I thank referees 1 and 2 and the editor Dan Goldberg for their comments and the time and effort behind them. The manuscript has now been significantly improved in response to these comments.

Referee 1 (R1):

In this very constructive review, R1 was impressed by the abilities of such a simple model but also suggested improvements/corrections in the present manuscript and improvements/enhancements for a future, version 2 of the model.

General comment:

"..It would help to promote the model if more discussion can be added describing specific applications that the model could contribute to, for which 3-D models are not feasible..."

In a revision in progress I revised and expanded the last paragraph of the discussion to now read :

I conclude that the DAIS model can be used profitably with care in a number of possible applications. This very fast, semi-analytical model is well suited for very long hindcasts that would not be feasible with 3-D models. One such possibility would be a study of AIS emergence and subsequent evolution over Cenozoic time. The DAIS model is also well positioned to be a component in integrated assessment modeling by allowing extensive sensitivity studies, using Monte Carlo-type analyses for example (Hargreaves and Annan, 2002; Applegate *et al*., 2012). After all, more than 10 million, 1000 year model simulations can be run in a day on a single computer processor. Such a study would determine the relative importance of and provide Probability Density Functions for the parameters in the model (Table 1). It should be remembered that the model has been calibrated and validated here for conditions no warmer than the last interglacial for which ice flux at the grounding line is essentially the only ice loss term. If future warming becomes sufficiently strong, ablation will also become an important factor in AIS mass balance. Work is underway to apply the model to the Greenland Ice Sheet for which ablation has been an important ice loss term during interglacial periods (Applegate *et al*., 2012). Such an application will allow improved calibration of the model parameters controlling ablation.

Specific Comments:

1. *"The real WAIS and EAIS are more like two separate ice sheets separated by theTransantarctics, than one single dome. ...For problems involving marine WAIS collapse, it might be a better approximation to treat them as two separate symmetric domes,. Or perhaps half-domes, with a no-flow wall representing the Transantarctics in each.....This could be left to future work (not needed for this paper). ."*

It is not always clear in what direction and how far one should go to enhance such a simple model as DAIS. But this "ansats" is well worth considering in a future, version 2 of the model as R1 suggests.

2. "*The two-parameter space explored here is reasonable, involving two of the most important processes for glacial cycles: parameterized flux across the grounding line (gamma), and ocean melting under floating ice (alpha). As a suggestion for further work, the model would be very suitable for a Large-Ensemble approach (e.g., Hargreaves and Annan, Clim. Dyn. 2002; Applegate et al., The Cryo., 2012; Briggs et al., The Cryo., 2013), for instance using a Monte Carlo Markov*

Chain technique. This would systematically determine the importance of other model parameters, and provide pdf's of parameter ranges,"

This had always been a plan for future work and was mentioned (but too briefly) in the last paragraph of the discussion. Now the revised last paragraph of the discussion, copied above, adresses this more fully as R1 requests.

3. *" From the perspective of parameter space, the sequence of presentation in sections 3.1 and 3.2 almost seems reversed. Fig. 7, showing the full ranges of acceptable alpha and gamma, should drive the choices of the runs shown in Fig. 6. Regardless of the order of the figures, more time series should be shown as in Fig. 6, for alpha-gamma values both in the center of the cluster of red dots in Fig. 7, and also on the right-hand side of the cluster...".*

The original organization was meant to show in a stepwise and hopefully pedagogical fashion what the consequences are of each improvement (parameterized flux across the grounding line and ocean melting under ice shelves) of the original (but corrected) Oerlemans model and in what way each improvement helps to capture AIS behavior as reflected in the data constraints. So I prefer to keep the figure order as is. However, in the revised version I follow R1's suggestion and add a fifth time series to Fig. 6 for a model run on the right hand side of the cluster of acceptable runs in Fig. 7. This is then a run for a higher value of alpha approaching that found in Schoof (2007; JGR).

4. *" In section 2.1, it is unclear if the coefficient beta (dependence of mass balance on elevation below the runoff line, Eqs. 4 and 7) represents summer melting via atmospheric lapse rate, or solely height-variations of precipitation (via orographic forcing? pg. 1796, lines 11-14). This point comes up much later (pg. 1808, lines 4-6), which suggest that there is no melting in the simulations in the paper. It would help to make this clearer in section 2.1..".*

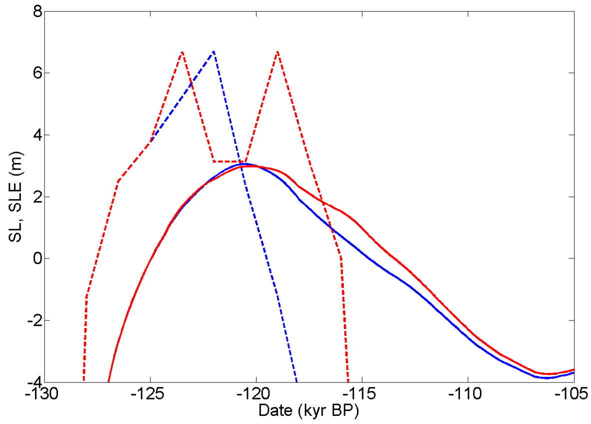
Both effects play a role in determining beta. To clarify this, the corresponding text in section 2.1 has now been expanded to read:

Sublimation has been disregarded here. The value of β depends on a combination of (1) height variations of precipitation and (2) height variations of summertime melting coupled to the atmospheric lapse rate (Oerlemans, 2008). The problem can be easily reformulated in terms of an equilibrium height, h_e,

5. "*The self-similar profiles of ice thickness seem to preclude the possibility of including a few-thousand-year bedrock lag due to asthenospheric relaxation. This may be a significant omission if bed depths in the grounding zone strongly affect ice fluxes (as discussed on pg. 1807). Is there a way to build a crude lag into the model?"*

As R1 notes, the extension of the model to address bedrock lag would not be straightforward given the self-similar profiles of ice thickness upon which much of the development of this model rests. One might expect that this effect would be most critical when addressing a marine WAIS collapse. Therefore it would to be better to address this in a future version 2 of the model that would include a treatment more focuesed on the WAIS itself (see Specific comment 1 above).

6. *"Recent observational studies of sea level around the Last Interglacial (LIG) find a double-peak structure (O'Leary et al., Nat. Geosc., 2013; Kopp et al., Nature, 2009). This is not in the prescribed sea-level curve in Fig. 5a (black curve). Could it significantly influence the model's response around LIG?"*



To address this question I made an additional simulation using case 4 parameter values (Table 1; Fig. 6) and a double peaked, LIG sea level based on the results in R1's two references. The figure to the right shows the results (blue lines are for the original case 4; red lines are for the new calculation; dashed lines are the applied sea level forcing (SL); solid lines are the sea level equivalents (SLE) calculated by the model). The double peak forcing does not affect the amplitude of the model response. Rather it only extends the response somewhat at the end of the LIG. This can be understood from the discussion of Fig. 6 (line 16, pg. 1803 to line 11, pg. 1804). "...the key model feature for simulating an LIG-like ice loss is subsurface ocean temperature leading to ice flow acceleration.." rather than the increased sensitivity of ice flow to sea level rise. A sentence on this was inserted (lines 7-9; pg. 1804) in the revised version to read :

Maximum SLE during the LIG was 3.07 m. When rerun using a double-peak structure for sea level rise across the LIG (Kopp et al., 2009), case 4 yields a very similar maximum combined with a 500-1000 year extension of high SLE. When rerun using the original Waelbroeck *et al*. (2002) sea level curve across the LIG (Appendix A; Fig. 5), case 4 yields a somewhat lower maximum of 2.31 m.

Technical comments:

"pg. 1793, line 8; pg. 1803, line 20: loose/loosing should be lose/losing".

Corrected.

"pg. 1800, line 21: It would help to alert readers at this point that higher values of gamma will be considered in later sections. The value of 2 mentioned here is much lower than those in Schoof (2007) (as noted later)."

I prefer to make this point at an even earlier stage and have revised the paragraph after Eq. (11) on page 1798 to read:

where T_o is the ocean subsurface temperature adjacent to the AIS, α is a partition parameter and γ is the power for the relation of ice flux to water depth. Furthermore, T_f is the freezing temperature of sea water, $T_{o,0}$ is a present day reference T_o and R_0 is a reference R. In the analysis to follow, the parameter α will be taken to vary from 0 to 1 (but values become unphysical toward the upper limit of this range as there will be flow at the grounding line even when T_o approaches T_f). The parameter γ will be taken to vary from 1/2 to 19/4. This can be compared to an O5 value of 1 and a value of 15/4 from the results of Schoof (2007; his preferred value for p is 19/4 where $F \propto H^p$ and therefore from Eqs. (9) and (11), $\gamma = p$-1). The value for $T_{o,0}$ was obtained as described in

Appendix A and the value for R_0 was taken from the steady state solution of the O5 model ($\alpha = 0$; $\gamma = 1$) with present day forcing (T_a= -18°C; SL=0; see below and Table 1).

Note that the original value of γ ascribed to Schoof (2007) on line 23; pg. 1798 was incorrect and has now been treated correctly in the text above. Figures 6 – 8 have been revised for the ranges of α and γ in the text above.

"pg. 1804, lines 12-14: The statement "includes a several meter SLE ice loss at melt water pulse 1A, forced by ocean subsurface warming and sea level rise across the pulse" is misleading (even though the next sentence says the result is consistent with a Northern Hemispheric source). The real MWP1A only lasted _500 years, and the rise in the model curves during any 500-year window in Fig. 6b is at most _0.5 m, not several meters."

Point taken and corrected to read:

includes an SLE ice loss of less than one meter at melt water pulse 1A, forced by ocean subsurface warming and sea level rise across the pulse.

"pg. 1807, lines 22-25: The suggestion here seems correct, that the different response in the Pollard and DeConto (2009) model at 125 and 90 ka is due to the different ocean forcing. There, the January insolation component produced a very large positive peak at 90 ka, which is absent in the ocean temperature time series here (Fig. 5, blue curve, with a peak at 125 ka). This points to the need for further work on ocean forcing on these timescales. The reconstructed ocean time series in the Appendix still seems uncertain, but acceptable in the absence of better methods.

No action needed

"pg. 1810, lines 5-15. It would be interesting to add a sentence or two briefly describing the DCESS ocean model used in the ocean time-series reconstruction."

To address this I revised and expanded these lines to read:

Then I force the low-mid (0 - 52°) and high (52 - 70°) latitude DCESS model ocean with corresponding atmospheric temperature series constructed from the global series using amplification factors (0.928 and 1.266, respectively) and mean reference period temperatures (20.55 and - 4.39°C, respectively), all taken from DCESS model simulations (Shaffer *et al.*, 2008; Fig. A1b). In the model, sea ice extent is diagnosed from zonal profiles of atmospheric temperature and only the sea ice-free part of the high latitude ocean exchanges heat with the atmosphere. The DCESS model ocean with 100 m vertical resolution includes parameterized overturning, horizontal mixing, small-scale vertical mixing in the low-mid latitude zone and enhanced vertical mixing in the high latitude zone. Parameter values have been calibrated by fitting to ocean temperature and carbon 14 observations (Shaffer *et al.*, 2008). The model has only one high latitude zone but the poleward extent of that ocean zone matches the equatorward extent of Antarctica. The subsurface ocean temperature, T_o, used for the forcing of the DAIS model is then taken to be the average temperature in the depth range 200 – 800 m of the high latitude ocean zone (Fig. A1b and Fig. 5). The value for $T_{o,0}$ in Table 1 is the mean T_o for the period 1961-1990.

Referee 2 (R2):

In this review, R2 finds the subject matter highly relevant and the application of such a simple model useful. R2 has fine-combed the manuscript and has produced a very long list of combined specific/technical comments that I will address one by one (in addition to R2's general comments).

General Comments:

"*The paper is fairly well written but would greatly benefit a careful re-edit; some of the sentences are difficult to interpret and the presentation style of repeated phrases is inconsistent. Please be consistent with either subsurface ocean temperature or ocean subsurface temperature. In addition, I only think you need to mention high latitude once. Be consistent with using AIS rather than Antarctic Ice Sheet. Once the acronym is introduced use it*."

In the revision, subsurface ocean temperature has been replaced by ocean subsurface temperature. Likewise, Antarctic ice Sheet has been replaced by AIS everywhere except at the beginning of the Introduction and Discussion and where two abbreviations follow each other. These are questions of flow and estetics

"*Most figures would benefit from a graticule*".

After a review of the figures I find that Figs 1, 4 and 7 need improvement for legibility and these improvements are being made in the revision.

"*The table needs some cleaning up to properly reflect the variables and parameters inthe article text. See specific comments but please also re-review. Use SI units; i.e., not cmˆ-3*."

Table 1 is now revised to separate model constants and model parameters. SI units have been adopted in both tables with the exception of yr (instead of s) to better connect with the original Orlemans publications and better reflect the time scales involved in the problem.

Specific/technical comments:

"P1792, L1-L6: I appreciate it is difficult having acronyms within acronyms but is there anyway this sentence could be re-constructed to follow the traditional format of having the acronym after the phrase has been used."

Revised to read:

Model hindcasts of Antarctic Ice Sheet (AIS) sea level equivalent are forced by reconstructed Antarctic temperatures, global mean sea level and high-latitude, subsurface ocean temperatures, the latter calculated using the Danish Center for Earth System Science (DCESS) model forced by reconstructed global mean atmospheric temperatures.

"P1793, L6 -> L8: is there a reference for the 'snow fall' statement, or at least could you discuss a little more".

Revised and expanded to read:

A common feature of many of these models is an increase in AIS ice mass in response to warming. This is a consequence of increased snow fall as warming leads to more precipitable water in the atmosphere that falls as snow for cold Antarctic temperatures.

"P1793, L8: Add a comma after However"

Comma added to revised version.

"P1793, L29: Is this a new DAIS model or just the DAIS model"

The word "new" eliminated in the revised text.

"P1794, L1: sea level or global sea level?"

The revised text now reads "..global sea level.."

"P1795, L10: I am confused by the equation and the text after the i.e. 'i.e. for R > (b0 −SL)s−1 = rc, the distance from the continent center to where the ice sheet enters the sea.' Please be clear what rc refers too. Please be careful with comma placement. rc is not in the table."

This text has now been revised for clarity to read:

The second term on the right hand side of Eq. (3) is only considered for a marine ice sheet, that is for $R > r_c$ where r_c is the distance from the continent center to where the ice sheet enters the sea. From Eq. (1) , $r_c = (b_0 - SL)/s$.

As seen in the text, r_c is a quantity derived from model parameters and therefore is not included in Table 1.

"P1796, L1: What is c?"

This follows from Eq. (5) and Table 1. But to clarify more I expanded the text immediately below Eq. (5) to read:

where h_0 is the runoff line height for $T_a = 0$. The values in Table 1 for h_0 and the coefficient c were taken from mass balance studies (O4).

"P1796, L1, Where does the Precipitation law come from?"

O5. The text immediately below Eq (6) now reads:

where P_0 is the (ice equivalent) precipitation at 0°C and the value of the coefficient κ is chosen assuming a relationship between columnar water content (that increases exponentially with temperature) and precipitation (O5).

"P1796, L14: Integration of B as formulated above over the ice sheet surface yields -> Integration of B, as formulated above, over the ice sheet surface yields"

Rather, I simplified this to read:

Integration of *B* over the ice sheet surface yields

"P1796, Eq8. If this equation has previously been presented incorrectly and you are correcting it, it would be useful to see a few more steps in the derivation, for example, in the appendix."

There is no need to insert an extra appendix for this geometric calculation. Rather I expanded the text starting on line 22 of pg. 1796 to read:

Note that the signs of the last two terms of Eq. (8) differ from those in comparable Eqs. (16) and (18) of O3. This is due to sign errors in the original work. The correct signs in the last three terms of Eq. (8) follow from Eq. (2) and the integration of the runoff term in Eq. (4) from r_R to R under use of $\int r(R-r)^{\frac{1}{2}}dr = 2\left\{\frac{(R-r)^{\frac{5}{2}}}{5} - \frac{R(R-r)^{\frac{3}{2}}}{3}\right\}$. I also confirmed the validity of Eq. (8) by comparing with numerical integration of Eq. (4).

"P1796, L25: Now? Do you mean Now, with the corrected signs, i.e., the updated model... P1796, L25: What do you mean by an Antarctic temperature. Do you mean yearly average? P1796, L22: 22 degC? Is that correct?"

These points have been addressed in a revised text. 22 degC is correct for complete deglaciation but apparently confusing. The revised text now reads:

This correction to the original work leads to much reduced runoff and, for the O5 parameter values, an AIS model much less sensitive to climate at warm temperatures. In the corrected model, complete Antarctic deglaciation occurs at a T_a of about 8°C higher than in the original model (O4) and about 4°C higher than in a 3-D thermomechanical model (Fig. 2; Huybrechts, 1993).

In addition to be more exact in the definition of T_a, I revised line 6, pg. 1795 to read:

The mean annual air temperature reduced to sea level and averaged over Antarctica, T_a,

"P1796, L27: You make no comment as to the validity of the Huybrecht's model given recent modelling advances."

No need to go into this here. I only make a comparison for the temperature of complete deglaciation the AIS for which recent advances in modelling the AIS as a marine ice sheet are irrelevant since near complete deglaciation the AIS is a continental ice sheet.

"P1796, L28: 'might be undertaken but I defer this to future work'-> possibly just drop this phrase, if it will be future work say so, if it might be undertaken in the future, is sounds unlikely"
"P1797, L11: Is there a reference for this?"

The revised text now reads:

Further improvements/adjustments of the model runoff formulation/calibration will be undertaken in future work. For the present I will concentrate on revised formulations of the other main component of the model: ice flux across the grounding line. This has been the dominant process for ice loss from Antarctica over ice age cycles and will continue to be so in the near future (Pollard and DeConto, 2009).
..
P1797, L15: Why the increase in bo?

This is motivated in the sentence that follows the one you refer to.

”P1797, L16: Never seen the word throughput in this context. Any distinction from mass flux?”

See page 283 of Oerlemans (2005, Antarctic Research) for ”mass throughput” used this way. It makes more sense to me than mass flux that could be interpreted too locally.

”P1798, L16: Reference for the ’proportional to the production of ocean flow speed and ocean temperature beneath the ice shelf, both of which increase linearly with ocean warming.’ Specifically I’m asking about the ocean flow speed.”

The reference is given in the preceeding sentence: Holland et al. , J. Clim, 2008. For clarity the revised text now reads:

Below I am guided mainly by the comprehensive, 3-D ocean general circulation model study of Holland *et al*. (2008) who find a quadratic dependency on this temperature difference for a wide range of shelf-slope topographies: They find the melt rate is found to be proportional to the product of ocean flow speed and ocean temperature beneath the ice shelf, both of which increase linearly with ocean warming.

”P1799, L6: What do you mean by remote forcing of the ice sheet?”

For clarity, the revised text now reads:

The ice sheet is now also forced by ocean subsurface temperature, T_o , that is determined by a combination of local and remote conditions.

”P1799, L13: Do you mean the whole equation within curly brackets or just the last term within curly brackets. I think you mean the latter. Possibly use the word ’within’ rather than ’in’ for the last word on L13”

For clarity, I revised this text to read:

The term multiplied by ε_2 in Eq. (13) is only considered for a marine ice sheet ($R > r_c$).

Likewise, for consistency I revised and shortened the lines after Eq. (14) to read:

where the terms multiplied by ε_2 are only considered for a marine ice sheet.

”P1800, L1: ’spanning glacial times into past (and future) global warming conditions.’ -> I think you mean past warmer and cooler global climates and that the past warmer climate is analogous the predicted future conditions, but please specify.”

The sentence containing this phrase has been revised and shortened to read:

Figure 3 shows the distribution of AIS ice volume for two different steady state model solutions as functions of Antarctic temperature and sea level for large ranges of T_a and SL that span past and possible future conditions.

”P1800, L4: Sect. 3 has not been discussed yet. Maybe state at the end of the sentence "this will be discussed in section 3" or "discussed in greater detail in section 3"

This text has been revised to read:

Present day ice volumes for transient model solutions slightly exceed this steady state value since these solutions are still responding to past temperature and sea level rises. This will be discussed in more detail in Section 3.

P1800, L13: Volumes decrease rapidly for still warmer temperatures as summertime melting becomes important -> Beyond 5-7degC volumes decrease rapidly as summertime melting becomes important
P1800, L13: ’summertime melting’...It’s not clear that the model has seasonal temperature forcing.”

I find the text as is quite satisfactory but I still made a few changes. Melting has been parameteized in terms of annual mean temperatures but the melting itself occurs in summer. Still , there is no real need to include the word summertime here. The revised text reads:

Steady state ice volumes increase for warming up to 5 – 7°C above present day, reflecting increased snow fall and accumulation for increasing temperature. Volumes decrease rapidly for still warmer temperatures as melting becomes important.

”P1800, L18: ’Increased continental area is needed for reduced snowfall to balance ice flux at the grounding line then’ I am not clear what this sentence is saying. -> I think it means ’Due to the reduced snow fall, increased continental area is needed to balance the ice flux at the grounding line’”

The text has been revised to read:

Due to the reduced snow fall, increased ice sheet area is needed to balance the ice flux at the grounding line.

”P1800, L22: To be clearer change 1. to (1) and 2. to (2) also A higher-order -> a higher-order and Ice flow increase -> ice flow increase”

These suggested changes were adopted in the revised text.

”P1800, L25: ’In this case the third forcing variable – high latitude, subsurface ocean temperature, To – also comes into play’ Change sentence to something like: ’In this case high latitude ocean subsurface temperature, To, becomes influential.”

The revised text now reads:

In this case high latitude, ocean subsurface temperature, T_o, also comes into play.

”P1800: Where did Eq 15 come from?”

This is now expressed in more detail in the sentences before Eq. 15 that now read:

For the calculations upon which this figure is based I related T_o to T_a by applying a second-order polynomial fit to values of these temperatures from reconstructions over the past 240,000 years (Appendix A). A best fit with a RMSE of 0.16 °C was found for

”P1801 L3: on temperature for cold temperatures? Please re-write. Start sentence with ’During colder periods...
P1801, L6: ’can keep up with’ -> ’balances the’ and remove ’to reach that balance" at the end
P1801, L9 for that balance -> for equilibrium”

These changes were made in the revised text.

”P1795, L6 ’The mean Antarctic temperature reduced to sea level, T a’ But in the table there is no ’Ta’, just ’Ta,0’ which is also ’Present day T a reduced to sea level’. This needs to be cleared up.”

There is no need to include T_a in Table 1 (nor SL and T_o for that matter). These are the time-variable forcings of the model and are clearly described as such in the text. Table 1 is reserved for model constants and parameters.

”P1801, L21: What do you mean by first calibration? What is the second?
P1801, L22: ’waxing and waning’ to me implies strengthening and weakening, maybe stick to growing and decaying or growing and shrinking of the ice sheet
P1801, L23: model -> reconstructed or modelled”

The text was revised to address these points and now reads:

The strategy I use to calibrate the DAIS model is to compare hindcasts of sea level equivalent (SLE) from growing and shrinking of the modeled AIS over the last two glacial cycles to paleoreconstructions that provide constraints on this SLE.

P1801, L27: Use semi-colons for the list after the colon

Semi-colons have now been inserted where needed after the colon.

”P1802, L14: less than this for LIG global 15 temperatures less than 2 deg above present day ->less than this given that, during the LIG, global temperatures were more than 2 deg C above present day

P1802, L16: a LIG not an LIG"

The text was revised to addressed these points and now reads:

their contribution to LIG sea level rise was probably considerably less than this given that, during the LIG, global temperatures were no more than 2 °C above present day (Fig. A1). Taken together, these two sources can explain a LIG sea level rise of at most 1 m.

"P1803, L127: Reconstruct the sentence beginning at the end of this line. It is not clear."

Assuming you mean P1802, L27, I revised this sentence to read:

Since temperatures had been slightly warmer than present for thousands of years by the mid-Holocene (Marcott *et al*., 2013), the ocean may have been warmer and less ice than present may have been found in mountain glaciers and ice caps and perhaps also on Greenland (Vinter *et al*., 2009).

"P1804, L4: an AIS size -> a HOL AIS size"

This line (but on page 1803 not 1804) has been revised to read:

, this implies an AIS size then about 2 - 4 SLE above present.

"P1804, L7: maybe use the word configuration rather than setup"

I replaced setups with configurations (page 1803 not 1804).

"P1805, L8: Qualify this sentence: 'This is an appropriate initial condition for the interglacial conditions at 240 kyrBP.' i.e., who says, or why, is it appropriate."

The argument is contained in the word "interglacial" (page 1803 not 1805). To express this more clearly I revised this sentence to read:

This is a reasonable initial condition for 240 kyr BP during an interglacial period not unlike the present one.

"P1803, L15: I would like to know where you got your SLE to volume conversion factor."

I included this in the revised text that now reads:

With the use of Eq. (13), ice sheet volume was then calculated and converted to SLE whereby an SLE of 57 m was taken to correspond to the ice volume of the above steady state model solution, 24.78×10^{15} m^3 (assuming ice and sea water densities from Table 1 with seawater replacing ice below sea level).

"P1803, L23: Now the timing -> In this case, the timing
P1803, L25: Not sure what you mean by commitment"

The text here was revised to meet these points and now reads:

In this case, the timing but not the amplitude of the SLE target at the LIG is achieved. As the model now responds more rapidly to sea level change, much less future sea level rise remains from continuing model response to the last deglaciation (Table 2).

The wording in Table 2 was also changed accordingly.

"P1804, L23: Antarctic Ice Sheet -> AIS, be consistent"

Not changed. See my answer above to R2's first general comment.

"P1804, L25: considered above -"considered in section 3.1."
P1804, L25: Why not use the same colouring scheme for the marker dots as in Fig. 6?"

These are good suggestions and were implimented in the text and in the figure. The revised text now reads:

The four cases considered in section 3.1 are plotted as colored dots with the coloring scheme as in Fig. 6.

"P1806, L4: Antarctic Ice Sheet -> AIS, be consistent"

Not changed. See my answer above to R2's first general comment.

"Fig 6 caption: Is there a better phasing than blowups?

In this caption the word "blowups" has been replaced by "enlarged sections"

"P1807, L7: As shown in Fig. 1, the depressed bed of the DAIS model slopes in this manner. -> As shown in Fig. 1, the depressed bed of the DAIS model slopes in this manner at the periphery of the ice sheet."

This extra wording has been added to the revised text.

"P1808, L23 complex model -> Pollard and DeConto model"

This change has been made in the revised text (but page 1807 not 1808).

"P1808. L2: robust confidence intervals on projections. I would question the use of the word robust given the simplicity, which admittedly is the strength, of the model. Please comment.
P1809, L3: conditions as warm as -> conditions no warmer that
P1809, L8: 'But this is a limitation shared by many other more complex models of the Antarctic Ice Sheet.' This is a very strong statement, please quantify a bit more, or refer to some paper that has analysed the ability of the more complex models to predict or fail to predict WAIS collapse"

These comments refer to the last paragraph of the Discussion that now has been rewritten under consideration of these comments (see my answer above to R1's general comment). Note that no mention is made of WAIS collapse in this revised paragraph. Model limitations with respect to this collapse were already discussed two paragraphs above.

”Appendix
P1803, L3: There are no detailed and reliable time series for sea level around Antarctica so I fall back upon global mean sea level estimates for SL - > last two words superfluous.
P1809, L27 ’For this task I take a very simple approach that however is in tune with the scope of the present paper and model’ -> For this task I take a simple approach in tune with the scope of the present paper and model”

These changes have been implimented in the revised text (all on page 1809).

”P1810, L19 Use brackets around 1. and 2. for ’1. referenced to the 1961–1990 period by referencing to the mean temperature anomaly in the period 500–1500BP from Mann et al. (2008) and 2. divided by”

These changes have been implimented in the revised text.

”P1811, L2: ’For this I used the relation T = −51.5+0.0802 [CH4(ppb)] from a linear regression of referenced Greenland temperature on Antarctic methane for the period 150–122 400BP.’ Do you have a reference or justification for this?”

R2 is right that this should be justified more in the text. This has been done and the revised text now reads:

For the period 128700 – 240000 BP, this temperature was estimated from a proxy based on methane data from an Antarctic ice core (Spahni *et al*., 2005). Global atmospheric methane increases during glacial times are due largely to enhanced emissions from boreal wetlands in response to boreal warming (Fischer *et al*., 2008). A linear regression of Greenland temperature on Antarctic methane for the period 150 - 122400 BP results in the relation T = -51.5 +0.0802 [CH_4(ppb)].

Editor Dan Goldberg (DG):

DG comments:

”The author should take the comments of the Anonymous Referee seriously, and I am in agreement with the majority of the comments. In particular
(1) the suggestion that the author give more detail about why such a simplified approach is preferable to a slightly more complex model.
(2) The suggestion of using an ensemble approach to understand the importance of more parameters than the two examined. The author must show the utility of such a simplified approach, as contributions to this journal must constitute innovative modelling approaches; such an application might do so, as large ensembles would not be as feasible with nonzero-dimensional models.”

These comments were addressed in a revised final paragraph to the Discussion presented above under R1’s general comment.

”(3) The anonymous reviewer has made a good suggestion regarding lag of bedrock adjustment. I think with a little effort this can be implemented without resolving a single spatial dimension.”

This was addressed above under R1’s specific comment number 5.

”In addition, the referees did not comment on this (though it may have been implied by referee #1), but your construction of buttressing, and the loss thereof, is a bit strange. Taken at face value you have an axisymmetric ice shelf extending from an axisymmetric ice sheet – and this can only exert backstress (i.e. affect S, your grounding line ice speed) by exerting "hoop" stress, which depends on ice sheet radius but i doubt is significant for reasonable curvatures, ice shelf thicknesses, and ice shelf velocities. If, on the other hand, you are intending this to be a gross approximation for a number of embayed ice shelves (i.e. Filchner, Ronne, Ross, Amery), the backstresses of which would respond strongly to high melt rates, you should be more clear on this.”

The second interpretation is the one I have in mind. To clarify this, in the revised text I have added a sentence to the paragraph ending on line 2 of page 1799. The added sentence reads:

In the spirit of the original work by Johannes Oerlemans (O3; O4; O5), the ice speed formulation in Eq. (11) is meant to capture the bulk effect of all individual ice streams and associated embayed ice shelves around the AIS periphery.

”I agree more explanation should be given on how the DCESS ocean model calculates ocean temperatures – particularly since it is the continental shelf waters that are in contact with ice shelves.”

This was addressed above in response to R1’s last technical comment.