Dear authors,

Thank you for addressing the points raised in the previous round of reviews and for submitting a revised version of your manuscript.

This is an extensive study that addresses several important research questions using complementary information from the analysis of field data and numerical modelling. The manuscript is significantly improved from the previous version and the two strands of the study are now clearly linked. The inclusion of additional tables is very useful and the restructuring that has taken place in several sections has improved the flow and coherence of the text. You have included additional detail in the methods sections, but many points remain unclear, and you will see a large number of queries in relation to this section below. The results are generally clearly summarised but justification for a couple of the key points mentioned in the abstract require a little more care when interpreting and communicating the results – these are addressed in my major points below.

I know that you will be frustrated to receive another detailed review. None of the points should require additional work to be carried out and therefore my decision is 'publish subject to minor revisions', but addressing the points below will require some careful edits in terms of explaining your methods and interpreting and reporting your findings.

Pippa Whitehouse (Editor)

Major points

Drygalski Ice Tongue (abstract line 7, section 5.1): as far as I can tell, presence or absence of the ice tongue cannot be inferred from the flowline modelling because the ice tongue does not play a role in controlling the dynamics/thickness of the glacier, it does not provide any buttressing. To make statements about the history of the ice tongue you would need to demonstrate that the observed thinning history cannot be replicated unless the ice tongue was present from a specific time. Since the data/modelling presented in this study does not provide any insight into the history of the ice tongue, please review the relevance of any related text about local ocean conditions (e.g. lines 441-447).

The evidence for the presence/absence of the ice tongue does not come from the modelling, but from the lowest exposure ages at Hughes Bluff. Thus, this is an interpretation from field data and not a modelling interpretation. We have already cited the relevant studies from Terra Nova Bay that support our conclusion. We have clarified our interpretation and discussion point in the text

Text changed to: The new geological reconstruction of ice surface elevation changes at Hughes Bluff shows a rapid lowering of the David Glacier at 6.5 ka and a period of slow thinning from ~6-4 ka. Given the marked slowdown in thinning rate from ~6 ka at Hughes Bluff (Figs 6B and 7B), we suggest that stabilisation of the Drygalski Ice Tongue occurred after ~6 ka. This finding is consistent with Orombelli et al. (1990) and Baroni and Hall (2004), who mapped a series of raised beaches along the TNB coastline that mark beach depositional processes in an open ocean setting (i.e. no grounded ice) initiating at 7.2 ka. Stevens et al. (2017a) show that the modern Drygalski Ice Tongue is essential for the development of the modern TNB Polynya. Thus, If the Drygalski Ice Tongue formed at ~6 ka as our exposure chronology from Hughes Bluff and the raised beach chronology from TNB suggests, it is likely that the TNB polynya has also existed since this time.

Relative timing of retreat and thinning: the abstract (lines 8-9) states that 'simultaneous thinning along the Transantarctic Mountains occurred ~3 ka after the retreat of marine-based grounded ice'. I could not find any explicit support for this statement in the results, discussion, or conclusions. A good place to address this issue might be in section 5.3 (e.g. around lines 506-510), drawing on information presented in the results. See also comments relating to lines 512-514.

We now provide the range of data-ice sheet model mismatch of ~4-7 kyr. We now unify this in the abstract and give it another mention in section 3.5.

Abstract text changed to say: Our work, along with ice thinning records from adjacent glaciers, shows simultaneous glacier thinning in this sector of the Transantarctic Mountains occurred 4-7 ka after the peak period of ice thinning indicated in a suite of published ice sheet models.

Modelling methodology: despite useful edits to the methods section, many aspects of the modelling remain unclear, as evidenced by the large number of minor queries below. As you address these queries, please review the order that information is provided to the reader and make sure that the following are clearly stated: which parameters or variables are fixed, which are tuned to fit modern/LGM constraints, which vary over time/space, what is the experiment duration, how/when are changes in model forcing implemented?

Addressed in 'minor' points below.

Data-model comparisons at Mt Kring: Section 3.1 states that bedrock at the summit of Mt Kring (~300 m above the ice) has an exposure age of ~550 ka and that the highest erratics are ~180m above the ice (exposure age ~7.2 ka). Text on lines 344-345 implies that you assume the site has experienced ~200 m of Holocene thinning, but text on line 434 implies that you assume the ice surface was above the summit of Mt Kring at the LGM. Please clarify upper and lower bounds on the magnitude of Holocene thinning at this site to enable comparisons to be made with the modelling.

Text in Section 3.5 changed to 'at least 171 m of thinning'. Lower bound is modern ice surface.

We remove 'the level of Mt. Kring' on line 433 as section 5.1 discusses coastal thinning and impacts on local oceanography.

In various places (e.g. lines 391, 407) you state that the modelling agrees well with the data constraints at Mt Kring or modern observations (e.g. line 393). However, the final modelled ice surface at Mt Kring is several hundred metres below present in many of the experiments (this mismatch is acknowledged in relation to one of the experiments on line 401). I suspect the statements about a good fit are based on the fact that the modelled ice surface passes through the data constraints for some experiments (e.g. M3, fig. 9B; MS1-3, fig. 11B), but the continued thinning predicted by these experiments is in stark contrast with the fact that the present surface of the ice sheet is ~30 m below the lowest data point (fig. 4). Please review your assessment of the model fit to Holocene changes at Mt Kring.

We agree we need to be more careful here. We have now made table 3 and the text in section 4 consistent. For LBR experiments to indicate '…results in an unrealistic final ice surface elevation 100s of metres below observed modern ice surface'. For combined experiments, we include: 'For experiments MS1-MS3, the modelled upper ice surface reconstruction at Mt. Kring lies below the modern ice surface.' Further, we have referenced this effect in our model set up to highlight that during basal traction tuning, this effect is present but minimised.

<u>Minor points</u>

Abstract and Introduction

Line 14: text states that retreat and thinning is 'initiated by *interactions* between enhanced sub-iceshelf melting and reduced lateral buttressing'. This is not supported by the results of experiments M2/M3/S2/S3 (see figures 9 and 10) which show that retreat and thinning can be initiated solely by enhanced basal melt or a reduction in buttressing. Edit text to remove the implication that interactions between processes are necessary to initiate retreat.

Text changed to say: 'We show that glacier thinning, and marine-based grounding line retreat is controlled by either enhanced sub-ice shelf melting, reduced lateral buttressing, or a combination of the two, leading to Marine Ice Sheet Instability.'

Lines 15-16: Figure 8 demonstrates that rapid thinning *is* captured in previous large-scale modelling efforts; perhaps clarify what you mean by 'this period' (line 16)

Text changed to say: 'Such rapid glacier thinning events during the Mid- Holocene are not fully captured in continental or sector- scale numerical modelling reconstructions for this period.'

Lines 41-48: simplify this bullet point. Suggest starting at "Ice load reconstructions constrained by cosmogenic dating…" and say that the reconstructions are needed to model GIA, which (i) may play an important role in controlling ice sheet grounding line dynamics, and (ii) is needed to interpret gravity-based estimates of contemporary ice mass balance

We prefer to keep the text as it is – we are leading in from the general geological perspective and then going into detail about the processes and then how cosmogenic nuclides can help. We think this flows better than the alternative suggestion.

Line 72: text in this paragraph repeats and expands on earlier material. Consolidate the text and review the overall structure and flow of the Introduction.

We have consolidated the text. We believe this improves the overall structure and flow of the introduction.

Line 82: 'evidence of a lingering ice shelf' – when?

The point of this statement is to highlight the lack of coherent chronologies in the Drygalski Trough. Thus, we cannot say when. Text changed to: 'Marine sediment cores from…provide evidence of a lingering ice shelf…14C ages mean that the timing of this ice shelf presence is uncertain.'

Methods

Line 109: 'sufficient erosion' – sufficient for what? Text changed to: 'significant erosion'.

Line 114: 'would have been elevated to the former ice margin prior to deposition' – text is a little ambiguous, please clarify the process described here

Text changed to 'would have been brought to the glacier surface by upward-flowing ice before being deposited at the ice margin.

Line 151: equations 5 and 6 are revised versions of equation 3 – which version is used in this study? In general, try to be more specific about how the equations presented here are used in your study.

We add text: We also incorporate a width term, including f_{lat}, a lateral buttressing factor, to account for lateral buttressing along a coupled ice stream-shelf. Thus, as in previous applications of this model (Nick et al., (2010), Jamieson et al., 2012 and Whitehouse et al., 2017) the final modelled depth (*H)* and width *(W)* averaged ice flow *(u)* is computed using the following equation:

We also modify equation 6 and include corresponding description of $f_{lat.}$

Equation 2: define all terms as soon as they are used

All terms in equation 2 defined directly after being used.

Line 163: '…equation *3* is modified…'

Changed to: 'For an ice stream, equation 3 is modified by including…'

Line 172: how is the statement about mapping geomorphic features related to the equations?

The offshore glacial geomorphology (for example the distribution of mega-scale glacial lineations) provides evidence of the former width of the ice stream, and provides the justification for having a width term in the equations. In the text, we clearly say that the width term (e.g. the equation) allows modelling of changing width as observed in the geomorphology.

We change text: 'To determine offshore trough width, we map all glacial geomorphic…' and draw attention to a more detailed description of the flowline and width follows on in section 2.2.1.

Line 180: MISI does not consider the role of the ice shelf, so it is not clear what sort of feedbacks are described here. Do you seek to understand how ice shelf buttressing can modify the MISI process?

Changed: we delete 'associated with marine ice sheet instability'. This is because we are looking at grounding line changes overall and how ice shelf changes may impact it and thus as you note, MISI is not the key here.

Line 180-181: how is a reduction in lateral buttressing implemented within your model?

See above regarding inclusion of f_{lat} term and corresponding description.

Line 190: method used to define the onshore flowline-parallel width is unclear and not replicable

Text changed to: 'We then construct a flowline that follows the centre of the ice stream and thereafter calculate ice stream width perpendicular to this flowline *(W)* and reaching to a defined lateral ice stream margin across the entire domain. Following Jamieson et al., 2012), the lateral ice stream margins in the offshore part of the domain are determined using geophysical data to map the distribution of trough-parallel mega scale glacial lineations (MSGLs) that are indicators of past ice stream flow, and thus show the width of the ice stream. Onshore, the lateral margins are defined by the valley width. The ice stream width perpendicular to the flowline is thereafter used to control the lateral stress applied by coalesced ice along the flowline (Eq. 6).'

Line 195: somewhere, it would be useful to explicitly state which parameters are tuned in the modern experiments. Some information is in Table 1, but this could be better signposted in the text

We modify text: '…we tune basal friction *(B)*, accumulation, sub-ice shelf melt rate and the rate factor *(A)* in order to reproduce, as closely as possible, the modern geometry and flow speed of the David Glacier.

Line 214: should basal melt rate be listed as a user-defined parameter on line 197? How is basal melt implemented within your model? (no mention of this in section 2.2)

We have listed basal melt rate now – see above.

We also add new text immediately before section 2.2.1 which indicates how accumulation (due to a later comment) and sub-ice shelf melt rate are applied thus: 'Accumulation in the model is applied using modern rates measured at the central flowline and multiplied to account for the ice stream width *(W)*. Accumulation is then scaled to represent warmer or cooler conditions in our experiments (see section 2.2.2). Where an ice shelf is present, sub-ice shelf melt rates are applied as a linear function of the depth of the ice shelf draught. From a minimum rate of 0.1 m yr^{-1} at 0 m (the ocean surface) SIMR increases to a maximum value at a depth of 500 m, and where ice shelf draught is deeper than 500 m, the maximum value is applied.'

Line 219: what is the modelled time period? Stated later, but reader needs to know before this point

Changed to: '…throughout a 2,000 year initiation period.'

Line 222-224: you state that you model basal stresses of 100 kPa, but then go on to say that Zoet et al. (2012) predict higher values which are consistent with your modelled values. The logic does not hang together.

We clarify the sentence thus: 'Resultant modelled estimates of basal shear stress approach 100 kPa in this setting. While difficult to constrain with in situ measurements, Zoet et al. (2012) suggest higher stresses such as these should be expected near the modern day grounding zone which is consistent with the modelled stress distribution in this study.'

Table 1: (i) units are missing for temperature and sub-ice-shelf melt rates. (ii) What is implied by a negative melt rate? (iii) Please expand on the fact that you only list 'maximum' basal melt rates. (iv)

Clarify whether the values listed in the final column relate to glacial or deglacial conditions – it may be useful to document in the caption how/when the different conditions are applied in the model.

(i) Added degrees C, m/yr. (ii) Removed negative signs throughout manuscript, (iii) This should now be clear from the newly added section indicated how sub ice shelf melt rates are applied. (iv) We change deglacial to glacial.

Change table 1 caption to say: ' Parameter values used for sensitivity experiments indicating choices for model tuning to modern conditions over a spinup period and subsequently applied deglacial conditions. SIMR=sub-ice shelf melt rate. Accumulation values reported as percentage of RACMO2 (van Wessem et al., 2018).'

Line 230: need to explicitly state that the model does not account for isostatic deformation (along with other model limitations that only become apparent on line 412

Changed to: '…without introducing uncertainty involving variable along-flow isostatic response and dynamic topography associated with the long-term evolution of the Antarctic subglacial topography (Stern et al., 2005; Whitehouse et al., 2019; Paxman et al., 2019) because our ice flow model does not adjust for isostatic deformation as it evolves.'

Note that we do not indicate the other limitations at this stage and prefer to leave them in-situ.

Lines 235-236: In the authors' rebuttal, it is explained that this text describes the approach used to ensure a stable grounding line during the first 7,500 model years. However, that is not what is implied by the text, which includes confusing information about tuning transient changes to ensure a stable LGM (how does this relate to model time?) configuration. How can applying transient changes in temperature and accumulation result in a stable configuration? How is the tuning carried out? What is 'the modelled period'?

'Transient' was not the correct word choice, and neither was 'stable'. Text changed to: 'To account for environmental changes during deglaciation, accumulation and internal ice temperature are tuned over the modelled period to ensure an LGM configuration which is consistent with geological constraints and which has a grounding line that is not moving significantly.'

The detail of the tuning and the period are further down in the paragraph, so we do not clarify them further.

Line 243: why are we told that accumulation was 75% of modern at 15 ka BP? How does this information relate to the forcing applied in the numerical experiments?

Text changed to: 'We use a scaling relationship between modern accumulation patterns and estimate that accumulation at the start of the model run was roughly 75% of modern accumulation (Veres et al., 2013).'

Line 244: 'we increase this value' – what value?

Text changed to: 'Internal ice temperature is increased through time to represent the increase in temperature that occurred during deglaciation. The internal ice temperature during deglaciation is not known and for this study, so we used values of -25C for the first 7,500 model years and -20C during the remaining 7,500 model years as a way to represent an appropriate amount of warming in the ice column.'

We remove the final sentence of this paragraph indicating we robustly tested accumulation and temperature, which we did not.

Line 246-247: it is not clear how accumulation changes over time are applied within the model

We have added some detail immediately prior to section 2.2.1 which indicates: 'Accumulation in the model is applied using modern rates measured at the central flowline and multiplied to account for the ice stream width *(W)*. Accumulation is then scaled to represent warmer or cooler conditions in our experiments (see 2.2.2).'

Line 247-248: 'we are able to demonstrate…' – this result (which perhaps belongs in the results) is not robust because you do not investigate the impact of varying the temperature or accumulation values within the model, despite acknowledging (line 254) that they are poorly constrained

We agree and remove the sentence: 'By accounting for changes in accumulation rate and internal ice temperatures during deglaciation, we are able to demonstrate these were not responsible for driving modelled grounding line retreat.'

Line 250: 'user-defined parameters…' – explicitly state what these are and what values they take

Text changed to: 'Deglacial sensitivity experiments use a range of accumulation and internal ice temperature forcings representing the potential scale of change experienced through a deglaciation (Table 1), to explore transient changes in lateral buttressing reduction (LBR) and sub-ice shelf melt rates (SIMR) in order to isolate their relative influence on glacier thinning and retreat.'

Line 250: 'optimised…forcings' – how are they optimised?

Text modified – see above – e.g. 'optimised' not the right phrasing.

Line 257: it is not clear how progressive changes in forcing are applied – over time within a single experiment or by running a suite of experiments, each with different boundary conditions?

Text changed to: 'We therefore run a suite of individual experiments that allow us to initiate grounding line retreat by either linearly increasing SIMR or decreasing lateral buttressing over a 500 year period, with each model run applying different perturbed values for SIMR or LBR (Table 1). For combined forcing experiments (MS1-3), we alter both SIMR and lateral buttressing as above until the grounding line retreats to a near modern configuration.'

Line 265: 'a forcing perturbation is applied' – be more explicit about what this entails

Text changed to: 'All sensitivity experiments run for 15 kyr with an initial spin up period lasting 7.5 kyr for SIMR forcing and 8.5 kyr for LBR forcing, at which point the forcing perturbation relating to increased SIMR or reduced lateral buttressing is applied for the remaining modelled period.'

Results

In a few places, additional information is needed to explain your interpretation of the field data:

- Line 296: ‘Two bedrock exposure ages … suggest significant wet-based glacial erosion’ – the reason for inferring wet-based erosion from these ages is not clearly stated

  Text changed to: ‘Two bedrock surface exposure ages sampled from rochés moutonnées at the highest and lowest outcrops…’

- Line 298: ‘…displays extensive glacial erosion which suggests the ice thickness at the LGM was considerably greater than 230m’ – again, more information is needed to explain why such erosion could not have been carried out by a thinner ice sheet

  Text changed to: The fact that well developed landforms of glacial erosion occur at the highest outcrops at Hughes Bluff, including evidence of abundant basal sliding and plucking, indicate that the ice thickness was considerably greater than 230 m..

- Lines 311-312: ‘…suggest either a thin cover of cold-based ice or ice-free conditions…’ – not clear how you reach this conclusion (noting that you didn’t mention the potential for a thin layer of cold-based ice when discussing bedrock ages from the summit of Mt Kring)

  Text changed to: In an effort to identify higher elevation glacial activity and long-term erosion history, field work was undertaken along the northern flank of David Glacier from the D’Urville Wall area (Mt. Neumayer to Cape Phillipi) (Fig. S5)

- Lines 312-313: ‘High elevation bedrock samples are much younger… suggests burial by nonerosive ice’ – several aspects of this sentence are unclear: which 'high elevation' samples are you referring to, where is the 'nearby' site with older bedrock ages at a similar height and which site do you suggest was buried by non-erosive ice?

  We clarify in the text: Bedrock exposure ages from D'Urville Wall area (including Mt. Neumayer and Cape Phillipi) do not allow a precise estimate of the past ice surface along the northern flank of David Glacier. Supported by geomorphic evidence from Hughes Bluff, which indicates ice thicker than 230 m, and the LGM limit of ~400 masl derived from drift deposits in TNB, we suggest the past ice surface was between 300 and 649 meters higher than today in this area (Stuiver, Orombelli1990, DiNicola2009) (Fig. S2).

Line 362: in this context, ICE-6G is an ice sheet reconstruction, not a post glacial rebound model

Line 366: how do you determine the ice thickness change for each model? Do you calculate the thickness change over a specific time period, or do you calculate the difference between the minimum and maximum ice thickness at any time during the model run?

Data constrained thickness values incorrect, changed in text. Model thickness values calculated during deglacial phase.

Text changed to: ‘At Mt. Kring, the average modelled thickness change throughout the deglacial phase for all models in Fig. 8A is 190 m ± 117 m, which compares well with the 173 m thickness change derived from our ice thinning chronology. In contrast, at Hughes Bluff, we capture only 171 metres of thickness change over the deglacial period and the average modelled thickness change for all models in Fig. 8B is 623 m ± 142 m.’

Line 367: figure 4b implies that the highest Holocene erratic at Mt Kring is ~170m above the present ice surface, i.e. there has been at least 170m thinning during the Holocene (not 144m). Similarly, where does the value of 181 m come from for Hughes Bluff (line 368)?

See previous comment and associated change in text.

Lines 387-390: text implies that experiments are carried out for melt rates between -2 and -10 m/yr, but such experiments are not listed in table 2 or shown in figure 9. The results presented here do not provide convincing evidence that -11 m/yr is the threshold value for triggering grounding line retreat

Text, table and figures are consistent but maybe not clear to provide convincing evidence that 11 m/yr is threshold to trigger grounding line retreat to near modern conditions.

Experiments are carried out for melt rates between 2 and 10 m/yr but they show very similar results: grounding line pinned to sill, upper surface above Hughes Bluff. We do not include these experiments because we would like to avoid 8 extra panels that show the similar results. We prefer to show model results where significant changes can be observed in the plots. Previously, we chose to show the lower SIMR value that initiates a change whereas it seems clearer to show the experiment with higher SIMR values.

Experiment M2 has been updated with model output corresponding to SIMR of 10 m/yr (in table, text and figure 9A).

Model M1: text on lines 389-391 implies that M1 predicts rapid grounding line retreat and that the results agree well with Mt Kring data constraints, but this is not supported by figure 9

Text changed to: 'For melt rates between 2 and 10 m/yr (exp M1, M2 respectively), grounding line retreat is initially rapid but…'

Line 397: does the 'further ice shelf debuttressing' take place after the 4% reduction in buttressing, i.e. during the same experiment, or in a completely separate experiment? Also, is a reduction by 40% the minimum value required for grounding line retreat to modern, or is this is simply an example of an experiment that showed full grounding line retreat?

We delete the word 'Further'. We confirm that the 40% reduction seems to be the minimum level of debuttressing required to retreat the grounding line to near modern if LBR on its own is being applied, but we note further down in the paragraph that it does not result in the correct final surface elevation.

Line 401: 'this simulation' – which simulation?

Text changed to: 'At Mt. Kring, modelled rapid thinning is synchronous with Hughes Bluff yet, the 40% LBR simulation (S3) results in an unrealistic final surface elevation 100s of metres below observed modern surface elevation.'

Table 3: (i) over what time period is 'modelled grounding line retreat rate' calculated? (ii) suggest listing the data-constrained thinning rates at each site, to allow comparison with modelled rates

(i) We include an * in the table header and then define in the caption. Text included in caption: *indicates the modelled retreat rate is calculated during the deglacial phase of experiments. The retreat rate is calculated during the deglacial phase of the experiment. (ii) We include a + in the table header for Max. thinning rates at MK and HB. Text included in caption: +Data constrained Max. thinning rate at Mt. Kring (MK) and Hughes Bluff (HB) is 0.19 m/yr and 2.06 m/yr, respectively.

Discussion

Line 424: take care when talking about 'matching periods of thinning' – the numerical experiments simply explore the response to an instantaneous perturbation to the boundary conditions. Your comparison could be taken to imply that Holocene change along the David Glacier was driven by a single, sudden change in local conditions. Also, it is not clear what it means for modelled retreat to 'match' the onshore thinning, are you implying that they occur at the same time?

Text changed to: Combined forcing experiments show a similar rate, magnitude and duration of onshore thinning compared with our onshore geologic records constrained…

Line 508: does the two-phase grounding line retreat result in two phases of onshore thinning?

'compares well' changed to 'aligns with phases of enhanced and reduced thinning identified in our onshore reconstructions at Mt. Kring and Hughes Bluff…'

Lines 512-514: the text implies that thinning initiated prior to grounding line retreat. However, text on lines 515-517 implies that grounding line retreat preceded onshore thinning. Which is correct?

We suggest grounding line retreat initiates onshore thinning. Text changed to: 'This synthesis suggests that beginning at 7.5 ka, the grounding line unpins from the sill at the mouth of the David Fjord, the David Glacier and proto-Nansen…'

Lines 528-530: be more explicit about the fact that you apply an instantaneous change to boundary conditions rather than applying time-evolving forcing

Text changed to: 'Modelled patterns of grounding line retreat correlate well with patterns of onshore thinning,…'

Figures, grammar etc.

Check for minor grammatical issues, e.g. words missing, singular/plural errors, sentences that do not make sense, use of hyphens in compound adjectives (e.g. sea-level rise, sub-ice-shelf melt)

Ensure that any new text is carefully incorporated into the existing text

Figures are very clear. Check the use of brackets when including citations in figure captions. Support the text in the Results and Discussion with more references to figures. I don't think the Drygalski Trough is labelled on any figure.

Drygalski Trough now labelled in Figure 1 and 2.

Figure 2: Aviator Glacier (AG) is not labelled on the figure, REG is not defined in the caption

Aviator Glacier (AG) and Mariner Glacier (MG) now labelled in figure. Rennick Glacier (REG) now defined in caption.

Figure 8: it is not possible to identify the source of all the lines, e.g. what is the difference between Kingslake2018 and Kingslake2018_WDC; what is Lowry2019_EMV? Check y-axis label on lower plot

We agree the Kingslake2018 and Kingslake2018_WDC (WAIS divide accumulation forcing) are difficult to distinguish in Figure 8B. They are the same from -22-~-13ka and from -13ka to 0 ka, only have minor differences. We suggest to keep them as they are. We have included in the caption, Kingslake2018_WDC uses WAIS Divide accumulation forcing and Lowry2019_EMV is a model in which Enhanced Mantle Viscosity is applied.

Plotting error corrected for y-axis on lower plot.

Figures 9-11: (i) label the field sites on one of the panel A plots, (ii) consider indicating the position of the present-day grounding line and labelling the 'prominent sill' on one of the panel A plots, (iii) state which axis each of the lines in the top plots of panel B relate to and make it clear that there is no relationship between melt rates and % buttressing, (iv) why do melt rates seem to vary randomly through time in the top plots of panel B?

(i) Field sites labelled in top panel Figs 9A, 10A and 11A (ii) Grounding line and sill labelled in top panel Figs 9A, 10A and 11A, (iii) include in caption: no relationship between LBR and SIMR, (iv)

Clarified text on 265-67: In order to simulate the natural variability that might be expected in an ocean forcing record, we apply a fluctuation of up to 0.5 m/yr magnitude using a random noise generator and this variation is then added on top of the step increase in forcing that is already applied.

Further, we include the following text in caption for Figs 9, 10 & 11: For SIMR cases, we varied the SIMR perturbation within a 0.5 m/yr window at 500 year temporal frequency in order to simulate short-lived pulses of relatively warmer or cooler water mass changes.