Reviewers' comments:

Reviewer #1 (Remarks to the Author):

A. Summary of the key results

The key results of this paper are that for the first time, the amount of melting from the bottom of floating Antarctic ice shelves in the Amundsen Sea Embayment (ASE) has been directly and accurately quantified. This is significant because the ASE is the region of Antarctica from where mass loss is greatest, but to date only coarse, indirect estimates of loss have been available. Being able to directly quantify at a much finer resolution is a significant advance. The successes of this approach have also revealed substantial regional mass loss from beneath three study glaciers (Kohler, Smith and Pope) and the authors use such broad regional changes to argue that this supports the hypothesis of an influx of warm ocean water, which is responsible for this loss. They also explore variability between the three study glaciers, and note differences that are attributable to variations in subglacial and submarine topography, which controls the ease with which warm ocean waters can access subglacial locations.

B. Originality and interest

This strikes me as novel and of significant interest. We have known about the susceptibility of ASE to mass loss, and thus its importance. We have also known about the likely role of warm ocean waters being responsible for melting at depth. However, the fact that the authors are able to directly quantify the magnitude of loss (and to do so with a fair degree of confidence in their data) is a substantial forward step. Carrying out similar observations in other parts of Antarctica would seem to be the next obvious step.

C. Data & methodology: validity of approach, quality of data, quality of presentation

I think the approach is neat and valid. Having an Operation IceBridge (OIB) survey line flown on two separate occasions provide a useful and extremely valuable source of comparison-data. It is a shame that subsequent data comparisons rely only on crossover points, and so are limited to a few locations. I do perhaps have some reservations about the hypothesising as to what has and occurring from a relatively limited dataset, however, this is the limitation of the available data, and I think the authors do a decent job of making the most of what is available to them.

Possibly my most significant criticism of this work is the way that the data is presented in the series of figures, and most importantly with respect to their Figure 1a. I find this very hard to appreciate - it took me a great deal of time to extract all the relevant information from, which leads me to think that perhaps too much is displayed here, and so confusion sets in. First off, the grey-scale background is very high contrast, and this, straight away, has the effect of making everything else in the image harder to see. Secondly, the colour-scheme showing ice bottom elevations is very confusing. It is hard to see partly because of the high-contrast background, but primarily because of the choice of colour-scheme for this AND for the grounding-line locations. These are far too similar. It is also not clear on Figure 1a whether the repeat flightpath is the whole coloured line. It's also tricky to differentiate the AGASEA line. Also, (on 1a), the 'dots' indicating profiles over each glacier are also lost amongst the background. I'd like to see a clearer delineation of the glaciers under consideration, and to see these marked on 1c,

Moving on, Figures 2 and 3 contain important information regarding the three study glaciers. I am a bit confused therefore why they are not all treated equally. For instance, why is Smith glacier given a figure all to itself (with three parts) whereas Kohler and Pope Glacier share Figure 3, and only have 2 parts each? Some consistency would help. I also think that having arrows indicating which way is up/down stream would be useful. I know this is easily worked out, but it just aid with the ease of viewing and understanding each figure. Finally, on Figure 2b, it's a little confusing that the 2002 bed seems to be made up of a partially dotted blue line...just like the 2002 flotation level.

On Figure 4, the blue mask makes it impossible to see the radargram beneath it.

Figure 5 appears in a strange location. It's the second figure referred to in the text so it is odd to me that it appears as the last figure. I believe it should be the second. As with figure 1, I find the figure quite difficult to interpret. The background is, again, too high contrast. The choice of colours for the grounding line and ice bottom elevation change is poor - they are simply too similar. I also think that greater clarity in the associated caption would help - for instance, the 'epoch' changes are presumably at crossover locations. This is not particularly clear. Finally, the OIB path is very hard to see - grey against a grey background, with grey grid lines too! Another question about this figure - why are there epoch changes indicated where the OIB path is not intersected? The caption tells us that observations are from OIB crossovers, so it makes no sense to show epoch changes where there is no OIB line indicated.

D. Appropriate use of statistics and treatment of uncertainties

I am not a statistical expert, but in my limited opinion, I am happy that the statistics and errors presented are appropriate. I would urge that the Editor seek the opinion of other reviewers of this though!

E. Conclusions: robustness, validity, reliability

There is quite a lot of speculation...lots of hypotheses are proposed based on the fairly limited dataset. However, I think this is just about acceptable. The authors present some interesting data and interpret it soundly, and then postulate what the significance might be. I am reasonably happy that the conclusions drawn are largely robust enough. However, there are a few locations where some improvement regarding clarity and evidence could be made, and I deal with these in the next section where I go through more minor points that I would like to see rectified.

F. Suggested improvements: experiments, data for possible revision

Here, I suggest point-by-point improvements that should be made:

1. L13-14: I'm confused how the authors can say that between 2009 and 2014, melt rates are 'similar' but then say that this results in a 'slower' rate of mass loss. This seems contradictory to me.
2. L47: I think talk of 'the loop' is not particularly clear, and the location and position of a flightline that is reflown needs to be made clearer.
3. L54: the authors talk about how past studies have been carried out at much coarser resolution. I appreciate that the work presented here is, on the one hand, at much finer resolution, but the fact that there are only a relatively small number of spot-locations (cf. Figure 5) actually means that some interpretation is drawn from quite a coarse set of points. I'd welcome some greater consideration of this.
4. L63: I'd like to know where the '10-35 m' uncertainty comes from.
5. L82: a brief explanation of the relevance and importance of the 'Coriolis-favoured side of the cavity' would be useful.
6. L87: when talking about mass loss here, it would be helpful if the authors could say that this is (presumably) from the base. They go into this in greater detail in the methods section, but I think

some greater clarification here is desirable.
7. L93: mention of retreat between 1996 and 2009, but there is no 1996 data on Figure 3b.
8. L99: slightly strange to talk about 'less faithful correspondence'. Can you please elaborate and explain further?
9. L102: The grounding line looks to me (on Figure 3d) to be grounded at 0km. 'Around 1km' is a bit vague, and not really what the figure shows.
10. L114: I'm a little uneasy about the term: 'strongly suggest'. I'd say that it doesn't really do much more than it suggesting this as a possibility. More study is required to truly ascertain this link.
11. L125: this discussion of a ridge that might block the cavity is a bit of a problem for the key interpretation here isn't it?
12. L146: I think it's perhaps a little misleading to use the term 'differently' here. Surely it's the fact that the response was uniform and widespread that provides the evidence for concluding that there is an oceanic warming effect. To therefore go onto talking about 'different' responses perhaps undermines this a little.
13. L154-159: I'd quite like to see more evidence for the existence of this topographic feature, but appreciate that perhaps it's beyond the scope of this piece of work.
14. L176: further evidence of making a lot of suggestions/hypotheses with little evidence. I guess this is okay, but I wonder if the confidence should be toned down a little.
15. L181: I'm a little confused. Does Figure 3d show the ice bottom or the bed surface? Clearly, when grounded, the two are the same, but not when ice is floating.
16. L188: This mention of a 'prominent bed topographic rise' is a bit vague. Can it not be clearly marked on the figure?
17. L211: remove this mention of 'spaceborne sounding radars'. For observations of the earth's surface, this would be an amazing achievement but they are not yet viable.
18. L242-244: This sentence is not clear and needs rephrasing.

G. References: appropriate credit to previous work?

Referencing seems fine to me. It's done appropriately and accurately, as far as I am aware.

H. Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

The paper is generally very well written. It is free of typographic or grammatical errors, for which the authors are to be congratulated. As outlined above, slightly greater clarity could be provided if (on occasions) further explanation is provided. Primarily though, understanding would be enhanced by improving some of the figures.

Reviewer #2 (Remarks to the Author):

This is an interesting paper that reports rapid thinning in the grounding zones of ice shelves in the Amundsen Sea Sector of West Antarctica. The focus is on ice shelves that have received comparatively little attention to date. Neighbouring features have captured more of the headlines, because of the size of their inland catchments, but ocean-driven change has been even more rapid on the floating ice shelves discussed here. Thus the findings are of broad interest, despite the more limited potential of the smaller inland catchments to change eustatic sea level. I therefore think that the paper is suitable for inclusion in Nature Communications, but I would recommend some work to improve its clarity prior to acceptance.

There are four main areas of potential improvement:

1) The overall structure of the paper could be improved. It took me several reads to really follow everything. I think the main problem is that, although the text is reasonably well organised, the figures are not well arranged in support, and figure calls are not sequential. For example, Figure 5 is mentioned repeatedly and the first instance is before Figures 2-4. Figure 1c is not mentioned until the Methods sections. Figure 2 covers two sections from one ice shelf, while Figure 3 covers two ice shelves. Figure 4 is hardly used, and does one of the panels appear in Figure 3 anyway? Overall, the need to go backwards and forwards between Figures and the difficulty in working out how they relate makes for a disrupted read. For example, the Figure 1 caption tells me that coloured dots indicate the ends of the profiles in Figures 2 and 3, but beyond that I am left to work things out for myself. They are colour-coded, but that could be clearer and a description of the colour coding would have helped, and some of the dots are common to two profiles? Also indicating which end is which, especially for Figure 2 would be helpful.

2) There are some confusing statements about how the calculations were made. On lines 60-70 there is a statement that observations of thickness change give a direct measurement of ice loss. I agree that thickness measurements are much preferable to elevation measurements, which must be converted to thickness through an assumption of free floatation. However, you still need to account for the thickness change that comes about through changes in ice dynamics and surface accumulation. Later on in the Methods sections (and lines 84-85) there is a discussion of these other processes, but the text on lines 60-70 (and line 44) appears to contradict this.

3) I didn't find the explanation of the differences seen on Smith Glacier during the later stages of the observations very convincing. The added depth of the new grounding line does give a slightly greater potential to melt, but I don't see why the water temperature should be any higher. The waters have flowed in from the same source, at least for Smith and Pope, haven't they? I also think that the concept of a deep reservoir of warm water beneath the ice shelf is flawed. If the water drives melting it must be flushed out by the associated decrease in density and be replaced by new more dense water. Furthermore, thermodynamically it cannot work. The sensible heat stored in a 100 m thick layer of water that is 4 degrees above the freezing point is sufficient to melt a layer of ice only 5 m thick. To sustain melting the cavity below the ice shelf must be flushed. It seems to me that a more likely explanation is simply that the geometry of the bed maintains the downslope retreat of the Smith Glacier grounding line. That exposes more of the glacier bed to melting, and the geometrically-driven increase in melting outweighs any reduction that might result from reduced thermal forcing, at least in the grounding zone where the observations are focussed.

4) In the methods section there is a discussion of the uncertainty in the ice thickness measurements, but no mention of the uncertainty in the horizontal positioning of the measurements. Where there is steep basal topography, horizontal positioning errors can give large biases in what are assumed to be "repeat" thickness measurements. I am in no way questioning the validity of the results presented here. The thickness changes are so big that the majority of the signal must be real. However, I would have liked to have seen a discussion of the potential errors introduced by positional uncertainty. Also, in allowing a 200 m offset between "repeat" measurements, how much potential error does this introduce, given the maximum spatial ice thickness gradients observed?

More minor comments:

Line 14: The melting rates are "similar" to what? If they are the same as the melting rates from 2002-2009, how can they explain the stabilisation in the of rate mass loss?

Line 82: What do you mean by "Coriolis-favoured" side of the cavity?

Line 212: I would not describe radar sounding of ice thickness as "an entirely new approach".

Reviewer #3 (Remarks to the Author):

Review of 'Rapid submarine ice melting in the grounding zones of ice shelves in West Antarctica', Khazendar et al.

• What are the major claims of the paper? Are the claims novel?

This paper presents a new set of direct measurements of ice shelf thinning from three major glacier-shelf complexes (Smith, Pope, Kohler) in coastal West Antarctica, calculates the fraction of thinning caused by ocean-driven basal melting and relates this to other time series of glacier flow rate and grounding-line migration. The authors conclude that melt rates rose to become well above the balance rate at all three sites in the 2002-2009 period, driving strong thinning that explains the rapid grounding line retreat and glacier acceleration through the early 2000s reported elsewhere, then maintained a similar melt rate at the three sites after this, explaining a previously-observed regional slowing in the rate of glacier acceleration at this time. At Pope and Kohler, grounding line retreat largely stopped or was reversed in this later period while continuing strongly at Smith, and this contrast in response to the sustained melting is, the authors say, because Smith has a particularly deep subglacial trough that is well suited to funneling warm, dense water into contact with the ice, while at Pope and Kohler, the grounding lines retreated along shallower troughs. This interpretation fits with expectations of such systems but is novel in providing spatially extensive and directly observed melt rates in the key grounding zones.

• Will the paper be of interest to others in the field?

These direct observations are valuable to the field because, as the authors note, the only previous widespread ice shelf melting measurements are indirect (e.g. from surface lowering), which means that they have important limitations in the areas that are of most interest to ice sheet dynamics: the grounding zones. Indirect measurements are either potentially biased (e.g. where ice shelves are incorrectly assumed to be free-floating) or are spatially limited to areas known to be free floating, away from the grounding zones. Direct measurements from these zones therefore help clarify the important relationship between ocean forcing (likely brought on by changes in regional climate) and the ice sheet dynamic response, which has global significance in terms of sea level rise. There is intense interest within glaciology in understanding the drivers of ice sheet loss in this region, in predicting how the forcing will change, and in particular how ice sheet loss will evolve (either as a stable, forced response or as an unstable collapse if forcing crosses a stability threshold).

• Will the paper influence thinking in the field?

These observations are most likely to be of value to numerical modelers seeking to tune models of ice sheet dynamics with more realistic ocean forcing to match observed dynamic behavior. They will improve rather than revolutionize this work. We can hope that they will also inspire future airborne radar campaigns to extend these observations to larger areas and over longer time periods.

• Are the claims convincing? If not, what further evidence is needed?

The core results showing ice shelf thinning around the 2002-2009 period presented in Figure 2 are clear and compelling. I suggest some minor improvements below.

The claim that the observed rapid then slower regional glacier acceleration and various grounding line migrations are explained by the temporal pattern in these melt rates is not, however, supported by the results. The issues are:

1) the authors claim that their observations support the argument for a 'large increase' in melting in the mid-2000s, but these results do not show melt rates prior to the 2002-2009 period, so no change can be demonstrated. It is not possible to tell whether melt rates increased, decreased or stayed the same.

2) for the 2009-2014 period, the authors claim that melt rates remain similar to the 2002-2009 period and that this explains other observations of a slowing in the rate of acceleration of glacier flow at this time, and both the 'stabilization' of the Pope and Kohler groundling lines as well as the continued rapid retreat of the Smith grounding line. The problem with this assertion is that it is not clear from Figure 5 that the melt rates did remain largely the same at all three sites post 2009. For example on the shelf in front of Kohler Glacier, melt rates appear at this time to vary over short distances from ~10 m thickening to ~70 m thinning per year, and on Pope, from ~10 m thickening to ~40 m thinning per year, in time periods that overlap (e.g. 2009-2014 and 2011-2014). In both cases, the results presented could in fact be interpreted to show an abrupt reduction in melt rate. Given the very rough ice shelf bases apparent in Figure 2, this is probably a sampling problem - the cross-over measurements used for the various 2009-2014 periods are point measurements and they are sparse.

Although the 2002-2009 snapshot of melt rates is compelling, the lack of data pre-2002 and the confusing picture post 2009 undermines the major conclusions about the way in which melt rates and bathymetry control grounding line migration and glacier acceleration. Perhaps the contrasts in grounding line behavior between Pope/Kohler and Smith and the regional reduction in glacier acceleration post 2009 are in fact due to variability in the melt rate after all - it's not clear from these results. The authors have a great spatial dataset of melt but a weak temporal one, and it does not seem possible to improve the existing temporal dataset.

An alternative approach would be for the authors to concentrate on the strengths of the 2002-2009 data and analyze the grounding line and flow rate changes in the context of the loss of buttressing implied by the thinning shown here and also by the interferometry results presented in the 'related paper', although this would be quite a different paper.

Other specific issues:

Line 73: Figure 2 suggests this should be -113.0 and -112.5 degrees.

Line 80: 'Another location...' - this is the same location covered by the above. (-113.1 degrees is grounded).

Throughout results: present thinning rate (m/yr) as well as total thinning (m) in each case.

Lines 90/91/97: claims for 'similarity' and 'persistence' of melt rates are stretching the results too far (see above).

Line 114-116: no evidence presented for melt rates pre 2002.

Lines 180 and 186: 'smallest grounding line retreat between 1996 and 2009', 'second farthest retreat by 2009' - Smith Glacier has no 2009 GL reported here (only Kohler and Pope) so these statements are not meaningful.

Throughout - perhaps 'marine' or 'oceanic' rather than 'hydrographic'?

Line 211: spaceborne radar - this is not introduced anywhere or referenced, seems out of place. This would be 'entirely new' but can't be brought into paper here.

Methods:

Radar sounding resolution: as the authors do, it is conventional to report a radar resolution which is usually calculated as a fraction of the radar wavelength. However in cases such as this where a single, prominent reflecting horizon is detected (such as an air-ice or ice-ocean interface), it is the ranging precision that is more relevant. Resolution applies to the minimum distinguishable distance between two similar reflectors. Ranging precision is a function partly of wavelength but also of sampling rate of the receiver and the sharpness of the interface, and may be better than the quoted resolution, as perhaps suggested by the reported {plus minus}10 m 'vertical uncertainty'. The thickness uncertainty would be the combination of ranging uncertainties for the upper and lower surfaces. While this is a better measure of the uncertainties relevant here, it is not likely to differ very much from the quoted resolution and so would not change the main results.

Line 236: I don't think phase-sensitive radar measurements are discussed elsewhere in the text.

Line 269: the thinning of slow-flowing areas used to calculate the surface-processes component of the total ice shelf thinning is apparently measured at considerably greater altitude than the shelf surface (on Mount Murphy and in the Kohler Range of mountains). It is unreasonable to assume that this is representative of compaction/accumulation/ablation rates on the ice shelf surface because compaction and ablation are strongly temperature dependent and accumulation is strongly slope and altitude dependent. It would be better to identify low-lying slow-flowing areas or failing that, use climate- and compaction-model output. The difference from the quoted 3-5 m is, however, unlikely to be large enough to change significantly the basal-melt results (up to several hundred meters thinning).

Figure 2b: show the 2010 intersections (given in Figure 5) as well as the 2004 ones.

Figure 2c: show the 2002 and 2009 intersections from downstream of the 1996 GL (given in Figure 1) as well as the upstream ones.

Figure 3b: show the 2014 intersections (given in Figure 5).

Figure 3d: show the 2010 and 2014 intersections (given in Figure 5).

Show the 2002-2009 cross profiles for Pope and Kohler as well as the long profiles given in Fig. 3.

Figure 4: Label the x-axis, align the y-axes for parts a, b and c.

Figure 5: show the 2009 GL given in Figure 3. Differentiate the colour bar between values that are positive and negative, report the elevation rates somewhere (e.g. supplementary table or on the figure) as it's difficult to read off from the colour bar.

Reviewer #1 (Remarks to the Author):

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We much appreciate this perceptive general remark that sums up well how we approached the available data.

Possibly my most significant criticism of this work is the way that the data is presented in the series of figures, and most importantly with respect to their Figure 1a. I find this very hard to appreciate - it took me a great deal of time to extract all the relevant information from, which leads me to think that perhaps too much is displayed here, and so confusion sets in. First off, the grey-scale background is very high contrast, and this, straight away, has the effect of making everything else in the image harder to see. Secondly, the colour-scheme showing ice bottom elevations is very confusing. It is hard to see partly because of the high-contrast background, but primarily because of the choice of colour-scheme for this AND for the grounding-line locations. These are far too similar. It is also not clear on Figure 1a whether the repeat flightpath is the whole coloured line. It's also tricky to differentiate the AGASEA line. Also, (on 1a), the 'dots' indicating profiles over each glacier are also lost amongst the background. I'd like to see a clearer delineation of the glaciers under consideration, and to see these marked on 1c,

We fully agree, and have taken the following steps to improve Fig.1a:
- A background MOA image with much lower contrast is now used.
- Ice bottom elevations along the survey paths are no longer shown. These are plotted in detail in the subsequent figures, so this is not a big sacrifice and it helps lessen the confusion surrounding color use.
- Colored circles are no longer used to mark the start and end points of transects, but instead the letters S-S`, P-P` and K-K`.
- The number of the latitude and longitude lines is reduced, and the lines are made dotted instead of solid to further reduce the clutter of the figure.
- Only the 2002/2009 OIB path is now shown in Fig. 1a. The AGASEA path is now shown in panel 1b, along with the OIB 2002/2009 path in a different color.
- The data of the old Fig. 5 are now shown in Fig. 1a. This is discussed in more detail below.

Changes to Fig. 1b:
- This panel now shows the vicinity of the study area in more detailed.

- The above modification now allows showing both the OIB and AGASEA paths in this panel in different colors to better distinguish between the two.
Fig. 1c now shows the glacier names.

Moving on, Figures 2 and 3 contain important information regarding the three study glaciers. I am a bit confused therefore why they are not all treated equally. For instance, why is Smith glacier given a figure all to itself (with three parts) whereas Kohler and Pope Glacier share Figure 3, and only have 2 parts each? Some consistency would help.

We have now created a new figure 4 for the two panels of Kohler Glacier (old Figs 3c and 3d), so Pope and Kohler glaciers each has its own figure (Figs 3 and 4). Old Fig. 4 is now Fig. 5, and old Fig. 5 is incorporated into Fig. 1a. The reason the Smith Glacier figure has three panels is that, unlike Pope and Kohler, OIB flight paths in 2002 and 2009 traversed this glacier across-flow (Figs 1a and 1b). We resorted to the AGASEA 2004 data to obtain a profile of the glacier along-flow, which the third panel shows. OIB paths in the case of Pope and Kohler were along-flow.
We summarize here numbering changes of the figures between the two versions of the manuscript:

Old		New
Fig. 3c	→	Fig. 4a
Fig. 3d	→	Fig. 4b
Fig. 4	→	Fig. 5
Fig. 5	→	Fig. 1a

I also think that having arrows indicating which way is up/down stream would be useful. I know this is easily worked out, but it just aid with the ease of viewing and understanding each figure.

We have now added arrows to Figs 2c, 3b, 4b and 5c showing the direction of ice flow.

Finally, on Figure 2b, it's a little confusing that the 2002 bed seems to be made up of a partially dotted blue line...just like the 2002 flotation level.

Indeed. We now alert the reader to this at the end of the caption of Fig. 2b. Otherwise, we hope that if it is accepted that using the same color for the surface and the bottom of the ice does not create confusion, then there will be no confusion between the bottom of the ice where it is partially dotted and the floatation level at the surface.

On Figure 4, the blue mask makes it impossible to see the radargram beneath it.

We made the mask more transparent to allow a better view of the radargram.

Figure 5 appears in a strange location. It's the second figure referred to in the text so it is odd to me that it appears as the last figure. I believe it should be the second.

The data of this figure are now shown in Fig. 1a, so this resolves the issue of this figure's location.

As with figure 1, I find the figure quite difficult to interpret. The background is, again, too high contrast. The choice of colours for the grounding line and ice bottom elevation change is poor - they are simply too similar.

As described above in detail, we hope that the steps taken to improve the clarity of Fig. 1 also make these data easier to view.

I also think that greater clarity in the associated caption would help - for instance, the 'epoch' changes are presumably at crossover locations. This is not particularly clear. Finally, the OIB path is very hard to see - grey against a grey background, with grey grid lines too! Another question about this figure - why are there epoch changes indicated where the OIB path is not intersected? The caption tells us that observations are from OIB crossovers, so it makes no sense to show epoch changes where there is no OIB line indicated.

We have now expanded both the caption of this figure and the description of the data in Methods to address these points. In the caption of Fig. 1a we now explain in more detail what was done at each crossover point. In Methods (Lines 243-245) we explain that the post-2009 tracks are not shown and provide a website where they can be viewed. If that is not sufficient, we can produce a figure (or more) showing the intersecting tracks from the post-2009 years to include in the Supplementary Information. Showing those tracks in the current Fig. 1a would clutter it considerably.
In Fig. 1a itself and the caption we also replaced the word "Epoch" with "Time interval" for more clarity. We use the same terminology in the new Supplementary Fig. 1 and Supplementary Table 1.
The measures to improve the visibility of the OIB path and grid lines are described above.

D. Appropriate use of statistics and treatment of uncertainties

I am not a statistical expert, but in my limited opinion, I am happy that the statistics and errors presented are appropriate. I would urge that the Editor seek the opinion of other reviewers of this though!

E. Conclusions: robustness, validity, reliability

There is quite a lot of speculation...lots of hypotheses are proposed based on the fairly limited dataset. However, I think this is just about acceptable. The authors present some interesting data and interpret it soundly, and then postulate what the significance might be. I am reasonably happy that the conclusions drawn are largely robust enough. However, there are a few locations where some improvement regarding clarity and evidence could be made, and I deal with these in the next section where I go through more minor points that I would like to see rectified.

F. Suggested improvements: experiments, data for possible revision

Here, I suggest point-by-point improvements that should be made:

1. L13-14: I'm confused how the authors can say that between 2009 and 2014, melt rates are 'similar' but then say that this results in a 'slower' rate of mass loss. This seems contradictory to me.

The statement referred to slower "increases" in mass loss, as can be seen on Line 14 of the old text. This line no longer appears in the new, shorter version of the Abstract. The relation between the sustained melting rates and slower increases in ice loss is discussed in detail in the main text.

2. L47: I think talk of 'the loop' is not particularly clear, and the location and position of a flightline that is reflown needs to be made clearer.

Agreed. We have now removed this mention of the "loop" (and also removed from Methods), and Fig. 1a now only shows the 2002/2009 OIB repeat flight line, and no longer that of AGASEA (which is now shown in Fig. 1b), to make the reflown lines clearer.

3. L54: the authors talk about how past studies have been carried out at much coarser resolution. I appreciate that the work presented here is, on the one hand, at much finer resolution, but the fact that there are only a relatively small number of spot-locations (cf. Figure 5) actually means that some interpretation is drawn from quite a coarse set of points. I'd welcome some greater consideration of this.

The crossover data covering the years 2009-2014 do offer a small number of locations compared with the much higher resolution of the 2002-2009 data. We now note this explicitly early in the manuscript (Lines 37-39). We believe that there are enough measurements, taken together for the three glaciers, to make carefully considered inferences, as we argue in detail below (in response to Reviewer #3).

4. L63: I'd like to know where the '10-35 m' uncertainty comes from.

Done. This is now explained in detail in Methods (Lines 247-256), referred to on Line 68 and mentioned in the caption of Fig. 1a.

5. L82: a brief explanation of the relevance and importance of the 'Coriolis-favoured side of the cavity' would be useful.

We were referring to the idea that the Coriolis effect leads to higher melting on one side of the cavity relative to the other. After deliberating this point, however, we reached the conclusion that it is best to remove the mention of the Coriolis effect and to restrict ourselves to noting the observed high melting rate at that side of the cavity (Lines 86-87). While Coriolis could be a factor in concentrating melting at that side of the cavity, other factors cannot be excluded. These include the bathymetry, the velocity of circulation and the shape and dimensions of the cavity. Examining these factors we believe is beyond the scope of the current study.

6. L87: when talking about mass loss here, it would be helpful if the authors could say that this is (presumably) from the base. They go into this in greater detail in the methods section, but I think some greater clarification here is desirable.

Done (Line 92). We checked other similar mentions in the manuscript, and they all already explicitly specify this as bottom ice loss.

7. L93: mention of retreat between 1996 and 2009, but there is no 1996 data on Figure 3b.

The 1996 grounding line is indicated in Fig. 3b by a short line at km 0. We should have explicitly mentioned this in the caption. We have now added to the caption of Fig. 2c a description of the lines marking grounding line locations, and mentioning that the 1996 line is shorter when bed topography is not known at the location.

8. L99: slightly strange to talk about 'less faithful correspondence'. Can you please elaborate and explain further?

Done (Lines 104-106).

9. L102: The grounding line looks to me (on Figure 3d) to be grounded at 0km. 'Around 1km' is a bit vague, and not really what the figure shows.

Clarified (Line 107).

10. L114: I'm a little uneasy about the term: 'strongly suggest'. I'd say that it doesn't really do much more than it suggesting this as a possibility. More study is required to truly ascertain this link.

We fully agree with Reviewer #1 that more study is required to ascertain this link (e.g., having records of changes in the properties of ocean water entering the cavity, which as far as we are aware are not available for the early 2000s and preceding years, or very sparse temporally and spatially; mapping ocean bathymetry that would allow warm water to reach the grounding zones, which probably would be achieved in the near future thanks to OIB gravimetry measurements, and other fieldwork efforts). On the other hand, we have endeavored to be careful with our language and did not claim that the findings "prove" the link, but rather "strongly suggest" it. We think that this choice of words is justified. As cited in the manuscript, several previous studies have found concurrent acceleration of mass loss from the ASE region around the mid-2000s, a time coincident with our study period, and those studies suggested changes in the ocean as the likely main culprit. Our direct observations of the bottom mass loss that occurred in that time interval present a strong piece of evidence in support of that hypothesis.
We much appreciate, and share, Reviewer #1's wariness of inflated claims, but we remain convinced that this is not one.

11. L125: this discussion of a ridge that might block the cavity is a bit of a problem for the key interpretation here isn't it?

Not quite. In the sentence directly preceding this one (Lines 151-153) we invoke the possibility that warm water could also be reaching the grounding zone through the Crosson Ice Shelf cavity. Furthermore, a ridge need not be continuous, and could have gaps that would allow the passage of warm water.

12. L146: I think it's perhaps a little misleading to use the term 'differently' here. Surely it's the fact that the response was uniform and widespread that provides the evidence for concluding that there is an oceanic warming effect. To therefore go onto talking about 'different' responses perhaps undermines this a little.

We agree that this first sentence in the paragraph can be confusing, so we removed it and the following sentences explain in detail the differences in glacier evolutions that we address (Lines 172-174).
We are reassured that Reviewer #1 agrees that the widespread, concurrent responses of the glaciers in the ASE support the idea of an oceanic warming effect.

13. L154-159: I'd quite like to see more evidence for the existence of this topographic feature, but appreciate that perhaps it's beyond the scope of this piece of work.

Actually, that is why we are referring to ref. 13 and pointing out Fig. 3f in that paper as it shows the general outline of this topographic feature. The bedrock in that work was possible to infer using a mass conservation technique thanks to the presence of velocity measurements from before the retreat of the grounding line and submergence of the uncovered bed with ocean water.

14. L176: further evidence of making a lot of suggestions/hypotheses with little evidence. I guess this is okay, but I wonder if the confidence should be toned down a little.

Agreed. We have now removed the part about SG rapid melting persisting in the future (Lines 196-198).

15. L181: I'm a little confused. Does Figure 3d show the ice bottom or the bed surface? Clearly, when grounded, the two are the same, but not when ice is floating.

The floatation lines shown in the same figure suggest that between kilometers 0 and -8 the glacier was near floatation or floating in 2002 and floating in 2009. We discuss this region in Lines 104-110.

16. L188: This mention of a 'prominent bed topographic rise' is a bit vague. Can it not be clearly marked on the figure?

Done. Its location is now described clearly in the text (Line 209).

17. L211: remove this mention of 'spaceborne sounding radars'. For observations of the earth's surface, this would be an amazing achievement but they are not yet viable.

Done (Lines 228-229). We believe that it is viable, and papers have been published describing how this could be achieved, but we agree that this is not the forum for that debate.

18. L242-244: This sentence is not clear and needs rephrasing.

Done (Lines 268-270).

G. References: appropriate credit to previous work?

Referencing seems fine to me. It's done appropriately and accurately, as far as I am aware.

H. Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

The paper is generally very well written. It is free of typographic or grammatical errors, for which the authors are to be congratulated. As outlined above, slightly greater clarity could be provided if (on occasions) further explanation is provided. Primarily though, understanding would be enhanced by improving some of the figures.

Reviewer #2 (Remarks to the Author):

This is an interesting paper that reports rapid thinning in the grounding zones of ice shelves in the Amundsen Sea Sector of West Antarctica. The focus is on ice shelves that have received comparatively little attention to date. Neighbouring features have captured more of the headlines, because of the size of their inland catchments, but ocean-driven change has been even more rapid on the floating ice shelves discussed here. Thus the findings are of broad interest, despite the more limited potential of the smaller inland catchments to change eustatic sea level. I therefore think that the paper is suitable for inclusion in Nature Communications, but I would recommend some work to improve its clarity prior to acceptance.

There are four main areas of potential improvement:

1) The overall structure of the paper could be improved. It took me several reads to really follow everything. I think the main problem is that, although the text is reasonably well organised, the figures are not well arranged in support, and figure calls are not sequential. For example, Figure 5 is mentioned repeatedly and the first instance is before Figures 2-4. Figure 1c is not mentioned until the Methods sections. Figure 2 covers two sections from one ice shelf, while Figure 3 covers two ice shelves. Figure 4 is hardly used, and does one of the panels appear in Figure 3 anyway? Overall, the need to go backwards and forwards between Figures and the difficulty in working out how they relate makes for a disrupted read. For example, the Figure 1 caption tells me that coloured dots indicate the ends of the profiles in Figures 2 and 3, but beyond that I am left to work things out for myself. They are colour-coded, but that could be clearer and a description of the colour coding would have helped, and some of the dots are common to two profiles? Also indicating which end is which, especially for Figure 2 would be helpful.

We have taken the following steps to address these issues:
- The data of the old Fig. 5 now appear in Fig. 1a, which contributes to making figure calls in the text appear in order.
- Fig. 1c is now mentioned the first time early in the manuscript (Line 29), as it should have been.
- The old Fig. 3 is now divided into two figures, one for each ice shelf.
- The panel that used to appear in old Fig. 3, which repeated the panel in old Fig. 4c, is now removed to avoid repetition and simplify the figure.
- Fig. 1a no longer uses colors to mark the start and end points of transects, but instead the letters S-S`, P-P` and K-K`, which helps distinguish the transects in Fig. 1a and helps indicate which end is which in Figs 2, 3 and 4.

2) There are some confusing statements about how the calculations were made. On lines 60-70 there is a statement that observations of thickness change give a direct measurement of ice loss. I agree that thickness measurements are much preferable to elevation measurements, which must be converted to thickness through an assumption of free floatation. However, you still need to account for the thickness change that comes about through changes in ice dynamics and surface accumulation. Later on in the Methods sections (and lines 84-85) there is a discussion of these other processes, but the text on lines 60-70 (and line 44) appears to contradict this.

We now make the need to account for surface mass balance and dynamic thinning clear early in this section (Lines 65-67), including a reference to Methods where those aspects are discussed in detail, and have been expanded in the revised manuscript.

3) I didn't find the explanation of the differences seen on Smith Glacier during the later stages of the observations very convincing. The added depth of the new grounding line does give a slightly greater potential to melt, but I don't see why the water temperature should be any higher. The waters have flowed in from the same source, at least for Smith and Pope, haven't they? I also think that the concept of a deep reservoir of warm water beneath the ice shelf is flawed. If the water drives melting it must be flushed out by the associated decrease in density and be replaced by new more dense water. Furthermore, thermodynamically it cannot work. The sensible heat stored in a 100 m thick layer of water that is 4 degrees above the freezing point is sufficient to melt a layer of ice only 5 m thick. To sustain melting the cavity below the ice shelf must be flushed. It seems to me that a more likely explanation is simply that the geometry of the bed maintains the downslope retreat of the Smith Glacier grounding line. That exposes more of the glacier bed to melting, and the geometrically-driven increase in melting outweighs any reduction that might result from reduced thermal forcing, at least in the grounding zone where the observations are focussed.

We appreciated this highly informative comment by Reviewer #2, in particular the estimates of melting rates given.
We in no way were proposing that the water would stagnate in the grounding zone, and are aware of the role of circulation and flushing in sub-ice-shelf cavities (we hope that the previously published work of several of us, some of which is cited in the manuscript, demonstrates that). Given the reviewer's comment, we likely did not express this idea well, including unhelpful choice of words on our part such as "pooling". We have now modified and rearranged this section (Lines 174-185). We are reassured that Reviewer #2 seems to agree with the role of bed topography that we are proposing as part of the explanation of enhanced SG melting.

4) In the methods section there is a discussion of the uncertainty in the ice thickness measurements, but no mention of the uncertainty in the horizontal positioning of the measurements. Where there is steep basal topography, horizontal positioning errors can give large biases in what are assumed to be "repeat" thickness measurements. I am in no way questioning the validity of the results presented here. The thickness changes are so big that the majority of the signal must be real. However, I would have liked to have seen a discussion of the potential errors introduced by positional uncertainty. Also, in allowing a 200 m offset between "repeat" measurements, how much potential error does this introduce, given the maximum spatial ice thickness gradients observed?

We agree, and we certainly should have included the across-track spatial resolution. We now give the range of the across-track spatial resolutions, and the factors that could affect those values (Lines 236-237). As can be seen, the 200-m offset that we use is more than 3x smaller than the best possible across-track resolution. We impose this strict limit to minimize the effects of tracks not exactly repeating, including those of ice bottom slopes as Reviewer #2 points out. We now address this point on Lines 239-242.

More minor comments:

Line 14: The melting rates are "similar" to what? If they are the same as the melting rates from 2002-2009, how can they explain the stabilisation in the of rate mass loss?

As the original Abstract was shortened to conform to Nature Comm's guidelines, this part is no longer mentioned.
We explain the slower grounding line retreat, and slower increases in mass loss, by bed topography and glacier geometry and their interaction with ocean conditions (Discussion section).

Line 82: What do you mean by "Coriolis-favoured" side of the cavity?

We were referring to the idea that the Coriolis effect leads to higher melting on one side of the cavity relative to the other. After deliberating this point, however, we reached the conclusion that it is best to remove the mention of the Coriolis effect and to restrict ourselves to noting the observed high melting rate at that side of the cavity (Lines 86-87). While Coriolis could be a factor in concentrating melting at that side of the cavity, other factors cannot be excluded. These include the bathymetry, the velocity of circulation and the shape and dimensions of the cavity. Examining these factors we believe is beyond the scope of the current study.

Line 212: I would not describe radar sounding of ice thickness as "an entirely new approach".

True. Now corrected to ice-shelf thickness "changes" (Line 230).

Reviewer #3 (Remarks to the Author):

Review of 'Rapid submarine ice melting in the grounding zones of ice shelves in West Antarctica', Khazendar et al.

• What are the major claims of the paper? Are the claims novel?

This paper presents a new set of direct measurements of ice shelf thinning from three major glacier-shelf complexes (Smith, Pope, Kohler) in coastal West Antarctica, calculates the fraction of thinning caused by ocean-driven basal melting and relates this to other time series of glacier flow rate and grounding-line migration. The authors conclude that melt rates rose to become well above the balance rate at all three sites in the 2002-2009 period, driving strong thinning that explains the rapid grounding line retreat and glacier acceleration through the early 2000s reported elsewhere, then maintained a similar melt rate at the three sites after this, explaining a previously-observed regional slowing in the rate of glacier acceleration at this time. At Pope and Kohler, grounding line retreat largely stopped or was reversed in this later period while continuing strongly at Smith, and this contrast in response to the sustained melting is, the authors say, because Smith has a particularly deep subglacial trough that is well suited to funneling warm, dense water into contact with the ice, while at Pope and Kohler, the grounding lines retreated along shallower troughs. This interpretation fits with expectations of such systems but is novel in providing spatially extensive and directly observed melt rates in the key grounding zones.

• Will the paper be of interest to others in the field?

These direct observations are valuable to the field because, as the authors note, the only previous widespread ice shelf melting measurements are indirect (e.g. from surface lowering), which means that they have important limitations in the areas

that are of most interest to ice sheet dynamics: the grounding zones. Indirect measurements are either potentially biased (e.g. where ice shelves are incorrectly assumed to be free-floating) or are spatially limited to areas known to be free floating, away from the grounding zones. Direct measurements from these zones therefore help clarify the important relationship between ocean forcing (likely brought on by changes in regional climate) and the ice sheet dynamic response, which has global significance in terms of sea level rise. There is intense interest within glaciology in understanding the drivers of ice sheet loss in this region, in predicting how the forcing will change, and in particular how ice sheet loss will evolve (either as a stable, forced response or as an unstable collapse if forcing crosses a stability threshold).

• Will the paper influence thinking in the field?

These observations are most likely to be of value to numerical modelers seeking to tune models of ice sheet dynamics with more realistic ocean forcing to match observed dynamic behavior. They will improve rather than revolutionize this work. We can hope that they will also inspire future airborne radar campaigns to extend these observations to larger areas and over longer time periods.

• Are the claims convincing? If not, what further evidence is needed?

The core results showing ice shelf thinning around the 2002-2009 period presented in Figure 2 are clear and compelling. I suggest some minor improvements below.

The claim that the observed rapid then slower regional glacier acceleration and various grounding line migrations are explained by the temporal pattern in these melt rates is not, however, supported by the results. The issues are:
1) the authors claim that their observations support the argument for a 'large increase' in melting in the mid-2000s, but these results do not show melt rates prior to the 2002-2009 period, so no change can be demonstrated. It is not possible to tell whether melt rates increased, decreased or stayed the same.

- We now cite the findings of Paolo et al. (2015; ref. 14 in the original manuscript) to support the idea of increased melting in 2002-2009 compared with the previous years (Lines 145-147). This study quantifies average ice-shelf thickness changes over the period 1994-2012. In their Figs S1 and S3, the thinning rates of both the Crosson and PIG ice shelves, respectively, increase in the period 2003-2008 compared with the preceding years: roughly1999-2003 in the case of Crosson; and 1994-2001 in the case of PIG. These changes were detected despite that fact that, as the authors describe, their methods could not quantify thinning in the grounding zones, which our results show can be large. This could be one reason why Dotson Ice Shelf does not show as a clear change as Crosson and PIG in Paolo et al, although even in the case of Dotson there is a hint slower melting before the early 2000s in their data.

- As cited in the manuscript, several previous studies have found an acceleration of mass loss from ASE region around the mid-2000s, a time coincident with our study period, and those studies suggested changes in the ocean as the likely main culprit. It is difficult to think of other factors that could explain the well-documented, near synchronous acceleration of mass loss from nearly all of the major glaciers in the ASE other than common forcing by the ocean through increased heat flux. Our direct observations of the bottom mass loss, due to out-of-balance basal melting, that occurred in that time interval present a strong piece of evidence in support of that hypothesis. Our claim that the findings "support the hypothesis" is reasonable and restrained.

2) for the 2009-2014 period, the authors claim that melt rates remain similar to the 2002-2009 period and that this explains other observations of a slowing in the rate of acceleration of glacier flow at this time, and both the 'stabilization' of the Pope and Kohler groundling lines as well as the continued rapid retreat of the Smith grounding line.

We explain the post-2009 evolution of the glaciers, including the stabilization of the grounding lines of Pope and Kohler, by the interaction of the hypothesized oceanic changes *and* bed topography, not only the melting rates. This idea is discussed at length in the Discussion section of the manuscript.

The problem with this assertion is that it is not clear from Figure 5 that the melt rates did remain largely the same at all three sites post 2009. For example on the shelf in front of Kohler Glacier, melt rates appear at this time to vary over short distances from ~10 m thickening to ~70 m thinning per year, and on Pope, from ~10 m thickening to ~40 m thinning per year, in time periods that overlap (e.g. 2009-2014 and 2011-2014). In both cases, the results presented could in fact be interpreted to show an abrupt reduction in melt rate. Given the very rough ice shelf bases apparent in Figure 2, this is probably a sampling problem - the cross-over measurements used for the various 2009-2014 periods are point measurements and they are sparse.
Although the 2002-2009 snapshot of melt rates is compelling, the lack of data pre-2002 and the confusing picture post 2009 undermines the major conclusions about the way in which melt rates and bathymetry control grounding line migration and glacier acceleration. Perhaps the contrasts in grounding line behavior between Pope/Kohler and Smith and the regional reduction in glacier acceleration post 2009 are in fact due to variability in the melt rate after all - it's not clear from these results. The authors have a great spatial dataset of melt but a weak temporal one, and it does not seem possible to improve the existing temporal dataset.

- Actually, we do agree with Reviewer #3 that the results presented could be interpreted as a reduction of melting rates post-2009 in the cases of Pope and Kohler. We say exactly that on Line 162 of the manuscript. That they are lower would be even more supportive of the idea that grounding line retreats of these two glaciers slowed down, or were reversed to some extent, because of a combination of lower melting rates and bed topography. We refrained from emphasizing this more favorable interpretation precisely because of our appreciation of the limitation of the data post-2009 compared with 2002-2009. We believe that there are enough measurements, taken together for the three glaciers, to justify characterizing the post-2009 melting rates as similar to those of 2002-2009, as we argue next.

- We respectfully disagree with some of Reviewer #3's descriptions of the post-2009 data, as follows:

Of 11 points on Kohler, 8 have melting rates of 13-33 m/yr (Supp. Fig. 1 and Supp. Table 1), and 6 of those are between 13 and 19 m/yr. These values agree well with Kohler's 14-29 m/yr rates from 2002-2009, and do not vary widely as depicted by Reviewer #3. Point #23 shows thickening, which is expected because of grounding line advance there, while points #24 and #25 we cannot explain and are described as anomalous in the text (and both are rates estimated over a 1-year interval, so with the highest uncertainty).

In the case of Pope, the two sets of two points each do not occupy the same locations. Only points #3 and #4 are on the 2002/2009 flight trajectory and their post-2009 melting rates are 38 and 30 m/yr (Supp. Table 1), which accord with the 29-36 m/yr range of the 2002-2998 period.

In the case of Smith, it is remarkable that all the 4 most inner points in the cavity (#14, 15, 16 and 17) show the highest melting rates of 44-83 m/yr (Supp. Table 1), which again fits well with the 40-70 m/yr from 2002-2009, and reflect the higher melting taking place in the deeper parts of the cavity, as we describe in the text.

An alternative approach would be for the authors to concentrate on the strengths of the 2002-2009 data and analyze the grounding line and flow rate changes in the context of the loss of buttressing implied by the thinning shown here and also by the interferometry results presented in the 'related paper', although this would be quite a different paper.

What Reviewer #3 describes would be a logical next step to build on the results presented here. We had carried out a fairly similar investigation in ref. 46.

Other specific issues:

Line 73: Figure 2 suggests this should be -113.0 and -112.5 degrees.

Well spotted. Now stated more accurately as -113.05 and -112.50 degrees (Line 80).

Line 80: 'Another location...' - this is the same location covered by the above. (-113.1 degrees is grounded).

Modified as suggested (Lines 86-87).

Throughout results: present thinning rate (m/yr) as well as total thinning (m) in each case.

Done everywhere, except the Abstract.

Lines 90/91/97: claims for 'similarity' and 'persistence' of melt rates are stretching the results too far (see above).

We address this point above.

Line 114-116: no evidence presented for melt rates pre 2002.

We address this point above.

Lines 180 and 186: 'smallest grounding line retreat between 1996 and 2009', 'second farthest retreat by 2009' - Smith Glacier has no 2009 GL reported here (only Kohler and Pope) so these statements are not meaningful.

Indeed. We now use the years 1996 and 2014 as grounding line locations in those years exist for all three glaciers.

Throughout - perhaps 'marine' or 'oceanic' rather than 'hydrographic'?

Done; changed to oceanic throughout.

Line 211: spaceborne radar - this is not introduced anywhere or referenced, seems out of place. This would be 'entirely new' but can't be brought into paper here.

Mention of the spaceborne radar is now removed (Lines 228-229).

Methods:

Radar sounding resolution: as the authors do, it is conventional to report a radar resolution which is usually calculated as a fraction of the radar wavelength.

However in cases such as this where a single, prominent reflecting horizon is detected (such as an air-ice or ice-ocean interface), it is the ranging precision that is more relevant. Resolution applies to the minimum distinguishable distance between two similar reflectors. Ranging precision is a function partly of wavelength but also of sampling rate of the receiver and the sharpness of the interface, and may be better than the quoted resolution, as perhaps suggested by the reported {plus minus}10 m 'vertical uncertainty'. The thickness uncertainty would be the combination of ranging uncertainties for the upper and lower surfaces. While this is a better measure of the uncertainties relevant here, it is not likely to differ very much from the quoted resolution and so would not change the main results.

We agree with Reviewer #3 that, in practice, the provision of the range estimates from a single, prominent reflecting horizon can be significantly better than the typically-reported bandwidth-limited range resolution. As such, we regard the current treatment as conservative, which (as the reviewer rightly notes) still does not change our results. Therefore, we much appreciate the reviewer's confidence that our observations are likely even more robust, and keep the current conservative treatment of uncertainty in the manuscript.

Line 236: I don't think phase-sensitive radar measurements are discussed elsewhere in the text.

We have modified these sentences (Lines 260-262) to make it clearer that their purpose is to acknowledge previous efforts related to this work either in the observation method applied (airborne sounding radar in Greenland), or what was being measured (bottom ice loss in Antarctica measured, in situ, with phase-sensitive radar).

Line 269: the thinning of slow-flowing areas used to calculate the surface-processes component of the total ice shelf thinning is apparently measured at considerably greater altitude than the shelf surface (on Mount Murphy and in the Kohler Range of mountains). It is unreasonable to assume that this is representative of compaction/accumulation/ablation rates on the ice shelf surface because compaction and ablation are strongly temperature dependent and accumulation is strongly slope and altitude dependent. It would be better to identify low-lying slow-flowing areas or failing that, use climate- and compaction-model output. The difference from the quoted 3-5 m is, however, unlikely to be large enough to change significantly the basal-melt results (up to several hundred meters thinning).

We agree. The hindrance was that the flight paths did not cross any grounded, slow flowing areas at altitudes similar to those of the grounding zones. We took the following steps:

- We have created the new Supplementary Fig. 2 that shows surface elevation change but also the surface elevation along the flight path.

- We now comment in the main text on the difference of elevation between the grounded areas considered and the grounding zones and possible effects on surface processes (Lines 125-129).
- We now cite new ref. 49 which shows that in the study area, surface elevation in the slow flowing parts generally exhibited no surface elevation change or thickened between 2003 and 2007 (Lines 129-130).
- We conclude from the above that the contribution of surface processes to the thinning we observe, if anything, is even smaller than the 3-5 m.

Figure 2b: show the 2010 intersections (given in Figure 5) as well as the 2004 ones.
Figure 2c: show the 2002 and 2009 intersections from downstream of the 1996 GL (given in Figure 1) as well as the upstream ones.
Figure 3b: show the 2014 intersections (given in Figure 5).
Figure 3d: show the 2010 and 2014 intersections (given in Figure 5).

All implemented. We re-plotted the figures to introduce the intersection points as can be seen in Figs 2b, 2c, 3b and 4b, respectively.

Show the 2002-2009 cross profiles for Pope and Kohler as well as the long profiles given in Fig. 3.

The cross profile for Pope is actually already shown to the east of Smith in Fig. 2b. We have now labeled it, but with smaller font so as not to detract attention from Smith, the main subject of this figure. We did not include the cross profile for Kohler because the 2002 and 2009 deviated from each other along that transect so there were not much data to show, as can be seen in the figure below.

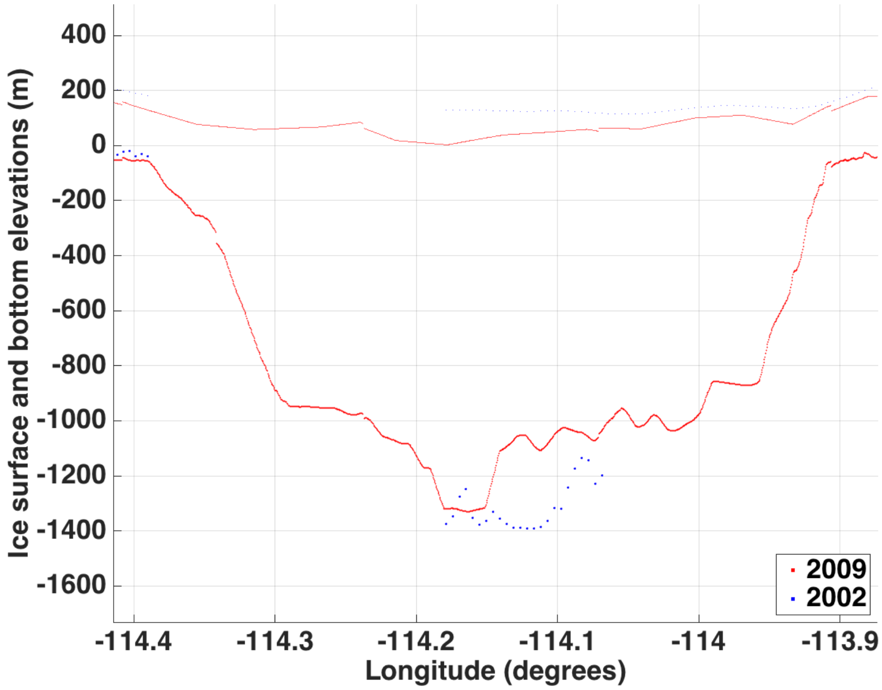

Figure 4: Label the x-axis, align the y-axes for parts a, b and c.

Done (Fig. 5).

Figure 5: show the 2009 GL given in Figure 3. Differentiate the colour bar between values that are positive and negative, report the elevation rates somewhere (e.g. supplementary table or on the figure) as it's difficult to read off from the colour bar.

Done:
- The color bar has been modified to make it easier to distinguish between positive and negative values (Fig. a1).
- We have now created a Supplementary Table 1 and a Supplementary figure 1 to be included in a Supplementary Information document (attached) to report the bottom elevation change rates. In the supplementary figure, a simplified version of Fig. 1a, each location is given a number, and the table provides the rate at each location.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

I am very happy with the way that the authors have responded to my queries, comments and concerns, and as a result, think that this manuscript is just about ready. All of my concerns have now been addressed to my satisfaction, and I think that the manuscript will make a very good and interesting addition to the literature. I have two minor issues to raise (see below) but beyond that, I congratulate the authors on their work.

1) I did, however, spot one typo - line 254, I think it should be 'average' rather than 'averages'.

2) Figure 1 - I still think that there needs to be a better link between the colour-scale on the right-hand side and the fact that this is relevant for the circles, diamonds, squares and triangles. I'm also still not over-keen on the fact that colours used to mark the grounding line also appear on the colour-scale, even though these are not linked.

Reviewer #2 (Remarks to the Author):

In this revision the authors have responded well to the major criticisms of the earlier reviews, but some more minor problems remain. I think the general reorganisation of the figures has greatly improved the overall clarity and the general flow of the presentation is now much easier to follow. However, that seems to have highlighted the more minor problems, which only became clear to me through the more fluid reading that the improvements allow.

Taking the points in order:

1) Line 34 and numerous places elsewhere:
The use of the term "sounding radar" (using "sounding" as a participle/adjective?) is rather unusual and reads awkwardly to me. I think in every case "radar sounding" (using "sounding" as a gerund) would be preferable.

2) Line 51 "Melting rates ... are similar":
I'm sorry, I still don't follow what you mean by this. If the observations "agree" with "slower increases in mass loss", shouldn't the melt rates be lower? Or are you trying to say that melting rose rapidly, then plateaued? But, I don't see how you can say that. During the 2002-2009 period you only have the integrated melt, don't you? So you cannot infer a time history?

3) Line 54-55 "... warmer waters flooding the greater depths ..."
Is this short-hand for saying that waters of the same temperature provide a greater source of sensible heat when they are at greater depth because the freezing point is lower? I agree that it's hard to say this in a few words, but as it stands the sentence implies the temperature of the water will rise simply because it flows to greater depth (it will slightly because of compression, but that is a small effect).

4) Line 63 and numerous place elsewhere:
The terms "ice-shelf bottom measurements", "bottom ice loss", "bottom ice ablation" are really confusing. I took the latter two to mean basal melting, which is exactly what the last one says (how can bottom ablation mean anything else than mass loss across the bottom interface?). But that isn't what is meant at all. Actually "ice thickness measurements" and "ice thinning" would be a much clearer and more accurate descriptions of what is meant (I think).

5) Line 70 "... or knowledge of ice flow speeds and surface mass balance as is the case ..."
This unbalanced comparison adds to the confusion. Measurements of surface elevation change

require an assumption of hydrostatic (or arguably more correctly isostatic) equilibrium in order to derive ice thickness change. Direct measurements of thickness clearly do not require any assumptions. But to derive bottom melting rates from ice thickness change requires knowledge of ice flow and surface mass balance in both cases.

6) Line 77 and throughout this section:
It is really important to talk about "ice thinning" rather than "bottom ice loss" here. When I read this section I took "bottom ice loss" to mean "basal melting" and was surprised when the following section went on to discuss contributions from surface processes and dynamic thinning, both of which I assumed had already been dismissed as negligible. The last paragraph of this "bottom ice loss" section seemed to confirm my misunderstanding because it talks about "out-of-balance melting". Maybe this should come later? Also didn't reference 4 calculate total melt (i.e. the combination of steady state and "out-of-balance" melting)?

7) Line 79-80 and numerous places elsewhere:
I think it would be much clearer to state longitudes as E/W rather than +/-. The authors seem to have confused themselves, referring to "latitudes" on this occurrence. But if you use E/W there is no need to state whether you are referring to longitude or latitude.

8) Line 89-90 "Roughly 10-15 % ..."
Doesn't this belong in the next section?

9) Line 142 "... bottom melting observed ..."
Finally we get to "basal melting", but actually it has never formally been quantified. All the quantitative discussion was about rates of ice thinning. Maybe shifting the sentence mentioned in 8) to the end of the section on "Contributions of surface processes and dynamic thinning" would set the reader up for a discussion of basal melting?

10) Line 162 "... similar to, or lower than ..."
Does this mean the same as "similar" in 2) above?

11) Lines 166-171:
This is a new idea, introduced and passed over in two sentences. For an inter-disciplinary journal such as Nature Communications that is much too quick. Either drop it entirely, if you don't have room, or expand it, if you have.

12) Lines 179:
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I still think "an entirely new approach" for monitoring ice thickness change is an exaggerated claim. Direct measurement of ice thickness by radar sounding is a decades-old technique and for much of that time has been much more widely used than measurements of surface elevation and an assumption about floatation. Use of the technique to measure ice thickness change is uncommon, but that's only because the change needs to be so large (unless phase-sensitive radar is used).

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Reviewer #3 (Remarks to the Author):

Following the initial review of this manuscript, the authors have modified and improved the text and figures presented and I am satisfied that they have responded to my queries and criticisms.

Reviewer #1 (Remarks to the Author):

I am very happy with the way that the authors have responded to my queries, comments and concerns, and as a result, think that this manuscript is just about ready. All of my concerns have now been addressed to my satisfaction, and I think that the manuscript will make a very good and interesting addition to the literature. I have two minor issues to raise (see below) but beyond that, I congratulate the authors on their work.

1) I did, however, spot one typo - line 254, I think it should be 'average' rather than 'averages'.

Corrected (L. 262).

2) Figure 1 - I still think that there needs to be a better link between the colour-scale on the right-hand side and the fact that this is relevant for the circles, diamonds, squares and triangles. I'm also still not over-keen on the fact that colours used to mark the grounding line also appear on the colour-scale, even though these are not linked.

We have now changed the colors of the grounding lines to make them as distinct as possible from those of the color scale, while still reasonably visible. As a result, we have modified Figs 1a, 1c and Supplementary Fig. 1.

Reviewer #2 (Remarks to the Author):

In this revision the authors have responded well to the major criticisms of the earlier reviews, but some more minor problems remain. I think the general reorganisation of the figures has greatly improved the overall clarity and the general flow of the presentation is now much easier to follow. However, that seems

to have highlighted the more minor problems, which only became clear to me through the more fluid reading that the improvements allow.

Taking the points in order:

1) Line 34 and numerous places elsewhere:
The use of the term "sounding radar" (using "sounding" as a participle/adjective?) is rather unusual and reads awkwardly to me. I think in every case "radar sounding" (using "sounding" as a gerund) would be preferable.

Agreed, and changed throughout the text as suggested.

2) Line 51 "Melting rates ... are similar":
I'm sorry, I still don't follow what you mean by this. If the observations "agree" with "slower increases in mass loss", shouldn't the melt rates be lower? Or are you trying to say that melting rose rapidly, then plateaued? But, I don't see how you can say that. During the 2002-2009 period you only have the integrated melt, don't you? So you cannot infer a time history?

We have now added the word "averaged" because it is indeed the averaged melting rates that we are comparing from the two time periods. We also changed "agrees with" to "is consistent with", which makes more clear that this is a brief statement involving other aspects (L. 51-52). Those aspects are discussed in detail in the main text (Lines 162-172).

3) Line 54-55 "... warmer waters flooding the greater depths ..."
Is this short-hand for saying that waters of the same temperature provide a greater source of sensible heat when they are at greater depth because the freezing point is lower? I agree that it's hard to say this in a few words, but as it stands the sentence implies the temperature of the water will rise simply because it flows to greater depth (it will slightly because of compression, but that is a small effect).

We have now rewritten this sentence at more length to ensure clarity (Lines 54-57).

4) Line 63 and numerous place elsewhere:
The terms "ice-shelf bottom measurements", "bottom ice loss", "bottom ice ablation" are really confusing. I took the latter two to mean basal melting, which is exactly what the last one says (how can bottom ablation mean anything else than mass loss across the bottom interface?). But that isn't what is meant at all. Actually "ice thickness measurements" and "ice thinning" would be a much clearer and more accurate descriptions of what is meant (I think).

We agree and apologize for the confusion. We have taken the following steps to address the issue of the terminology we employ:

- We moved the section “Contributions of surface processes and dynamic thinning” so it is now before the section “Rapid bottom ice loss in the grounding zones” in order to explain the terminology and pave the way for the subsequent material.
- We now explain at the end of the section “Contributions of surface processes and dynamic thinning” why we use “bottom ice loss” and “out-of-balance melting” interchangeably, as we should have done.
- We added two lines at the beginning of the section “Contributions of surface processes and dynamic thinning” to define total ice thinning.
- We removed the two instances in the manuscript of the use of “bottom ice ablation” (L. 67 and 203).

5) Line 70 "... or knowledge of ice flow speeds and surface mass balance as is the case ..."
This unbalanced comparison adds to the confusion. Measurements of surface elevation change require an assumption of hydrostatic (or arguably more correctly isostatic) equilibrium in order to derive ice thickness change. Direct measurements of thickness clearly do not require any assumptions. But to derive bottom melting rates from ice thickness change requires knowledge of ice flow and surface mass balance in both cases.

We generally agree with this pertinent remark, and have now modified those sentences to reflect that (L. 71-76). We still believe that the indirect methods usually applied do require more assumptions and approximations than what is needed here to infer melting rates from direct thickness change observations, but we are content to focus on the assumption of hydrostatic equilibrium and its importance, which has been the main point of this paragraph to begin with.

6) Line 77 and throughout this section:
It is really important to talk about "ice thinning" rather than "bottom ice loss" here. When I read this section I took "bottom ice loss" to mean "basal melting" and was surprised when the following section went on to discuss contributions from surface processes and dynamic thinning, both of which I assumed had already been dismissed as negligible. The last paragraph of this "bottom ice loss" section seemed to confirm my misunderstanding because it talks about "out-of-balance melting". Maybe this should come later? Also didn't reference 4 calculate total melt (i.e. the combination of steady state and "out-of-balance" melting)?

We changed “bottom ice loss” to “thinning” or “ice-shelf thinning” in two instances in this section where we thought it was more appropriate (L. 115 and 139). We retained “bottom ice loss” in the other instances as explained in point (4) above.
Ref. 4 calculated both the total melting and the steady-state melting. The latter is discussed in detail in the Supplementary Material of that paper (Section 10 of Ref. 4),

including a figure that shows ice-shelf steady-state melting rates around Antarctica (Fig. S4 of Ref. 4).

7) Line 79-80 and numerous places elsewhere:
I think it would be much clearer to state longitudes as E/W rather than +/-. The authors seem to have confused themselves, referring to "latitudes" on this occurrence. But if you use E/W there is no need to state whether you are referring to longitude or latitude.

Well spotted. Corrected to “longitudes” (L. 109).
We will be happy to change from +/- to E/W if Nature Comms style requires that, in case of acceptance. We do have a preference for the +/- system for its clarity and economy of expression (no need for letters), and it is wide usage.

8) Line 89-90 "Roughly 10-15 % ..."
Doesn't this belong in the next section?

Indeed. Corrected. Now the section “Contributions of surface processes and dynamic thinning" precedes this section, as explained in point (4) above.

9) Line 142 "... bottom melting observed ..."
Finally we get to "basal melting", but actually it has never formally been quantified. All the quantitative discussion was about rates of ice thinning. Maybe shifting the sentence mentioned in 8) to the end of the section on "Contributions of surface processes and dynamic thinning" would set the reader up for a discussion of basal melting?

Implemented as recommended. Now the expanded section “Contributions of surface processes and dynamic thinning" precedes this section, as explained in point (4) above.

10) Line 162 "... similar to, or lower than ..."
Does this mean the same as "similar" in 2) above?

Line 51 Has now been modified to mention “similar or lower”.

11) Lines 166-171:
This is a new idea, introduced and passed over in two sentences. For an inter-disciplinary journal such as Nature Communications that is much too quick. Either drop it entirely, if you don't have room, or expand it, if you have.

We have now expanded this paragraph, as suggested (L. 178-180).

12) Lines 179:
I still don't see how "strong topographic confinement" can be favourable to promoting the access of CDW to the ice shelf base. Don't you need the cavity to be open to flush it efficiently?

We agree that this could be ambiguous. We have now modified the sentence for more clarity (L. 187-188).

13) Lines 182-185:
This seems very misleading. I think it implies that since the temperature of the water on the shelf is warmer at 1000 m than that at 500 m, it must be even warmer down at 2000 m. That simply is not true. The water in the cavities, however deep they are, cannot be any warmer than the warmest water found outside the cavities. There is no heat source in the cavities.

We respectfully disagree with Reviewer #2 on this one: Our statement in no way implies that.

13) Lines 191-198:
The arguments are really confusing here and the misuse of the term "ablation" does not help. I think what you are trying to say is that as ice goes afloat it must experience a dramatic increase in basal melting, so will thin rapidly as a result.

Modified as suggested (L. 200-204), and the term "ablation" was dropped.

14) Line 199-201:
I still cannot see why the size of a "reservoir" of warm water has any relevance. A reservoir of any size will cool unless it is flushed. The efficiency of the exchange determines how rapid the melting will be, not the size of the reservoir.

Agreed that the word "reservoir' could be misinterpreted. We have now removed it and rewrote the sentence to convey the idea more clearly (L. 207-209).

15) Line 210:
There is an opening bracket "(", but no closing one. Where is it supposed to be?

Corrected (L. 218-219).

16) Lines 225-227:

Again this is misleading. In the grounding zone, the possible lack of free floatation affects the accuracy of ice thickness changes deduced from surface elevation changes. This of course affects the accuracy of the basal melting estimates based on those estimates of ice thinning. However, the uncertainty in the other components that go into the calculation of melting are identical, irrespective of whether the thickness change is measure directly or inferred from elevation change.

But everything we mention in those lines (233-235) has to do with whether the assumption of free floatation is valid or not and its effect on the accuracy of derived bottom melting rates, something with which Reviewer #2 clearly agrees. While we promptly agreed with the reviewer's argument in point (5) above, we sincerely cannot see why the objection here.

17) Line 229:
I still think "an entirely new approach" for monitoring ice thickness change is an exaggerated claim. Direct measurement of ice thickness by radar sounding is a decades-old technique and for much of that time has been much more widely used than measurements of surface elevation and an assumption about floatation. Use of the technique to measure ice thickness change is uncommon, but that's only because the change needs to be so large (unless phase-sensitive radar is used).

We agree with this comment and believe that what we are saying is not much different from Reviewer #2's arguments. We removed the word "entirely" in order to approach Reviewer #2's position even more (L. 237-239). In the sentence immediately preceding the one where we mention the new approach, we do make the point that we could detect bottom ice loss with radar sounding because it is so large. In the remainder of the sentence in question we describe what makes this approach new: the use of radar sounding on regional or continental scales to monitor such ice loss; it is not the use of radar sounding to detect ice thicknesses, or their changes, which has been done before (but on a more limited spatial scale) as Reviewer #2 points out and as we describe in the manuscript, with references.

Captions to Figures 2, 3 and 4:
You use two different definitions of the transect for panel (a) and (b) in each case. I think the transects are the same across the two panels, but the wording makes it seem otherwise.

Indeed—corrected.

Figure 1 (and in Supplementary Information):
Now we have yet another term introduced. Is "bottom ice elevation change" actually what you mean by "bottom ice loss"? That is a concern, because that is not the same as "ice thickness change". But the latter is what you need if you want to

determine basal melting (unless all your numbers should be understood as equivalent thicknesses of seawater?). This emphasises the point that more precise and clearer terminology needs to be used throughout to avoid any ambiguity or confusion.

In these cases, we thought that the term “bottom ice elevation change” is suitable as there are a few locations where ice thickness has actually increased, as can be seen in the figure, as well as in the Supplementary Information.

Reviewer #3 (Remarks to the Author):

Following the initial review of this manuscript, the authors have modified and improved the text and figures presented and I am satisfied that they have responded to my queries and criticisms.

End of letter.

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have done an excellent job at responding to the second round of comments, and the paper is much clearer as a result. I would recommend publication as is, although I did notice a couple of very minor, editorial points:

Line 208: "… their grounding zones did not develop grounding zones that are as susceptible …" could be improved. Maybe "… their grounding zones were not as susceptible …"?

Figure 4 caption: The last sentence "Inset shows … grounding zone" is now obsolete and should be deleted.

Figure 5 caption: Should include the information removed above, i.e. these are 2009 MCORDS data. Also would it be possible to use the same horizontal scale (distance, with zero at the 1996/2014 grounding line) to make it clearer that the profiles in figures 4 and 5 are the same? Failing that make it clear in the caption.

Supplementary Figure 1: Is this necessary? Isn't the information identical to that shown in Figure 1?

Supplementary Figure 2: Make it clear that this is a repeat of Figure 2, just extended at either ends Maybe mark on the points S and S'?

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Caption modified as suggested.

Supplementary Figure 1: Is this necessary? Isn't the information identical to that shown in Figure 1?

Supplementary Figure 2 (which was Supp. Fig. 1) differs from Fig. 1a in that the points are numbered in order to locate their information in Supplementary Table 1. Including those numbers in Fig. 1a would clutter it greatly. We also believe that having both the table and the related figure in the same document makes it easier to use the information.

Supplementary Figure 2: Make it clear that this is a repeat of Figure 2, just extended at either ends Maybe mark on the points S and S'?

Done. The letters S-S' are now shown in Supplementary Fig. 1 (the old Supp. Fig. 2) and described in the caption.

Note about further minor modifications to the Supplementary Information section:

The order of the supplementary figures was reversed in response to an editorial comment.

In the caption of Supp. Fig. 1 (according to the new figure numbering), the word top/bottom "panel" was replaced with top/bottom "plot" to emphasize viewing this figure as one.

In Supp. Table 1, the presentation of the x and y coordinates was modified to be the same as that of the map in Supp. Fig. 2 to which the table refers. Hence, the coordinates are presented without the use of exponential notation.

End of letter.