Glaciological setting of the Queen Mary and Knox coasts, East Antarctica, over the past 60 years, and implied dynamic stability of the Shackleton system
Thompson et al. (2021)

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.

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.

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.
- **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).

- **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.
- **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.
- **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!

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 . . .''
- L17: Shackleton Ice Shelf (use capitalization because it is a proper name)
- 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.
- 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."
- 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.
- 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.

- L32-L34: The sentence “it is clear . . . Shackleton system” can be removed.
- 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.
- L34: Insert comma after “accelerating retreat”.
- 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).”
- L45: Insert comma after “WAIS”
- 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).
- L52: Change “it is supplied by . . .” to “Major outlet glaciers drain into this ice shelf system, including Denman, Scott, Northcliffe, Roscoe, and Apfel Glaciers.”
- 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.
- L65: Change “just above” to “just upstream of”
- 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.
- 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.
- 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.
- 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).
- L96: Remove “in response to coupled ocean and atmospheric forcing”

- 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).
- L107: The “th” on 10th should be a super-script.
- L111: change “;” to “:”
- 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.
- L120: Insert comma between “methods” and “feature tracking”
- L121: “We use image . . .” Use present tense
- L124: “ . . . and the quality of the velocity map is maximized”
- L126: Change “allowed” to “allows”
- 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?
- 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).
- L213: Cite figure 1b here, it shows the high concentration of surface features on the Denman Ice Tongue very well.
- L218: Should this first sentence be citing figure 5 (figure 6 shows the rift on the Shackleton-Roscoe shear margin)?
- 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).
- L229: Replace “some changes” with wording that is a bit more definitive.
- 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).
- 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)
- 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.
- L267: Change "rise in the ocean floor" to "local topographic high in ocean bathymetry".
- 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 . . .".
- 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.
- 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.
- L291: change "groundling" to "grounding"
- 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.).

- 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.
- L340: See previous comment about L285.

Figure Comments:

- General comment: units of ice speed should be changed from m/day to m/year to fit the convention of existing literature.
- Figure 1: In panel-a, the color bar should be flipped upside down so that the color-bar is in increasing order. I believe this panel could also be zoomed in to show more detail of the Knox Basin, rather than showing the entirety of the Sabrina and Aurora basin, as these are not the focus of the paper. Some of the text in panel-b is tough to read because of the numerous crevasse-lines (i.e. Scott Glacier, Denman Glacier). In the legend, I believe the last entry should be “grounded ice” rather than “grounding line”.
- Figure 2: I think this figure can be removed so that it is combined with figure 1. Perhaps in figure 1, you could include a dotted or dashed line to denote the model domain.
- In Figures 3-7, it is difficult to know where we are on the Shackleton Ice Shelf System. It would be helpful to have a small inset in each figure that shows the entire floating ice shelf and a box that shows the domain of each figure.
- Figure 9: I really like this figure, I found myself going back to it throughout reading the manuscript because it does a great job at illustrating your velocity results. Very well done!
- Figure 10: This figure is blurry for some reason, please update with a clear copy.
- Figure 12: Change from real years to model simulation years and change font color so that it is easy readable. Also, in the caption you say that you disintegrate “all floating ice in the Denman Glacier shelf”, which confuses me because in the methods section, you say you apply a ~1000 m/yr melt rate across all floating ice (not just Denman’s ice tongue). Please specify if you are applying this melt rate to all floating ice.
- Figure 13: This figure is also blurry and the x-axis label got cut off. In the top panel, it would be useful to use the other y-axis to show the volume loss in units of cm sea level rise equivalent. Also, in the caption, you say that this is discharge from Denman Glacier, but it is not clear over which bounds the authors are integrating the volume loss (i.e. are

the authors using a flux gate along the Denman Glacier grounding line, or it this actually ice volume loss integrated across the entire domain?). It is a bit odd to me that the main goal of this paper was to observe and model the entire Shackleton Ice Shelf System, but in the captions of figures 12 and 13, the description is only about Denman Glacier (what about retreat and acceleration of Scott Glacier?). This description/analysis is inconsistent with the scope of the paper.

References:

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