Response to Reviewer 1: tc-2020-25 by Kelly Hogan et al. "Revealing the former bed of Thwaites Glacier using sea-floor bathymetry"

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

tc-2020-25-RC1: Matteo Spagnolo

General comments:

1. The main conclusions that emerge from the discussion, and in particular the idea of soft and hard bedrock highs and their implication to ice dynamics, are very important and yet there is little mention to them in the abstract and no mention at all in the title. I would therefore recommend incorporating this and other highlighted points in the abstract and consider an alternative, more result-focused, title.

We have rewritten the Abstract and Title to highlight the main conclusions of the work rather than list the work done. This also addresses comment for Line 429 to include numbers on cross-sectional areas in the Abstract (we have now included these numbers) and for Line 507 asking for the variable composition of pinning points to be alluded to in the title.

2. Evidence of glacial erosion on the flat-topped highs does not necessarily implies that the flat top morphology is due to erosion. It could be simply related to the presence of harder horizontal layers in a sedimentary rock. Nonetheless, the role of a more or less thick blanket of glacial sediment that could be eroded and its importance for a potential feedback mechanism remains valid.

We have taken this comment about erosion by an ice shelf on board, also noted in the short comment, and made the following additions to the text to consider the various possibilities for the flat-topped morphology of the highs. We have toned down and further explained our interpretation of the flat tops; we have also added a short paragraph to "Section 3.3: Bathymetric highs and ridges" to explore the alternate explanations namely, that the morphology is inherited from previous subglacial erosion or that rugged bedrock features were mantled by some amount of glacigenic sediment that levelled this topography. The paragraph reads:
"We note, however, that alternative explanations are possible for this morphology, namely that the flat tops are an inherited feature produced by erosion down to horizontal bedrock strata or that rugged bedrock highs, which are typical of the inner Amundsen Sea shelf (cf. Nitsche et al., 2013), were mantled by some thickness of glacigenic material that levelled the topography below. The former is relatively easy to discount accepting that the inner shelf of the ASE is composed of crystallite basement with seismic-reflection profiles showing that northward-dipping sedimentary strata only occur on the middle and outer shelf (cf. Graham et al., 2009; Gohl et al., 2013). In this setting close to the current TG grounding zone, it is perhaps easier to conceive of the latter explanation that rugged bedrock features were mantled by glacigenic material delivered to the area when the grounding zone was located on or near the highs, and then flattened by some degree of glacial compaction and/or erosion as it was overtopped by TG and the subsequent Thwaites Ice Shelf. This is consistent with our suggestion for the formation of these flat tops as we cannot tell from our data either what sediment thickness occurs on the highs or how much erosion by took place, and we acknowledge that amount of ice-shelf erosion may have been low only skimming

unconsolidated material from the surface of the highs. The presence of GZW and glacial lineations on the highs, and sub-bottom profiler data (Fig. S4), confirms that at least some thickness of unconsolidated material occurs on the highs but seismic-reflection profiles would be required to fully capture the internal structure of these features."

Despite the comments, we are happy to agree with R1 that the potential feedback mechanism or erosion from the tops of the highs remains valid! We have added some clarifying text to Section 6.2"Implications from sea-floor morphology" to make the feedback mechanism clearer to the reader, naming the erodible highs as "soft" and the crystalline/bedrock highs as "hard" (the latter is the same as the original text) and we have explained the relative timing of this feedback mechanism with the text below. We also now referred to the concept of soft/hard pinning points in the Abstract.

3. Generally speaking, there are a number of sentences that area bit vague, and I have highlighted places where the authors should make an effort, if possible, to quantify mentioned differences, similarities, significant implications for, etc. This is especially important in terms of CDW. How much the refined topography of this new bathymetry redraws the estimates of CDW incursion towards Thwaites grounding zone?

We have addressed individual sentences in the specific comments listed below. Regarding the significance for CDW inflow, although we appreciate this comment, and agree that it would be great to assess to significance of the new bathymetry on the inflow of CDW, to do this robustly is the subject of an oceanographic study and probably beyond the scope of this paper. To fully quantify the implications for CDW influx requires an ocean circulation model (e.g. Nakayama et al, 2019) that implements the high-resolution bathymetry data and is calibrated by CTD data in the troughs (non-existent yet!). Nevertheless, we have tried to make a first pass attempt at quantifying the change in heat flux for the two cross sections over T2, i.e. the one from the older gravity-derived bathymetry and for our new MBES dataset. These "first-pass estimates" use data from oceanographic studies to provide temperature and flow velocities through the troughs, thus allowing us to estimate heat fluxes. This is now documented in Supp. Info. section "Oceanic heat flux calculations", Table S2, and is discussed in Section 6.1. We have also added information on critical sill depths, and channel widths at these sill depths, along sea-floor troughs T2-T4 that may act as conduits for CDW to the Thwaites GZ in Section 3.2 and new Fig. S3 to show the long profiles/sills and channel widths. This suggestion about critical sill depths was also made during discussions with our oceanographer colleague (now co-author Anna Walhin) and hopefully our additions provide useful information for future oceanographic studies.

4. The spectral analysis description (as the entire manuscript), is interesting and very well written but comes across as rather technical, and a departure from the rest of the manuscript. I recommend the authors to look into ways of making it more accessible to the wider glaciology community, perhaps by moving some its technicality to the supplementary materials and/or by taking greater advantage of an illustrative example. On the other hand, I had the impression that some of the key parameters used in the analyse are not fully explained, but this could all go into the supplementary material.

We appreciate this comment (which is also in line with comments made by R2) and we have now attempted to better integrate the spectral analysis work into the manuscript. We prefer not to move material from this section to the Supplementary Materials because the derivation of power spectra and its relationship to basal drag is a key component of this work that broadens its appeal to glaciologists and "over-ice" geophysicists alike. It is also important to lead the reader through the derivation so that the results can be linked to specific parts (behaviour of some parameters) in the derivation. Thus, we have made the following changes to address this comment:

- *We have added a new paragraph in the Introduction that introduces the use of MBES datasets for both glacial landform mapping and its potential for bed roughness analyses.*
- *We have separated out the methods and results sections of the spectral analysis sections (new Sections 5.1 and 5.2) to lead the reader through the process more clearly and provided more text to explain the most important parameters, what the periodograms represent, and how our results link to other studies of subglacial roughness.*

Specific comments in tc-2020-25-RC1-supplement.pdf:

We have addressed each of the comments in the PDF document supplied by Reviewer 1. If we agreed with the comments we have accepted the change this is noted only briefly. Important comments requiring a significant change and comments that we refute are listed here with our full response below.

Line 48-49: I would order these aspect with a better logic. In facts, I would have mentioned increased ice shelf calving/disintegration of ice shelves first which then induce reduced buttressing, increased upstream ice flow and grounding zone retreat.

We have reordered the text as suggested by R1.

Line 78: [Re TGT has periodically advanced and calved] over which period of time?

We have added the timescale of the Thwaites Glacier Tongue advance and calving (multi-decadal).

Line 122: this [sounding density] is pretty amazing but it would be more informative if you would provide a resolution range based on the water depth range you had in the area, and perhaps the average or median value as well. Otherwise this mentioned high resolution is at odd with the choice of a 50m gridded DEM.

We now also provide sounding densities (on the sea floor) based on the maximum depth range of our working area (~1200 m) to illustrate the range in spacings, as well as examples of sounding densities for some of the older MBES systems used to acquire the data in the final grid (see Table 1). We have added the following sentences to "2: Geophysical Methods" to explain the choice of 50 m grid cell size:

"Note that the sounding spacing achievable by each MBES system varies considerably depending on the system setup with older systems generally attaining lower spatial resolution. For example, at 1200 m water depth and a 60° beam angle, the Kongsberg EM120 MBES would achieve an across-track sounding spacing of only 22 m, and the Seabeam 2112 MBES only 35 m. Together, these two systems were responsible for acquiring 5 cruises worth of data in the area (Table 1)."

"Ultimately, and to accommodate the different resolutions of the original datasets, the bathymetric sounding data were gridded in MB-System using a Gaussian weighted mean filter algorithm to produce an isometric 50-m digital elevation model (DEM) for the sea floor on the southern ASE shelf."

Line 146: what dictated the choice of the location of these profiles [additional profiles for spectral analysis]?
We have now added the following description of why the bed profiles were chosen to the spectral analysis methods section (new Section 4.1):
"Profiles were selected based on their location along the central glacier trunk, and their quality in terms of continuity and along-track resolution. The profiles from the Dotson-Getz Trough, offshore from the Getz A Ice Shelf (Fig. S2b), were selected as representative of a sedimentary palaeo-ice stream bed characterised by mega-scale glacial lineations (MSGL) (Graham et al., 2009; Spagnolo et al., 2014). These were extracted from a MBES dataset fully described by Larter et al. (2009) and Graham et al. (2009)."

Line 198: [Dimensions of the lineations are] very much like MSGL. What is their spacing? Or do you recognise these as erosional or depositional?
We have added the spacing between lineations (crest-crest 200-500 m). We do not have a good feel for whether these features are erosional or depositional; their dimensions are consistent with mapped MSGL or glacial lineations (e.g. Spagnolo et al., 2017) and we think it would be difficult to discern from our relatively small patch of lineations to determine whether they are erosional or depositional features – this is also in line with the findings of Spagnolo et al. that this is difficult to determine and may be a combination of processes! Ultra-high resolution data from the AUV missions flown on NBP19-02 (to be worked up and published) may shed more light on this.

Line 230: [troughs…have been variously modified by ice] and perhaps water as well? or can you exclude this entirely?
We have added modification by subglacial water flow as a possibility here.

Line 231: [Re channel widths] measured how? over how many profiles etc.?
The methodologies for deriving trough and channel metrics are given in Kirkham et al. (2019) which is referenced in the Geophysical methods section (2.1); however, we have added the total number of cross sections analysed here for information. We also add a line to the methods section pointing the reader to Fig. 2 of Kirkham et al. which very clearly describes how the channel metrics are measured in graphical form.

Lines 257-259: I would like to see a brief discussion on this specific, and rather interesting point. As I think this might help with their overall interpretation.
We have added several sentences to discuss the different heights of the flat-topped highs, notably the similar heights of features in Pine Island Bay and in front of Thwaites Glacier, and that highs with different heights along one flowline would be of interest for distinguishing between pre-existing topography and ice dynamics (assuming that the bedrock composition is the same, which is thought to be the case for the inner Amundsen Sea).

Line 264: Despite evidence of glacial erosion, you cannot be sure that the highs flattened top

was produced by the erosion of the moving ice necessarily. For instance, they could simply be the expression of selective erosion around bedrock horizontal structures, as we see in many (generally non glaciated) onshore structures. A further possibility is that the highs top is depositional, mantled by a considerable amount of glacial deposit which leveled a more rugged, underneath topography. I would like you to consider this possibility and discuss it, if you haven't done already.
This comment is linked to general comment 2 – please see full response to that above.

Line 266: If I recall correctly, Damon David observed flat topped ridges below the current Pine Island ice shelf, although perhaps they are of different size. It is all in his 2017 paper
The flat-topped mounds surveyed by Autosub AUV under Pine Island Ice Shelf are much smaller in scale and interpreted as glaciotectonic rafts of sedimentary material and so are not directly comparable to the flat highs that we see here. Still, we thank the reviewer for pointing us toward a possible analogous feature!

Line 293: [glacigenic sediment was transported] by what?
We have qualified transport of material down-slope was by gravity-driven processes (although we cannot be more specific than this based on our morphological data alone).

Line 307: did you consider doing this for different orientations, as in Spagnolo et al., to quantify basal drag relative to ice flow direction?
We did do power spectra and basal drag analyses for 6 across-flow lines as well as along-flow lines (see Figs. S5a, b). This is discussed in (new) Section 5.2 and we have added a sentence about across-flow vs along-flow roughnesses for the MSGL area. One future study that we have already considered is spectral analyses of roughness for, say 8, orientations around the compass (like a Rose diagram) on a grid of the bathymetry data to assess the anisotropy (or not) of bed roughnesses. This, however, is beyond the scope of this current work, we just need to get a student or post-doc to do it now!

Section 4.2: This section is interesting and well exposed but extremely technical and a big jump from the previous, largely descriptive, part of the paper. Part of the technicality is intrinsic, and also it is clear that one needs to read Schoof paper to fully appreciate this section. However, I wonder if a further, better effort could be made to keep the non-expert reader better engaged with this section. Perhaps the most technical aspects could be moved into the supplementary and an easier-to-grasp/simplified explanation be maintained here. Or else, could the use of an example, which is already partly incorporated here, be further exploited, to show what each (component of the) equation means in practice?
We fully address this in our response to general comment 4 above (see new Sections 5.1 and 5.2).

Line 407: This is kind of obvious to most of us, but for the wider audience I wonder if you should have stressed this important point earlier on as well, when you described landforms that are clear evidence of grounded ice.
We have added a statement at the end of Section 3.1 (Glacial landforms) that the mapped area represents the former bed of TG.

Line 418: Could you specify by how much [gravity-derived bathy underestimates seafloor

depths], on average?
We have added average numbers for the differences between the new MBES gridded bathymetry and gravity-derived bathy from Millan et al. and IBCSO, 119 m and 65 m, respectively.

Line 431-433: Could you quantify [the significance of underestimating CDW volumes by underpredicting trough depths], rather than generically saying that it is significant?
We have addressed this fully in our response to general comment 3 above.

Line 469: Is this to say that TG was less dynamic than its neighbors? I would say this more explicitly, if so.
This is not what really what we are saying. All the evidence to available date (i.e. before any ITGC dates come through) points towards a similar retreat history for Thwaites Glacier to that of Pine Island Glacier and is nicely collated and summarised by Larter et al. (2014). The more recent history (last century or so) has only been speculated about, and should be illuminated by ITGC, but it has been hypothesised that Thwaites ice shelf unpinned from the highs in front of it 55-150 years ago (Tinto and Bell, 2011), which is similar to the unpinning of PIG ice shelf from a submarine ridge ~30 years ago. We have added the following sentence to the text to clarify this:
"This retreat history is in line with what we know about ice-sheet retreat more generally in the Amundsen Sea, where rapid grounding-zone retreat occurred from 15 to10 ka to reach near modern limits (Hillenbrand et al., 2013; Larter et al., 2014); however, more marine dates and terrestrial thinning histories will certainly provide more clarity."

Line 483-485: "Our interpretation of a proportion of unconsolidated sedimentary substrate, and thus low density material, on the H2 and H3 highs may explain why bathymetries derived from gravity over-estimate the height of some of these features (Figs. 8a, b)." Could you explain why?

Although we believe that the confusion here was caused by an error in the text - if we consider Figs 8a, b and d the heights of the highs are sometimes underestimated but not OVER ESTIMATED – we have elected to remove this sentence. We have discussed this with our airborne geophysicist co-author who noted that the gravity inversion actually did a good job of predicting the heights of H2 and H3 as the free air anomaly goes down as you move west along the highs. Thus, we have removed that sentence and added a linking sentence about the interpretation of sediment on the highs as an explanation for their flat-topped morphology:
"All of the landform evidence we present here, supported by cores and acoustic sub-bottom profiles, suggest that the tops, fronts and sides of the H2 and H3 highs are mantled by some thickness of sediment, probably over a bedrock core. Seismic-reflection profiles would be needed to determine the internal structure of these features and sediment thicknesses."

Line 507: This is such an important outcome and I think you should hint at it somewhere in the title
We have changed the title to: "Revealing the former bed of Thwaites Glacier using sea-floor bathymetry: implications for warm-water routing and bed controls on ice flow and buttressing"

Line 514: but how different would the topography need to be in order to obtain considerably different roughnesses? Would, for example, all ice stream bedrock beds (no MSGL) have a comparable roughness?
This is definitely an interesting question. It was certainly a little surprising that all of our bed profiles (onshore and offshore, along-flow and across-flow, MBES and radar) had similar roughness properties. In a future study it would be interesting to compare our results with the findings of, for example, Jordan et al. (2017) who looked at the roughness of bedrock terrains in N Greenland and determined bedrock bed roughnesses with differing power law scaling behaviour.

Line 527-528: This is also very interesting and makes me wonder if there are specific conditions/thresholds above which an ice stream is able to ignore topography and below which is forced to follow it. It would be great if we were able to quantify these.
We agree with the reviewer that it would be great if we could quantify how "big" the topography needs to be before it steers the ice or, conversely, how thick the ice needs to be to ignore the topography and/or when cavitation occurs. It is certainly intuitively that thicker ice would be less sensitive to being steered by large-scale bed topography (e.g. O Cofaigh et al., 2010). It may be that numerical modelling as part of ITGC, and perhaps over this offshore terrain, will provide new insights on this.

Line 545: I do not quite see the need to stress this aspect. I would expect bed types to be the same, whether onshore or offshore. Or perhaps I am missing the point..?
We take this point that offshore vs onshore is not the issue so we have replaced "in onshore areas" with "beneath the modern glacier".

Line 565-566: I think this sentence is overselling. Could you distinguish between grain sizes from high res bathymetry?
We have modified the language in this sentence to the below to not oversell our findings and to avoid repetition:
"These analyses add to our understanding of across-flow contributions to basal drag or hydraulic potential (e.g. Muto et al., 2019a), and allow us to consider the spatial variability of bed types (e.g. sedimentary vs. hard beds), particularly where sea-floor sediments are cored for ground-truthing."

References:
Jordan et al. (2017): Self-affine subglacial roughness: consequences for radar scattering and basal water discrimination in northern Greenland. The Cryosphere, 11, 1247–1264, 2017. doi:10.5194/tc-11-1247-2017

O Cofaigh et al. (2010): Large-scale reorganization and sedimentation of terrestrial ice streams during late Wisconsinan Laurentide Ice Sheet deglaciation. GSA Bulletin (2010) 122 (5-6): 743–756.
https://doi.org/10.1130/B26476.1