General comments:

Neuhaus and colleagues present a new modelling approach to constrain post-last glacial maximum grounding line behavior in the Ross Sea sector of West Antarctica. In this study, the authors explain previously published (Kingslake et al., 2018) radiocarbon data from subglacial and sub-ice-shelf sediment samples using a two-phase model for radiocarbon input and decay to determine the timing of grounding line retreat beyond sampling sites beneath Whillans, Kamb, and Bindschadler ice streams. The timing of re-advance over the same sites was determined using previously published basal temperature gradients (Engelhardt, 2004), porewater chemistry profiles (at Whillans Subglacial Lake; Michaud et al., 2016), and geophysical evidence (at the grounding zone of Whillans and Kamb ice streams; Horgan et al., 2013; 2017). Given that both the style (Swinging gate vs. saloon door vs. marine-based vs. retreat and re-advance; reviewed in Halberstadt et al., 2016) and timing of grounding line retreat in the Ross Sea Embayment (recently reviewed in Prothro et al., 2020) is the subject of active debate in the community, this re-assessment of previously published data has the potential to draw interest from both modern- and paleo-glaciological researchers. However, several points necessitate significant revision before this manuscript is accepted for publication.

Major comments on scientific content:

1. *Assumptions of the radiocarbon model: The main assumption of this study is that any radiocarbon present in subglacial sediment samples in this region comes from the marine environment. This assumption is supported by two previous studies—Kingslake et al. (2018), which is cited in the main text, and Venturelli et al. (2020) which is only cited in the figures. The authors should include further discussion of these two papers in the explanation of their assumptions to make it clear that this point is well-established in previously published literature. The model assumes that radiocarbon was added to these sediments at a constant rate while exposed to the marine waters (line 17-18 of supplement). This assumption should be justified with respect to the proposed mechanism of radiocarbon input to these sediments (228-229 of main text)—was radiocarbon addition set to a constant rate because it is physically realistic for fecal pellets and faunal necromass to be deposited at a constant rate or because it is the simplest way to model input? How much would a variable rate of radiocarbon input change the timing of grounding line retreat? What is the uncertainty that this assumption imposes on the result? In the independent evolution of ^{12}C in this model, further explanation of carbon supply should be included. There is reference to radiocarbon free material on continent with reference to a previous study (Tulaczyk et al., 1998); however the authors set carbon input to 0 after the grounding line re-advances. How would the transport of radiocarbon-free material by the overlying ice streams impact model outputs? A sensitivity test of variable vs. constant carbon inputs would demonstrate that this assumption is sound, similar to what was done for the ice temperature model.*
    1. *Authors' response: Yes, we agree that it is established in the literature that the radiocarbon present in our subglacial sediments comes from a marine environment. We have added a sentence to section 2.3 clarifying that this idea has been previously established. We have also added further reference to, and discussion of, the Venturelli et al. (2020) paper throughout our manuscript. We assumed that radiocarbon was added to the sediments at a constant rate for model simplicity. However, we do examine a wide swath of accumulation rates in our model runs. We like the reviewer's suggestion of sensitivity testing of variable carbon input (for both phases) and have added it to the supplemental.*

Varying the rate of 14C and 12C during phase 1 and of 12C during phase 2 does not alter our results significantly.

2. *Details about radiocarbon model: Whereas the authors do a good job of laying out equations used in this model and provide a graphical depiction of model outputs (figure 6, and S1), significant details of the modeling methods are missing, preventing me making a careful assessment of this key piece of the manuscript. The caption of figure 6 provides a percentage of simulated Fm values that match measured values, however nowhere in the figure caption or text is it stated how many model runs were performed. As written, the manuscript lacks explanations for why the (maximum) 4% match between modeled and measured Fm shown in figure six is significant. Does this mean the model only reproduced the measured values 4% of the time?*
    1. *Authors' response: We agree that including the number of model runs is key information for the reader. We have therefore added the total number of model runs for the radiocarbon model to the caption for the figure presenting the radiocarbon model results. The percentages for our radiocarbon model results are small because we run a very large number of simulations. For each combination of Ti and To, we run the model for different starting concentrations of 12C and different rates of accumulation for 12C and 14C. We choose these ranges based on the results from running phase one of the radiocarbon model for RISP and WGZ (the two sub-ice shelf sites). Thus, for every combination of Ti and To, we run the model roughly 30,000 times. For combinations of Ti and To that produce hundreds of model matches, the percentage of model matches is still small.*
3. *Proposed mechanism of radiocarbon input:* The authors state that radiocarbon in these sediments "likely" comes from fecal pellets and faunal necromass. Have intact fecal pellets or diagnostically Holocene macro/micro/meiofaunal parts been observed in previous micropaleontological investigations of these sediments (e.g. Harwood et al., 1989; Scherer, 1991; Scherer et al., 1998; Coenen et al., 2019) or subsequent investigation herein? If micropaleontological evidence for Holocene particulate carbon input does not exist, it should be made clear that this mechanism is assumed, and the assumption should be defended in this discussion of this manuscript.
    1. *Authors' response: It is well established in the literature that carbon input to the sediments in a marine environment comes from fecal pellets and faunal necromass (Kingslake et al., 2018; Turner, 2015 [Progress in Oceanography]). The abundance of fauna living in the water column at WGZ supports the idea the carbon input to the sediments at our field sites came from fallen faunal necromass and fecal pellets. To help support this in the manuscript, we have added citations from Kingslake et al. (2018) and Turner (2015).*

4. *Uncertainty in retreat ages:* In its current form, the manuscript lacks any quantification of the uncertainty in modeled retreat ages. The interpretation presented in this manuscript suggests grounding line retreat 4,000 years before present at Whillans Ice Stream and 2,000 years before present at Kamb and Bindschadler ice streams. Based on the figures 6 and S1, it seems that the presented values may be the mean output of the radiocarbon model, however it is not clear from the methods or results if this is true. At the very least, it would benefit the manuscript to provide quantitative uncertainty of model outputs. I note that a "frequency of successful model runs" is included in figure eight, but there are no values tied to the color bars. It is therefore impossible to assess the weight of these results or contextualize them with previous studies. Given the noted assumptions in both the radiocarbon and temperature models, a full propagation of uncertainty should be presented to support the presented retreat and re-advance timing. Error bars should be

added to the presentation of timing in figures 7 and 8 and discussion should be added in text about whether or not the modeled timing in this study falls within error or previous studies from this region (Kingslake et al., 2018; Venturelli et al., 2020). The authors are specifically using their modeled retreat and re-advance timing to designate a climate-related forcing mechanism instead of previously suggested sea level and glacioisostatic forcings, but without any presentation of uncertainty it is unclear how reliable this alternative explanation is.

   1. *Authors' response: We have added in error bars to the timing of grounding line retreat and re-advance. We include those error bars in the figures where it is relevant, and include them in the text when we present those numbers.*

5. *Interpretation of geochemical data:* It is well-established that the subglacial sediments in this study contain a mixture of past marine and terrestrial microfossils (e.g. Coenen et al., 2019). Though this point is acknowledged in this manuscript (lines 169 and 301), the authors use bulk geochemical analyses (C:N ratios, Fm, δ13C) to make interpretations about the origin of organic material in the samples herein. At present, the manuscript lacks sufficient discussion of how a multi-source mixture would appear in the results of these geochemical analyses. Furthermore, the assertion of a marine source of organic material is not consistent with the shaded boxes in figure 9. It would benefit the explanation of new data generated herein to compare measured δ13C values to δ13C values for particulate organic carbon and sedimentary organic carbon in the contemporary Ross Sea (e.g. Villinski et al., 2000). This information would improve the presentation of data in figure 9, and further explanation should also be included in the discussion.

   1. *Authors' response: Yes, the subglacial sediments contain a mixture of pre-glacial terrestrial input and marine input. Based on the on the location of the subglacial samples in the d13C vs. C:N plot, we surmise that the majority of the organic material derives from the terrestrial sediment and only a small amount comes from marine sources. We have added a sentence clarifying this in section 4.2. We agree that there is room for further discussion of d13C and are working on analyses for this.*

6. *Temperature model:* In the current form, the manuscript lacks quantification for the uncertainty of re-advance timing. The sensitivity testing (Supplement section 2) indicates that the authors considered uncertainty in assumptions of this model and may therefore be more reliable than the retreat timing determined with the radiocarbon model. The constraint on re-advance timing has wide ranging importance from providing a constraint on Holocene ice dynamics to aiding in the interpretation of microbial data—a point that is very well illustrated in paragraphs on lines 372-394. However, the significance of this important result is overshadowed by a lack of error bars.

   1. *Authors' response: We have calculated error for the temperature analysis and now present it in the relevant figures. We also include the error bars in the text whenever we present our results for timing of grounding line re-advance.*

Uncertainty must be included for both the timing of retreat and re-advance to assess what new knowledge is presented in this manuscript. Without an explanation of the uncertainty in these age ranges, it is impossible to determine if modeled results are significantly different spatially (i.e., the difference between Whillans and Kamb/Bindschadler ice streams herein), temporally (i.e., Is there any overlap in the modeled timing of retreat and readvance at any site?), or from the many studies surrounding the chronology of grounding line retreat in this region (most notable to the sites included in this study: Spector et al., 2017; Kingslake et al., 2018; Venturelli et al., 2020). A discussion of all of these points must be included to set this study apart from previous work in this region. If the timing presented in this paper is significantly statistically different than previous studies in this region, the inclusion of uncertainty has the potential to make this paper impactful for a wide-ranging scientific audience.

Line/Technical comments:

43: Smith et al., 2019 (doi: 10.1038/s41467-019-13496-5) provided a comprehensive review on this topic and should be cited here.

Author's response: Thank you for bringing this paper to our attention. We agree that this paper address the lack of data from below ice shelves and the need for more in order to provide more comprehensive grounding line retreat histories, and therefore should be cited here. We have thus included it.

43: You can/should state that this is the enduring paradigm in the Ross Sea Sector, and that this has been recently challenged [Bradley et al., 2015; Kingslake et al., 2018; Venturelli et al., 2020 (you could even add in Greenwood et al., 2018 if you wanted to include the EAIS portion of the Ross Sea Embayment)]. However, "scientific consensus" is too strongly phrased given the large body of work detailing the active debate in this region. As written, this also contradictory to the "disagreements" discussed in section 4 of the supplement. Some comprehensive reviews (e.g. Prothro et al., 2020 and Halberstadt et al., 2016) of this debate are not cited here or throughout the manuscript, but should be to provide better context on the state of knowledge.

Authors' Response: We agree that "scientific consensus" is too strongly worded. We have re-worded this paragraph to better reflect the evolution of ideas about post-LGM grounding line positions in the Ross Sea. We have also included the suggested citations.

Additionally, we have decided to modify section 4 for the supplement (saloon door vs. swinging gate) and move it into the main text. While we still examine the compatibility of our results to the swinging gate and saloon door models, we now also include a discussion of the role that bathymetry may have played on the grounding line retreat in the Ross Sea. We extend the conclusions of Halberstadt et al. (2016) and Prothro et al. (2020) that the grounding line retreated first in the troughs to the area currently under the Ross Ice Shelf. Bathymetry under the Ross Ice Shelf shows a large trough alongside the Transantarctic Mountains. We surmise that this trough aided the rapid retreat

63-66: There should be some detail added about why you conjecture re-grounding as the explanation for observed unsteady thermal state. Other processes that may explain this observation should be mentioned in text or even tested with your temperature model. A demonstration of how you ruled out these other processes would add strength to the statement in this sentence.

Authors' Response: We agree that this section deserves an explanation as to why we suspect that the unsteady thermal state is due to the grounding of an ice sheet. The high basal temperature gradients observed at KIS, BIS, and UC indicate that there is cold ice present at the base of the ice. The way to get cold ice at the base of the ice is by either lowering the surface temperature, increasing advection (either vertical or horizontal) or by changing the basal boundary conditions. Engelhardt (2004) ruled out lowering of the surface temperature and increased vertical advection (accumulation), settling on a combination of increased horizontal advection and basal melt. However, he required an extremely large increase in horizontal advection and basal melt in order to reproduce the observed temperature gradients. Thus, we believe our explanation of ice sheet grounding (i.e. changing basal boundary conditions) to be more realistic. We have added in this explanation to the manuscript to rule out other processes and justify our choice of boundary conditions.

75-77: Do you plan to include your MATLAB code in the supplement? The reference here and elsewhere in the manuscript make me curious to see it.

Authors' response: We can upload the MATLAB code to GitHub. We are working on cleaning up the code to make it presentable.

87: Explanation for the assumed ice shelf thicknesses (500-1000 m) is needed. If these are based on the modern thickness distribution of Ross Ice Shelf, a citation should be included.

Authors' Response: Yes, we assumed these ice shelf thicknesses based on the modern ice thickness at the Ross Ice Shelf grounding line. We have added in a statement clarifying this.

91-93: Explain why you assume basal freeze on to have occurred after re-grounding. Do you have any age constraint on accretion? Justification for this assumption should be added.

Authors' Response: We do not have age constraints on the basal ice, but we assume that when this ice was part of an ice shelf there was abundant basal melting. Begeman et al. (2018) and Marsh et al. (2016) presented basal melt rate measurements at the grounding zone of the Whillans Ice Stream that showed that there are very high rates of basal melt near the grounding line. Thus, we surmise that any basal ice that may have existed prior to grounding line retreat melted away when the ice was floating. Because there is basal ice present at our field sites now, we conclude that it must have frozen after the sites re-grounded. We agree that an explanation of this is needed, and have thus included one in the manuscript.

95-99: It would benefit this manuscript to add a brief explanation of why you set 10,000 years as the model boundary. I realize that this is tied to the work of Kingslake et al. (2018), but it would improve the clarity of this paper to add a sentence or two to explain this so the reader does not have to search through another paper for this explanation.

Authors' Response: We originally chose to examine the grounding line position over the past 10,000 years because Kingslake et al. (2018) modelled the grounding line retreat over our field sites 9.7 kya. However, we have since decided to alter that time window to cover the past 8000 years rather than 10,000 years because we wanted to use grounding lines which have been interpreted from measurements rather than models (Lee et al., 2017; McKay et al., 2016; Spector et al., 2017). These studies place the grounding line several kilometers north of our field sites ca. 8000 years ago, thus we believe that the grounding line could not have retreated over our field sites earlier than 8000 years ago. We agree that the reader deserves an explanation that does not require them to search through another paper. We have therefore changed the date from 10,000 years to 8000 years, and included a more thorough description of why we chose this date.

98: Should "advanced" be swapped for "retreated" here?

Author’s response: Yes, this should read “retreated” as opposed to “advanced.” We appreciate the reviewer for pointing out this mistake. We have corrected it in the manuscript.

102: Here and throughout, “Subglacial Lake Whillans” should be changed to “Whillans Subglacial Lake” to align with the official place name established in 2018. Further details can be found with the direct link provided below:
https://geonames.usgs.gov/apex/f?p=138:3:::NO::P3 ANTAR ID,P3 TITLE:19707,Whillans%20Subglacial%20Lake

Author’s response: Thank you for pointing out to us the correct naming convention for Whillans Subglacial Lake. We have changed all instances of “Subglacial Lake Whillans” to “Whillans Subglacial Lake” to reflect the proper title.

136, 144: Provide further explanation for the instantaneous change in boundary conditions. Do you assume that there is no transitional phase in which seawater and subglacial water are mixed as the grounding line re-advances, as has been proposed for grounding lines in this region? (e.g. Horgan et al., 2013 doi: 10.1130/G34654.1)

Author’s response: We thank the reviewer for brining to our attention the paper on estuarine conditions below the Whillans ice Stream. We agree that the transition between sub-ice shelf and subglacial conditions is more nuanced than represented in our model. Due to tidal pumping, marine waters are actually able to enter the subglacial system upstream of the grounding line. However, for convenience, we chose to represent this transition as instantaneous in our model. We have added a statement to this effect in the manuscript for clarification.

169: “Meaningless” should be changed to “Chronologically meaningless”. The presence of radiocarbon in subglacial sediments was used in Kingslake et al (2018) to challenge the enduring paradigm of grounding line retreat in this region. That alone makes those data meaningful, and your work in this manuscript has the potential to add further value.

Author’s response: The reviewer makes a good point here. The radiocarbon ages are meaningful, even though Kingslake et al. (2018) were not able to use them to constrain the timing of grounding line movement. We have changed the wording so that we do not diminish the importance of the age results.

176: Sediments intended for geochemical analyses of acid insoluble organic material are conventionally decarbonated using hydrochloric acid. Some explanation should be added about why a different method is used here. Important details about the size of samples (mg? g?) decarbonated using this method are noticeably absent.

Authors’ Response: TOC was estimated directly with C-EA-iRMS instead of indirectly by difference (TOC=TC-TIC). To prepare a bulk sediment sample for direct TOC measurement carbonate extraction, the higher pH of sulfurous acid (pH=4.5) is more specific than hydrochloric acid (pH<1). Carbonate is digested at pH 4 to 5, and at its very low pH, hydrochloric acid may also digest some of the organic material we are trying to measure. Thus, sulfurous acid was used instead.

The sample size used to make the measurements was 8-10 mg of bulk sediment. We have added in this piece of information to help clarify our methods.

225: Space between number and unit (100 g)

Author's response: Thank you for pointing this typo out. We have added the space.

228-229: I make note of my questions surrounding your radiocarbon input mechanism above (#3), but the statement about advection here raises further question. Are you assuming that particulate carbon is being advected under Ross Ice Shelf from the open marine environment or elsewhere in the sub-ice-shelf environment?

Authors' response: Ultimately the radiocarbon-bearing organic matter comes from the open marine environment in front of the Ross Ice Shelf. Organic matter originating from the subglacial environment contains radiocarbon-dead matter.

307: Provide explanation when stating that something is "surprising". If the primary source of sedimentation at your sub-ice shelf sites is melting of basal debris from the overlying ice shelf that you assume accreted following grounding line re-advance, one could expect these samples to be geochemically similar.

Author's response: If one came to this manuscript with the mindset that the sites which are currently subglacial have been so since before the LGM, it would be surprising to find that these sites have similar Fm values to sites which are currently located in the ocean. However, given that the idea that the grounding line retreated over these sites during the Holocene is now established in the literature (Kingslake et al., 2018; Venturelli et al., 2020) this is perhaps less surprising. We will therefore remove the word "surprisingly."

334-340: This paragraph makes it seem like you are assuming that sediments present at the surface when your samples were collected are the same sediments that were present at the surface when grounding line retreat occurred. If I am interpreting your assumption correctly, provide some context for why significant sediment accretion would not have occurred in the ~2000-4000 years between your modeled retreat and sample collection.

Authors' response: Hodson et al. (2016) found no evidence of sediment erosion or deposition at this field site (SLW). Additionally, there has not been thorough vertical mixing of the subglacial till layer due, in part, to the presence of a shallow lake preventing contact between the sediments and the ice, and to the fact that Ti is short. Thus, we expect that the topmost sediments at SLW were deposited when the site was exposed to the ocean. We have added in a sentence to the manuscript clarifying this.

365: Simkins et al., 2018 (doi: 10.5194/tc-12-2707-2018) would be a good citation to strengthen the grounding zone wedge sentence here.

Author's response: We agree that this paper strengthens our argument that GZWs can form rapidly (i.e. multi-year to decadal time scales) and have thus cited it here. Thank you for bringing this paper to our attention.

403-405: Should add an explanation of the proportion of inputs needed to result in the values shown in figure 9.

Authors' response: Based on the position of the subglacial sediments on the d13C vs. C:N plot, we believe that the majority of the organic matter originates from pre-glacial terrestrial C3 plants. However, the sediments also contain a small amount of marine input that is responsible for the radiocarbon. Bu that input is small enough that it does not cause the samples to display a marine signature in the plot. We have added a sentence that make it clear that the marine input is small relative to the pre-glacial terrestrial input.

Figure 2: Error bars for geochemical data (Fm, %TOC, C:N) are needed (and should be propagated through all analyses).

Author's response: We have added error bars to the plots of TOC and C:N. We did not include the error bars for the measurements of Fm because they are smaller than the symbols, and we made note of this in the figure caption. We had accounted for the error in Fm in our analyses, but had not accounted for error in TOC measurements. We therefore re-ran our radiocarbon simulations. The changes in the results were very slight (i.e. did not alter our conclusions) but we did remake Figures 6, 7, and S1 so that they were in keeping with our findings.

Figure 4: Note how many temperature model runs were performed.

Author's response: The total number of temperature model runs performed was 808,000. We have added this number to the figure caption to give the reader more information about our methods.

Figure 5: Are these results for ionic diffusion modeling efforts of all chemical parameters noted in Table 1? How many model runs were performed?

Author's response: In Figure 5 are shown the stacked results of the ionic diffusion modelling of all six chemical parameters noted in Table 1. The total number of model runs used to create this figure was 19,926. We have added this information to the figure caption because we agree with the reviewer that this information is important for the reader to better understand our results.

Figure 6: It is stated in the caption that the model runs are stacked for each core, but it is not indicated how many model runs were performed.

Authors' response: For each core, a total of 103,495,644 model runs were performed. To obtain the number of model runs shown for each field site, one must multiply that number by the number of cores located at each field site. We have added these numbers to the figure caption to provide further context.

Figure 7: Add uncertainties for timing indicated by colored lines. Are they within error of measured values of Venturelli et al (maroon error)? It would be interesting to supplement this point in your figure with some explanation in the discussion.

Authors' response: We have added uncertainties to our estimates of timing of both grounding line retreat and re-advance. Our estimates of the timing of grounding line retreat over SLW and WIS line up nicely with the estimates of grounding line retreat over WGZ from Venturelli et al. (2020). There is some overlap in those timings, although generally it would appear that the grounding line retreated over WGZ earlier than SLW or WIS, which is to be expected as it is over 100 km upstream of those sites. We agree that this is a useful observation, and have thus added it to the discussion of timing of grounding line retreat in section 4.1.

Figure 8: Do the color bars for frequency of successful model runs in 8a have any associated number values? Explanation should be included in the figure caption for what designates a successful model run for grounding line retreat. Figure clarity would be improved by adding a key for the shape points and line colors in 8a rather than an explanation in the caption.

Authors' Response: The color bars are simply the probability density plots for timing of grounding line retreat seen in Figure 7. We have added this information to the caption to clarify what constitutes a successful model run. We have also added numbers to the key so that the frequency of successful model runs can be more easily discerned. We also recognize that the term "frequency of successful model runs" might be confusing because we have been using "model matches" or "positive model matches" throughout the manuscript. We have therefore changed the wording in the figure to "percentage of model matches for grounding line retreat."

Given the number of symbols used in panel a, we find that adding a legend actually makes the figure too busy. We therefore have decided to keep the description of the symbols in the figure caption.

Figure 9: The x-axis label says C_{org}/N_{tot}, but the caption says $C_{org}:N_{org}$ It seems from the supplement and methods that only Total Nitrogen (TN) was measured. Can you clarify?

Author's response: Thank you for pointing out this inconsistency. The figure caption is incorrect. We plotted $\delta^{13}C$ vs. $C_{org}:N_{tot}$ in Figure 9. We have corrected the figure caption to reflect this.

Supplement 143-144: The "Saloon Door" comes from Ackert (2008). This citation should be added in addition to the already included citations for later discussions of this model.

Authors' Response: Thank you for providing us with this citation. We have included it to this paragraph. Additionally, we have expanded the discussion to include discussion of the findings from Halberstat et al. (2016) and Prothro et al. (2020). Because of these changes, we feel that this section is relevant to our manuscript and have therefore moved it to the main text.

Below are citations included in this review that are not already included in the manuscript:

Ackert, R. (2008, October). Swinging gate or Saloon doors: Do we need a new model of Ross Sea deglaciation. In *Fifteenth West Antarctic Ice Sheet Meeting, Sterling, Virginia* (Vol. 811).

Coenen, J. J., Scherer, R. P., Baudoin, P., Warny, S., Castañeda, I. S., & Askin, R. (2020). Paleogene marine and terrestrial development of the West Antarctic Rift System. *Geophysical Research Letters*, *47*(3), e2019GL085281.

Halberstadt, A. R. W., Simkins, L. M., Greenwood, S. L., & Anderson, J. B. (2016). Past ice-sheet behaviour: retreat scenarios and changing controls in the Ross Sea, Antarctica. *The Cryosphere*, *10*, 1003-1020.

Horgan, H. J., Alley, R. B., Christianson, K., Jacobel, R. W., Anandakrishnan, S., Muto, A., ... & Siegfried,

M. R. (2013). Estuaries beneath ice sheets. *Geology*, *41*(11), 1159-1162.

Prothro, L. O., Majewski, W., Yokoyama, Y., Simkins, L. M., Anderson, J. B., Yamane, M., ... & Ohkouchi, N. (2020). Timing and pathways of East Antarctic Ice Sheet retreat. *Quaternary Science Reviews*, *230*, 106166.

Simkins, L. M., Greenwood, S. L., & Anderson, J. B. (2018). Diagnosing ice sheet grounding line stability from landform morphology. *The Cryosphere*, *12*, 2707-2726.

Smith, J. A., Graham, A. G., Post, A. L., Hillenbrand, C. D., Bart, P. J., & Powell, R. D. (2019). The marine geological imprint of Antarctic ice shelves. *Nature Communications*, *10*(1), 1-16.

Villinski, J. C., Dunbar, R. B., & Mucciarone, D. A. (2000). Carbon 13/Carbon 12 ratios of sedimentary organic matter from the Ross Sea, Antarctica: A record of phytoplankton bloom dynamics. Journal of Geophysical Research: Oceans, 105(C6), 14163-14172.

Author's Note: We would like to thank the reviewer for a very thorough review, and for providing excellent feedback. We also appreciate the recommendations for citations.