Review of Neuhaus et al – Did Holocene climate changes drive West Antarctic grounding line retreat and re-advance?

Firstly, my apologies that this review is right up against the deadline that I was given – I had some unexpected personal circumstances.

Secondly, for contextualising some of my several comments on figures it is worth noting that I have a mild but common colour deficiency. It might be helpful for the authors to know that as a general rule when small colour symbols (or thin lines) are placed on top of other colours it can be extremely difficult to distinguish the colours and match them to a key. There is now wide debate on this in scicomms literature and some good reviews available to guide authors e.g. https://www.climate-lab-book.ac.uk/2014/end-of-the-rainbow/ – most of R, GMT, MATLAB etc now have colour-vision- friendly palettes available.

This paper aims to address the question of timing of a grounding line retreat and re-advance in the Ross Sea sector. This has been part of a debate for some time on retreat history of the region but this paper particularly follows on from the work of Kingslake et al (2018) which attempted to constrain retreat timing but which yielded dates for retreat that were inconsistent with significant amounts of other observations.

In this paper, the authors take three main approaches to constraining past timing of retreat and readvance: and in each case comparing model simulations to archived (and some new) measurements from subglacial and sub-ice shelf sample sites. First, they use temperature modelling of the basal ice to constrain the timing of grounding line readvance. Secondly they use iconic diffusion modelling of the topmost portion of subglacial sediment to constrain timing of readvance. And thirdly they use modelling of radiocarbon content of subglacial sediment to constrain the period of ocean exposure between grounding line retreat and readvance. The authors then go on to draw some conclusions on the likey forcing factors and conclude that the drivers for retreat and readvance were climatic rather than delayed ice dynamics and glacioisostatic adjustment, as suggested by Kingslake et al.

The paper is based on an interesting idea which has potential to provide insight into the grounding line history, and I very much like the use of archived samples and long-standing measurements. The results will be potentially of interest to a broad community, but they need further explanation and some additional clarity (especially on uncertainty) before the paper should be accepted for publication.

Broad issues

Model details. The derivation of model equations is mostly well dealt with but some other details such as numbers of runs, uncertainty (see below), and how to interpret some of the output (especially in some very rich figures), are not yet clear enough.

Authors' Note: We appreciate the feedback on interpreting the figures. We recognize that some of our figures contain a lot of information and we have added further explanations in the captions to try and assist the reader in interpreting them. We have also added information about the number of model runs to the relevant figures (Figures 4, 5, and 6). We have also calculated the uncertainty in the timing of grounding line retreat and not only added them to the relevant figures, but added them to the text whenever we discuss the timing of grounding line retreat or re-advance.

Model assumptions – in a number of places it is difficult to follow if assumptions/choices are made for model simplicity or are based on a comprehensive observational dataset. For example, a single porosity value of 0.4 (line 126) seems surprising: marine sediments vary significantly in porosity and I imagine that subglacial sediments do too. Similarly the supplementary gives a single value for geothermal heat flux but without discussing any likely range or sensitivity of the model to this parametrisation. Whilst the choice of parameters may be helpful it is difficult to assess the model results without knowing the uncertainty bounds created by uncertainty in parametrisation. Some sort of sensitivity or uncertainty analysis of the model is needed to try and understand the range in final exposure/readvance durations. For example, the plots in Fig 6, which I read as a form of probability density plot translate into single thin lines on figure 7 but without uncertainty bounds included.

Authors' Note: In our models we chose parameters based on a combination of observed and assumed constraints. We will adjust our language to make it more clear which are which. We have also performed sensitivity tests of the models and have found that changing our assumptions does not alter our results that much. We are including these sensitivity tests in the supplemental.

Unmodelled processes: What would be the effect of sediment accretion and/or deformation following grounding line readvance? Given this is the 'type area' for actively deforming till layers it probably needs some qualitative comment on the likely effects of such deformation on the radiocarbon and/or ionic diffusion observations. If it is only simple shear then there would be no vertical movement but till accretion is possible and this would likely lead to addition of deep interior radiocarbon-dead carbon. I think some qualitative discussion on possible post-depositional changes would be important here, as the implicit assumption is that the sediment being sampled was the same sediment exposed as the cavity was closed.

Authors' Note: We agree that our manuscript would benefit from discussion of the effects of sediment accretion and/or deformation. We are working on including this. We have performed sensitivity testing of the radiocarbon model where we allowed sediment deposition (in the form of ^{12}C) to occur during phase 2 (grounding line re-advance). This did not alter the results of the radiocarbon modelling noticeably. We are adding this sensitivity testing to the supplemental.

C:N rationale and treatment: The explanation of why C:N is being measured and analysed does not become clear until the Results in Section 3.3, when Figure 9 is called (out of sequence) and the different fields for marine and terrestrial become clear. I suggest putting Fig 9 much earlier and calling it from the methods where it will be possible to explain the concepts behind measuring C:N ratios and del-13C. This explanation can also then make clear the reasoning as to why terrestrial plants field matter (I presume this is because the radiocarbon-dead material is assumed to come from long-dead terrestrial plant material in bedrock beneath the WAIS but I couldn't see it made explicit anywhere). Note also comments below on use of weight:weight and atom:atom ratios on the same plot.

Authors' Note: Thank you for pointing out that we called Figure 9 out of sequence. We like the suggestion of referring to Figure 9 in the methods as a way to clarify why we're interested in d13C and C:N measurements. We have therefore included a few sentences in the paragraph where we first mention the d13C and C:N measurements that explain how the relationship between d13C and C:N can be used to clarify the origin of the sediments. Incidentally, we do mention in section 3.3 that the sediments consist of a mixture of marine organic matter (which is responsible for the radiocarbon) and *dead C3 plant matter left over from before the continent was glaciated. Because this was not clear enough, we added a line to section 4.2 where we specify that the bulk of the organic matter in the sediments comes from the radiocarbon dead terrestrial matter, which explains the location of the subglacial samples on the d13C C:N plot. The marine matter is important because it contains the radiocarbon, but it is makes up a relatively small proportion of the total organic matter.*

There was some confusion as to the units of C:N throughout the manuscript. We have cleared this up so that all values are now presented in atom:atom (see comments to Figure 2).

Presentation of (radio)carbon data on sediments: It would be helpful to include analytical error bars plus site means and standard deviations on this plot. There is a lot of discussion of 'average' values and ranges and so the descriptive statistics should be presented.

Authors' Note: We have added error bars to Figure 7 indicating the extent of our uncertainties. Additionally, in the text we now include the error bars whenever we talk about the timing of grounding line retreat or re-advance.

Paragraph 307-318 – I've misunderstood something here: I couldn't follow the description of the dataset in this paragraph against Fig 2, which is the only figure cited. It seems to suggest that Fm values at the different sites are variously similar or different but in ways I couldn't see. For example, Fm values at KIS and BIS are supposed to be similar but those at WIS and SLW different from the former. I disagree – there is only one value at BIS and that is similar to 2 of the three values at KIS but very different to the third value, and the range at KIS looks comparable to ranges at WIS and SLW, albeit with a very small n. If this is actually referring to model output, where is it illustrated? Apologies if I have missed something here.

Authors' Note: We made a mistake when plotting Figure 2a, which accounts for the confusion here. Our analysis in the text was correct, and that can now be more easily understood by looking at Figure 2a. We appreciate the reviewer for bringing this to our attention so that we could fix it.

I think the same section uses 'statistically independent' (which has a very specific meaning) when 'statistically distinguishable' might be what is actually meant.

Authors' Note: Yes, the reviewer is correct. This should be 'statistically distinguishable' rather than 'statistically independent.' We have corrected that in the text.

Comparison to climatic forcings: Without seeing the uncertainty bounds of the results it is difficult to assess the robustness of the conclusions. To be clear I think the paper raises some really interesting questions about the driving factors for grounding line retreat but without a greater assessment of model output sensitivity and uncertainty on durations/dates it is difficult to comment firmly on the conclusions. Whilst I understand why the authors are drawing out differences to Kingslake et al I think it is important to note that Kinglake et al used an ice sheet model to explain forcing mechanisms and they found that they could not initiate readvance without including some sort of GIA processes or buttressing from ice rise formation. Their ice sheet model was forced by a similar temperature record to the one described in Fig 7 and so a simple comparison of timing of events probably needs to be tempered by a discussion of whether temperature changes would be sufficient to initiate readvance. I also note the use of Hall et al (2006) sea ice record as a proxy for ocean warming – there may be better records of Holocene ocean temperatures such as Cunningham et al (1999) (The Holocene) which are based on oceanic proxies alone (Hall et al note that their record may reflect both oceanic and atmospheric forcing).

Authors' Note: We have now added error to the timing of grounding line retreat and re-advance.

Kingslake et al. (2018) were unable to re-create grounding line re-advance without GIA, although in their sensitivity testing they found that altering the ocean parameters had a large effect on the timing of grounding line retreat and re-advance. The results of Lowry et al. (2019) directly contradict Kingslake et al. (2018). Our results are more consistent with the results of Lowry et al. (2019), and dissecting why that is the case is beyond the scope of our paper. We are working towards helping to establish an observational basis for Holocene grounding line dynamics in the Ross Sea embayment. Our results disfavor the idea of Kingslake et al. (2018) that glacioisostatic adjustments were the most important to the Holocene dynamics of grounding lines in this region. If that were the case, then the re-advance should happen early in the Holocene when rebound rates were fast. Instead, our evidence points to alignment between grounding line retreat and re-advance phases with warm and cold (respectively) climatic phases observed in the regional paleoclimate proxies. We cannot comment on why different ice sheet models differ in their reconstruction of Holocene grounding line dynamics in our study region.

We appreciate the reviewer suggesting the Cunningham et al. (1999) study. We have included the bounds on their warm period in Figure 7 (which contains our results for the timing of grounding line retreat and re-advance) as well as discussing the timing of this warm period compared to our results. The grounding line retreat over SLW and WIS falls decidedly during their warm period in the mid-Holocene. The retreat over KIS and BIS coincides with that warm period within error. But the grounding line re-advance over all four sites falls within the cool period proposed by Cunningham et al (1999). We will still keep discussion of the two warm periods proposed by Hall et al, but we believe that our argument for oceanic influence on grounding line position Is strengthened by adding the Cunningham et al. (1999) paper to our discussion.

Some line-by-line issues:

44 – this hasn't been a consensus for quite a while, starting probably with Bradley et al 2015 but also including Matsuoka et al 2015 (Earth Science Reviews) which discussed smaller-than-present configurations and the GIA and climatic mechanisms that would explain Crary ice rise etc.

Authors' Note: We agree that it is too strong a statement to say that the idea of unidirectional post-LGM retreat was the consensus until 2018. We intended to convey that the idea of post-LGM grounding line retreat and re-advance has only been in the literature relatively recently (within the past 5-6 years). We have re-worded the paragraph to make this more clear. We have also added in the suggested citation. Thank you.

58 – the comparison to Greenland is not as useful as it might be – the main part of the GrIS that retreated and readvanced in the Holocene is in the SW where it is mostly a terrestrial margin, and so unlike the WAIS ocean and GIA forcing would not be possible.

Authors' Note: We chose to mention the Greenland Ice Sheet in our introduction because there is already evidence that the Greenland Ice Sheet experienced Holocene fluctuations, which we are arguing WAIS did as well. Irrespective of the exact mechanisms, both show sensitivity to the relative minor Holocene climate fluctuations. Thus, we feel that this comparison is relevant to the introduction.

92 – "chose the freezing point of freshwater" (insertion of 'fresh' reduces confusion as to whether you were just sticking with seawater (line 83) after Phase 1). Also, is there an argument to look at a 3-phase model where the heat flux sets the basal temperature in a subsequent Phase 3 (a la Bindschadler) after a period of basal freeze-on

Authors' Note: We agree that this should read "the freezing point of freshwater," to avoid any confusion that we might be examining the freezing point of ocean water. We have thus made the change in the manuscript.

We did consider using the geothermal flux as a basal boundary condition, but we ultimately chose not to do this because we know there to be lots of water present under these sites. Using the geothermal flux to constrain the basal temperature gradient only works when there is no water and ice is frozen to the bed. Fisher et al. (2015) found that when water is present, the geothermal flux and the flux of heat through the base of the ice are not the same. It would be difficult to recreate the steep observed basal temperature gradients using the geothermal flux as the basal boundary condition because it would quickly lower the basal temperature gradient. The results using that basal boundary condition would indicate that the ice has been grounded for an even shorter period of time than we currently estimate. Therefore, our results are actually a conservative estimate of the length of time the ice has been grounded.

98 – 'retreated' rather than 'advanced' otherwise I don't think this makes sense

Authors' note: 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.

148 – not equation 7

Authors' Note: Thank you for pointing out this inconsistency. We actually intended to refer to equation 3. We have made the correction so that we now refer to the correct equation.

240-244 – I'm not quite sure what this is describing – can you explain the inference that comes from the grounding line constraint of 8000 and why the preceding 4000 years is used.

Authors' Note: In order to constrain some of the variables used in the radiocarbon model (A and N_0) we ran the model for RISP and WGZ. We needed to assume a time window over which the grounding line could have potentially retreated over WGZ and RISP. We previously chose 10,000 years ago as the starting estimation based on the grounding line retreat model in Kingslake et al. (2018). We then added 2000 year error bars to that time, which is how we ended up modelling the radiocarbon input at RISP and WGZ 8000-12000 years ago. However, we have decided to change time window we examine to 8000 years ago to base the timing of grounding line retreat over RISP and WGZ on dates from Spector et al.

(2017) and McKay et al. (2016) which place the grounding line at Ross Island 8.6 kya and at Beardmore Glacier 8 kya. We chose to make this change so that our dates are based on geologic dating methods rather than modelling. Our previous time window did include 8000 years ago. We now will examine grounding line retreat over WGZ and RISP 8000 ± 1000 years ago. We have changed the wording in this paragraph to explain this more clearly.

242 – should this read 'earliest' rather than 'latest'?

Authors' Note: No, this should read "latest." We see how this could be confusing because 12,000 is a larger number than 8000. But 12,000 years ago is earlier than 8000 years ago. Thus, we will keep this as "latest."

293- add reference 14/12 value as a line to figure 2a.

Authors' note: By definition, the "modern" fraction of modern radiocarbon is 1. Fm is measured in reference to the modern ratio of ^{14}C to ^{12}C. We have added a parenthetical clause clarifying this in the text. However, adding a reference line to Figure 2a would prove difficult as the measurements of Fm for our sites are very far from Fm=1, and extending the y-axis to include 1 would prevent us from seeing any variation on the measurements of Fm between our sites. Thus we have chosen not to include the reference line in Figure 2a.

297 – add ratio as a line to Fig 2c

Authors' note: We agree that it is useful to compare the C:N from our sites to the reference value for the ocean, and have thus added this value as a reference line in Figure 2c.

298 – Fig 9 – called out of sequence (see comments above about reordering this figure)

Authors' Note: We have moved Figure 9 as per the reviewer's suggestion. See earlier comment regarding the new ordering of this figure..

308 – I don't see eight sample points – I see 8+3

Authors' Note: Thank you for pointing out this mistake. First, there are in fact seven rather than eight total measurements of Fm from SLW and WIS. The mean and standard error calculations are correct, but we miscounted the number of points during the writing process. We have corrected this to read "seven" instead of "eight." Secondly, there was a mistake in the way that we plotted Figure 2a, resulting in more points appearing at WIS and SLW. This did not affect our analysis in any way, but we have corrected the figure so that the correct points are plotted.

325-327 – use of ‘model’ to refer to (I think) 2 or 3 different models. Would really help to specifically refer to radiocarbon, temperature or ionic models – I lost track of the logical thread here.

Authors’ Note: We agree that the fact that we refer to three separate models in this paragraph can be confusing. We have therefore taken up your suggestion of specifically noting which model we are referring to throughout the paragraph.

328 – do ‘model matches’ on axis of Fig 7 mean the same as ‘positive models’ here

Authors’ Note: Yes, these are the same. We have used ‘positive model outcomes’ and ‘model matches,’ which we realize is confusing. We have therefore decided to just use ‘model matches’ for consistency.

323-324 – refer to Figs 7 and 8 together

Authors’ Note: We are slightly confused about the line numbers presented here. It does not make sense to include references to Figures 7 and 8 here as the conversation largely revolves around the half-life of radiocarbon and using the temperature and ionic diffusion models to constrain T_i. However, if the reviewer is referring to lines 331-333, we agree that both Figures 7 and 8 contain relevant information and should therefore both be referred to. We have therefore included the reference to Figure 7 as well as all of Figure 8 (rather than just Figure 8b) to lines 331-333.

404 – what is ultimate origin of terrestrial plants?

Authors’ Note: The carbon that shows signatures of C3 plants is likely from tundra flora that was present before Antarctica was glaciated. We would expect to see evidence of past C3 plants in Antarctica because they are typically found in cold, wet environments, much like Antarctica was before it was fully glaciated. In fact, plant fragments and pollen have been found in sediment cores collected at our field sites (Coenen et al., 2019). To convey this in the text, we specified that the C3 plants were pre-glacial. We also added a sentence specifying that while the majority of the carbon in the sediments derives from dead C3 plant matter (which explains the position on the d13C vs. C:N plot), the radiocarbon comes from a small amount of marine matter which was deposited when the grounding line retreated.

408 – the values of C:N at UC are lower than at WGZ

Authors’ Note: The reviewer makes a very good point. The C:N measurements from UC are, in fact, lower than those from WGZ. It is likely that WGZ receives an input of subglacial sediments that cause the samples to have higher C:N ratios. We have added a statement in the text clarifying that the UC samples have a lower C:N value than WGZ and that part of the explanation could be due to this subglacial input.

415 – Bradley et al 2015 give a series of observations that support retreat and readvance including observation of (unstable) readvance of grounding lines on reverse slopes, amongst others.

Authors' Note: While Bradley et al. (2015) discuss observations that support the idea of recent grounding line re-advance, their study focuses on the Weddell Sea. We were only considering the Ross Sea in the discussion in this paragraph, although we did not specify that clearly. So discussion of the findings of Bradley et al. (2015) is not entirely relevant to this paragraph, although we do cite them elsewhere in the manuscript. We have therefore added information letting the reader know that we are only discussing the Ross Sea region in this paragraph.

Figures and Tables

Figure 1

I can't see a cyan diamond. Would be helpful to define abbrevations in the caption to save constantly referring back to the text.

Authors' note: We apologize that the cyan diamond is not clearly visible. We have changed the color to white and changed the shape to a star so that it is hopefully more visible. We have also added the full names of the field sites to the caption as suggested. Additionally, we changed color scale on the bathymetry so it more closely resembles the scale for the bathymetry shown in Figure 4a of Tinto et al. (2019).

Figure 2

add reference lines (see above)

Authors' Note: We agree that adding a reference line to Figure 2c showing the typical ocean C:N is useful for putting our C:N measurements in context, and have thus included the line in Figure 2c. However, we have opted to not include a reference line in Figure 2a showing the modern standard Fm. The modern Fm is by definition 1, and extending the y-axis to include this would make it difficult to compare the values of Fm between the sites.

Caption suggests C(org):N(org) but I think it should be C(org):N(total).

Authors' note: Thank you for pointing out this typo. We have corrected it so that the caption now reads C_{org}:N_{tot}.

Caption – '...matter from.....'

Authors' note: We thank the reviewer for pointing out this typo. We have corrected it.

Caption suggests the C:N ratios of data in the paper are plotted as atom:atom but looking at the fields used from Lamb et al (2006) I believe they were plotted in the original paper as weight ratios. So the weight:weight column in Table S1 needs to be plotted, not atom:atom. I think this will lead to a shift of *1/1.17 for all your data points. All text, including caption, plus results and conclusions will need to be checked to see if the correction changes anything in text.

Authors' Note: There was some confusion with the numbers included in the table. The values of C:N for the RISP samples were atom:atom, but the values for the other samples were wt:wt. We have corrected this so that all values are shown in atom:atom. However, in Figure 9, all values of C:N were previously plotted as atom:atom as were the fields from Lamb et al. (2006). Thus the changes made to the table do not affect our results and conclusions.

Figure 3

Labels for T(o) and T(i) should be centred over their durations on the diagram, not placed at their end points otherwise there is confusion that they are dates not durations.

Authors' Note: We appreciate the reviewers suggestion of centering T_i and T_o. We have added brackets above the diagrams so that it is more clear that T_i and T_o refer to durations of time.

Figure 4.

Thin colour lines not clearly discernible on top of background colour palette for temperature gradient

Not clear why there are 7 lines for 3 sites – I thought it was perhaps for different cores ? If so, please note this in caption (I couldn't find how many cores there were at UC).
It would be very helpful to give the observed values in a table somewhere – otherwise it relies on ability to read off a contour from these colour plots. This would also allow the uncertainties in the observed gradients to be provided.

Authors' Note: Thank you for pointing out that the lines are not easily discernible over the colormap showing the results of the temperature gradient. We decided to change the way that we present the results so that they are more easily understood by the reader. We now present histograms showing the distribution of the model matches (i.e. dates when the model output fell within 10% of the observed basal temperature gradients). We have also included a representation of the median and 32^{nd} - 68^{th} percentiles in the upper portion of the figure to better illustrate the uncertainty. Also, in the process of improving the presentation of results from the temperature model, we noticed a mistake which we have now corrected. Thus, the results from the temperature model are slightly altered.

Figure 5.

y-axis caption – not clear. Number of what ? (runs ?) Overlapping with what ? (presumably within 2-sigma 'match' to observations ?) Is this number or a percentage ? (looking at the next figures you use percentages)

Authors' Note: Thank you for pointing out that the label on the color bar is not clear. What we meant to convey with "Number of Overlapping 2σ" was that the color shown on the figure corresponded to the number of ionic profiles produced by the model that fit the observed ionic profiles within 2σ. We were stacking the results of six chemical parameters. What we were really trying to say was the "number of model matches." We agree that that is much less complicated to understand, and we have changed the label to "Number of Model Matches."

Figure 6.

These are quite rich diagrams and I can see the potential but it would be very helpful to include a caption that walks the reader through interpretation. E.g. "The key shows the percentage of xx model runs that............Fig 6c shows that at KIS, ocean exposure durations, T(o) of approx. 1200- 2200 yrs are preferred by the model but the model provides less constraint on preferred grounding line readvance duration, T(i)"

756 - I can't follow what this means – produced *what* simulated values of Fm and TOC ? (presumably all the runs produced simulated values of these two variables?). And what do you mean by 'fit measured values' – within 2-sigma ? Some other measure? How many runs of the model do these % correspond to?

Authors' Note: We agree that these figures contain a large amount of information, and therefore deserve more explanation in the caption. We like your suggestion of walking the reader through the interpretation, and have thus included it in the caption. We have also added the total number of model runs performed for each field site to the caption. Finally, we have added the bounds on T_i for each field site as determined from ionic and temperature diffusion modelling. This should help explain how we arrived at the curves shown in what was formerly Fig. 7.

Figure 7

Worth reversing x-axis so that time goes same way as in rest of paper.
What are the thin curves plotting ? – I'm presuming it is the optimum ('highest percentage') values of To and Ti taken from Fig 6? How does the stacking work ? The results in fig 6 look like they are combined results for each site already.
These curves in Fig 7 also need some expression of uncertainty on them – for example Fig 6b shows that the curve for WIS should have broader uncertainty ranges than the curves for KIS in Fig 6c but at present this is not reflected in Fig 7.
Explain which constraint is plotted in the solid bars for T(i) – is it the temperature modelling?
As with figure 6 – the caption could be more helpful to a reader – it took me some time to work out that (I think) it is the peaks in the thin curves that I am looking at for most likely exposure-readvance duration combinations and which therefore are the most likely date for initial retreat past the site?

Authors' Note: We agree with the reviewer that this figure contains a lot of information and could therefore be confusing without a clear figure caption guiding the reader to the interpretations. We have therefore re-worded the caption to be more descriptive. The colored curves are plotting the probability distribution of the results from the radiocarbon modelling. The difference between Fig. 6 and this figure is that we add T_i and T_o together and only examine the model results that fall within the error estimates for T_i obtained through temperature and ionic diffusion modelling. To clarify which results we mean, we *have added dashed lines to Fig. 6. What we were trying to convey by calling the curves "stacked" was that all cores are taken into account for each field site, but the reviewer is correct that we have already done that for Fig. 6. Thus, we have removed that confusing statement in our re-wording of the figure caption. We have also added error estimates to both the timing of grounding line retreat and grounding line re-advance. We also added a statement clarifying that the estimates of T_i presented in this figure come from the results of the ionic and temperature diffusion modelling. Finally, the way that time has been presented on the x-axis in this plot is the same as in Figures 4-6. We therefore think it makes sense to keep it in this configuration.*

Figure 8

The blue and orange colour bars for frequency of successful runs have no scale. Cyan diamond not visible to me
Add WIS and BIS/KIS flowlines to panel b.

Authors' Note: We apologize that the cyan diamond is not visible to you. We have changed it to a white star to make it more visible. The color bars correspond to the probability density plots in Figure 7. We have added in percentages for context. We are working on incorporating the flowlines from WIS and KIS/BIS to panel b.

Figure 9

See notes above. ND not needed in axis label for x-axis

Authors' Note: We have removed [ND] from the x-axis label.

Supplementary

77 – Celsius

Authors' response: Thank you for pointing out this typo. We have corrected it.

Eqn S14 – should this be m(b) rather than m?

Authors' response: No, in this instance "m" stands for "meter" and is part of the unit of measure [ºC/m]. It is not the variable m_b.

147 – if your model cannot distinguish between the two conceptual models then should say so

Authors' Note: Yes, our results are compatible with both the "swinging gate" and "saloon door" models. We have added in a statement articulating this. We have also decided to modify this section and move it to the main text because we feel that it is relevant to our discussion. In addition to comparing our results to the swinging gate and saloon door models we include a discussion on the role that bathymetry plays on grounding line retreat. We build on the conclusions made by Halberstadt et al. (2016) and Prothro et al. (2020) that the grounding line retreat initiated in the troughs and extend it below the current Ross Ice

Shelf. There is a large trough that runs along the Transantarctic Mountains, and we surmise that this could have aided the rapid grounding line retreat seen in Spector et al. (2017).

155 – the reference here to provenance relates to flowline (and geology) differences rather than any differences in timing behaviour and so I don't think it is relevant

Authors' Note: We agree that the reference to flowline and geologic differences is perhaps not relevant to this section. However, we have also slightly changed this section to include discussion of the sensitivity of grounding line retreat to bathymetry. In making those changes we removed this reference because we felt that it did not contribute to the conversation.

Table S1
units missing from several columns

Authors' response: Thank you for noting the omission. We have added units to the columns where the units were missing.

Make core nomenclature clearer e.g. in first row is this the data for RISP Core 7 at a depth of 46.6cm or for core 7-4 at 6.6 cm ?

Authors' note: We apologize for the confusion. We have reformatted the column and added dashes to make it more clear which numbers represent the core number and which represent the bounds on the section of the core.