

The Cryosphere Discuss., author comment AC1
<https://doi.org/10.5194/tc-2021-265-AC1>, 2022
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Reply on RC1

Sarah S. Thompson et al.

Author comment on "Glaciological history and structural evolution of the Shackleton Ice Shelf system, East Antarctica, over the past 60 years" by Sarah S. Thompson et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-265-AC1>, 2022

We thank Anonymous Referee #1 for their considered and detailed review of our manuscript. Below we respond to each comment, with the anonymous referee's original comments shown in italic text and our response in bold text.

Summary: The manuscript by Dr. S. Thompson and colleagues presents a two-pronged study aimed at (1) reconstructing the 60-year surface feature record and 2014-2021 surface ice velocity field of the Shackleton Ice Shelf System via analysis of satellite imagery, and (2) modeling the future response of the upstream grounded glaciers feeding the Shackleton Ice Shelf System to near-instantaneous disintegration of this ice shelf via a 400-year projection made using the BISICLES ice sheet model. The authors conclude that the Shackleton Ice Shelf System has not changed significantly over the 60-year observational period (aside from a localized acceleration in surface ice velocity near the ice front of Scott Glacier) and that the future upstream glacier response to collapse of the Shackleton Ice Shelf is minimal relative to changes projected across other East Antarctic basins.

I find the extension of the surface-observational record both spatially (to the neighboring Scott Glacier, Roscoe Glacier, and greater Shackleton Ice Shelf region) and temporally (from 2017- 2021) to be the primary strength of this manuscript, as this information is very useful to the ice sheet modeling community. However, I have significant concerns regarding the scope of the paper and the applicability of the numerical modeling work, which make the manuscript difficult to follow. First, I find significant overlap in the analysis of surface features of the Denman Ice Tongue between this manuscript and that of Miles et al. (2021) (e.g. ice front positions, patterns of rifts, and location of pinning points on the Denman Ice tongue, as well as the calving of the large tabular icebergs in the 1940's and in 1984). While the manuscript does properly cite Miles et al. (2021), the repetition of the analyses and findings makes up a significant portion of this study and thus reduces the novelty of the manuscript. The manuscript should be reorganized to have a greater focus on the spatial and temporal extension of the surface observation record.

We will restructure the manuscript to clarify the novelty of the extension in spatial and temporal observations. Our aim was to provide full context to the observations we report and discuss, rather than simply citing previous work. We will make the distinction requested.

In addition, the numerical modeling portion of this manuscript is rather disconnected to

the scope and findings of the rest of the paper and is not robust enough to support the authors' conclusions. The first half of the manuscript analyzes short-term and fine-scale changes of features on the floating portion of the Shackleton Ice Shelf System; however, the authors then model the 400-year response of the grounded regions of mainly Denman Glacier to near- instantaneous disintegration of this ice shelf. This disconnect between the focus on observing small-scale ice shelf features across the entire Shackleton Ice Shelf System and modeling grounding line retreat and volume loss of Denman Glacier (without mentioning of the response Scott and Roscoe Glacier) makes following the progression of the manuscript very difficult. If modeling is going to be included in this study, it needs to complement the rest of the manuscript (i.e. model how future ice flow responds to changes in the surface features discussed in the first half of the manuscript). Furthermore, from the analysis of a single model simulation, the authors imply that the Queen Mary and Knox coasts are relatively stable and insensitive to reasonable forcing in the next 400 years (see L313, I am also assuming this is the "implied dynamic stability" referenced in the title). I don't believe the authors can claim stability of the system and make a statement about sensitivity without modeling the system's response to realistic forcing perturbations. Overall, I believe the modeling portion of this manuscript needs to be either redone so that it supports the observational-focus of the paper or separated and made the focus of a secondary manuscript.

It is apparent that the authors have put a lot of effort into the text and figures in the manuscript; however, because of my significant concerns over the scope of the manuscript, its connection to the modelling work, and the key takeaways, I suggest that major revisions (or perhaps a resubmission) are needed before the manuscript can be considered for publication in The- Cryosphere.

We thank Anonymous Referee #1 for their detailed comments on the modelling section of the manuscript, which are echoed by those of Anonymous Referee #2. The authorship team have discussed this at length and appreciate the limitations of the modelling approach highlighted here and in the general comments. The logic in the flow of our original manuscript from observation to modelling lies in the following. As explained further under the 'Title' section below, our observations revealed only minor changes of structural changes in the Shackleton Ice Shelf System over the past 60 years. Our inference is therefore one of a rather stable system (not withstanding previous authors' observations of some dynamic variability) and indeed, this is confirmed by our BISCLES modelling experiments in that even upper limit conditions, i.e. complete loss of all floating ice that may buttress the grounded ice, lead to only minor simulate change relative to those elsewhere in Antarctica.

Nonetheless, given the matching comments by both anonymous referees we feel that – subject to advice by the journal editor, the best approach may be for us to separate the observations from the modelling, with the latter forming the focus of a subsequent manuscript. As suggested by the anonymous referee(s) we will therefore remove the modelling aspects from this manuscript and focus on the observations that already make up the majority of the current manuscript. The modelling proportion will then form the basis of a subsequent manuscript as suggested.

General Comments:

Title: I don't believe the title accurately describes the presented work. I am unsure what the authors mean by "glaciological setting", I think wording that describes the analysis would be better suited to use in the title. I also think it is a bit misleading to claim that the authors are studying the entire Queen Mary and Knox coasts, when only the Shackleton Ice Shelf system is analyzed. Lastly, I am not sure what the authors mean by "implied

dynamic stability". Is this stability over the entire 60-year observational period (which would be inaccurate because grounding line retreat (~5 km, Barancato et al., 2020), floating and grounded ice accelerations (Miles et al., 2021; Rignot et al., 2019), and accelerated ice discharge (Rignot et al. 2019) have been observed over this timeframe), over the 400-year modeling period (which, as stated above, I do not think the authors can claim based on the results presented), or between 2018-2021 (following the ice velocity results)? It is difficult to suggest a new title right now because significant changes to the scope of the manuscript need to be made.

The inference of an 'implied stability' is based on our observations of little dynamic change over the past 60 years, as well as reduced modelled sensitivity to even extreme events such as complete removal of all floating ice. It is natural for systems such as the Shackleton Ice Shelf to show some dynamic variability although, as explained in the manuscript, our observations and modelling cannot confirm that the variabilities observed by previous work necessarily herald major future change. We recognise that the same time that field data are too sparse to make this conclusion with confidence and am therefore recommending that focused programs of field observation are urgently needed. Either way, we will reflect on the current title and revise it appropriately.

Ice sheet model validation: As the manuscript presents one of the first regional ice sheet models to make future projections of the Shackleton Ice Shelf system, it is critical that it is properly described and validated. It is not enough to only show the mismatch of observed and modeled surface ice velocity in order to validate your ice sheet model, one also needs to know how the modeled and observed grounding line positions and ice discharge values compare. In the initial model solution, there are extensive grounded regions along Denman's ice tongue and floating pockets along Denman's grounded ice stream (figure 11) that are not seen in observations (Barancato et al. 2021; Morlighem et al., 2020). Such errors in the initialization of the model can propagate to the transient solutions, so it is critical to have a well-calibrated model that matches present day observations.

The ice sheet model description section (L130-L177) is lacking details that would be needed to ensure that this modelling work reproducible. Some examples of missing methodological descriptions are as follows: which 2D stress balance approximation is used (e.g. SIA, SSA, a combination of both), do the ice stiffness and basal friction coefficient change in time, how is the grounding line tracked (e.g. sub-element parameterization), how is basal melt applied numerically to partially floating elements if using a sub-element grounding line parameterization (e.g. to the entire element, to only the floating part of the element), etc. These details are very important and should be included (perhaps it would be better to give a complete model description as a supplement or appendix). For examples of the types of information needed, one could refer to the ISMIP6 Antarctica publication (Seroussi et al. 2020) or this recent manuscript submitted to The-Cryosphere Discussions (Castleman et al. 2021).

Agree and we will make this the focus of a separate manuscript.

Units of speed: When referencing speeds of both ice and rift/ice front propagation in the main text and figures, the authors switch between m/day and m/year (see lines 258 and line 270 for examples of each). For ice speed, the convention is m/year, so I think it would be best to abide by this convention so that your results can be easily compared to other values in the literature (change in both the text and in figures). Also, when referencing the unit "year", please stick to either "year" or "a", as both were used in the manuscript.

May we clarify that m/year is commonly used when the temporal resolution of available data is greater than one year. For this reason, when describing the

movement of structural features and ice frontal positions we have used the unit m/year to describe the longer-term trends as the measurements are based on data with annual or multi-annual temporal frequency. When describing the ice speed data from feature tracking, we have deliberately used the unit m/day because we are using much higher temporal resolution data. While we are happy to include annual trends for the data, ice speed does vary on much shorter timescales and by simply changing the ice speed data to m/year we would lose this valuable information.

Data availability statement: Please add a data availability statement at the end of the manuscript, as to abide by The-Cryosphere's data policy. All of the links in sections 2.1 and 2.2 should be moved to the data availability statement. In addition, a link to the BISICLES ice flow model, as well as links to all datasets used in the simulation, should be added to this statement if the modeling portion is to remain in the manuscript.

We are happy to make this change.

Grammar: When reading through the manuscript, I noticed a fair amount of spelling and grammar mistakes (especially missing commas, which would help the readability of the text). I tried to point them out as I found them in the specific comments, but it is possible I missed a few!

The whole authorship team will all check the manuscript thoroughly for spelling and grammatical errors.

Specific Comments:

L13-L38: In general, I think the abstract is a bit long and should be condensed. Below, I suggested a few sentences that can be removed and/or shortened.

We are happy to make these the changes.

L15: change to `` . . . on understanding the controls driving Denman Glacier's dynamic evolution, although . . . ''

We are happy to make these the changes.

L17: Shackleton Ice Shelf (use capitalization because it is a proper name)

We will make the change throughout the manuscript.

L22-L23: Remove "in response to coupled ocean and atmospheric forcing". Coupled forcing suggests that your ice sheet model is coupled to an atmosphere and/or ocean model, which it is not.

This section will be removed from the manuscript as outlined in the response to the general comments above.

L31: I make note of this later in the results section, but the authors should not use real years to describe the output of their modeling work because it is not a realistic simulation. Instead of saying "in the third century from now", it would be better to say "in approximately 300 years into the model simulation."

This section will be removed from the manuscript as outlined in the response to the general comments above.

L31: Please check the computation of the 6 cm of sea level rise, I computed $40 \text{ Tt} = 40000 \text{ Gt} / 3600 [\text{Gt/cm}] = 11.11 \text{ cm}$ sea level rise equivalent ice mass, but it is possible that my math is off! Is this the sea level contribution from just Denman Glacier, or from the entire model domain? I believe this is from the whole domain; however, in the previous sentence, you discuss the grounding line of Denman Glacier, so it is a bit confusing. Please specify.

This section will be removed from the manuscript as outlined above.

L32: I would hesitate to say that 6 cm of global sea level rise equivalent ice volume loss is "small" in comparison to other areas of East Antarctica. First, I don't believe there are any published studies that have run regional transient simulation of the EAIS through 2400, so we cannot compare. Also, 6 cm is on the upper limit of the ISMIP6 projected contribution of the entire Antarctic Ice Sheet to global sea levels by 2100, so this contribution from a single EAIS glacier by 2400 must be fairly significant.

This section will be removed from the manuscript as outlined above.

L32-L34: The sentence "it is clear . . . Shackleton system" can be removed.

We are happy to remove the sentence.

L34: Here you conclude that there is potential vulnerability of the system to accelerating retreat, but further along in the manuscript (L313), you say that the modeled domain is relatively stable and insensitive to reasonable forcing in the next 400 years. These statements conflict and left me confused about the message of the manuscript. Perhaps it would be more consistent with the rest of the manuscript to say that these data are needed to improve model initialization and validation.

We agree and will alter the manuscript to reflect the main point that we don't know enough about the system to be able to accurately model potential vulnerabilities.

L34: Insert comma after "accelerating retreat".

Happy to make the change.

L41-L44: These first two sentences can be combined and condensed, which I think would be a bit easier on the reader. Perhaps something like: "It has long been perceived that the East Antarctic Ice Sheet is the stable sector of Antarctica (citations); however, it has now emerged that the Aurora and Wilkes subglacial basins of the EAIS have been contributing to sea level rise since at least the 1980s, with Aurora contributing 1.9 mm and Wilkes contributing 0.6 mm (citations)."

We agree that the change increases the clarity and are happy to make the change.

L45: Insert comma after "WAIS"

Happy to make the change.

L45 and L47: You are referencing both BedMachine Antarctica (Morlighem et al., 2020) and Bedmap2 (Fretwell et al., 2013) for your values of sea level potential. As BedMachine is the most up-to-date dataset, I would stick to just using the BedMachine citation throughout the manuscript (unless of course you are using the BedMap2 dataset in the paper).

Bedmap2 was used in the first draft of the manuscript in the current version we updated to BedMachine. The inclusion was an oversight, and we will remove the Fretwell reference.

L52: Change "it is supplied by . . ." to "Major outlet glaciers drain into this ice shelf system, including Denman, Scott, Northcliffe, Roscoe, and Apfel Glaciers."

Happy to make the change.

L57: Cite Morlighem et al. (2020) instead of Rignot et al. (2019), as the BedMachine publication lists the most updated inventories of glacial ice volume.

Happy to make the change.

L65: Change "just above" to "just upstream of"

Happy to make the change.

L73: Adusumilli et al. (2020) show melt rates peaking at approximately 120 m/yr along Denman Glacier's deep grounding zone. Please check the value reported in your manuscript (6 m/yr), I think this might be a typo.

The value > 6 m/yr was identified from Figure 1 in Adusumilli et al. (2020) as there is no mention of Denman Glacier in the manuscript or supplementary materials. Shackleton Ice Shelf is listed in Table 1 of the supplementary materials with a basal melt rate of 1.8 ± 1.9 m/yr for 1994-2018. Since the manuscript was submitted we have identified an additional reference, Liang et al. (2021), who estimate melt rates exceed 50 m/yr near the Denman Glacier grounding line for 2010-2018 and we will update the manuscript to include this reference.

Liang, Q., Zhou, C., and Zheng, L.: Mapping Basal Melt Under the Shackleton Ice Shelf, East Antarctica, From CryoSat-2 Radar Altimetry, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 14, 5091-5099, 10.1109/JSTARS.2021.3077359, 2021.

L82-L85: Please remove "A satisfactory explanation . . . with the nearby Totten Glacier." I think this interrupts the flow of the introduction and does not serve the rest of the paper, as the focus is not to determine where the high melt rates are being forced from.

Happy to make the change.

L92: What does it mean to put previously observed dynamic changes in the Shackleton system into the wider regional context of the Queen Mary and Knox coasts? The observational and modelling components of this study do not investigate changes beyond the Shackleton Ice Shelf System, so I think that claiming to frame the regional context of the entire Queen Mary and Knox coasts is a bit misleading. As stated above, I think the really exciting science presented here is the extension of the observational record to other sectors of the Shackleton Ice Shelf and to 2021. So I think this sentence should reflect that.

We agree and are happy to make the change.

L95: I do not think we are testing the sensitivity of the domain, as this would require further model runs (such as a control simulation and variance of the ocean forcing).

We remove this sentence from the manuscript as the modelling will be the focus of a separate manuscript as discussed in detail above.

L96: Remove "in response to coupled ocean and atmospheric forcing"

We are happy to remove this sentence from the manuscript.

L101-L120: Remove links in the main manuscript and add them into a proper data availability statement at the end of the manuscript (see general comments).

We are happy to this change to the manuscript.

L107: The "th" on 10th should be a super-script.

Happy to make the change.

L111: change ";" to ":"

Happy to make the change.

L100 and L112: I think the sentences would read better if you did not use the parentheses at the end of the sentence. For instance, L100 would read as: ". . . using standard GIS techniques following the methodology of Glasser et al. (2009)." The multiple sets of parentheses is confusing for the reader.

Happy to make the change.

L120: Insert comma between "methods" and "feature tracking"

Happy to make the change.

L121: "We use image . . ." Use present tense

Happy to make the change.

L124: ". . . and the quality of the velocity map is maximized"

Happy to make the change.

L126: Change "allowed" to "allows"

Happy to make the change.

L146: Change "horizontal rate of strain tensor" to "horizontal strain rate tensor"

This section will all be removed from the manuscript as outlined above.

L151: Change "rate-strain" to "strain-rate"

This section will all be removed from the manuscript as outlined above.

L163: Please give references to previous BISICLES studies that have used this initialization method.

This section will all be removed from the manuscript as outlined above.

L173: Change to "The single future simulation follows the methodology of Marin et al. (2019)."

This section will all be removed from the manuscript as outlined above.

L174: "Under" should be lowercase

This section will all be removed from the manuscript as outlined above.

L174: I had assumed that the melt rate was 1000 m/yr across all floating ice, but here you say that the melt rate reaches 1000 m/yr in places. How is the basal melt rate computed? Does the melt rate vary in space and/or with the geometry of the ice shelf?

This section will all be removed from the manuscript as outlined above.

L195: These rift-systems along the Shackleton Ice Shelf are really fascinating! It is so interesting that the two rift-systems have almost identical shapes (with system-1 being larger than that of system-2).

Yes indeed, we completely agree.

L213: Cite figure 1b here, it shows the high concentration of surface features on the Denman Ice Tongue very well.

Happy to make the change.

L218: Should this first sentence be citing figure 5 (figure 6 shows the rift on the Shackleton-Roscoe shear margin)?

Yes, this first sentence should reference figure 5, we thank Anonymous Referee #1 for noticing the mistake and will correct it and check all figure references thoroughly to make sure they are correct.

L222: I am having trouble figuring out which rift you are describing in this line (the one on the western side of Scott Glacier). Since you are highlighting this particular rift, it would be helpful to highlight it or point it out in figure 5c if possible (perhaps an arrow or pointer next to it so that it is easily identifiable by the reader).

We agree that it is difficult to distinguish between the different rifts discussed in this paragraph and will ensure that they are all clearly labelled in figure 5c to allow distinction.

L229: Replace "some changes" with wording that is a bit more definitive.

The whole sentence currently reads 'Across the whole system there are some changes in the shear margins between the various inlet glaciers and the main body of 230 the Shackleton Ice Shelf.' We will change the sentence to read

'We observe changes of different magnitude in all of the shear margins of the Shackleton Ice Shelf System over the period of observation.'

L244: Why did you decide to compute the velocity difference between one year (2019-2020)? It seems like this would not give very interesting results because that is not enough time for the system to respond to a forcing perturbation (aside from the northern point of Scott Glacier's floating extension, which looks like perhaps it is undergoing a calving event).

We report the difference in ice speed between 2019-2020 because in this paragraph we are focusing on the most recent changes that have not previously been reported. We think Anonymous Referee #1 is referring to a perturbation to the flow of ice by, e.g. atmospheric or ocean forcing, or a calving event which are unlikely to show much change on the timescale of one year. We acknowledge that this is different in Greenland where there are strong year on year and even seasonal changes in response to transient ocean warming/cooling. Figure 9 provides the recent higher resolution data in the context of longer-term changes, and we will amend the text to clarify that we are only looking at the most recent changes in this paragraph. Recent changes are discussed in the context of longer-term ice speed variability (shown in Figure 9) in the subsequent paragraph (L249-260).

L254: Change m/day to $m \text{ day}^{-1}$ to be consistent with the rest of the paper (ultimately should be $m \text{ year}^{-1}$, see general comments)

We apologise for the inconsistency in unit form and will check the manuscript carefully to maintain $m \text{ day}^{-1}$ and $m \text{ year}^{-1}$.

L255: "Speeds ~ 10 km either side of the grounding line . . ." confuses me a bit. This sentence makes it sound like you are talking about grounded ice as well (since 10 km on either side of the grounding line would extend 10 km upstream into grounded ice), but I believe you are talking about points 10 and 11 in figure 8a. Perhaps it would be better to say "Speeds up to ___ km downstream of the grounding line show . . .". I also think it would be helpful to reference the specific points in figure 8a that you are discussing (e.g. in L258, "close to the ice front (point 13 in fig. 8a)"). L265: The format of ((b) in Fig. 10)(Furst et al., 2015) is a bit crowded. Instead of using double parentheses, change to (label-b in Fig. 10, Furst et al., 2015). Same with L266 and L267.

We agree that the statement is confusing as point number 10 is approximately located on the grounding line (as identified from Measures data). We will change the sentence to read 'There is no observable change in speed >10 km downstream of the grounding line of Scott Glacier (points 10 and 11 in Fig. 8a).'

L267: Change "rise in the ocean floor" to "local topographic high in ocean bathymetry".

Happy to make the change.

L269-L283: When describing the model results, I would stray away from using actual years (e.g. ". . . of Denman Glacier occurs after 2150 . . ."), as you are modeling with unrealistic forcing. Instead, I would change this to something like ". . . of Denman Glacier occurs 150 years into the model simulation . . .".

This section will be removed from the manuscript as outlined above.

L276: The dynamic response of the system seems pretty significant, as Denman Glacier retreats more than 100 km upstream and Denman and Scott Glaciers end up connecting around a topographic high.

This section will be removed from the manuscript as outlined above.

L285: This first sentence of the discussion section contradicts existing literature (e.g. Barancato et al. 2020; Miles et al. 2021), which have cited patterns of grounding line retreat and ice velocity change that appear to be ocean induced since the 1970s or so. This study only looked at changes in surface features through the 60 year observational period and velocity changes over the past ~15 years; however, it seems that changes

outside of those presented in this paper are occurring over those timescales. As such, I don't think the authors can claim, based on the presented manuscript, that the Queen Mary and Knox coasts have not changed significantly in the last 60 years.

Our own reconstructions of longer-term ice flow velocity change (Luckman, unpublished data) very much match those reported previously, e.g. in Miles et al. (2021). We refrained from repeating these matching observations here but perhaps should provide them as a wider framework to place our more recent inferences of (no substantial) ice flow velocity within. Pre-2000 data points are sparse and as we emphasize later in this paragraph (lines 291-292): "An increase in ice flow speed was observed just upstream of the Denman Glacier grounding line between 1972-4 and 1989 and, to a lesser extent, through to 2008 (Miles et al., 2021, their Fig. 3c). Variability in ice flow speed then became insignificant through to 2016-17 (Miles et al., 2021, their Fig. 3c), a pattern that has continued since (Fig. 8b, 9b) and accordingly we cannot identify any related change in the structure of the system (Fig. 3)". Whilst an increase in ice flow speed of the Denman Glacier clearly occurred between the 1970s and 2017 detailed examination of the timing of this change (as matched by our own feature-tracking based inferences) reveals that almost all of it happened sometime between 1972 and 1989, with very little change since. We will review this aspect of the manuscript and make appropriate revisions, including additional (own) ice flow velocity data as maybe required.

L291: change "groundling" to "grounding"

Thank you pointing the mistake out, happy to make the change.

L310-315: I don't think the authors can make this claim based on a single transient model run of the Denman/Shackleton system. The Denman Glacier grounding line is currently retreating under present day forcing conditions (Barancato et al. 2020) and this retreat could be susceptible to the marine ice sheet instability, as the bed upstream of the current grounding line position is retrograde. Your model results show > 100 km of grounding line retreat by 2310 over Denman Glacier, which is significant. However, you did not test the response of the system to realistic forcing over the same timeframe, so we cannot make a statement on the sensitivity of the system. Lastly, the Aurora and Wilkes subglacial basins are not included in the model domain, so this last statement is a speculative conclusion rather than one based on your modeling results and should not be included in the manuscript.

This section will be removed from the manuscript as outlined above.

L317-L319: It is a great addition to include model limitations in the discussion section; however, unless you are running a thermal model, I do not believe that the geothermal heat flux is used by the ice sheet model. In addition, the ocean conditions and bathymetry will not impact your model run because you assume near-instantaneous disintegration of the floating ice shelf. In the modeling results that you presented, I would expect the results to be primarily impacted by mesh resolution (I am assuming you are not using adaptive mesh refinement, so as the grounding line retreats upstream, the size of the elements will most likely become larger), poorly constrained basal friction and ice stiffness parameters that do not change in time, use of a 2D stress balance approximation instead of a higher order model, etc. It would be helpful to the reader to know exactly how the limitations you listed impact your model (e.g. poorly constrained basal hydrology leads to a poorly constrained basal friction parameter, ice properties impact the ice stiffness parameter, etc.).

This section will be removed from the manuscript as outlined above.

L321-L337: This paragraph lost me a bit. I understand the comparison to Totten Glacier, but I do not think it is appropriate to dive into such a detailed discussion of the subglacial conditions of Queen Mary Land because it does not connect to the rest of the paper. If the authors want to speculate on the subglacial conditions, they need to tie it back to the conclusions of the paper (i.e. its impact on enhanced ice shelf basal melting rates near the grounding line, reducing basal friction at the ice-bed interface, etc.). Without that obvious connection to tie back to the rest of the paper, this paragraph seems out of place and left me confused.

Both anonymous referees were left confused by this paragraph and it is clear that we need to much better clarify its logic connection with our observations reported earlier. Our discussions as a whole are intended to place our observations within the wider framework of the governing atmospheric, oceanographic and subglacial (incl. solid earth) settings, setting the scene for possible future investigations of key properties and processes that we do not understand well at present (of which there are many). We will do this by comprehensively revising the paragraph and the parts of the manuscript that it connects to.

L340: See previous comment about L285.

Please see our reply above.

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2021-265/tc-2021-265-AC1-supplement.pdf>