Response to Reviewers: tc-2020-25 by Kelly Hogan et al. "Revealing the former bed of Thwaites Glacier using sea-floor bathymetry"

The comments of the two reviewers and the short comment are listed below with our responses below *in blue italics*.

Comments in tc-2020-25-SC1-supplement:

Lines 185-218: I appreciate the thorough description of landforms here, but often found it hard to link the text to the figures (and differentiate landforms) the way the authors did. I understand that looking at these things is interpretive, and at times as much an art as a science, but I think it would be helpful if the authors put forth an image for a type example for each of the described features. Crag-and-tails, glacial lineations, grooves, gulleys, channels, troughs, grounding-zone wedges, slide scars, crescentic scours; linking them to the figures is often very challenging, and figuring why one elongate feature is called a groove and one a lineation in these data is often difficult to do. When I was faced with my own data from swath radar under Thwaites, trying to differentiate features, I really wished I had a clearer articulation in the literature of how others defined features in their data from morphology alone.
We appreciate that the interpretation of glacial landforms (here submarine) is not in every reader's skillset and we want this paper to be accessible to a wide readership. As such, we have made a new Supplementary Figure (new Fig. S1) to give type examples of the main glacial landforms discussed in the text and noted in this short comment. We have used examples from the Amundsen Sea Embayment, and from this dataset wherever possible, to make them as relevant as possible for this paper.
Beyond this, we strongly recommend that readers interested in glacial landforms consult the "Atlas of Submarine Glacial Landforms: Modern, Quaternary and Ancient", Geol. Soc. Memoir 46, edited by J.A. Dowdeswell et al. which has numerous type examples of glacial landforms from the marine realm.

Lines 195-197: These moats are gorgeous, and really interesting to think about. In the reviews of our swath radar paper at Thwaites (Holschuh et al., 2020), we were challenged on the interpretation that they must be carved by water, and have spent a fair amount of time since thinking about that problem. There are reasons to believe that ice might be the primary actor here. Is there a reason you only mention meltwater and till slurries, when Graham and Hogan list "meltwater erosion, erosion by a saturated till slurry, or the direct action of mobile basal ice"?
Agreed! This was an oversight on our part and we have now added mobile basal ice to the text. Unfortunately, we do not have room to go into a detailed discussion of how these features form, however, we think about this often too and are interested in doing more on this topic. One question that we come back to is why these features are observed in some areas and not others, and particularly in areas where you might expect less basal melt? We have also noted differences in moat morphology (e.g. the absence of a downstream obstacle) that are hard to explain. These are definitely topics for future research.

Lines 477-480: What specifically indicates that flat-topped surfaces are erosional? Given that tablelands have been described in many places under Antarctica (as far back as Drewry

1975), I think more evidence might be required to call them planed-off. In general, features that act as ice rises are thought to have been areas of uniquely low erosion rates (Matsuoka et al., 2015). The fact that they interrupt deep glacial troughs seem to imply that those features are in fact more resistant than their surroundings. I would just like more (or clearer) evidence before arguing there is some new, unique positive feedback here, distinct from existing discussion of erosion / ice-flow feedbacks (e.g., Kessler et al, 2008).
*We have addressed a similar comment about the origin of the flat tops from R1, and discounted a the flat tops as hard sedimentary surfaces based on seismic-reflection data for the inner Amundsen Sea shelf. We have also added new text to both the descriptions of this morphology and its interpretation in Section 3.3 and its implications for ice dynamics in Section 6.2 to explain that our suggestion is not of a large amount of erosion occurring rather that some glacial sediment was deposited on the highs when they were at the GZ and then the motion of TGT over them may have promoted slope failures and skimmed sediment from their tops. We also discuss that for the duration of observations (55 years) the TGT has moved quickly over the area of sea floor highs (i.e. THwaites ice shelf has not formed an ice rise) acknowledging that these sites are considered to have low erosion rates. Hopefully, our expanded discussion of build up of sediment at the GZ, and then its potential for failure/erosion but the subsequent ice shelf is clearer than our original text. This is certainly distinct from erosion/ice-flow feedbacks of grounded-ice flow with arguably higher erosion rates in areas of thicker ice (e.g. Kessler et al., 2008) as we are discussing the motion of an ice shelf over relatively recent GZ sediments. However, we also state clearly that we do not know if this mechanism has occurred at THwaites, only that it is **possible** (as R1 also states!).*

Lines 493-495: This was a problem we were having comparing swath radar data with the terrestrial record – sediment in-fill of crescentic features was making it hard to evaluate their true depths in the paleo record. Definitely interesting to see the same challenges here!
Agreed! The crescentic scours are arguably a bigger problem than the troughs, seismic-reflection or acoustic methods can at least tell us whether there is sediment infill in the troughs but the small-scale of the crescentic scours means that they are not well imaged by our shipborne methods. Coring attempts in a moat around a drumlin in Marguerite Trough (see Kilfeather et al. 2011; GSA Bulletin 123) did not really hit the sediment in the moat; AUV studies over these features (including sub-bottom profiling, maybe as part of ITGC-TARSAN…) would definitely help see what is in them!

Line 509: Again, it seems unlikely that (after all of Antarctica's growth and retreat cycles) we might catch a very transient pinning point now. Doesn't it seem more parsimonious that there is no such thing as a particularly weak pinning point? Either that, or the authors should expand on the idea that erosion of pinning points requires ungrounding (maybe higher velocities in ice shelves/ice rises, as opposed to fully grounded ice are required to erode the underlying pinning point, or slump events are a required precursor, and so this feedback is unique to ice rises as opposed to the general erosion/ice flow feedback already described in the literature).
We are not sure that we agree with this comment. In response to comments from R1 we have added text to expand on the idea of an erodible pinning point in this setting and we think that this may help address this comment also. At Thwaites, we know that the GZ was on/near the highs probably for a long time flushing all that sediment down between the highs, into the fans, when it is possible that rather a lot of glacigenic sediment built up at the

GZ on the highs (we have a GZW there). At some point, the GZ retreated and there was a transition to an ice shelf, would this not keep bulldozing sediment from the tops of the highs esp if the ice shelf accelerated? Wouldn't the GZ sediments provide a perfect chance to make a soft pinning point? Failures on the highs, which could have occurred as the GZ retreated off these features, would only promote instability of grounding on the high as material would be moved downslope rapidly in discrete events. Hopefully, our new text clarifies that it is the sequence of events at Thwaites (GZ on the highs, build up of sediment, retreat of GZ, rapid flow of Thwaites Ice Shelf over the high) that opens up the possibility of this mechanism occurring. Thus, we now highlight that higher ice shelf velocities would be required and that at Thwaites we have no evidence of this in the observational record (e.g. TGT has continued to move rapidly over the highs for the past 55 years). We also stress that this is only a possibility, not that it definitely happened. Of interest may be that we acquired new seismic reflection profiles from Pine Island Bay this year that indicates variable composition of seafloor highs in the area.

Line 532-533, 537-539, and 544-545: Without seismic data or rock cores, I do not think you have the data required to validate Muto et al.'s work (although I do think it has interesting implications for your data set). Muto was looking at features within a region of the Thwaites bed that, if interpreted morphologically, would have been assumed to be uniformly hard bedded. Below, you can see a figure from our swath radar paper (Holschuh et al., 2020), that shows that the bed looks like in the vicinity of Muto et al.'s seismic line:
You can see the upstream region, characterized by crag-and-tails and MSGL, is uniformly weak in the seismic data. It is in the downstream half of the Thwaites grid that is described in detail by the authors, showing that (in a region that might be interpreted as uniformly hard by the morphology alone), the lee and stoss sides of bed features show variable bed properties. I think the only way to actually validate the Muto et al. study is to look in more places with coincident high-resolution morphological data and acoustic property or rock property measurements, it is not possible to validate or contradict their results with morphological data alone.
The reviewer is right that we are not looking explicitly at an area thought to be uniformly hard bedded as in Muto et al (2019 a, b). However, when we consider the length scales of our crag and tails on H3 in particular (<5 km) they are more in line with the "hard bedded terrain" of Muto et al. (2019) than the large crag and tails further upstream (>12km) and we think the comparison is still reasonable. However, we acknowledge that without coring/seismic profiles we cannot validate Muto's work and we have changed the emphasis of our wording so as not to say that we are validating Muto et al.'s work but that our results are consistent with their findings. Thus, we replace ""We note that these features exhibit the same correlation …" put "We interpret morphological characteristics of these features as being consistent with the correlation of morphology with bed type that has been described from on-ice seismic reflection profiles for TG (Muto et al., 2019a, b), although we recognize that high-resolution seismic reflection data would be required to confirm this". We also slightly change the emphasis of this section to point out the similarity between the crag and tails in Holschuh et al (2020) and under the Rutford Ice Stream as the most comparable terrains to our data.

Line 563-564: I have always been jealous of how nice multibeam data look – you are right that conventional radar sounding and seismic sounding can't compare. But swath radar data are finally giving sea-floor observations competition! I know you mention the substance of the Holschuh et al., 2020 paper below, but some of the predecessors deserve mention here. (Paden et al., 2010; Jezek et al., 2011).

We have added these references in Section 6.2 when introducing swath radar (but before talking about swath radar at Thwaites where we refer to the Holschuh paper.

Line 566-567: Again, I'm not sure you have the data required to do more than assume variability in bed type.

We take this on board (see above comment to 532-533) and have changed the wording from "These analyses … and allow us to constrain the spatial variability of bed types" to "These analyses … and, in combination with high-resolution seismic data and ground-truth from sediment cores, have the potential to constrain the spatial variability of bed types".

Line 591-594: I worry that there is something that I missed– do you have direct observation of substrate type from acoustics or coring? If so, that needs to be described in more detail, because I really think Muto et al., 2019 cannot be validated without them.

In the case of the MSGL terrain analysed we do have direct observations of substrate type from sub-bottom profiles. The profiles show that the MSGL are sedimentary are included as a supplementary figure in Larter et al. (2009) and, additionally, high-resolution seismic profiles over the area are included in Graham et al. (2009).

Lingering Questions:

Because we are interested in moats generally, we noticed a commonality between your data and our swath radar data at Thwaites. Moats on the leading edge of bedforms often meet exactly at the center of a downstream mot, at the head of a new bedform. I find this to be a really curious pattern – any thoughts on why this might be the case?

This is definitely an interesting/exciting observation! Could one possibility be that whatever process/material that causes the erosion of the moat (ice/water/slurry), which then extends around the sides of the obstacle (as the moat continues there) would be the focus for erosion should another obstacle on the bed be met? We have started work to assess the morphological variation in crescentic scours (and their "obstacles") which may shed more light on how common this pattern really is. I have not, however, noticed this exact pattern before… We would certainly be very happy to continue this discussion offline and compare notes on the moats!

As one last note – due to the highlands you've pointed out (H1/H2/H3), the main trough and pathway for CDW to route in toward the ice-sheet terminus is actually to the true west of the modern Thwaites shelf. Do you have any thoughts on what implications that has for Thwaites retreat? It looks as though there are available high-spots for shelf regrounding to the west, but perhaps the Thwaites tongue was never resilient enough, given its closer proximity to this CDW pathway? I think some more discussion of the oceanographic implications of these data could be a really useful addition.

This is a really interesting point that I think will be discussed in detail in both future THOR-ITGC papers that look at the retreat history for Thwaites and possibly by the oceanographers looking at circulation here. It may be that as ice retreated across the shelf that the large Thwaites Trough "funnelled" warmer water to access at Thwaites GZ promoting retreat *there. The chronological constraints on retreat that will come out from the THOR project should illuminate when this occurred and whether it was coincident with retreat at PIG. Pine Island Bay is a wide and generally deep embayment that may have simply been flooded with warmer water during retreat. In the modern setting, a paper with our oceanographic colleagues shows CDW accessing the Thwaites cavity from east of the EIS so it may be that this branch of inflow became more important over time, as ice stepped back. We look forward to tackling these questions in future papers that use this bathymetry as their backdrop!*

References:

Graham, A. G. C., Larter, R. D., Gohl, K., Hillenbrand, C.-D., Smith, J. A., and Kuhn, G.: Bedform signature of a West Antarctic palaeo-ice stream reveals a multi-temporal record of flow and substrate control, Quaternary Science Reviews, 28, 2774-2793, 2009.

Kilfeather, A. A., et al. (2011): Ice-stream retreat and ice-shelf history in Marguerite Trough, Antarctic Peninsula: Sedimentological and foraminiferal signatures. *Geological Society of America Bulletin* 2011;123;997-1015. doi: 10.1130/B30282.1.

Larter, R. D., Graham, A. G. C., Gohl, K., Kuhn, G., Hillenbrand, C.-D., Smith, J. A., Deen, T. J., Livermore, R. A., and Schenke, H.-W.: Subglacial bedforms reveal complex basal regime in a zone of paleo–ice stream convergence, Amundsen Sea embayment, West Antarctica, Geology, 37, 411-414, 2009.