach of deriving the recent grounding line positions by calculating hydrostatic equilibrium from ice elevation and ice thickness data. This is a frequently used method in peer reviewed literature (e.g. Enderlin and Howat, 2013; Fricker, 2002; Münchow et al., 2014; Tinto and Bell, 2011). Typically the point of first stable hydrostatic equilibrium is located a few hundred meters seaward of the interferometric grounding line (Rignot et al., 2011). An exception are a few ice streams which have an ice plain (i.e. a region upstream of the grounding line with low surface slopes that is only lightly grounded and where the ice is very close to hydrostatic equilibrium). Here, hydrostatic analysis may place the grounding line some kilometers upstream of the real grounding line (Corr, H. F. J. et al., 2001; Crabtree and Doake, 1982). However, in our datasets we do not see any signs for the presence of an ice plain on Fleming Glacier.
In order to demonstrate the applicability of the hydrostatic height anomaly method, we have conducted the same calculations as on Fleming Glacier for the glaciers in the Cabinet Inlet region (Larsen C Ice Shelf) (Fig. 1, 2). Here coherence is also maintained in Sentinel-1 6-day temporal baseline interferograms and no long term grounding line migration has been observed since the last ERS measurement in 1996. This enabled us to compare our hydrostatic calculations with interferometric grounding lines from different epochs. Fig. 1 shows that the hydrostatic calculations are generally in good agreement with the DInSAR measurements. The differences between the 1996 and the 2016 DinSAR grounding line are mainly due to short term tidal grounding line migration. The mean difference between the first hydrostatic equilibrium and the closest DInSAR-grounding line is ~ 300 m.
We have also added a sentence on the estimated uncertainty of grounding line positions derived from hydrostatic height anomalies to S3:
For all possible sensor-depending errors of H_i=750 m the Monte Carlo simulation yielded a standard deviation of < 12 m for Δe. Thus we assumed the total uncertainty of Δe to be ~12 m. Consequently, we assigned locations on our OIB and PIB profiles to be freely floating ice, if the calculated values of Δe lay within this range. In the vicinity of the grounding zone the TDX global DEM has a minimum gradient of 1°, and therefore we estimate the uncertainty in the horizontal position of the transition from grounded to freely floating ice to be ~700 m.

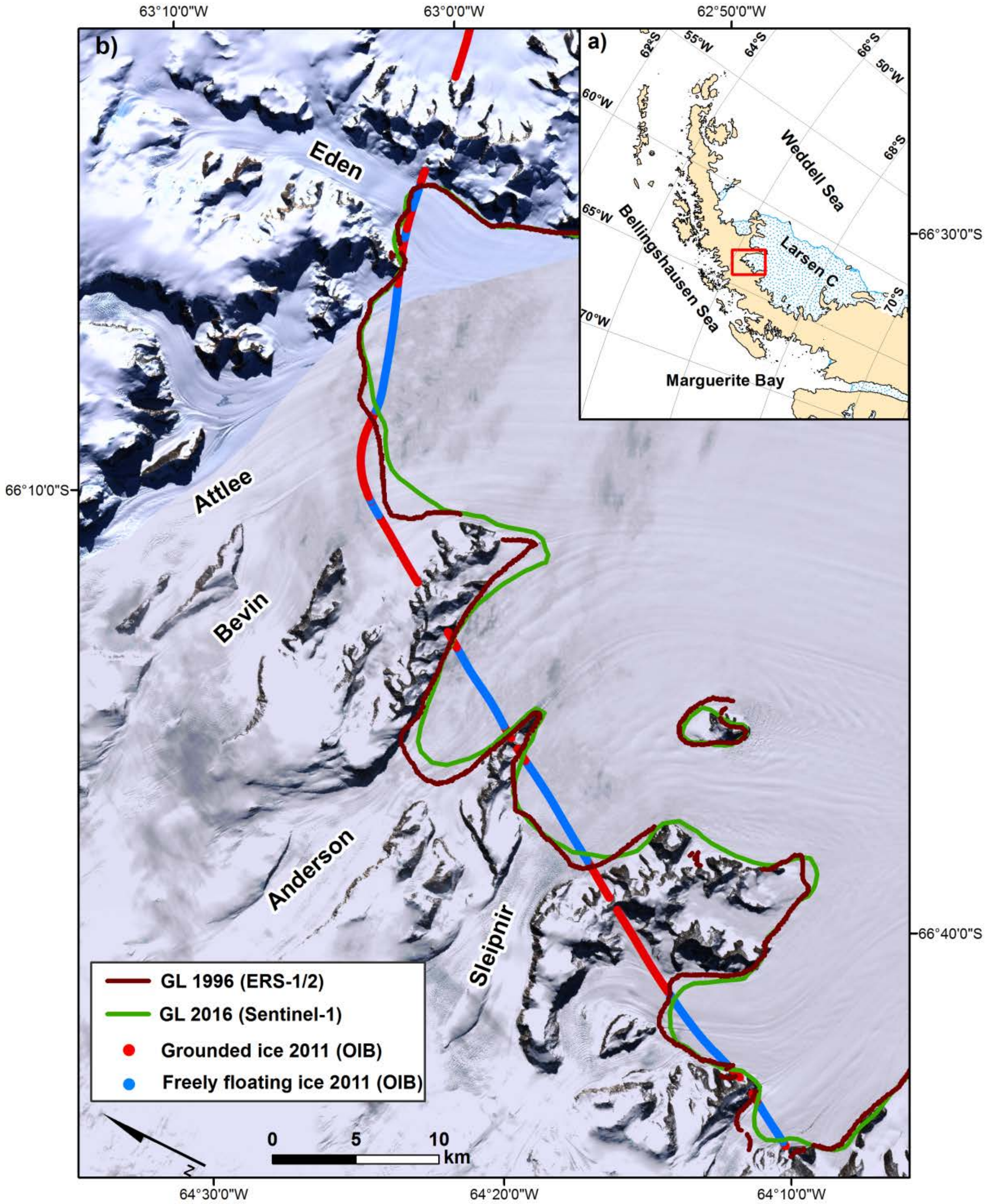

Figure 1: (a) Location of the study area at the Antarctic Peninsula. Map base: SCAR Antarctic Digital Database, version 6.0. (**b)** Comparison of floating and grounded ice from hydrostatic height anomalies with grounding lines derived from DInSAR. Brown line: grounding line in 1996 from ERS-1/2 (Rignot et al., 2011). Green line: grounding line in 2016 from Sentinel-1 a/b acquisitions on 2016-09-27, 2016-10-03 and 2016-10-09. Blue and red dots: Freely floating and grounded ice after acceleration as derived from OIB laser altimeter and ice thickness data on 2011-11-14. Background: USGS Landsat Image Mosaic of Antarctica (LIMA).

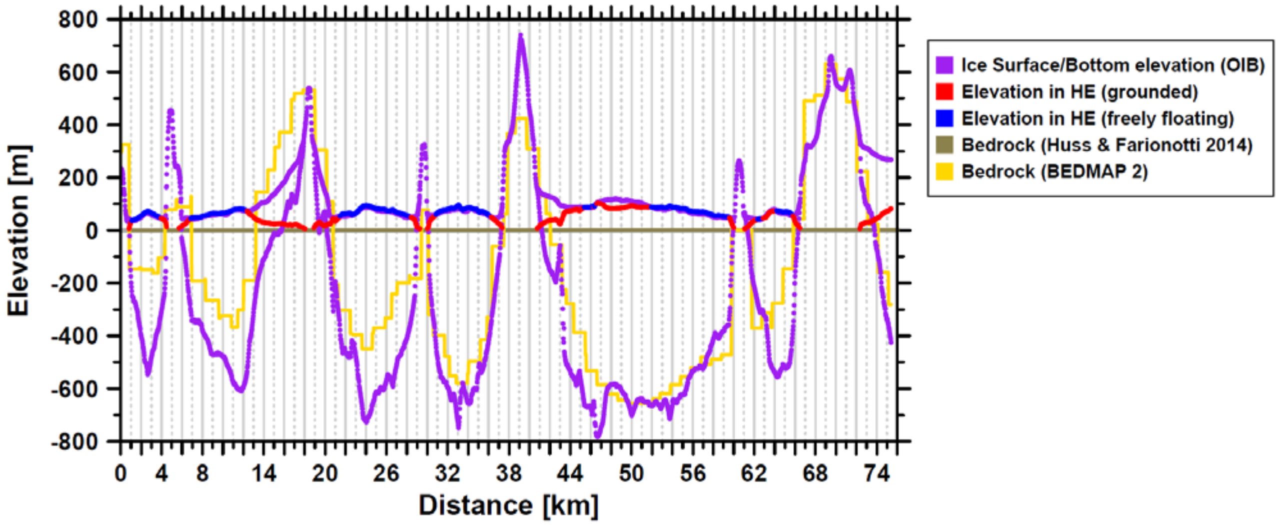

Figure 2: Fulfillment of the hydrostatic equilibrium assumption from hydrostatic height anomaly calculations along the OIB profile from 2011-11-14. The profile extends from southwest to northeast on the map (Fig. 1). Purple dots: PIB/OIB ice surface/bottom elevations. Ice surface elevation is taken from PIB/OIB ATM measurements and ice bottom elevation is calculated by subtracting OIB/PIB ice thickness from ice surface elevation. Yellow line: Bedrock elevation from Bedmap 2 (Fretwell et al., 2013). Red and blue dots: calculated ice surface elevation in hydrostatic equilibrium e_{he} and information on hydrostatic equilibrium (blue: freely floating ice, red: grounded ice)

P8 L3 – The Figure 2 z-scope is not easy to interpret. I suggest the authors re-plot this information as a standard x/y line plot of the time series of flow line ice speeds, with an inset showing change in calving front position.

We understand the reviewer`s concerns. However, in our view a standard x/y line plot which contains all of our 175 measurements would be very confusing. Furthermore the huge amount of lines would make a distinguishable color coding impossible. Thus we think that our plot is a clear way to show a chronologic time series of all our measurements. The plot also reveals the timing and the propagation of pronounced acceleration very well. However, for additional information we have added a graph of smoothed median center line velocities to the plot.

P8 L14 – Poor sentence wording. Edit.

We have changed the sentence into:

Since 2008 the glacier tongue never has exceeded the 1996 grounding line anymore

P8 L28 – 8 to 14 km upstream of which location. Edit sentence to be more precise.

All distances in the text are relative to the 1996 grounding line. We have stated this at the beginning of section 5: "Distances in the subsequent text are given in reference to the grounding line of 1996." We hope that the reviewer is fine if we do not change the text here.

P8 L30 – Edit text to quantify 'lower parts'.

We have changed the section into:

A tendency to lower negative or even positive elevation change rates could be observed on the lower parts of the joint Fleming and Seller glacier tongue between 0 and up to ~9 km upstream. However the pattern was not as clear as on Airy Glacier, where a distinct area of low ice thinning rates could be detected between 0 and ~4 km upstream.

P8 L31 – Edit manuscript to remove all 'could be detected' wording. You are stating what results you have observed, so it 'has been detected', not the less affirmative 'could be'.

We thank the reviewer for pointing this out. We have changed it throughout the text.

P9 L4 – The scatter on figure 5a is very large and must be addressed given that it is significantly greater than the previously stated errors. I recommend that a) distance markers are annotated onto the ATM and ICESat track locations on Figure 4 so we can see how this corresponds to the x axis distance scale in Figure 5. My interpretation of figure 5a is that the elevation change measurements are unusable between 0 and 20 km of the grounding line, which looks like its about up to the 'g' on the Fleming annotation on figure 4. This is the key area of interest, so vastly limits the usefulness of these datasets. B) state the method used to calculate the lines of best fit, e.g. moving average, polynomial fit? How has the clearly erroneous data been removed? C)

The large scatter of the ATM and the CAMS data between 0 and 20 km of the grounding line is due to the undulated and heavily crevassed surface, where a purely horizontal displacement of crevasses can cause apparent positive and negative elevation differences. Many crevasses are up to 50 m wide and 300 m long. Due to the high spatial resolution and precision of the airborne laser altimeter data, these characteristics of the surface are well reflected in the signal. If looking at the ICESat elevation change of 2004–2008 , which by the way shows a very similar trend like the 2004–2008 ATM-CAMS data, indeed the scatter is smaller, but this is just because of the much lower spatial resolution of the data. Hence, more scatter does not necessarily mean that the data is more "erroneous". In general, the ATM and CAMS data have been shown to be very useful to calculate elevation change over Fleming Glacier (Gardner et al., 2017; Wendt et al., 2010; Zhao et al., 2017).

To better account for the large scatter, we now follow the previously published approach by Wendt et al. (2010) by applying a median filter to the data, before calculating the cubic regression functions. However, this did not change the results substantially. We have added a note on this to the text:

The large scattering of the data is due to the highly crevassed surface of the glacier tongue, where a purely horizontal displacement of crevasses can cause apparent positive and negative elevation differences. Therefore, a median filter was applied to the data before adjustment of a cubic function.

We have also updated Figure 5a and the numbers accordingly. Furthermore, we have followed the suggestion of the reviewer by adding distance markers to Figure 4.

P9 L10 – Based on figure 5a, stating that the CAMS-ATM show elevation change of 4.1 ± 0.2 m/a is not credible. The raw data shown in figure 5a shows that at this location the elevation change ranges between -0.5 to 7 m/yr, so the error of 0.2m/yr is effectively meaningless. Please revise the error estimate here, and throughout the rest of the results paragraph.

In order to minimize the influence of scatter, we now apply a median filter before calculating the cubic regression functions of elevation change. Furthermore, for all median ice thinning rates we now calculate the normalised median absolution deviation (NMAD) as a measure of the statistical dispersion ("error") of the input data. We have added a note on this to the results paragraph (5.2):

For all median elevation change rates presented below, we calculated the NMAD in order to account for the statistical dispersion of the input data.

P9 L12 – The fact that Fleming is thinning between 04-08, doesn't prove that the catchment hasn't reached an equilibrium since shelf collapse in 1989. The two effects may be entirely uncorrelated, so although its possible, without a continuous dataset I don't think it can be proven one way or the other. I recommend the authors revise this wording. Changing 'shows' to 'might suggest' would be more factually correct.

We agree. We have changed the sentence according to the reviewer`s suggestion.

P9 L22 - Although I don't like the method, it's clear how the hydrostatic equilibrium has been calculated along the airborne tracks. Can the authors clarify what method they have used to draw the grounding line connecting the dots in Figure 6? For example, according to their own data, a section of grounded ice on track 'c)' is included in the now 'floating' area. I recommend the authors revise the line as their data shows it isn't correct.

The reviewer is right. Originally we assumed that since the data used for calculating the hydrostatic equilibrium on track c was acquired in 2011, the area now may be floating. However, since there is no further evidence for this assumption, we have revised the line according to the reviewer`s suggestion and have updated Fig. 6 and Fig. S7 accordingly.

Following the suggestions of reviewer #2, we have also added some more information to the methods section on how the dots have been connected:
Wherever possible, we gave preference to information on hydrostatic equilibrium for the final decision of the recent grounding line location. For selected profiles across the glacier, recent and previous (2008) grounding line positions were estimated by combining evidence from elevation change, bedrock topography and surface velocity. In the remaining areas, the recent grounding line was deduced from combining information on elevation change rate patterns in the TDX/TSX 2011–2014 dh/dt map with information on bedrock topography.

P9 L30 – Based on the above comment, the number stated for the area of the floating shelf will also need to recalculated.
We have changed the number for the area of floating ice throughout the text to ~56 km ^{2}.

P9 L33 – Quantify 'several km'.
We have changed the sentence into:
The ridges reach up to ~ 9 km upstream of the 1996 grounding line.

P10 L25 – Although Turner et al 2016 shows that the long term air temperatures are decreasing the situation may be more complicated than that, and Sundal et al (2011) showed that a simple linear relationship between melt water vs lubrication is not currect, as melt induced speed-up can be offset by drainage efficiencies. I'd revise this text to avoid oversimplifying these relationships.
We agree with the reviewer that in Greenland, where strong surface melt in summer leads to enhanced basal sliding due to rapid migration of meltwater to the ice-bedrock interface, the acceleration effect may be offset by efficient drainage. However, the meltwater production on Fleming Glacier is generally not large enough to percolate to the bed (Rignot et al., 2005). No seasonal variation in surface velocities, which would indicate enhanced glacial sliding during summer, is observed. Furthermore, surface melt is likely further reduced due to decreasing air temperatures. Hence, since there is no enhanced basal sliding due to percolating surface meltwater on Fleming Glacier, the mechanisms as described by Sundal et al. (2011) play no role. Nevertheless, we have added a sentence on basal sliding and variations in drainage efficiency:
In Greenland seasonal velocity fluctuations have been linked to both enhanced basal sliding due to the penetration of surface melt water to the ice-bedrock interface and inter-annual differences in drainage efficiency (Moon et al., 2014; Sundal et al., 2011; Zwally et al., 2002).

P11 L4 – Remove sentence about basal melt. This hasn't been measured in this study so is just a generic assumption, and no reference provided to previous study evidencing statement.
We have largely rewritten the discussion, conclusion and abstract sections. In the revised manuscript we link our observations to high basal melt rates measured by Depoorter et al. (2013) and Rignot et al. (2013) and to exceptional phases of warm CDW intrusions in 2008/2009 and 2010/2011 found by Walker and Gardner (2017). Anyway, the sentence has been deleted during reformulation of this section.

P11 L8 – Same statement as above re basal melt inference.
Same answer as for P11 L4

P11 L11 – Really poor sentence wording. Edit to be more diligent with regards to terminology. Stability is a specific process, i.e. 'unstoppable' retreat that will continue to propagate even if environmental conditions returned to their original state. Glacier imbalance and grounding line retreat can occur stably. I haven't seen evidence presented in this paper of about the likely future instability of Fleming, so tighten up language.
We apologize for the poor wording. However, it is well known that if grounding line retreat occurs on ice resting on a retrograde bed, this can trigger a positive feedback loop of flow acceleration, dynamic thinning, increased calving, mass loss and further grounding line retreat

(“marine ice sheet instability” or “tidewater instability”) (e.g. DeConto and Pollard, 2016; Favier et al., 2014; Rignot et al., 2014; Schoof, 2012). The subglacial topography of Fleming Glacier reveals a trough 3-4 km upstream of the current grounding line, which has such a retrograde bed. This suggests that once the grounding line has exceeded the edge of the trough, instability is possible. Especially since further oceanic forcing is likely to continue in the future (see answer P12L18), this is a likely future scenario for Fleming Glacier. We have changed the sentence into:
Hence, if grounding line retreat exceeds the edge of this trough, destabilisation like on Thwaites Glacier and in the Pine Island Bay region (Favier et al., 2014; Rignot et al., 2014) is possible, which would involve further rapid grounding line retreat and amplified mass loss in the future.

P11 L12 – edit Thwaites
We have changed this accordingly.

P11 L18 – Again I feel the analysis here is overly simplistic. Wouters et al were the first to present the rapid thinning rates and mass loss from the Western Palmer Land region, but subsequent publications (Hogg et al 2017) have shown that only ~30% of this should be attributed to ocean induced dynamic imbalance. Revise text to reflect known complexity. Discussion general – the authors have stated results from other regions of WAIS/AP, however this really isn’t tied very coherently into how this impacts on the results they have presented on Fleming.
We understand the concerns of the reviewer. We have added a sentence on the publication of Hogg et al. (2017):
For the glaciers on Western Palmer Land Hogg et al. (2017) showed that ~ 35% of the ice loss after 2009 can be attributed to dynamic thinning triggered by ocean driven melt.
Additionally, we have substantially rewritten the discussion section, following the suggestions of reviewer #1 and #2. The timing of acceleration found by us is coincident with the timing of recently published exceptional warm CDW intrusions in 2008/2009 and 2010/2011 to Wordie Bay due to phases of strong westerly winds during strong La Niña/+SAM (positive Southern Annular Mode) events. Hence, we could substantiate the link between ocean forcing and our observed dynamic changes.

P12 L10 **–** as previously stated I do not think the authors have proven dynamic instability on Fleming. Imbalance maybe, but instability, no. Equally, attributing the signal to ocean induced dynamic forcing without properly evaluating any oceanographic or atmospheric data is poor. These interlinked processes are very complex, and really hard to disentangle. Although its entirely plausible that ocean forcing is responsible, I do not think the analysis presented in this paper has proven it.
See answer P11L18

P12 L18 – ‘ocean forcing is likely to continue’. Do the authors present any evidence to support this statement, or is it just a guess?
We thank the reviewer for this important remark. Oceanic forcing is likely to continue, since the SAM is expected to be shifted further poleward, which would foster conditions like in 2008/2009 and 2010/2011. We have changed the sentence into:
Pronounced oceanic forcing is likely to continue, since the SAM is forecasted to be shifted further poleward, which will foster conditions like those during the strong La Niña/+SAM events in 2008/2009 and 2010/2011 (Abram et al., 2014; Fogt et al., 2011; Walker and Gardner, 2017). Thus, further retreat of the grounding line and more dynamic thinning are expected on Fleming Glacier.

References

Abram, N. J., Mulvaney, R., Vimeux, F., Phipps, S. J., Turner, J. and England, M. H.: Evolution of the Southern Annular Mode during the past millennium, Nature Climate change, 4, 564–569, doi: 10.1038/nclimate2235, 2014.

Burgess, E. W., Forster, R. R., Larsen, C. F. and Braun, M.: Surge dynamics on Bering Glacier, Alaska, in 2008–2011, The Cryosphere, 6, 1251–1262, doi: 10.5194/tc-6-1251-2012, 2012.

Corr, H. F. J., Doake, C. S. M., Jenkins, A. and Vaughan, D. G.: Investigations of an "ice plain" in the mouth of Pine Island Glacier, Antarctica, J. Glaciol., 47, 51–57, doi: 10.3189/172756501781832395, 2001.

Crabtree, R. D. and Doake, C.S.M.: Pine Island Glacier and Its Drainage Basin. Results From Radio Echo-Sounding, A. Glaciology., 3, 65–70, doi: 10.3189/S0260305500002548, 1982.

DeConto, R. M. and Pollard, D.: Contribution of Antarctica to past and future sea-level rise, Nature, 531, 591–597, doi: 10.1038/nature17145, 2016.

Depoorter, M. A., Bamber, J. L., Griggs, J. A., Lenaerts, J T M, Ligtenberg, S R M, van den Broeke, M R and Moholdt, G.: Calving fluxes and basal melt rates of Antarctic ice shelves, Nature, 502, 89–92, doi: 10.1038/nature12567, 2013.

Enderlin, E. M. and Howat, I. M.: Submarine melt rate estimates for floating termini of Greenland outlet glaciers (2000–2010), J. Glaciol., 59, 67–75, available at: https://www.cambridge.org/core/services/aop-cambridge-core/content/view/8BA26CC3DEBE21F24ABF703A41A97C3A/S0022143000203419a.pdf/div-class-title-submarine-melt-rate-estimates-for-floating-termini-of-greenland-outlet-glaciers-2000-2010-div.pdf, 2013.

Favier, L., Durand, G., Cornford, S. L., Gudmundsson, G. H., Gagliardini, O., Gillet-Chaulet, F., Zwinger, T., Payne, A. J. and Le Brocq, A. M.: Retreat of Pine Island Glacier controlled by marine ice-sheet instability, Nature Climate change, 4, 117–121, doi: 10.1038/nclimate2094, 2014.

Fogt, R. L., Bromwich, D. H. and Hines, K. M.: Understanding the SAM influence on the South Pacific ENSO teleconnection, Clim Dyn, 36, 1555–1576, 2011.

Fretwell, P., Pritchard, H. D., Vaughan, D. G., Bamber, J. L., Barrand, N. E., Bell, R., Bianchi, C., Bingham, R. G., Blankenship, D. D., Casassa, G., Catania, G., Callens, D., Conway, H., Cook, A. J., Corr, H. F. J., Damaske, D., Damm, V., Ferraccioli, F., Forsberg, R., Fujita, S., Gim, Y., Gogineni, P., Griggs, J. A., Hindmarsh, R. C. A., Holmlund, P., Holt, J. W., Jacobel, R. W., Jenkins, A., Jokat, W., Jordan, T., King, E. C., Kohler, J., Krabill, W., Riger-Kusk, M., Langley, K. A., Leitchenkov, G., Leuschen, C., Luyendyk, B. P., Matsuoka, K., Mouginot, J., Nitsche, F. O., Nogi, Y., Nost, O. A., Popov, S. V., Rignot, E., Rippin, D. M., Rivera, A., Roberts, J., Ross, N., Siegert, M. J., Smith, A. M., Steinhage, D., Studinger, M., Sun, B., Tinto, B. K., Welch, B. C., Wilson, D., Young, D. A., Xiangbin, C. and Zirizzotti, A.: Bedmap2: improved ice bed, surface and thickness datasets for Antarctica, The Cryosphere, 7, 375–393, doi: 10.5194/tc-7-375-2013, 2013.

Fricker, H. Amanda: Redefinition of the Amery Ice Shelf, East Antarctica, grounding zone, J. Geophys. Res., 107, doi: 10.1029/2001JB000383, 2002.

Gardner, A. S., Moholdt, G., Scambos, T., Fahnestock, M. A., Ligtenberg, S., van den Broeke, Michiel and Nilsson, J.: Increased West Antarctic ice discharge and East Antarctic stability over the last seven years, The Cryosphere Discuss., 1–39, doi: 10.5194/tc-2017-75, 2017.

Griggs, J. A. and Bamber, J. L.: Antarctic ice-shelf thickness from satellite radar altimetry, Journal of Glaciology, 57, 485–498, doi: 10.3189/002214311796905659, 2011.

Hogg, A. E., Shepherd, A., Cornford, S. L., Briggs, K. H., Gourmelen, N., Graham, J. A., Joughin, I., Mouginot, J., Nagler, T., Payne, A. J., Rignot, E. and Wuite, J.: Increased ice flow in Western Palmer Land linked to ocean melting, Geophys. Res. Lett., 353, 283, doi: 10.1002/2016GL072110, 2017.

Krabill, W. B., Abdalati, W., Frederick, E. B., Manizade, S. S., Martin, C. F., Sonntag, J. G., Swift, R. N., Thomas, R. H. and Yungel, J. G.: Aircraft laser altimetry measurement of elevation changes of the greenland ice sheet. Technique and accuracy assessment, Journal of Geodynamics, 34, 357–376, doi: 10.1016/S0264-3707(02)00040-6, 2002.

Martin, C. F., Krabill, W. B., Manizade, S. S., Russell, R. L., Sonntag, J. G., Swift, R. N. and Yungel, J. K.: Airborne topographic mapper calibration procedures and accuracy assessment, Hanover, Tech. Rep. NASA/TM-2012-215891, 32 pp., 2012.

Miranda, N., Rosich, B., Santella, C. and Grion, M.: Review of the impact of ERS-2 piloting modes on the SAR Doppler stability, Proceedings Fringe '03, 1–5, 2003.

Moon, T., Joughin, I., Smith, B., Broeke, M. R., Berg, W. Jan, Noël, B. and Usher, M.: Distinct patterns of seasonal Greenland glacier velocity, Geophysical Research Letters, 41, 7209–7216, available at: http://onlinelibrary.wiley.com/doi/10.1002/2014GL061836/full, 2014.

Münchow, A., Padman, L. and Fricker, H. A.: Interannual changes of the floating ice shelf of Petermann Gletscher, North Greenland, from 2000 to 2012, J. Glaciol., 60, 489–499, available at: https://www.cambridge.org/core/services/aop-cambridge-core/content/view/5DA6EADBD7E2C73FC0B79505689D4D3D/S0022143000205960a.pdf/interannual_changes_of_the_floating_ice_shelf_of_petermann_gletscher_north_greenland_from_2000_to_2012.pdf, 2014.

Rankl, M., Fürst, J. Jakob, Humbert, A. and Braun, M. Holger: Dynamic changes on Wilkins Ice Shelf during the 2006-2009 retreat derived from satellite observations, The Cryosphere Discuss., 1–17, doi: 10.5194/tc-2016-218, 2016.

Rankl, M., Kienholz, C. and Braun, M.: Glacier changes in the Karakoram region mapped by multimission satellite imagery, The Cryosphere, 8, 977–989, doi: 10.5194/tc-8-977-2014, 2014.

Rignot, E., Casassa, G., Gogineni, P., Kanagaratnam, P., Krabill, W., Pritchard, H. D., Rivera, A., Thomas, R., Turner, J. and Vaughan, D. G.: Recent ice loss from the Fleming and other glaciers, Wordie Bay, West Antarctic Peninsula, Geophys. Res. Lett., 32, doi: 10.1029/2004GL021947, 2005.

Rignot, E., Jacobs, S., Mouginot, J. and Scheuchl, B.: Ice-shelf melting around Antarctica, Science (New York, N.Y.), 341, 266–270, doi: 10.1126/science.1235798, 2013.

Rignot, E., Mouginot, J., Morlighem, M., Seroussi, H. and Scheuchl, B.: Widespread, rapid grounding line retreat of Pine Island, Thwaites, Smith, and Kohler glaciers, West Antarctica, from 1992 to 2011, Geophys. Res. Lett., 41, 3502–3509, doi: 10.1002/2014GL060140, 2014.

Rignot, E., Mouginot, J. and Scheuchl, B.: Antarctic grounding line mapping from differential satellite radar interferometry, Geophys. Res. Lett., 38, n/a, doi: 10.1029/2011GL047109, 2011.

Schoof, C.: Marine ice sheet stability, J. Fluid Mech., 698, 62–72, doi: 10.1017/jfm.2012.43, 2012.

Seehaus, T., Marinsek, S., Helm, V., Skvarca, P. and Braun, M.: Changes in ice dynamics, elevation and mass discharge of Dinsmoor–Bombardier–Edgeworth glacier system, Antarctic Peninsula, Earth and Planetary Science Letters, 427, 125–135, doi: 10.1016/j.epsl.2015.06.047, 2015.

Seehaus, T., Marinsek, S., Skvarca, P., van Wessem, J. M., Reijmer, C. H., Seco, J. L. and Braun, M. H.: Dynamic Response of Sjögren Inlet Glaciers, Antarctic Peninsula, to Ice Shelf Breakup Derived from Multi-Mission Remote Sensing Time Series, Front. Earth Sci., 4, F01005, doi: 10.3389/feart.2016.00066, 2016.

Sundal, A. Venke, Shepherd, A., NIENOW, P., Hanna, E., Palmer, S. and Huybrechts, P.: Melt-induced speed-up of Greenland ice sheet offset by efficient subglacial drainage, Nature, 469, 521–524, doi: 10.1038/nature09740, 2011.

Tinto, K. J. and Bell, R. E.: Progressive unpinning of Thwaites Glacier from newly identified offshore ridge. Constraints from aerogravity, Geophys. Res. Lett., 38, n/a-n/a, doi: 10.1029/2011GL049026, 2011.

Walker, C. C. and Gardner, A. S.: Rapid drawdown of Antarctica's Wordie Ice Shelf glaciers in response to ENSO/Southern Annular Mode-driven warming in the Southern Ocean, Earth and Planetary Science Letters, 476, 100–110, doi: 10.1016/j.epsl.2017.08.005, 2017.

Wendt, J., Rivera, A., Wendt, A., Bown, F., Zamora, R., Casassa, G. and Bravo, C.: Recent ice-surface-elevation changes of Fleming Glacier in response to the removal of the Wordie Ice Shelf, Antarctic Peninsula, Annals of Glaciology, 51, 97–102, doi: 10.3189/172756410791392727, 2010.

Zhao, C., King, M. A., Watson, C. S., Barletta, V. R., Bordoni, A., Dell, M. and Whitehouse, P. L.: Rapid ice unloading in the Fleming Glacier region, southern Antarctic Peninsula, and its effect on bedrock uplift rates, Earth and Planetary Science Letters, 473, 164–176, doi: 10.1016/j.epsl.2017.06.002, 2017.

Zwally, H. Jay, Abdalati, W., Herring, T., Larson, K., Saba, J. and Steffen, K.: Surface melt-induced acceleration of Greenland ice-sheet flow, Science (New York, N.Y.), 297, 218–222, doi: 10.1126/science.1072708, 2002.