Response to Anonymous Referee #1

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 blue 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 have restructured the manuscript to clarify the novelty of the extension in spatial and temporal observations and included additional ice penetrating radar data. Our aim was to provide full context to the observations we report and discuss, rather than simply citing previous work. We have made the distinction as requested; specific changes are detailed in response to the comments below.

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 have removed the modelling aspects from this manuscript and focused 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. In response this comment and the removal of the modelling from the manuscript, we have changed the title of the manuscript which now reads

L1-2: Glaciological setting and structural evolution of the Shackleton Ice Shelf System, East Antarctica, over the past 60 years

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. All of the links in sections 2.1 and 2.2 have been removed and included in a data availability statement. Section 2.1, 2.2 and the data availability statement now read

L 91-120:
2.1 Structure and feature mapping from optical and SAR imagery
Surface structures and features of the Queen Mary and Knox coasts were mapped from satellite imagery using standard GIS techniques (following Glasser et al. (2009)). Structural features have been mapped every 6 months from February 2015 to February 2022 using freely available datasets from Landsat 8 OLI and Sentinel 2A and 2B, where cloud cover is <15%, in combination with Sentinel 1A and 1B GRD to improve spatial and temporal coverage. To include multi-decadal changes in the extent and structure of the whole system, we used several different datasets from three time periods, only choosing the datasets that covered the entire area of interest. These include the declassified ARGON KH-5 images acquired 16th May 1962, Landsat 1 MMS acquired 27th February 1974, Landsat 5 TM acquired between 10th and 12th February 1991 and the MODIS Mosaic of Antarctica (MOA) image map, a composite of 259 swaths of both Aqua and Terra MODIS images acquired between 01 Nov 2008 and 28 Feb 2009 (Scambos et al., 2007). Datasets were registered to the Sentinel-2 imagery as required.
Mapped features included, where visible: the ice-shelf or floating-glacier edges, rifts, crevasses and crevasse traces and longitudinal surface features following the methodology of Glasser et al. (2009). Interpretation of the optical imagery was performed using multiple band combinations to provide natural colour (Landsat-1 MSS bands 7-4-3, Landsat-5 TM bands 5-2-1 and Sentinel-2 bands 4-3-2) and for all imagery standard enhancement procedures (contrast stretching and histogram equalisation) were used to improve the contrast across features. The spatial resolution of the data sets varies from 10 m to 150 m and is thus a limitation on the minimum size and accuracy of the features mapped in each data set.

2.2 Feature tracking from Sentinel-1
Glacier surface velocities were derived using feature tracking between pairs of synthetic aperture radar (SAR) images acquired by the Sentinel-1 satellite. Using the standard Gamma software and following commonly adopted methods, feature tracking uses cross-correlation to find the displacement of surface features between pairs of images, which are then converted to velocities using the time delay between those images (Luckman et al., 2007). We use image patch sizes of ~ 1 km in ground range and sample at ~ 100 m in range and azimuth. Where the time-delay between images is sufficiently short, and surface change is minimized (for instance by very cold temperatures), trackable surface features include fine-scale coherent phase patterns (speckle) and the quality of the derived velocity map is maximized. We applied feature tracking to many image pairs and selected the best velocity map in terms of minimum noise and maximum coverage of high-quality matches for each year to provide mean annual velocity maps, including ice flow direction. We then produced percentage difference maps, scaled between +/- 10% but excluding data where the mean velocities are less than 0.2 m/day as uncertainties in velocity magnitude are around 0.2 m day-1 (Benn et al., 2019). This approach allows us to optimize the quality of the surface strain map derived from the surface velocity.

L 332-337:

Data availability
All satellite imagery used in this work are freely available as follows; Landsat 8 OLI, 5 TM and 1 (all downloaded from https://earthexplorer.usgs.gov/), MODIS Mosaic of Antarctica 2008-2009 (downloaded from https://nsidc.org/data/nsidc-0593/versions/2), Sentinel 2 A and B (all downloaded from https://scihub.copernicus.eu/dhus/#/home)
Sentinel 1A and B GRD (all downloaded from https://scihub.copernicus.eu/dhus/#/home) and ARGON KH-5 (downloaded from https://earthexplorer.usgs.gov/).

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

We are happy to make these the changes. The sentence now reads

L19-21: Recent attention has therefore been focusing on understanding the controls driving Denman Glacier's dynamic evolution, although knowledge of the wider regional context and timescales over which the future responses may occur remains incomplete.

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

We have made 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 has been 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 has been 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 Tt = 40000 Gt / 3600 [Gt/cm] = 11.11 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 has been 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 has been removed from the manuscript as outlined above.

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

We have removed the sentence from the manuscript.

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 have altered the manuscript to reflect the main point that we don't know enough about the system to be able to accurately model potential vulnerabilities. The sentences now read

L33-37: Given the potential vulnerability of the system to accelerating retreat into the overdeepened bedrock trough, better data recording the glaciological, oceanographic, and geological conditions in the Shackleton system are required to improve the certainty of numerical model predictions. With access to these remote coastal regions a major challenge, coordinated internationally collaborative efforts are required to quantify how much the Shackleton region is likely to contribute to sea level rise in the coming centuries.

L34: Insert comma after "accelerating retreat".

We have made 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 the sentence now reads

L51-53: The East Antarctic Ice Sheet (EAIS) has historically been perceived as the stable sector of Antarctica (Silvano et al., 2016); however, it has now emerged that the Aurora and Wilkes subglacial basins of the EAIS have been contributing to sea level rise since the 1980s, with Aurora contributing 1.9 mm sea level rise and Wilkes 0.6 mm (Rignot et al., 2019).

L45: Insert comma after "WAIS"

We have made 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 have removed the Fretwell references and replaced with Morlighem.

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

We have made the change and the sentence now reads

L48-49: The Shackleton system flows from a major drainage basin at the EAIS margin, located at the intersection of the Queen Mary and Knox coasts (Fig. 1a), including major outlet glaciers such as Denman, Scott, Northcliffe, Roscoe and Apfel.

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.

We have made the change, the sentence now reads

L53-54 The Denman Glacier alone is estimated to hold an equivalent of 1.5 m of sea level rise equivalent ice mass (Morlighem et al., 2020).

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

We have made the change, the sentence now reads

L60-63 Ice velocity data from the region are sparse before the late 2000s, but recent work identified an increase in ice velocity of the Denman Glacier of 16 % since the 1970s (Rignot et al., 2019), with an increase of 11± 5 % just upstream of the Denman grounding line between 1972-74 and 1989 and a more recent decrease of acceleration rate to 3 ± 2 % between 1989 and 2007-08 (Miles et al., 2021, their Fig. 3c).

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 have update the manuscript to include this reference. The sentence now reads:

L68-72: Meltwater production from basal melt of the Shackleton system (73 Gt year-1) between 2003 and 2008 rivalled that from Thwaites (98 Gt year-1) (Rignot et al., 2013). Satellite derived basal melt rates between 2010 and 2018 revealed high but localised melt rates of > 50 m year-1 close to the Denman grounding line (Liang et al., 2021), on par with basal melt rates in the Bellingshausen and Amundsen Sea (Adusumilli et al., 2020).

L415-416: 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.

We have removed the sentences from the manuscript.

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 the sentence now reads

L86-89: Here we place the previously reported dynamic changes in the Denman Glacier into the wider regional context of the Shackleton Ice Shelf system. We do so by presenting an improved biannual temporal frequency of observations in the last eight years (2014-22) integrating airborne radar data, new satellite observations of ice structure, changes in ice front position and ice-flow velocities, with known geometrical and glaciological constraints.

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 have removed this sentence from the manuscript as the modelling will be the focus of a separate manuscript, discussed in detail above.

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

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

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 have removed the links and added the information to the data availability statement at the end of the manuscript as described above.

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

We have made the change.

L111: change ";" to ":"

We have made 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.

We have made the change and the sentence now reads

L101-102: Mapped features included, where visible: the ice-shelf or floating-glacier edges, rifts, crevasses and crevasse traces and longitudinal surface features following the methodology of Glasser et al. (2009).

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

We have made the change.

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

We have made the change and the sentence now reads

L112-113: We use image patch sizes of ~ 1 km in ground range and sample at ~ 100 m in range and azimuth.

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

I think this is in reference to the spelling of maximised which has been changed. Apologies if we have misinterpreted the comment.

L126: Change "allowed" to "allows"

We have made the change.

L146: Change “horizontal rate of strain tensor” to “horizontal strain rate tensor”
L151: Change “rate-strain” to “strain-rate”
L163: Please give references to previous BISICLES studies that have used this initialization method.
L173: Change to “The single future simulation follows the methodology of Matin et al. (2019).”
L174: “Under” should be lowercase
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?

These sections have all been 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.

We have added the figure citation to the sentence which now reads

L167-168: The surface of the Denman Glacier is heavily featured with a combination of crevasses, flow lines and channel-like features (Fig. 1b).

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 (now 4), we thank Anonymous Referee #1 for noticing the mistake and we have corrected and checked 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. All rifts have been clearly labelled in what is now figure 4 to allow distinction and the paragraph now reads

L170-174: The floating portion of Scott Glacier is dominated by a series of rifts striking perpendicular to the flow direction (Fig. 4). The rifts initiate approximately 20 km down glacier of the grounding line and widen to ~ 2.5 km as they flow around the Taylor Islands. Between 2015 and 2022 the up-flow (southern) rift widens at a rate of ~ 200 m year-1 (Fig. 4a – labelled 1), while the down-flow rift narrows at a rate of ~ 100 m year-1 (Fig. 4a – labelled 2). The rift formation, widening and narrowing process is evident from 1962 through to 2009 (Fig. 4b). A rift on the eastern side of Scott Glacier, initiated from the eastern Scott shear margin, has increased in length toward Chugunov Island by ~ 5 km between February 2021 and June 2022 (Fig 4c -

labelled 3).

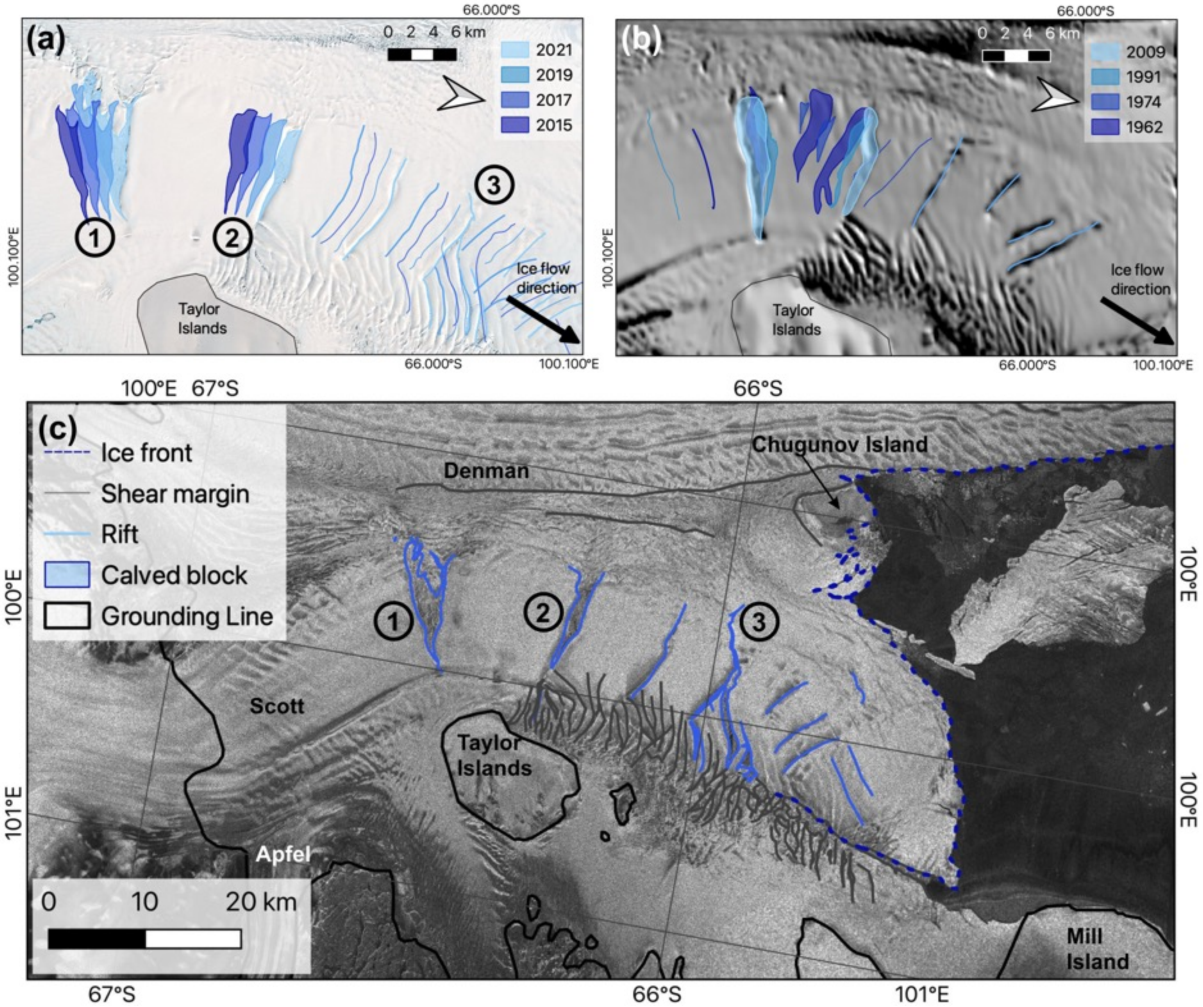

Figure 4: Evolution of the rifts on Scott Glaciers from (a) 2015-2021 mapped from Sentinel 2B acquired 27th February 2021, Landsat 8 OLI acquired 16th February 2019 and 25th March 2015 and from Sentinel 2A acquired 23rd February 2017 (Background: Sentinel 2B acquired 27th February 2021) and (b) 1962-2009 mapped from ARGON KH5 acquired 16th May 1962, Landsat 1 MMS acquired 27th February 1974 and Landsat 5 TM acquired 10th -12th February 1991 and from MODIS MOA acquired 1st November – 28th February 2009 – Modis MOA (Scambos et al., 2007) (Background: MODIS MOA). (c) The floating portion of Scott Glacier in June 2022, highlighting the iceberg that calved from the western front in April 2022 and the rifting across the eastern portion of the front towards Chugunov Island (Background: Sentinel 1A acquired 6th June 2022).

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

We have changed the sentence to read

L181-182: Across the whole system, small-scale changes have been observed in the shear margins separating the various inlet glaciers and along the main body of the Shackleton Ice Shelf.

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 to 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. We have now changed the difference maps to percentage difference maps, scaled to +/- 10 % as this provided the clearest visualisation of change and included a new Figure 7b showing the longest difference period we can produce 2021-2018 (this is as longer period as the data from Sentinel-1 allows), prior to 2018 the data is much nosier and the resulting difference maps are very messy. We have added another figure, new Figure 8, to show the annual difference maps 2021-2020, 2020-2019, 2019-2018 and 2018-2017. Figure 9 provides the recent higher resolution data in the context of longer-term changes, and we have amended the text to clarify that we are only looking at the most recent changes in this paragraph.

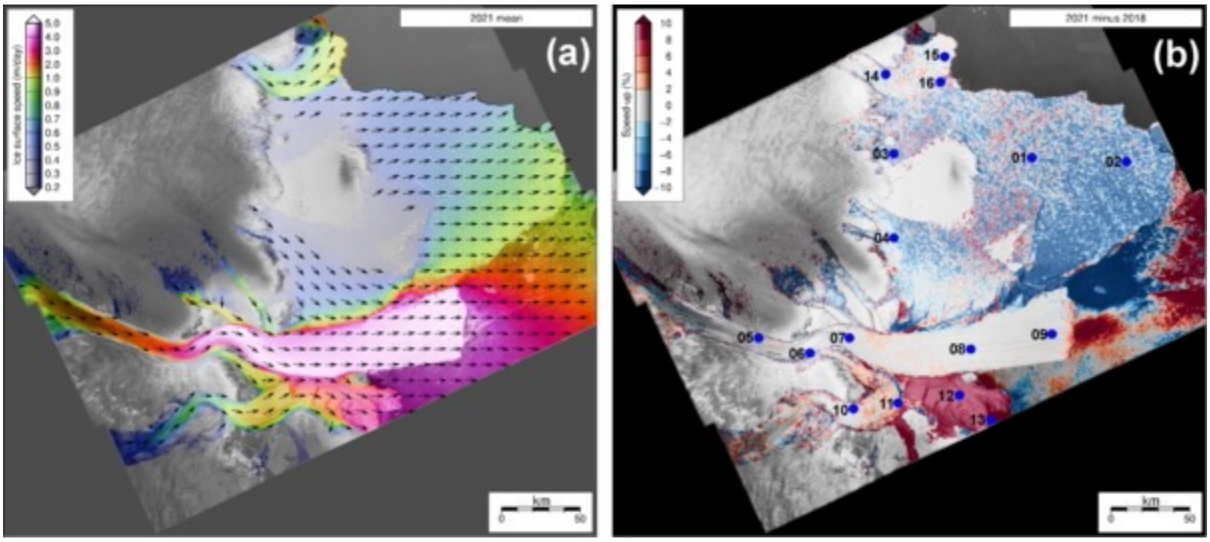

Figure 8: (a) Mean speed for 2022 with velocity arrows and (b) percentage difference in mean speed between 2021 and 2018, scaled between +/- 10%, with point locations illustrating the ice speed timeseries in Figure 10.

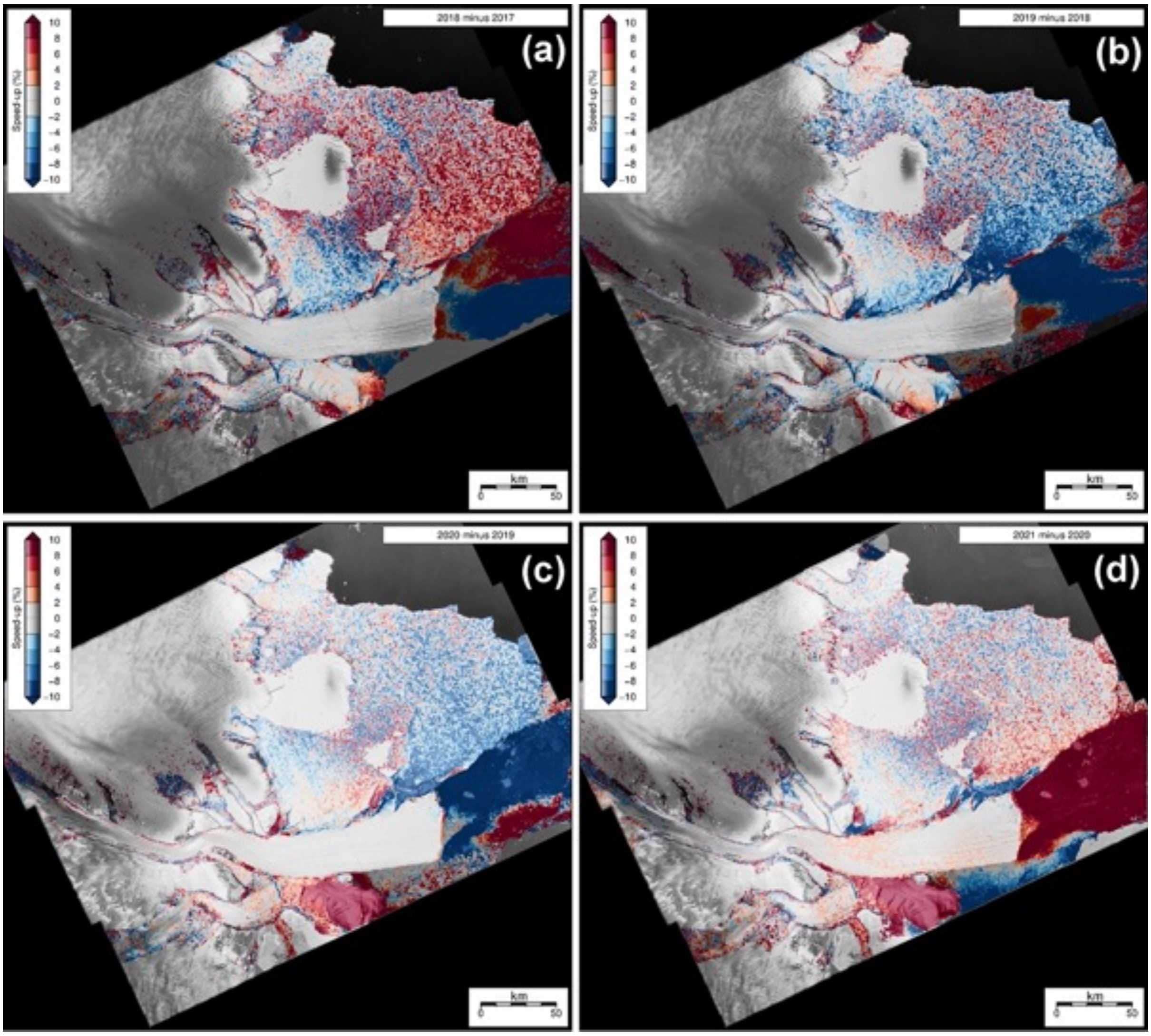

Figure 9: Percentage difference in mean speed between (a) 2018-17, (b) 2019-18, (c) 2020-19 and (d) 2021-20 scaled between +/- 10%.

L205-221: Mean ice speed derived at annual temporal frequency from Sentinel-1 data varies across the Shackleton System. In 2021, ice speed ranges from ~0.2 m day-1 in the area between the grounding line and Masson Island on the Shackleton Ice Shelf to ~5 m day-1 on the floating tongue of Denman Glacier (Fig. 8a). Surface ice speeds observed along Roscoe and Scott glaciers reach 1-2 m day-1 and 2-3 m day-1, respectively. Recent changes in ice speed, derived by differencing the mean speed across the whole system between 2021 and 2018, are confined to the seaward ~ 60 km of the floating tongue of Scott Glacier and the Shackleton Ice Shelf (Fig. 8b). On the floating portion of Scott Glacier increases of > 10 % occur across the outer 50 km (Fig. 8b). The western side of the Shackleton ice shelf, including the fast ice to the western side, appears to have decelerated over the same time period, where a change of between -4 % and -6 % is observed (Fig. 8b). There is some evidence of deceleration on the eastern side of the ice shelf, although the signal is unclear with values ranging between +/- 5 %. Annual ice speed percentage differences illustrate the increase in ice speed on the eastern flank of the ice front of Scott Glacier between 2017 and 2018 (Fig. 9a), which then appears to decelerate between 2018 and 2019, when a 4 % increase in the ice speed of the western flank of the ice front is observed (Fig. 9b). The increase in speed continues through 2019-2020 with a 10% acceleration from the ice front up to 45 km upstream and across the entire ice tongue width (Fig. 9c). The increase extends a further 15 km in the up-flow direction between 2020 and 2021 (Fig. 9d). There is more spatial variability in the annual speed percentage differences across the Shackleton Ice Shelf. The overall trend between 2018 and 2021 appears to be deceleration but there are small regions of acceleration and much of the variability is within the uncertainty bounds, thus complicating interpretation (Fig, 8b).

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

We apologise for the inconsistency in unit form and have checked the manuscript carefully ensuring m day^{-1} and m year^{-1} are used throughout.

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)").

We agree that the statement is confusing as point number 10 is approximately located on the grounding line (as identified from Measures data). We have changed the section to read

L225-237: Point locations on Shackleton Ice Shelf vary between ~ 0.2 m day^{-1} at the grounding line (point 3 and 4 in Fig. 8b) and ~ 1 m day^{-1} towards the front of the floating ice (point 2 in Fig. 8b), with no consistent temporal trends (Fig. 10a). Denman Glacier exhibits higher speeds, from < 2 m day^{-1} upstream of the grounding line (point 5 in Fig. 8b) to ~ 5 m day^{-1} on the floating tongue (point 9 in Fig 8b) but speeds remain constant through time at each point location (Fig. 10b). Scott Glacier has a similar spatial pattern with speeds increasing from ~ 1.2 m day^{-1} at the grounding line (point 10 in Fig. 8b) to > 4 m day^{-1} close to the floating ice front (point 13 in Fig 8b; Fig. 10c). There is no observable change in speed within 10 km of the grounding line of Scott Glacier (points 10 and 11 in Fig. 8b; Fig. 10c). However, the downstream 30 km of the floating ice tongue show significant acceleration from the beginning of 2020 through to May 2021 (points 12 and 13 in Fig. 8b; Fig. 9c). Over the 17-month period, ice speeds increase ~ 30 % to 2.5 m day^{-1} 30 km from the ice front (point 12 in Fig. 8b) and ~ 40 % to 3.2 m day^{-1} close to the front (point 13 in Fig. 8b; Fig. 10c). Roscoe Glacier has similar ice speed spatial patterns to both Shackleton and Denman, with slower speeds of ~ 0.4 m day^{-1} at the grounding line (point 14 in Fig. 8b), increasing to ~1.2 m day^{-1} close to the floating ice front (point 16 in Fig. 8b) and no significant change in speed through time (Fig. 10d).

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 have changed the labels and parentheses and the section now reads

L240-244: Pinning points have previously been identified at the front of the Roscoe - Shackleton shear margin (label-a in Fig. 10) and upstream of rift 2 on the Shackleton Ice Shelf (label-b in Fig. 11, Fürst et al., 2015). There is evidence of two additional pinning points, Chugunov Island at the front of the Denman-Scott shear margin (label-c in Fig. 11) and at the ice margin of Shackleton Ice Shelf (label-d in Fig. 10). The latter coincides with a local topographic high in ocean bathymetry (Arndt et al., 2013).

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

We have made the change and the sentence now reads

L244: The latter coincides with a local topographic high in ocean bathymetry (Arndt et al., 2013).

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 as been 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 has been 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 have provided 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 emphasise later in this paragraph (L282-286 below). 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.

L282-286: 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, 10b) and, accordingly, is not associated with change in the surface structure of the system (Fig. 2).

L291: change “groundling” to “grounding”

We have made the correction.

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.
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.).

These sections have been 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 have comprehensively revised the paragraph and the parts of the manuscript that it connects to. The paragraph now reads

L298-309: On the nearby Totten Glacier, inferences from combined geophysical exploration and numerical modelling are consistent with areas of high basal melt rate coinciding with significant grounding line retreat, possibly linked to channelized subglacial meltwater discharge (Dow et al., 2020). By analogy, the deep trough beneath the Denman Glacier is also likely to favour vigorous channelization of the subglacial meltwater system close to the grounding line. As freshwater outflow into the sub-ice shelf ocean cavity can locally enhance basal melt rates melting near the grounding line (Jenkins, 2011; Wei et al., 2020), the recently observed retreat of Denman Glacier towards the deepest continental trench on Earth could result from complex interplay between the ice, ocean, and subglacial environments. In addition, interaction of Denman Glacier with neighbouring Scott and Northcliff glaciers, as well as with the numerous surface features and pinning points highlighted in this work, could influence how this ice mass responds to future changes in forcing conditions. As this sector of Antarctica holds over 2 m of global sea level potential, it is critical that we continue to both support numerical modelling efforts of this region and monitor short- and long-term changes of the Shackleton system.

L340: See previous comment about L285.

Please see our reply above.

Response to Anonymous Referee #2

We thank Anonymous Referee #2 for their considered review of our manuscript. Below we respond to each comment, with the reviews original comment shown in italic text and our response in blue text.

Review: This manuscript uses a suite of remote sensing data to map changes in ice extent, structure and velocity across the wider Shackleton system. The manuscript compliments recently published work showing recent grounding line retreat and acceleration of Denman Glacier, but also includes detailed and novel observations across the wider understudied Shackleton system. The authors use these observations to conclude that there has been limited change across the Shackleton system across the observational time period. They also then simulate the response the Shackleton system to a hypothetical loss of floating ice to demonstrate the systems sensitivity to any future ice shelf loss.

Overall, I think the manuscript contains some interesting and novel observations of the Shackleton system that are worthy of publication. In particular I think the 50 year record of rifting evolution across the Shackleton Ice Shelf is a very nice contribution. The background here is while there has been some recent studies focussing on Denman Glacier, we know very little about the recent behaviour of the many other glaciers that feed the wider Shackleton system, so these results are valuable. However, at the moment I think these interesting results are somewhat lost in the manuscript. It seems like quite a jump and a distraction from discussing these detailed annual scale observations, to discussing the response of the Shackleton system to the hypothetical loss of all floating ice 400 years in the future, which then turns into a discussion as to how the deep trough of Denman may favour vigorous channelization of the subglacial meltwater system close to the grounding line. At the moment I am not sure what the main focus of the manuscript is. I think the manuscript would benefit from being more streamlined, with a greater focus on the novel observations. I have included some more detailed comments below.

These comments echo those of Anonymous Referee #1 and have inspired us to remove the modelling part of the manuscript and make it the focus on a follow-up manuscript instead. In making appropriate revisions to the manuscript as a whole this will allow us to re-focus the manuscript on the novel observations, as suggested by the Anonymous Referee #2.

Observations: The authors state that there have been no significant annual variations in ice flow speed across the Shackleton system. I would argue that the use of 'significant' is not appropriate, what is 'significant' variations greater than 50 m yr^{-1} 100 m yr^{-1} etc?.

In this context we are using significant to mean greater than the uncertainty and, in this case, we do not find that any changes are greater than the uncertainty. We have clarified this where possible throughout the manuscript.

The plots in Figure 9 give a good overview of the longer-term changes in ice flow speed across the region. However, because they are in m/day and the scales are somewhat stretched it is difficult to determine if there has or has not been any annual variations in ice flow speed. A variation of 0.3 m/day equates to around 100m yr, which would be larger than the uncertainty of the velocity products and would be an interesting result. Are there similar scale variations, particularly in the faster flowing sections of the Shackleton system, to my eyes it looks that there could be, but I could be wrong, I really cannot tell from the plot alone?

May we refer to our corresponding answer to Anonymous Referee #1.

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.

In summary, the use of both m/year and m/day is deliberate as it appropriately reflects the temporal resolution of the available data. Respectfully, in our experience it is usually incorrect to state that "0.3 m/day equates to around 100m yr" because our experience with tracking glacier flow speeds over several decades shows that velocities can change on a daily basis.

In Figure 8 the authors plot an ice speed difference map between 2019 and 2020, I think to illustrate the acceleration of parts of the Scott Glacier. I think these plots are useful in giving a broad overview of the changes in ice speed. Could they also do this over a longer time period, maybe 2000-2010 and 2010-2020 (brackets whatever the availability of velocity data allows)? This would probably be the best visualization of the speed changes over the observational period.

We have now changed the difference maps to percentage difference maps, scaled to +/- 10 % as this provided the clearest visualisation of change (and is in line with the reporting style of others e.g., Miles et al 2019). We have included a new Figure 8b showing the longest difference period we can produce 2021-2018 which as longer period as the data from Sentinel-1 allows, prior to 2018 the data is much nosier and the resulting difference maps are very messy. We have added another figure, new Figure 9, to show the annual difference maps 2021-2020, 2020-2019, 2019-2018 and 2018-2017.

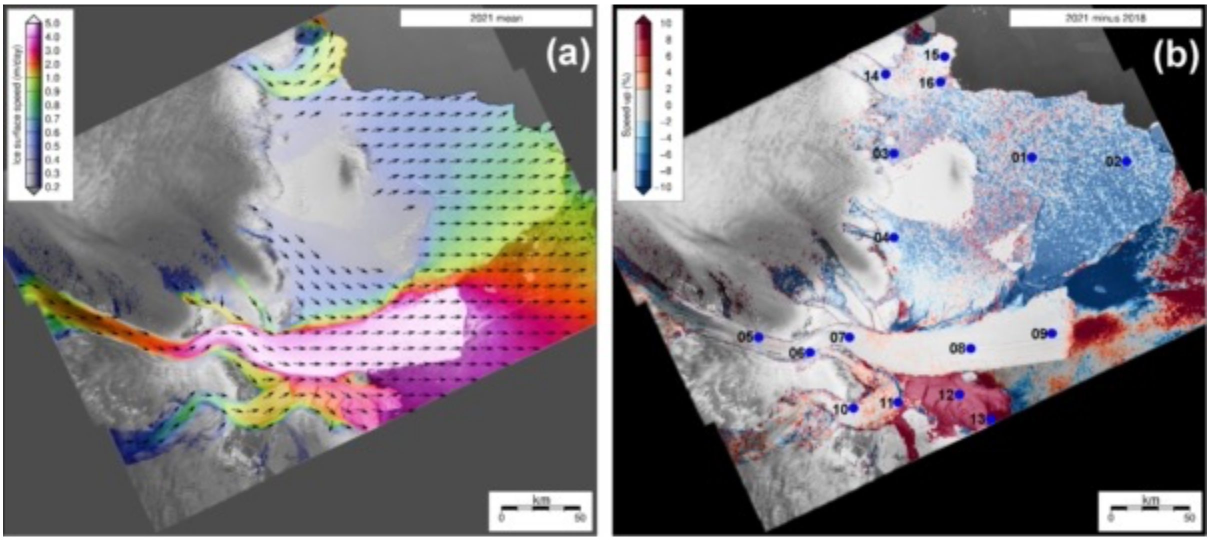

Figure 8: (a) Mean speed for 2022 with velocity arrows and (b) percentage difference in mean speed between 2021 and 2018, scaled between +/- 10%, with point locations illustrating the ice speed timeseries in Figure 10.

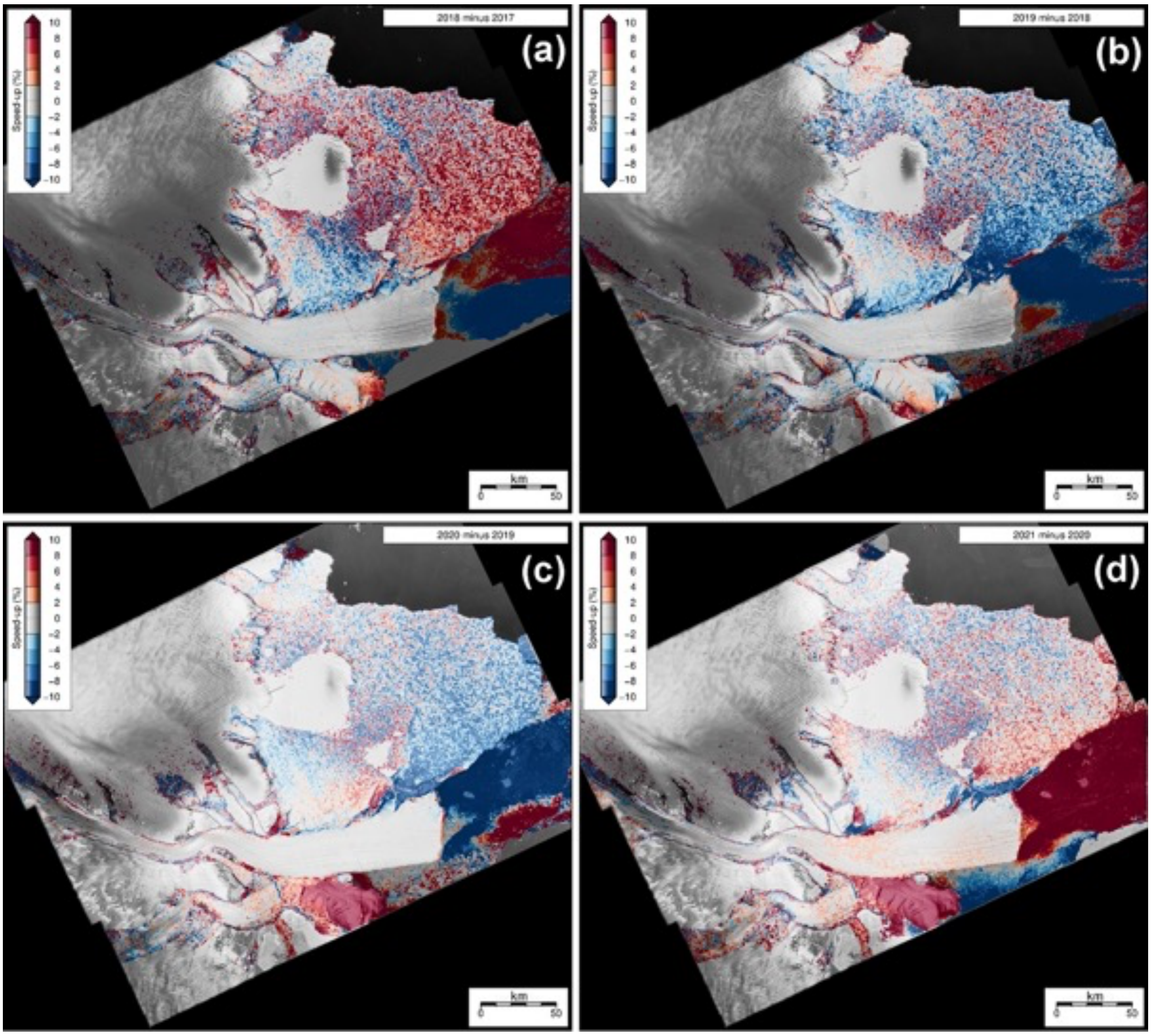

Figure 9: Percentage difference in mean speed between (a) 2018-17, (b) 2019-18, (c) 2020-19 and (d) 2021-20 scaled between +/- 10%.

One of the most striking observations is the migration of the shear margin near Chungunov Island over just a few years. This is a somewhat unique observation. The authors have collated this wide range of velocity data, while they have tended to focus of ice speed, could they also focus on the velocity directional data? Is this change in shear margin caused by the whole Denman ice tongue 'wobbling' or a more localised change to do with Scott?

We can see from Figure 3b that Denman tongue does not appear to be 'wobbling' and all of the evidence indicates that changes are confined to Scott, likely a combination of ice front calving close to Mill Island, the rift opening from Chugunov Island towards Mill Island and the speed increases. We know from previous experience plotting directional arrows onto individual ice speed maps provides a much clearer illustration of any changes in velocity direction. We have added ice speed directional arrows to Figure 8a (see response to previous comment), mean ice speed over the whole Shackleton system for the year 2021. We have also made an additional figure that includes mean ice speed and directional speed arrows for 2017, 2018, 2019 and 2020. We have not included this in the manuscript as it does not show any change in flow direction, but we would be happy to include in a supplement if needed.

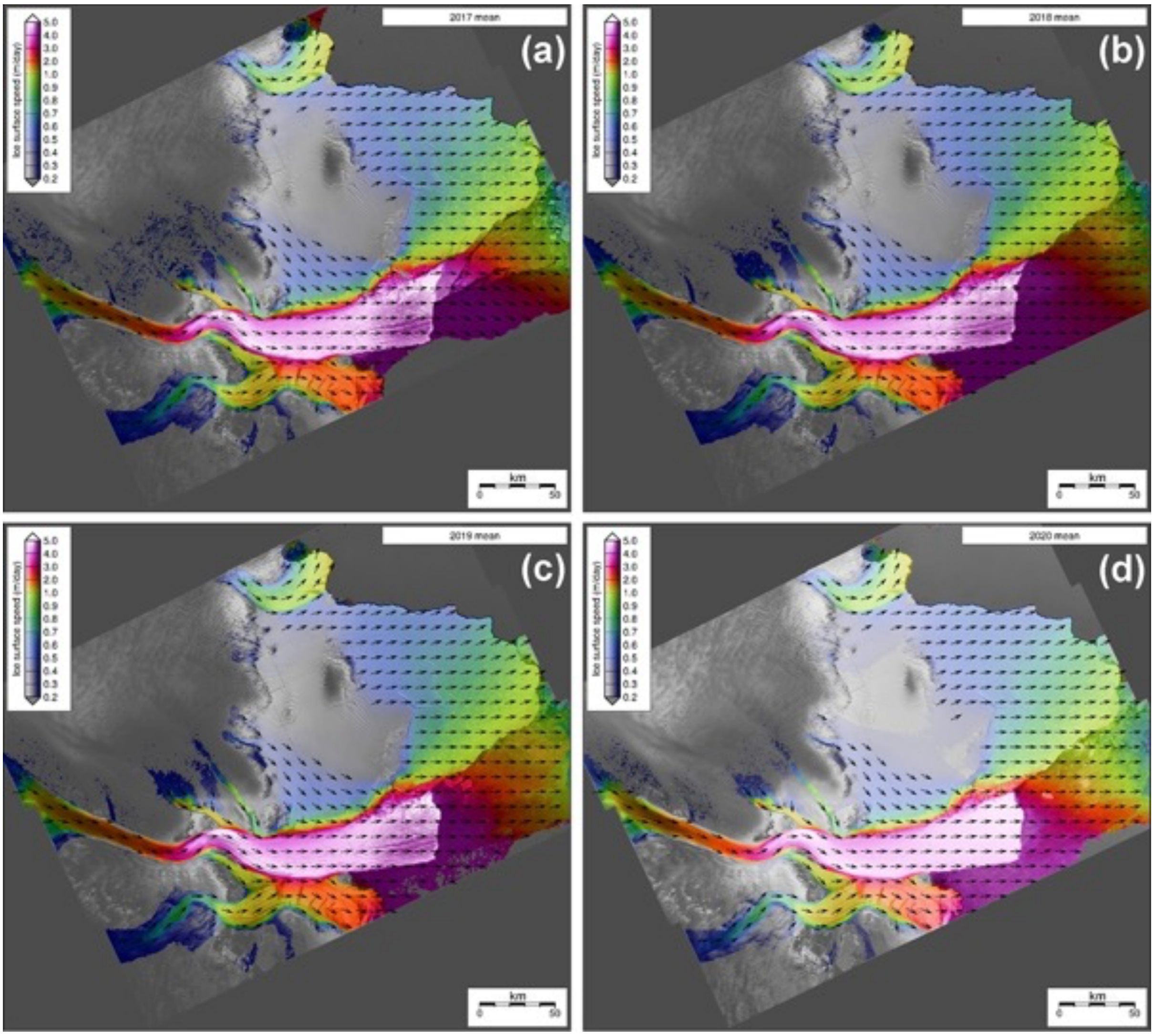

Figure S1: Mean speed with velocity directional arrows for the years (a) 2017, (b) 2018, (c) 2019 and 2020.

Modelling: My expertise lies in remote sensing, so I am not in a position to comment on the methodological details of the modelling. But I did find the description of the modelling experiment carried out to be lacking. In the discussion it is stated that:
'The upper limit scenario of forcing in our BISICLES model runs suggests that noticeable grounding line retreat occurs in the Denman Glacier over the simulated 400-year time period' But in the methods section there is no mention of an upper or lower limit scenario, nor any mention of the timescales of the simulation. Aside from the basic description of model, there is only a very limited description of simulation. It is essential that the details here are expanded.

In wider point, while I think it could be a useful contribution to repeat the experiment in Martin et al., but with BedMachine, I did feel the modelling appeared as somewhat left field in the manuscript and appears as a bit of a jump in the discussion from the main body of observations. The general tone of the manuscript is that very detailed remote sensing observations have shown limited changes in the floating ice in the Shackleton system... But then the manuscript jumps to.. 'now we simulate the unrealistic loss of all floating ice in the Shackleton system'... The scientific rationale for this is unclear to me? Of course, it is entirely up to the authors, but I would point out that I think the detailed observations have the potential to be a nice contribution alone.

The term upper limit was used to indicate the upper limit of change in the system by removing all of the floating ice, rather than the upper limit of a series of scenarios. May we refer Anonymous Referee #2 to our response to Anonymous Referee #1 for an explanation of the anticipated logic of connection between observations and modelling in our original manuscript.

Following up on these matching anonymous referee comments, and also explained in that response, we have separated the observational and modelling components into two manuscripts, removing the modelling from this manuscript which already largely focused on observations.

Specific comments:

Line 59: I would not describe the acceleration of Denman Glacier since the 1970s as a short- term fluctuation

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 have removed the 'short-lived description and emphasised the more recent stability in ice flow speed. The paragraph now reads

L56-64: Despite the lack of an embayment and with the exception of localised fluctuations in ice flow, the Shackleton system has so far shown few signs of major dynamic change, with its flow restrained by islands, ice rises, and ice rumples (Stephenson et al., 1989; Young, 1989). Analysis of Envisat data indicated that the Denman Glacier was thinning by 0.4 m year-1 upstream of its grounding line between 2002 and 2010 (Flament and Rémy, 2012), and a 5.4 ± 0.3 km grounding line retreat was detected between 1996 and 2017–2018 (Brancato et al., 2020). Ice velocity data from the region are sparse before the late 2000s, but recent work identified an increase in ice velocity of the Denman Glacier of 16 % since the 1970s (Rignot et al., 2019), with an increase of 11± 5 % just upstream of the Denman grounding line between 1972-74 and 1989 and a more recent decrease of acceleration rate to 3 ± 2 % between 1989 and 2007-08 (Miles et al., 2021, their Fig. 3c).

Line 62: 'the glacier' – please clarify in the text that you are presumably referring to Denman Glacier.

Yes, we are referring to the Denman Glacier and the sentence now reads

L58-59: Analysis of Envisat data indicated that the Denman Glacier was thinning 0.4 m year-1 upstream of its grounding line between 2002 and 2010 (Flament and Rémy, 2012),

Line 113: Landsat 1 was not mentioned in the above paragraph? Was it used?

Landsat 1 MMS acquired 27th February 1974 was used in Figure 5b and has been added to the paragraph describing the data used, which now reads

L97-100: Ice velocity data from the region are sparse before the late 2000s, but recent work identified an increase in ice velocity of the Denman Glacier of 16 % since the 1970s (Rignot et al., 2019), with an increase of 11± 5 % just upstream of the Denman grounding line between 1972-74 and 1989 and a more recent decrease of acceleration rate to 3 ± 2 % between 1989 and 2007-08 (Miles et al., 2021, their Fig. 3c).

Line 118-129: What software was used for the feature tracking?

The standard Gamma software was used, this information has been added to the manuscript which now reads

L110-112: Using the standard Gamma software and following commonly adopted methods, feature tracking uses cross-correlation to find the displacement of surface features between pairs of images, which are then converted to velocities using the time delay between those images (Luckman et al., 2007).

Line 164: That of Rignot (2011) – Do you mean the MEASURES ice velocity mosaic?

This section has been removed from the manuscript as outlined above.

Line 173-178: I think much more detail is needed here (see comment above)

This section has been removed from the manuscript as outlined above.

Line 189: Not sure 'remarkable' would be the word I would use here

We have removed remarkable from the sentence which now reads

L143: The floating ice front of Scott Glacier has experienced more variability than that of Denman or Shackleton (Fig. 2a).

Line 243-245: Its nice to see the changes between 2019 and 2020, but I think you could also expand to include 2000-2010 and 2010-2020, maybe even 2000-2020

As outlined above in response to general comments, we have now included 2021-2018 percentage difference map which as longer time period as the data from Sentinel-1 allows, prior to 2018 the data is much nosier and the resulting difference maps are very messy. We have added another figure, new Figure 8, to show the annual difference maps 2021-2020, 2020-2019, 2019-2018 and 2018-2017.

Line 250: Please use the correct citations for the Measures and ITS LIVE velocity products. I think the correct details can be found on each respective website.

The correct citations have been included for both and the sentence now reads

L223-225: Ice speed extracted from Sentinel-1 provides a timeseries between 2017-2021, which we extend back to 2002 using MEaSUREs (Mouginot et al., 2012, 2019; Rignot et al., 2011) and ITS_LIVE (Gardner et al., 2018, 2021) in locations where available to highlight variability through time across the system (Fig. 8 and 10).

Line 267: Is d Chugunov Island?

C is Chugunov Island and has been relabelled accordingly, the sentence now reads

L242-244: There is evidence of two additional pinning points, Chugunov Island at the front of the Denman-Scott shear margin (label-c in Fig. 11) and at the ice margin of Shackleton Ice Shelf (label-d in Fig. 10). The latter coincides with a local topographic high in ocean bathymetry (Arndt et al., 2013).

Line 269: The paragraph is discussing modelling results. Therefore, I think it needs a new appropriate subsection, currently it is under 'ice flow speed'.

This section has been removed from the manuscript as outlined above.

Line 285: 'Not changed significantly' What is 'significantly'? Observations of Denman have shown that its grounding line has been retreating and that it has been accelerating and losing mass, this is contradictory.

As explained in our response to Anonymous Referee #1 it is natural for systems such as the Shackleton Ice Shelf to undergo some variability. Within the framework of our regional observations of system structural evolution (and indeed modelling experiments) previous authors' inferences point to relatively minor dynamic changes that do not necessarily point to a system that is on its path to instability. We have tried to clarify the use of significant(ly) where used and changed this sentence to read

L246-247: Over the ~ 60-year period of observation, the Shackleton Ice Shelf system has undergone observable variability in velocity and structure, but there is no sustained longer-term change.

Line 285-302: I think you could be more detailed in this section. One of the key results here is the lack of propagation of the rifting across the Shakleton over the past 50 years, with the exception of some small activity over the past few years. This is a key result. What does this mean and how does it tie into the wider body of literature? In other regions rifting evolution has been key to ice shelf stability? How important could the melange that fills these rifts be (see recent Larour Larsen C paper in PNAS)?, etc..

There have indeed been a series of manuscripts that examine the importance of suture zones and their mélange fills on rift propagation, not least those on Larsen C cited here, as led by some of the current authorship team (Kulessa et al., 2014, 2019, both in Nature Communications), that pre-date the Larour et al. manuscript quoted here. We completely agree, rift mélange is indeed critical to ice shelf stability and have added an expanded explanation to the revised manuscript, which now reads,

Ln 264-276: The lack of significant rift propagation on the main body of Shackleton Ice Shelf appears directly related to the Masson Island suture zone. Indeed, there is growing recognition that the softer marine ice present in suture zones inhibits the growth of large-scale fractures, acting to stabilise ice-shelves by reducing local stress intensities (Kulessa et al., 2014; Larour et al., 2021; McGrath et al., 2014). Suture zones are structurally and mechanically heterogeneous and therefore particularly susceptible to micro-crack formation ahead of the rift tip, reducing the stress intensity around the rift tip and potentially causing rifts to be halted by suture zones (Bassis et al., 2007; Kulessa et al., 2019). In addition, stress intensities around the rift tip are likely to be reduced because of sea-water softened suture-zone ice, relative to harder meteoric ice that is colder and contains less liquid seawater (Kulessa et al., 2019). While current observations suggest suture zones promote stability by halting rift propagation, a strong relationship between the thickness of ice mélange and the opening rate of the rifts has been observed, indicating that ice mélange thinning rather than ice shelf thinning can promote rift propagation (Larour et al., 2021). The warmer temperature and increased sea water content of the ice mélange suggests it may be more vulnerable to thinning due to future surface and basal melting than the surrounding meteoric ice, potentially affecting rates of rift opening and propagation (Kulessa et al., 2014; McGrath et al., 2014).

Line 321-338: I struggle to see the relevance of this section to the manuscript. Fig 1a: The scale is a little difficult to make out because of the transparency

This paragraph left both anonymous referees confused. We had included it to set the scene for processes that may control the evolution of the ice shelf and for possible future investigations of key properties and processes that we do not understand well at present. We have simplified the paragraph and integrated it more closely with the main points of the manuscript. The section now reads

L298-308: On the nearby Totten Glacier, inferences from combined geophysical exploration and numerical modelling are consistent with areas of high basal melt rate coinciding with significant grounding line retreat, possibly linked to channelized subglacial meltwater discharge (Dow et al., 2020). By analogy, the deep trough beneath the Denman Glacier is also likely to favour vigorous channelization of the subglacial meltwater system close to the grounding line. As freshwater outflow into the sub-ice shelf ocean cavity can locally enhance basal melt rates melting near the grounding line (Jenkins, 2011; Wei et al., 2020), the recently observed retreat of Denman Glacier towards the deepest continental trench on Earth could result from complex interplay between the ice, ocean, and subglacial environments. In addition, interaction of Denman Glacier with neighbouring Scott and Northcliff glaciers, as well as with the numerous surface features and pinning points highlighted in this work, could influence how this ice mass responds to future changes in forcing conditions. As this sector of Antarctica holds over 2 m of global sea level potential, it is critical that we continue to both support numerical modelling efforts of this region and monitor short- and long-term changes of the Shackleton system.

Fig 10: The high strain rate band going across the Denman ice tongue – presume there is no evidence of any recent rifting in this location?

No there is not any evidence of rifting in the area, we have examined all of the imagery in detail, but it is an artefact. We have described this clearly in the figure description which now reads

Figure 11: Magnitude of the principal strain rate of the Denman-Scott-Shackleton system derived from Sentinel-1 data. N.B. The feature down flow of pinning point c on the Denman Tongue is an artefact, we see no evidence of rifting in the remote sensing data in this region (e.g., Fig. 2b, 7a).