Response to Reviewer 1:

In the revised manuscript entitled, "Did Holocene climate changes drive West Antarctic grounding line retreat and readvance", Neuhaus and colleagues have addressed most of the concerns presented in my first review. The authors have included uncertainties in their modeled chronological constraints on grounding line retreat and re-advance, along with significant differences in timing from the first version of this manuscript. As the objective of this manuscript is to shift away from the glaciological forcing presented by Kingslake et al. (2018) and toward a climate forcing, the timing and inclusion of uncertainties is very important. I note that though the peak model matches presented in the first version of this manuscript may be loosely correlated with modest changes in climate, a correlation between revised timing (with uncertainty) and changes in Holocene climate requires a more rigorous explanation than what is currently presented. My primary concerns remain:

1. Whillans Ice Stream is the only ice stream in the study with two sites along a flowline. This serves as an opportunity to test whether the model produces realistic results. I note that the upstream site (WIS) resulted in an earlier retreat time (4700 years ago) than the downstream site (SLW; 4300 years ago). Because this distribution of timing does not make physical sense, I am wondering if these were typed in backwards? Should SLW be 4700 years ago and upstream WIS be 4300 years ago? If this was not a mistake, this distribution of timing must be further explained. Additionally, there should be further discussion of why WIS and KIS/BIS sites result in significantly different retreat timing, as they are quite close together and would be experiencing similar changes in climate during the Holocene. The addition of Halberstadt et al. (2016)'s marine-based model indicates that the authors have considered differences in underlying geology/bathymetry. It should be stated clearly whether this is the assumed reason for differences in retreat.

Author's Note: With regards to the relative timing of retreat at SLW and WIS sites, our method simply has too large uncertainties to engage in such analysis. The reviewer quotes just a single age for each of these two sites (4300 years for SLW and 4700 for WIS) without any error bars, but the same reviewer implored us previously to quantify uncertainties on these ages, which we did. These uncertainties are large and explain why these SLW and WIS model ages appear to be 'out of order'. Clearly, the grounding-line retreat must have happened first at SLW and then over WIS. The fact that the two ages do not appear to confirm that is just a reflection of the large uncertainties associated with these model ages. Therefore, we prefer to emphasize the entire probability distribution functions (PDFs) that are shown in our Figure 8. The two curves for WIS and SLW clearly show that the likelihood of retreat over SLW and WIS is similar over a wide range of ages.

There are at least a few reasons why the grounding line may have retreated over WIS/SLW before KIS/BIS. For instance, the grounding line of Bindschadler and Kamb ice streams may have been slower to retreat because of the buttressing effect of Roosevelt Island, where the grounding line was 'hung up' until 3200 years ago (which we mention in section 4.1). In addition, the deep bathymetric trough along the transantarctic mountains continues up through the Whillans Ice Stream whereas bathymetry beneath Kamb and Bindschadler ice streams is not as deep. In section 4.3 we speculate that the grounding line retreats first in areas of deep bathymetry. We have added in a sentence to section 4.3 spelling this out more clearly.

2. Do the results support a claim of a climate forcing? Retreat timings computed at BIS and KIS sites significantly overlap with the re-advance timings. The explanation for re-advance is slight cooling observed in the region over the last 2000 years, however, I note that the re-advance timing of these two sites falls closely in line with warm periods in the Ross Sea (Hall et al., 2006; grey shaded box in

figure 8) that the authors state forced retreat at WIS and SLW earlier in the Holocene. The full range of uncertainty for retreat encompasses 4000 years, a range that exceeds any of the associated changes in climate being used to explain the observed retreat and re-advance.

Author's Note: We fully disagree with this assertion of the reviewer. Yes, our estimates of grounding line movements in the study region are associated with considerable uncertainties. We have never tried to hide this fact. But what is the alternative? Is it better to have no estimates at all? Error bars of 4000 years are large but what were the error bars on the scientific understanding of grounding line retreat and readvance in the study region before our manuscript was submitted? It is easy to complain about uncertainties in other people's work but can the reviewer, or anyone else, offer a better alternative? It will most likely be decades, if ever, before anybody is able to collect samples and data that can improve on our estimates. Is a complete lack of constraints the better option? On our Figure 8 we marked with a maroon arrow the result of Venturelli et al. (2020) in which they have estimated that the grounding line of Whillans ice stream retreated past its modern location sometime in the mid Holocene. The uncertainty on their estimate is not much smaller than the 4000 years of uncertainty on our estimate. Yet, GRL reviewers and editors did not deem their result to be pointless, as this reviewer is asserting with regards to our results. Venturelli et al. (2020) concluded based on their results that the grounding line retreated during warm phases of Mid Holocene and then readvanced during Late Holocene. This published conclusion is remarkably like ours even though it is based on fewer samples from fewer locations. Frankly, we are completely at a loss how it is possible to look at our Figure 8 and to claim that we have no basis for hypothesizing that climate warming drove grounding line retreat and climate cooling caused the readvance in the study region. And we emphasize in the paper that this is a hypothesis or a conjecture. After all, even our title ends with a question mark. However much one can complain about the large size of our error bars, the fact is that the error bars for all four estimates of grounding line readvance overlap with the Late Holocene cooling seen clearly in the WAIS Divide record. And all four error bars for our estimates of grounding line retreat overlap with the warmest part of the WAIS Divide record shown in Figure 8. Since when did it become controversial to hypothesize that grounding lines retreat during warm climate periods and advance during cold climate periods? We are taken aback by the suggestion of this reviewer that we should abandon this hypothesis and are not inclined to follow this suggestion. We believe that the reviewer did not present sufficiently meritorious reasons for us to do that. Scientific peer review should not be used as a backdoor to censorship of scientific ideas.

3. Assumptions of the model: Hodson et al. (2016) demonstrated that sediment transport occurs in a cm's thick layer of deformable till beneath Whillans Ice Stream. This mechanism could result in the transport of old (14C-free) carbon to (and deposition at) sites as ice streams flow over them. This point was overlooked in the manuscript and the response to review.

Author's Note: We apologize for not responding to this point previously. This was our omission. In terms of our mathematical model, the bulk of the C-12 input is incorporated in the time-independent component of equation S9, designated with the symbol 'N_o'. Since C-12 is a stable isotope, it does not experience time-dependent decay the way that C-14 does. Hence, any C-12 incorporated during subglacial erosion may as well be included in the time-independent coefficient 'N_o'. We clarify HERE AND NOW that N_o includes the initial C-12 and any C-12 that may have been incorporated during subglacial erosion following. We have also added in a statement clarifying this in section 1 of the supplemental.

Given that the reviewer references the result of Hodson et al. (2016), regarding the cm-thick deforming till layer, it is useful to further discuss the potential implication of this constraint for our work. First, this finding of Hodson et al. (2016) does not necessarily apply to '… till beneath Whillans ice stream' in general as it was based on a short sediment core recovered from the bottom of Subglacial Lake Whillans, a setting that may preferentially favor a thin zone of till deformation. It is not clear to us why this reviewer assigns such great significance to this finding because it seems to have limited implication for our results. The subglacial sediment samples that were used for the C-14 measurements come from a range of depths below the core tops, including some samples that come from >1 meter depths. Yet, all these sediment samples contain C-14. Moreover, we have compiled a plot of sample depths versus C-14 concentrations and there is no trend of the latter with depth. Although the reviewer does not make it clear in their comment, it seems that they have in mind the idea that C-14 is abundant at the top of the till and not present or less abundant deeper down. Our data do not support such a contention. Some of the highest C-14 fraction modern values from subglacial samples come from samples that came from deeper parts of the cores, not from core tops. And, as we said, the data does not give any hint about C-14 fraction modern decreasing with depth. Neither is there a significant variability in C-12 concentration with depth or from core to core. For instance, out of 9 sediment samples from beneath Whillans Ice Stream (SLW and WIS localities), seven have TOC of 0.3% and two TOC of 0.4%. This is remarkable homogeneity of C-12 concentration (which is the main component of TOC) with depth and across horizontal distance of about 100 km. So, even if the few-cm-thick deforming layer of Hodson et al. (2016) is causing erosion, the material that is being eroded has similar C-14 and C-12 concentrations as the material that is already incorporated in the thin deforming till layer. Hence, this erosion will not change our modeling results.

As the authors state in their concluding paragraph, this study presents an interesting and new use of old data. However, it is too strong of a claim to state that the modeled chronological constraints for retreat and re-advance can be explained by a climate forcing when viewed through the lens of the full model output. I recommend that acknowledgement of conjecture come before the last paragraph so that the efforts herein are not oversold as a new/alternative explanation for observed Holocene changes. I would suggest that the future version of this manuscript be reframed to focus on the methodological advances so that sufficient explanation for model assumptions, sensitivity, and uncertainty be presented in the main text.

Response to reviewer 2:

Comments on revised Neuhaus et al

The revised paper is substantially improved from the original version I reviewed and I believe that the authors have addressed almost all of my comments. The figures in particular are clearer, the captions explain them better and uncertainty is better represented throughout the figures. As I noted previously I think the paper is a novel contribution to the debate and provides important constraints for understanding Holocene grounding line retreat in the Ross Sea. I also would point out again the importance seeing novel work using innovative approaches to archived samples: this sort of study is very welcome.

In the abstract I think that it would be helpful to include the word 'primarily' or 'dominantly' when referring to climatic controls. The authors have not ruled out GIA processes, only that they suggest climate was dominant. Indeed in the introduction the authors only say that their results are 'consistent with a climatic forcing..'. So last sentence of abstract could read: Based on these results, we propose that the Siple Coast grounding line motions in the mid- to late-Holocene were primarily driven by relatively modest changes in regional climate, rather than by ice sheet dynamics and glacioisostatic rebound, as hypothesized previously (Kingslake et al., 2018).

Author's Note: We agree with this sentiment that we do not entirely rule out GIA, but rather suggest that our results are more consistent with climatic forcing. We have thus added in "primarily" to the abstract.

A similar edit is likely to be needed in discussion. This is important and scientifically prudent because the authors do not find a complete match and they cannot rule out some GIA control on readvance.

Author's Note: We have added in a similar "primarily" to section 4.4 of the discussion.

In my first review I noted that the fields on Fig 9 (now Fig 3) were plotted in Lamb et al (2006) using wt%:wt% not atom:atom (molar) ratios. My point was that there is a conversion between these two units (x1.17) and this needed to be applied if the authors were to plot their data from the atim:atiom measurements. The response claims that Lamb et al uses atom:atom and that therefore the plot in Fig 3 is unmodified. This is not my specialism but I have to note again I have checked and Lamb et al (2006) note very clearly in the text on their p.30 that they use wt% ratios unless stated otherwise (and their field diagram does not state otherwise):
"The weight ratio of organic carbon to total nitrogen (C/N) is normally measured alongside d13C, and can also help to distinguish carbon sources. Occasionally, the weight ratio is converted into an atomic/molar ratio by multiplying by 1.17; however, the ratio used is not always stated. Here, C/N refers to the weight ratio unless stated otherwise." Lamb et al (2006)
I am not a specialist in this field but unless I have missed something, the response implies that the Lamb et al (2006) fields and the data from this study continue to be plotted on different x-scales in Fig 3 and therefore are offset and the plot is in error. This may need someone more expert to take a look or to ask Angela Lamb directly.

Author's Note: We re-examined what we did to create this figure, and found that the reviewer is correct. We thought that we had converted from wt:wt to atom:atom, but realized we hadn't. We have made the necessary corrections.

Line 645: Additional

Author's Note: Thank you for pointing out this typo. We have corrected it.

Symbology on Fig 9 - the captions in the revised paper are much richer and more helpful than in the original submission but fig 9 still needs some further explanation of the shaded colour bars at the base of the figure - what does the shading mean, what are the red (?) boxes towards their younger ends and so on. Would be helpful to more clearly and explicitly match the symbology to the plots in Fig 8 e.g. by placing a dot at the optimal GL retreat timing (along with the lines for uncertainty) and a box and whisker for the readvance timing. This might need a small zoom in inset for this part of the figure

Author's Note: The shaded color bars were just a different representation of the histograms shown in figure 8. But we like the reviewer's suggestion of changing the symbology to match what is shown in figure 8. We have now changed the shaded color bars indicating timing of grounding line retreat to dots with error bars (as shown in figure 8), and added the boxes from the figure 8 box and whisker plots of timing of grounding line re-advance. We have also made sure to use the same colors that were used to indicate the four sites in figure 8.

The discussion of saloon door and swinging gate is now much better: it is nuanced, and points out clearly that the model results presented here cannot distinguish between these two canonical models. The discussion of bathymetric controls is good but I would note, however, that the dependence of retreat on bathymetry has been discussed in many of John Anderson's papers on Antarctic deglaciation for several decades and he should be cited here. The most recent paper, and perhaps most generally applicable is perhaps this one: Anderson,John B. et al.. 2019. Seismic and geomorphic records of Antarctic Ice Sheet evolution in the Ross Sea and controlling factors in its behaviour, Geological Society, London, Special Publications(2019),475(1):223 https://doi.org/10.1144/SP475.5 but there are several earlier examples.

Author's Note: We agree that John Anderson's work deserves a citation here. We have therefore added in the citation suggested.

Mike Bentley, July 2021