We thank the reviewers for their positive & constructive comments. We have addressed their concerns in the manuscript, as detailed below.

In addressing one of the issues raised, requiring that we plot the modelled and measured velocities together, we identified that the ice shelf velocities had a greater discrepancy than would be expected. This was due to a line in an input file mis-assigning the temperature structure in the ice shelf. We have re-run the experiments with the appropriate correction and find a modest change to the results, but this does not affect the findings or overall conclusions of the paper. The figures have now been updated to reflect the results from these re-run experiments.

Reviewer 1

1. The material in this article is quite complex and dense, and I understand the need and effort to keep the discussion clean and simple. However, one of the diffculties I had while reading through was that the authors mention (several) parameters in the model, but they never concretely state the model, the equations, etc. I would strongly suggest adding the minimum necessary equations to make the presentation self contained and to help the reader follow the discussion.

Response: In response to two reviewers' requests, we have included equations necessary to give context to the parameters discussed. The added description is as follows:

"We use the BISICLES ice sheet model, a detailed description of which is given in Cornford et al. (2013). BISICLES is a finite-volume model which applies a two-dimensional force balance approximation approach to the solution of the Stokes free-surface problem with the addition of a vertically-integrated stress component derived from the model of Schoof and Hindmarsh (2010) following:

$$\nabla.[\varphi h\bar{\mu}(2\dot{\epsilon} + 2tr(\dot{\epsilon})\boldsymbol{I})] + \tau^b = \rho_i g h \nabla s, \quad (1)$$

where φ is a stiffening or 'damage' factor, h is the ice thickness, $\bar{\mu}$ is the vertically averaged viscosity, $\dot{\epsilon}$ is the horizontal rate-of-strain tensor, I is the identity tensor, τ^b is the basal traction, ρ_i is the density of ice, g is acceleration due to gravity and s is the ice surface.

The horizontal rate-of-strain tensor is given by:

$$\dot{\epsilon} = \frac{1}{2}[\nabla\boldsymbol{u} + (\nabla\boldsymbol{u})^T], \quad (2)$$

where $\boldsymbol{u}$ is the horizontal ice velocity.

The vertically integrated effective viscosity ($\varphi h\bar{\mu}$) is computed from the vertically varying effective viscosity, μ:

$$\varphi h\bar{\mu}(x,y) = \varphi \int_{s-h}^{s} \mu(x,y,z)\mathrm{dz}, \tag{3}$$

where μ includes a contribution from vertical shear and satisfies:

$$2\mu A(T)(4\mu^2\epsilon^2 + |\rho_i g(s-z)\nabla s|^2)^{(n-1)/2} = 1, \tag{4}$$

where n is the flow rate exponent (n=3) and A is the ice-flow rheological parameter. A is calculated as a function of ice temperate (T) using the relation formulated by Hooke (1981):

$$A(T) = A_0 \exp\left(\frac{3f}{(T_r - T)^k} - \frac{Q}{RT}\right), \tag{5}$$

where A_0 = 0.093 Pa^{-3} a^{-1}, Q/R = 9.48 × 10^3 K, f = 0.53 K^k, k = 1.17 and T_r = 273.39 K, and using a 3D internal temperature field produced by Pattyn (2010) for T.

While the effective viscosity calculation incorporates vertically-integrated stresses, the component of mass flux due to vertical shearing has to be neglected because it significantly reduces the maximum stable time-step. The resulting model still out-performs the shelfy-stream approximation, however, when compared to full Stokes solutions of grounding line problems (Pattyn et al., 2012; Cornford et al., 2013). Resistance to basal sliding is governed by a linear viscous friction law so the basal traction is:

$$\tau^b = \begin{cases} -C\boldsymbol{u} & \text{if } \frac{\rho_i}{\rho_w} h > -r \\ 0 & \text{otherwise} \end{cases},$$

where C is a is a basal friction coefficient and r is the bed elevation."

2. The authors mention (pag 5482) that "to invert the model equations using as input measurements of present-day ice sheet geometry (Fretwell et al., 2013), temperature (Pattyn, 2010) and surface velocity (Rignot et al., 2011).". This is a bit confusing, as it suggests that the inversion for the parameter(s) is done based on all these data (observations), while later in the next paragraph the data misfit is defined based on surface velocity observations. Please reformulate.

Also, I suggest adding "satellite surface velocity observations" to make sure it's clear what are the data used in the inversion.

Finally, “invert the model equations" is not correct, if the purpose is to invert for the parameters present in these equations/model.

Response: The first sentence discusses the *inputs* required for the control problem. The next paragraph refers to the data used for the *misfit* (just the surface velocity observations). The paragraph has been slightly reworded for the third sub-point above. We have also added inserted “satellite surface velocity observations” as requested. We believe that the wording is now much clearer:

“As field measurements of C and A (φ) are not available, we follow the control method approach of MacAyeal (1992) **to determine their values, subject to the model equations and the** present-day ice sheet geometry (Fretwell et al., 2013), temperature (Pattyn, 2010) and surface velocity (Rignot et al., 2011).

This ‘control problem’ involves fitting modelled ice velocities to **satellite surface velocity observations** with the upper and lower ice surface topographies fixed.”

3. The underlying inverse problem would have been ill-posed even with one inversion parameter (field). Please reformulate (pag 5482, line 10).

Response: The paragraph has been re-worded as follows:

“In this case we use a gradient-based optimization method (non-linear conjugate gradient method) to iteratively minimize an objective cost-function quantifying the difference between observed and modelled velocities. **The basic optimization problem is underdetermined, because there are two unknown fields but just a single field of observations, and prone to produce large fluctuations in C and phi in response to noise in the observations.”**

4. The authors mention that the inverse solution is obtained with 16 (optimization) iterations. It would be beneficial to know what inverse solution method did the authors apply (i.e., steepest descent, nonlinear CG?). Also, please comment on the (relative) reduction in the norm of the gradient.

Response: We have included the method in the text, along with a comment on the reduction in the norm of the gradient:

“In this case we use a gradient-based optimization method **(non-linear conjugate gradient method)** to iteratively minimize an objective cost-function quantifying the difference between observed and modelled velocities.

…

As an initial guess we set φ equal to 1 everywhere and C equal to 100 Pa $m^{-1}a$ within ice streams, increasing as an exponential function of measured velocity where the ice flow velocity is less than 100 m a^{-1}. **The L2-norm misfit (integral of squares) was improved by a factor of 10 after 16 iterations. A further 16 iterations produces an improvement by only a factor of 1.5.** Figure 2 shows the results of the control problem after 16 iterations."

5. It would be useful to show the used (InSAR) observed surface velocities in the Filchner-Ronne Ice Shelf region and the corresponding recovered velocity fields (obtained with the inverse solution). This would give some more understanding to the reader about what the authors mean by: \the model is unable to match the observed ice fluxes at those ice streams which flow through well-defined narrow channels" (pag. 5485, lines 25).

Response: We have added a figure with both observed and modelled velocities for comparison, as requested.

6. There is a detailed (good) discussion on the mesh refinement. It would be nice also to show the mesh used for a typical simulation.

Response: The mesh has been added to the modelled velocity figure.

7. Grammar, typos, consistency, etc. consistency issues: lines 10: modeled speeds versus modeled ice velocities; t = x yr versus t = x; south-north versus South-North; grammar: pag. 5487 (lines 25): to apply.

Response: These minor edits have all been made. 'Speed' is changed to 'velocity'; 'South-North' changed to 'south-north'; 'years' added to 't = x years'; 'to' added to the sentence on line 25.

Anonymous Referee #2

Additionally, I have a few comments/suggestions for the authors:
Page 5479, lines 7-10: Please include some references here to support the statements in this paragraph.

Response: Example references have been added:

Uotila, P., Lynch, A. H., Cassano, J. J. & Cullather, R. I. 2007 Changes in Antarctic net precipitation in the 21st century based on Intergovernmental Panel on Climate Change (IPCC) model scenarios. J. Geophys. Res.. 112, D10107, http://dx.doi.org/10.1029/2006JD007482

&

Winkelmann, R., Levermann, A., Martin, M.A., Frieler, K. (2012) Increased future ice discharge from Antarctica owing to higher snowfall. *Nature* 492, p239-242.

Page 5481, line 13 (also Page 5485, line 6): Please add some justification or explanation for the use of 2000 year- long simulations.

Response: A sentence has been added to the last paragraph of the introduction to justify the 2000 year simulations:

"The model is run over a period of 2000 years in order to investigate the full response of the ice sheet to the perturbations, whilst balancing the assumptions made when setting up the domain and boundary conditions and the computational time of the model."

Page 5483, line 3: What are the time-steps of your simulations? Yearly?

Response: The timestep is chosen to satisfy the CFL (Courant-Friedrichs-Levy) condition. This sentence has been added to the end of section 2.1 – model description.

Page 5483, line 16: It would be helpful to add a figures illustrating the mesh and highlighting refined area.

Response: As per reviewer 1 this has been done.

Page 5487, line 15: This is the only place where Fig. 7 is mentioned in the results. Overall, a description of the results presented in Fig. 7 is lacking, and it is only the threshold behavior of Evans is mentioned. This figure is quite rich with information but it can be a little confusing to digest at first. More guidance for the reader would be helpful, especially in the results section. Doing so would be more effective, since figure is used as a key reference in parts of the discussion.

Response: The main purpose of the Fig. 7 is to demonstrate the strong threshold behaviour of Evans Ice stream compared to the others, which is why it is only mentioned once in the text.

Page 5487, line 25: found it most convenient “to” apply a uniform shift

Response: Changed

Page 5493, line 15: Is it possible to make some statements about how these results might extend to the rest of the West Antarctic Ice Sheet, or maybe other specific ice streams? Or perhaps about how these results could not extend to other locations in the WAIS? Would it make sense to characterize other ice streams, based on key parameters (like outlet narrowing) for predictability? Or are we still far from being able to do something like that? A short discussion on this would add to the broad impact of the findings presented here.

Response: The extension of the results to other ice streams is possible, though caution is required because there are many factors involved. Numerical modelling is the best approach to give the most robust assessment of the process behaviour of the system. A sentence has been added as follows:

“These factors should also be taken into account when assessing the relative vulnerability of other Antarctic ice streams to climate-change induced retreat. For example, the Siple Coast ice streams feeding into the Ross Ice Shelf are not constrained by fjord topography, so they may act in a similar way to the Institute & Moller ice streams. However, as many factors are involved in controlling ice stream response, such as the slope of the bed topography and the presence of ice rises in the ice shelves, the most robust approach is the discrete application of numerical modelling, as demonstrated in this paper.”

Also, adding a few sentences to discuss possible next steps would be quite valuable to the manuscript. In particular, what could be done (e.g. additional experiments/ observations) to nail down the different behaviors of the various Filchner-Ronne ice streams?

Response: Further geophysical measurements of the less-well surveyed ice streams is a major requirement for future work, and a sentence has been added as such:

“In order to further improve our understanding of the behaviour of major ice streams feeding into the Filchner-Ronne Ice Shelf (outside of the Institute & Möller ice streams) further geophysical observations are required to properly characterise the nature of the topography near the grounding lines.”

S. Price (Referee)

Minor Concerns
5477, 22-26: ". . . under warmer past climates" Is there clear evidence there were past retreats? Is there evidence that these past retreats coincided with warmer climates? If so, perhaps some refs. should be added to support these claims (or make it clear which of the refs. do support these claims). It sounds plausible, but could be supported better here.

Response: The paragraph is structured to present evidence of a smaller WAIS, including refs from Joughin et al., 2006; Ross et al, 2012 & 2014, which then support the possibility future retreat. Consequently the references in support of the claim are already included. However, further evidence can be added to support the claim from the ANDRILL sediment drilling project (Naish et al. 2009) and from previous ice-sheet modelling (Pollard & De Conto, 2009), which we now include as follows:

"These interpretations, **along with evidence from sea floor sediment analyses (Naish et al., 2009) and ice sheet modelling of multiple glacial cycles (Pollard & De Conto, 2009),** support the idea that the West Antarctic Ice Sheet has, under warmer past climates, been confined to a stable position occupying the highlands upstream from the currently glaciated basin beneath the trunks of the Institute and Möller ice streams."

5479, 21: Is it accurate to refer to BISICLES as a "3d" model? While the full 3d velocity field can be recovered (when accounting for an SIA-like vertical shearing component to the flow), in my understanding, it is most commonly applied in a 2d mode (SSA "star"), where the vertical shearing term only affects the depth-ave viscosity "seen" by the SSA solution. More importantly, is it being applied in a fully 3d mode here, or something closer to 2d? I see that the "SSA star" issue is discussed in a subsequent paragraph, so it is probably sufficient to simply link this discussion more clearly in the text.

Response: We have removed the reference to a 3d model to avoid confusion & added a clause directing the reader to the next section (which is now enhanced with appropriate equations).

(25-26): suggest using "block structured adaptive mesh refinement" to describe the AMR "method".

Response: Changed.

Also, it is not just the g.l. that dictates the refinement is it? Doesn't general dynamic complexity also come into consideration, e.g. regions of large strain rates get AMR as well?

Response: The model setup in this particular case is restricted the mesh refinement to grounding line areas. No changes are made.

5480, 15-17: Providing the values for C and m in the sliding law doesn't really help the reader if they can't see the form of the sliding law. Perhaps just show us the equation inline here? Also, if m is just set to 1, we probably don't even need to know about it.

Response: Equations have been inserted, as per reviewer 1

5481,3-9: It isn't clear to me why the inversion procedure breaks down when the ice sheet surface topography data is too detailed (relative to the bed topography). The ice flow model, a constraint in the inversion procedure, includes membrane stresses and thus should be serving to smooth these out. While I don't think this can / needs to be addressed here, it seems like something worth further attention (e.g., could it be "fixed" through regularization?).

Response: The referee is correct; the surface smoothing approach here was essentially a kind of regularisation. While we agree more work could be done on this issue, it doesn't affect the conclusions of the paper and no changes are made.

Section 2.4: (5482, 24-28) Clarify – is the 'relaxation' done to bring the init. cond. closer to being in equilib. with the SMB forcing? To reduce otherwise large and spurious values of the flux divergence field? Does ". . . two continuous fields " means "spatially continuous"? (5483,2-5) It took me multiple readings to start to understand what is being done here, but I still don't follow it all. I think I follow that the basal melt rate field required to maintain a SS in the ice shelf is diagnosed, and then divided into a g.l. and "ambient" component. This is then somehow tied to the location of the g.l., so that as the g.l. moves over time these fields move with it? I don't follow how this allows for a basal melt rate field that covers the entire shelf at all subsequent times (e.g., as the g.l. retreats, there would seem to be grid cells near the calving front that no longer have a melt/freeze rate associated with them). Maybe more importantly, it isn't clear to me why this basal mass balance (for the ice shelves) and the "compensatory" SMB on grounded ice are not adequate to maintain equilibrium for the reference experiment. It seems like they were designed for that purpose. At any rate, I think this section could be explained a little more clearly, even at the expense of adding additional text / figures. This initialization problem is of significant interest to a lot of other modeling groups right now, so even if it has not lead to ideal results here, a clear description of the problems and lessons learned in this study would be useful to other readers.

Response: We have added further clarification to this section. We no longer refer to a relaxation run. Originally, the relaxation run with the ice shelf held steady was used to dampen short wavelength features in the flux divergence over grounded ice, in order to find a long wavelength synthetic surface mass balance there. However, we found that the ice dynamics were essentially unchanged if we simply used the more physically plausible Athern 2006 SMB. Therefore, the revised results do not make use of a synthetic surface mass balance and the relaxation is no longer necessary from that point of view. The relaxation run is also not required for computing the melt rate parameters as relaxing the ice shelf without imposing a melt-rate just results in grounding line advance. We have also added more description of how the melt fields are calculated and combined. A paper has recently been published in The Cryosphere (Gong et al., 2014) which details the method, we have referenced the method to this paper below. We have also added further discussion of why the sub-ice-shelf basal mass balance is not enough to hold the ice sheet in equilibrium. The edits are shown below in bold:

Sub-shelf melt rates are determined from the initial flux divergence in the manner described by Gong et al., (2014), and are intended to hold the ice shelf close to steady state. Two **spatially** continuous fields are produced that each cover the entire model domain, one for application near to the grounding line (M_{GL}) and the other, the ambient field (M_A), relevant to floating ice away from the grounding line. **M_{GL} and M_A are determined by considering the flux divergence in regions close to and far from the initial grounding line respectively, smoothed to remove short wavelength features, and then extrapolated into the surrounding regions. This extrapolation allows the basal mass balance field to adapt to the change in grounding line location by effectively transferring the grounding line melt rate to the new grounding line location.** The two fields are then combined using a weighting parameter to transition smoothly at each new time-step (over a distance of approximately 35 km on the floating side of the grounding line) so that the maximum melting follows the grounding line as satellite measurements indicate (Rignot and Jacobs, 2002).

And later on page 4585:

Over the length of the control run, the result is that thickening occurs near to the grounding-lines of Rutford, Evans, Carlson Inlet and Foundation ice streams in particular (Figure 5a). **The difficulty in maintaining a steady state is due to the high amplitude and frequency variations that are required in the sub-ice-shelf basal mass balance to balance the flux divergence. The flux divergence field produced by the control-problem-derived ice velocity and geometry observations is noisy due to artefacts of interpolation and other sources of error in the ice sheet geometry. The high amplitude and frequency variations in the sub-ice-shelf basal mass balance field are unrealistic and, hence, need to be smoothed out, leading to the ice sheet not maintaining a steady state.** This problem inhibits our ability to make predictions and means that the results of the reference experiment cannot be viewed as a future prediction for a scenario of constant climate.

5483,19-22: This is a bit confusing as written. By “either” side of the g.l., I think you mean “both” sides (upstream and downstream)? And why 8 cells? When you refine at the g.l. you do so for more than just the cells that are on both sides of the g.l.? Does the 8 refer to before or after you divide by one half? If this is really important, perhaps a figure would help the description?

Response: The text has been reformulated as follows:

A single level of grid refinement reduces the cell size from dx to dx/2 (where dx is the initial grid size before refinement) for at a distance of at least 4*dx upstream and downstream of any grounding line within specifically defined parts of the domain. The distance 4*dx was found to be sufficient in earlier studies (Cornford et al 2013).

5485, Section 3.2: As above, not clear to me why the reference experiment fails to maintain a steady state. Wasn't it designed to do so?

Response: As above, we have to compromise between steady-state and a credible melt-rate.

5486,5-7: "The reference run . . . viewed as a neutral staring point . . .". It sounds like "neutral" might not be the right choice of words here. From the longer description of the reference state, given in 3.2, it doesn't sound like the reference run produces a steady state, which is what I think of when I read "neutral".

Response: "Neutral" is used here to avoid using the words "steady state". We are happy that the word neutral is appropriate here. No changes are made.

5491, 15-18: Is it worth mentioning the apparent similarities here between these ice streams and the "classical" Siple Coast ice streams flowing into the Ross?

Response: As per reviewer 2, a sentence has been added to the conclusion to refer to the Siple Coast ice streams as an example.

5492, 26: "indicate" -> "suggest"? "are CURRENTLY? situated very close to . . ."

Response: Changed, as requested.

Figures

The necessary detail from the figures was very difficult to discern in the print version of the manuscript. The online versions of the figures were better, but still required excessive zooming in to be able to see the necessary details discussed in the text and captions. For example, for Figs. 8 and 10, I had to zoom in to >300% to really see what was going on.

Response: This is partly due to the formatting of the Discussion paper pdf, we will make sure the figures are clearer in the final manuscript in conjunction with the typesetters. It should be noted that reader inspection of electronic figures is a major advantage of publishing in TC, so we don't see this as a major problem.

Fig. 2: Might be worthwhile reminding the reader which color (red or blue) is more sticky / slippery.

Response: (red is "sticky", blue is "slippery") has been added to the caption.

Fig. 4, caption: “Results of the 200 yr EXPERIMENT ON GROUNDLING LINE CONVERGENCE . . .”

Response: Changed, as required.

Figs. 5, 6, 7, 8, 10: for right column of line plots, showing ice loss vs. time, suggest adding a right-hand side vertical axis, that gives the volume loss in equivalent of cumulative SLR.

Response: This axis has been added to the revised figures.

Acknowledgements
While probably not strictly necessary, I know that the developers of the BISICLES model on the DOE side would greatly benefit from any acknowledgement of DOE’s role in developing the code. Co-author Cornford probably has access to something appropriate from previous papers (e.g., a recent Nat. Clim. Change paper contained such an acknowledgement).

Response: Acknowledgement added

Editorial Suggestions
p.5476 (Abstract) Lines 5-8: suggest adding something like “. . . and the ability to accurately model marine ice sheet dynamics . . .” (under the description of “significant advancements”).

Response: Changed

Line 10: note that the “perturbations” are not arbitrary? Label them as “reasonable” or “realistic”?

Response: ‘Realistic’ added.

p.5476 (Intro) Line 20: move Mercer ref to end of sentence?

Response: The Mercer citation refers to the principle of marine instability. By moving the reference to the end of the sentence it would read like Mercer’s work was considering the Weddell Sea sector. This has been left unchanged.

Line 23: It reads like “smooth” is needed for “retrograde” slopes, but I don’t think this is the case – you can still have retrograde without being smooth. Anyway, the definition of “smooth” seems arbitrary to me.

Response: Sentence structure changed to:

“Near to the grounding lines of these ice streams the bed topography has been observed to deepen significantly inland (a configuration often called a retrograde slope), and also to be smooth, with few potential pinning points (Ross et al., 2012).”

5477, 6: Add some observational &/or modeling refs. to section on loss of grounded ice following loss of buttressing (e.g. Scambos et al. folling Larsen B collapse?).

Response: (c.f. The Larsen B ice shelf collapse (Scambos et al, 2004)) added.

(27):A few more refs. for the modeling of ice shelf buttressing, like Goldberg et al. (and other refs therein?)

5478, 2: Payne et al. ref ... add a few more, e.g. recent paper by Joughin et al. (GRL,37,2010), recent work by ISSM group at JPL?

Response: The references in this sentence have been amended to address the two points noted above. The sentence in question is specific to the Amundsen Sea region, so the modelling references have been changed to account for the Amundsen Sea region. Payne et al., 2004 has been moved into the revised list of modelling references. The Goldberg reference is not Amundsen Sea specific and, to cover all ice shelf buttressing references, would lead to an overly long list.

“Several modelling studies (e.g. Payne et al., 2004, Dupont and Alley, 2005, Joughin et al., 2010) and a growing body of observational evidence (e.g. Pritchard et al., 2012) suggest that the thinning of ice shelves, and the consequent decrease in buttressing at the mouths of fast-flowing ice streams, is the main cause of the observed dynamic thinning identified in the Amundsen Sea Embayment region of West Antarctica”.

(5-6): “highest salinity . . .” Isn’t this usually referred to as “High-Salinity Shelf Water (HSSW)”?

Response: This has been changed in the text to High Salinity Shelf Water.

(14-15): “. . .increase by an order of magnitude” are there refs. to support this statement?

Response: This result comes from Hellmer et al. (2012); this reference has been added at the appropriate point in the sentence, as needed.

5479, 5: “constrained” -> “narrower”.

Response: we feel that constrained is a better word here, as it is consistent with the idea that the ice stream is not free to change its flow configuration laterally.

(7): “enhanced ocean warming” -> “changes in ocean warming” (not all anticipated future changes are going to lead to ocean warming).

Response: Changed

(13): “predicted” -> “expected” or “anticipated”? I don’t think we’re good enough yet to call these predictions.

Response: They are still predictions, but under certain scenarios. This clause has been added to the sentence:

“…perturbations on the order of those predicted to occur within the next few hundred years, **given certain scenarios (e.g. Hellmer et al., 2012)**.

5480, 7: If “n” is not ever referred to again here, then omit it? That is, you can just say “power law exponent set to 3”.

Response: Changed

(7-8): How different is the Pattyn formulation for A(T) from something more standard like that given in Paterson? Give the most fundamental reference here if similar.

Response: The inclusion of the model’s equations should make this issue now clear. The actual formulation referred to comes from Hooke (1981), which is now referred to.

5481, 12: “up to 2000 yr” is used here and in other parts of the text. This is confusing. I assume it means 2000 yrs into the “future”, assuming that time 0 is supposed to be something like today?

Response: This has now been addressed by clarifying the 2000 year model run time in the introduction (reviewer 2)

5482, 10: “containing” -> “quantifying”.

Response: Changed

(19): Is the optimization converged after 16 iterations?

Response: The optimisation reduces the misfit by a factor of 10 after 16 iterations, and a further 16 iterations only manage an additional factor of 1.5 – see the response to reviewer 1.

(19-22): A bit of a run on. Break up into two sentences?

Response: Broken into two sentences

5484, 3: suggest “. . . =4km) to 5 (maximum size of 4 km, minimum size of 125 m).”

Response: We feel this is clear as it is. No changes are made.

(23-24): “grounding line shape” -> “grounding line position”.

Response: Changed

5487, 21: suggest “Very little melting occurs on the Antarctica ice sheets even at sea level . . .”

Response: Changed

5488, 3.5: suggest section title, “Grounding line melting versus ambient shelf melting”

Response: Changed

5491,3: “steep walls” -> “steep rock walls”?

Response: Changed.

(7): “. . .which appear to place them CURRENTLY near to a critical threshold . . . and unstable RETREAT.”

Response: Changed

5492, 1: “inverse bed slope” -> “reverse bed slope”?

Response: Changed to retrograde to be consistent with the introduction.

(7-9): “ Trough width has previously . . . Even under conditions . . . stabilizing effect during retreat.” Shorten this? Could go with “While trough width has previously been show to XYZ . . . (reference), it is unlikely to . . . ABC . . . here.”

Response: I’m not sure the reviewer has interpreted the sentence correctly; it is not clear what correction is being suggested here. No changes are made. We don’t think there is any mistake or ambiguity in the existing text.

5493,2: “once retreat begins FOR THESE ICE STREAMS . . .”.

Response: Changed

(13-14): “. . .affect the responses of ice streams to EXTERNAL FORCING.”

Response: Changed