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.
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?
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.

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.
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, $\delta^{13}C$) 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 $\delta^{13}C$ values to $\delta^{13}C$ 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.
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.

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.

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.

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.

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.

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.

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.

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.

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

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

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)

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.

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.

225: Space between number and unit (100 g)

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?

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.

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.

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.

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

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

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

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

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.

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.

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.

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?

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.

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.