Response to Reviewer 1 (Adrian Luckman)

Initial comments

This study uses a time-series of high resolution DSMs to investigate the interplay between grounded ice, ice shelf, floating tongue and basal channels at Thwaites Glacier. This is a topic of great interest in the cryosphere and the research is very well conceived, investigated and presented. The methodology is mostly made clear (see comments below), substantial interesting findings are presented, and the whole paper may serve as a model for the use of precisely calibrated DEMs for investigating changes in the “Hydrostatic Boundary” at ice shelf-ocean interfaces.

We thank the reviewer for their supportive comments and address their suggestions below.

General Comments

It is understandable that the (very) recent paper by Eric Rignot (Widespread seawater intrusions…) is not mentioned in this study, probably because it was in review as this paper was being submitted. I recommend that the authors include this paper in their review not simply because it is relevant, but because it could serve to clarify the relationship between the transition zone between grounded and floating ice as detected by InSAR and the Hydrostatic Boundary as measured by DSM analysis. Professor Rignot’s paper finds evidence of seawater-induced vertical motion inland beyond the HBs in this paper and the discussion could be quite informative. The adoption of informal names of some sub-glacial features may also be appropriate. From the present high quality of argumentation and discussion I doubt it will take long to add this potentially valuable element.

This is similar to an apt comment made by Reviewer 2 as well, and we believe we have addressed both reviewers’ comments with the additions we’ve made. Yes, the paper by Eric Rignot et al. was published about a month after this manuscript was submitted, and we immediately started discussing how we could contextualize our results with theirs, so we certainly agree with the reviewers that it is relevant and should be included. While we don’t find strong evidence for seawater-induced vertical motion inland of the grounding zone, it is important to note that our DSM analysis is at a much coarser temporal resolution than that of Rignot et al.; the locations of the uplift/subsidence regions identified in this work have been added to several figures and we have added a new paragraph to the Discussion to address this (Section 5.3):

“One of the channels we observe, ThC7, initiates near where two subglacial drainage channels discharge to the ocean (Rignot et al., 2024). Using differential SAR interferometry, Rignot et al. (2024) observed several circular areas ~4–6 km in diameter with time-varying uplift and subsidence (10–20 cm). These features are located above subglacial topographic depressions that abut km–scale subglacial ridges. The major features are all adjacent to prominent subglacial drainage channels and resemble the isolated HBs we infer inland of the GZ in and around Cavities 1a, 6, 8, and 9 (Figs. 3–5b). Rignot et al. (2024) conclude that the filling and draining of the more inland features is driven by fluctuations in the subglacial water flow through the nearby channels. For the large ‘bull’s eye’ feature just above the grounding line (see Figure 4c in Rignot et al. 2024), however, they speculate that the vertical motion is due to tidally-forced seawater intrusion, which they suggest should cause enhanced basal melting. They do not specify the magnitude of this melt other than to say it should be much lower than 20 m yr^{-1}. If this non-steady melting is significantly above the background basal melt rate, we would expect to see a signature in the long-term thinning rates. Instead, the 2020–2023 elevation change data show thinning of 1–2 m yr^{-1} in the area surrounding the downstream feature with minor thickening (<0.5 m yr^{-1}) near its centre, providing little or no indication of enhanced melt (Figs. 5f, S7c). We also note that dH/dt derived from annual DSM mosaics does not provide the fine temporal resolution (up to sub-daily) over which uplift/subsidence features were observed in this study. We do not observe increased rates of thinning for most of these closed regions, even when they are near the main HB, suggesting that any enhanced melting due to incursion of seawater may not persist long enough to significantly impact the signal on multi-annual timescales for most of the glacier. An alternate hypothesis is that all of the circular features are driven by subglacial water flow rather than seawater intrusion. This hypothesis is supported by a strong gradient in the hydraulic potential between the grounding line and the ‘bull’s eye’ feature, which should drive the water toward – not away from – the ocean (Fig. S6). Seawater intrusion is also problematic because it needs to occur over an area where the predominant flow direction should be seaward to accommodate major subglacial outflows. These features likely fill and drain through exchange of water with the adjacent subglacial channels, similar to how lakes located much farther inland fill and drain (Smith et al., 2017). If this is the case, the pressure boundary condition where these channels meet the ocean should be subject to tidal modulation (10 kPa) sufficient to explain the observed ~10–20 cm uplift/subsidence (1–2 kPa).”

We have also added a reference to the Rignot et al. paper in the first paragraph of the introduction, as it provides further motivation for the timeliness of our GZ investigations, and adopted the use of “main trunk” rather than “embayment” for the TWIT GZ and other informal feature names for consistency with this and other papers.

Mostly, the remaining Thwaites ice shelf is referred in this paper to as “TGIS” (Thwaite Glacier Ice Shelf), but I detected some “TEIS” references (Thwaites Eastern Ice Shelf) which is my own preference because it acknowledges the former existence of a western ice shelf. Consistency is obviously required and you (and maybe the Editor) should decide which to use.

We agree that there was not enough consistency in the use of these acronyms. We have reworked the introduction, where we define the TEIS and TWIT as distinct portions of the shelf, and to define the TGIS as referring to both portions collectively. We have also checked the rest of the manuscript to ensure consistency throughout, making changes as appropriate.

Specific comments

Line 112: which “annual velocity map”?

We address this together with the following comment (L. 114).

Line 114: how can you have a “median of two”, and how do you define “summer quarters”?

We have rewritten this description of the methodology; hopefully the reviewer will agree that it provides more clarity: “The MEaSUREs annual velocity maps obtained for 2011–2015 are variable in their spatial coverage and quality, while the quarterly velocity maps obtained for 2016–2023 have more consistent coverage and better quality. To obtain annual velocity maps from 2011–2023 with more consistent quality, we initially take different approaches to filling data gaps and reducing noise in each dataset: for the 2011-2015 annual velocity maps, we take the average of each annual map and the velocity mosaic at each pixel; for the 2016-2023 quarterly maps, we take the average of each year’s Oct–Dec map and Jan–Mar map at each pixel.”

Line 137: please expand what you mean by “each independent continuous grounding line”

We have removed this phraseology as it was unnecessary. The sentence now reads “We track HBs at the continental grounding line and six pinning points (PP1–6) delineated in the IPY GL .”

Line 148: I admire that you have used ‘inclusive’ colour scales. It is best not to refer to the (subjectively received) colours in the main text but allow the figures to speak for themselves

Thank you for appreciating the colour scales, which we put a lot of work into selecting! We have removed this sentence and subsequent references to subjectively received colors in the main text.

Figure 1: The IPY GL and 2011 GL are apparently in the same colour and the former is probably obscured by the HB sequence. Some adjustments (or removals) are reauired here.

We have elected to superimpose the IPY GL over the HBs and change its symbology rather than have it covered by the HBs.

Line 208: “Remaining artefacts .. are filtered out”. Please elucidate.

We have specified that artefacts may be due to clouds or poorly co-registered strips: *"...*extreme values resulting from remaining artefacts from clouds or poorly co-registered strips in the annual mosaics are filtered out."

Line 236: "Several". Why not be precise here?

Good point. "Several" → "Six".

Line 261: "TEIS" and "TWIT". I would say these have gained enough currency for general adoption. But then I would.

This has been addressed in our response to the reviewer's second General Comment above.

Lines 279 and 286: seven, then eight basal channels?

Good catch. "Eight" was a typo persisting from a previous version when we were considering a less-convincing feature. There are only seven features that we consider to be basal channels; fixed.

Line 303: "by the end of the study period". You could help the reader here by giving precise time boundaries.

*We purposefully left this vague because some channels did not experience melting throughout the entire study period, although all did in at least one multi-year epoch. However, the reviewer's point is taken and we have replaced "by the end of the study period" with "within at least one multi-year epoch". As further detail is described in the following sections, we are comfortable with leaving this summary somewhat vague: "*Retreat of the HB exceeding 1 km occurred along all reference channels except for ThC6 and ice–column thinning and melting occurred near all channel intersections with the GZ within at least one multi-year epoch (Figs. 2–6)."

Line 305: I couldn't see how Figure 7 could be used as evidence here.

Figure 7 shows time-varying velocity and HB position along the reference channels in the vicinity of the GZ, and we did ***not*** *observe any strong patterns among the channels (e.g. Fig. 7 showed no evidence that the velocity increased more with more HB retreat). We have made this sentence more specific to reflect this, and to alert the reader that some individual channels may exhibit a connection between velocity and HB retreat*: "However, no strong relationships emerge between changes in velocity and HB retreat rates along all channels; notable correlations between changes in velocity and changes in HB position along individual channels are described in ensuing sections (Fig. 7)."

Line 405: "was extended .. arbitrarily". Please explain more precisely what you mean.

We revised this sentence for precision: "Thus, we manually extended the upstream end of the ThC6 reference channel about 5 km inland of the GZ to show retreat past the IPY GL."

Line 427: “or an error in the manual delineation”. This alerted me to the fact that I had missed that a manual step is involved in the method - I had assumed that the process was automated. Perhaps you could expand the methods section to explain this in a nit more detail and discuss the potential errors. Errors in manual steps are rather different from uncertainties in automated processing. I think this sentence needs some more nuance.

We have added an additional section addressing uncertainties and manual errors in the methods section (Section 3.5), and revised this sentence to provide more context:

“In 2022, this region only has coverage from one or two strips (Fig. S2), resulting in only two mappings of HB features from which the annual HB was manually delineated. Although some regions are covered by few strips in several years, we are more confident in HB positions that persist or display a pattern over several years, and one year of data does not provide sufficient evidence to conclude that this entire region was ungrounded in 2022.”

Line 442: extra brackets.

Good catch. Fixed.

Line 520: “volume of basal melt .. 3.5Gt”. Please give a time period as well as an area. To claim this as “ample” requires some more data or argumentation.

We have added additional details based on closer comparison of our results with the cited paper:

“The basal meltwater volume has been estimated at 3.5 Gt a^{-1} for the 189,000 km^2 Thwaites Glacier drainage basin, with most of the melt occurring within about 50 km inland of the GZ (Joughin et al., 2009). Our study area extends from ~10-100 km inland of the GZ, so ample subglacial water is available, and may discharge…”

Line 529: Figure S8a would need some more annotation to support this statement.

We have labeled the pinning points in Fig. S8a and marked the highest point of the bathymetry in this region with a ‘’ marker.*

Great work.

Adrian Luckman, 22nd May 2024

We thank Adrian wholeheartedly for his helpful and constructive comments, which we believe have greatly improved the readability and impact of this paper.