Firstly we would like to thank Petra Langebroek, 1 anonymous reviewer and the editor for their careful and fair reviews. We have responded to each of the reviewer's comments below. We have also repeated the simulations shown in Fig. 5 and Fig. 7 so that they are of the same duration as the other transient CO2 simulations.

Reviewer 1 – Petra Langebroek

General comments

1) How well do the modern climates of the same GCMs represent a realistic modern Antarctic ice sheet? Preferably by forcing Glimmer, and using the same set-up as for the Eocene simulations. – We have now included a section in 2.3 on GCM performance with modern boundary conditions, with the caveat that this may not have direct relevance to performance under Eocene boundary conditions:

"The GCMs used here have been evaluated previously against modern day observations (e.g. Thompson and Pollard, 1997). It should be noted that modern day performance may not be relevant to performance under Eocene boundary conditions. Connolley and Bracegirdle (2007) evaluated 4 of the GCMs used here (excluding GENESIS, CESM1.0 and FAMOUS) against 15 other GCMs (used in the IPCC AR4) for their performance compared with Antarctic re-analysis output. They assigned skill scores based on 5 variables (mean sea level pressure, height and temperature at 500 hPa, sea surface temperature, surface mass balance), giving a skill score between 0 (low skill) and 1 (high skill). Over the Antarctic region (defined as latitudes greater than 45°S), ECHAM5 had the highest skill score (0.45) of the 15 GCMs based on he 5 chosen variables, with HadCM3 (0.36) and CCSM3 (0.28) 4th and 7th, respectively, and GISS (0.11) 14th. ForAntarctic sea surface temperatures the skill of all of the models was low, in part due to the method used to measure skill, however ECHAM5, GISS and HadCM3 were in the top half of the 15 GCMs. HadCM3 had the joint best skill score for surface mass balance over the Antarctic, with CCSM3 and ECHAM5 also scoring highly (> 0.9), however GISS had a low skill score (0.07) (Connolley and Bracegirdle, 2007)."

Because of lack of treatment of floating ice shelves in the Glimmer ISM, which are an important component of the modern Antarctic ice sheet, we have not performed modern simulations using Glimmer.

2) How do the ice-free conditions in the GCMs affect the threshold results? How different would the GCM climates be if there was an intermediate or large ice sheet considered? Please discuss. We have made this clearer in section 2.1: "...a number of feedbacks on the climate system, such as changes in surface albedo, sea-ice and cloud cover (e.g. DeConto et al., 2007; Goldner et al., 2013). Although the lack of these feedbacks will not affect the threshold for the initial accumulation of ice from ice-free conditions, they may affect the rate at which full scale glaciation occurs.".

Also added to the conclusions: "The offline forcing method we have adopted does not take into account feedbacks on the climate system from the growth of an ice sheet, which could affect the glacial CO_2 threshold. However, our results with GENESIS are comparable to the earlier results of DeConto and Pollard (2003) using an asynchronous coupling method."

3) Orbital parameters:

- For the GCMs ran with different orbital parameters: which orbital setting is used in your equilibrium and transient simulations? – Clarified that the simulations shown in Figures 2 and 3 are with the astronomical configuration closest to modern, also shown in supplementary figure 1.

- HadCM3L does not simulate a different ice volume due to different orbital settings, but this is because it has too warm temperatures in general in order to grow an ice sheet. How large are the variations in ice volume, due to insolation changes, as simulated by GENESIS? – It is not that HadCM3L is warm in general, it is actually one of the coldest of the models over Antarctica (see Table 2), but that it has warm summers (hence the discussion about astronomical variability). We have now included a transient simulation including astronomical forcing in Figure 4 (method described in 3.2), using the climate output from GENESIS. Similar to the result found by DeConto and Pollard (2003) and Langebroek et al. (2009), this produces a glacial inception at a slightly higher atmospheric CO2 concentration.

- Or the other way around: how uncertain are the CO2 thresholds of Fig. 11, due to different orbital settings, and resulting different ice volumes? – The result using the climate output from GENESIS suggests that inter-model (and inter-model configuration) differences produce greater uncertainty than the astronomical configuration.

4) The CO2 thresholds can possible be better defined if we know which model represents the Eocene best. Which of the GCM simulated climates fits best to Eocene proxy data? I know there is not much data available, and you do not need to repeat Lunt's EoMIP paper (Lunt et al., 2012), but some discussion is needed. – We have now included a summary of the results in EoMIP towards the end of 2.3: "The simulations within EoMIP were also evaluated against proxy data (Lunt et al., 2012). The EoMIP simulations had closest agreement with proxy data at higher atmospheric CO_2 concentrations. The simulation with the closest agreement with the proxy records was the CCSM3 H simulation at 16× PIC. However, not all of the GCMs were run at the same atmospheric CO_2 concentrations, precluding a direct evaluation of model performance (see Lunt et al., 2012 for a detailed discussion of model performance). Additionally, atmospheric CO_2 is poorly constrained by proxy records in the Eocene (Beerling and Royer, 2011), making assessment of model skill in the Eocene problematic (Lunt et al., 2012).", this is in addition to the discussion in 3.6 on meridional temperature gradients.

5) I find the order in the paper confusing. This will be largely improved when the GCM climates (temperature, precipitation/snow fall, and seasonality) - the input for the ISM - are discussed before the equilibrium and transient simulations, and not afterwards. – We have made the changes suggested below to the order of the paper, however we feel that moving the discussion of GCM seasonality to after the ISM results may be more confusing, given that motive for this discussion would be unclear without having shown the ISM results first.

Specific and technical comments

Title should acknowledge the fact that the results are based on GCMs with Eocene boundary conditions. – Most of the results in the paper are relevant to general Antarctic glacial inception, the results of previous simulations for the onset of Antarctic glaciation at the Eocene/Oligocene boundary have been reproduced in the context of the whole Cenozoic (e.g. Figure 3 in Zhang et al., 2013, Phil. Tran. R. Soc. A., 371). Additionally, changing the title to "Uncertainties in the modelled CO2 threshold for Antarctic glaciation in the Eocene" could be misleading, as we discuss in the paper we are not stating that there was glaciation during the Eocene, but investigating the potential for glaciation. We would therefore prefer to keep the present title.

(page.line) 5702.3: name the ice sheet model used – Included reference to study, the ice sheet model does not have an official name

5703.5: ". . . ice-sheet and climate modelling studies. . . " – included

5703.8: ". . . glaciation of 2.8 x pre-industrial CO2 concentration (PIC) (~780 ppmv). . ." – changed

5703.13-15: mention glaciation threshold values of references – Included CO2 threshold from Langebroek et al., 2009, Huybrechts 1993 used a temperature perturbation.

5704.9: "ISM model description" – Now changed section title to 'Ice sheet model description'

5704.10: ". . . in this paper. The mechanics . . ." – changed

5704.12, 17: what do you mean with "whole ice sheet ISMs"? – changed

5705:1: why is there no basal sliding? Not prescribed or not occurring? Is there no bottom melting either? – Clarified that basal sliding is not prescribed. Bottom melting can occur.

5705.1-2: “. . . 20 km, and all simulations. . .” – Included

5705.5: vertical lapse rate is not a feedback, but a correction – Clarified that we are referring to height-mass balance feedback.

5705.7-8: not only due to spatial discrepancy, but also for (vertical) topography differences between ISM and GCMs, or? – Clarified that this correction is for vertical and spatial discrepancies.

5705.19: Emphasize that only the climate forcing and the GCM topographies are different. All other boundary conditions (also PDD factors and lapse rate adjustment) are the same in all simulations, right? – Correct, now emphasised.

5705.27: “. . . by e.g. DeConto and Pollard. . ." – Changed

5705.28: delete one “we” – Thanks for spotting, now removed.

5706.4: “thermal subsidence and plate movements”: how did they account for those? Or mention/quantify how they changed TOPO1 to TOPO3/TOPO4 due to thermal subsidence and plate movements. – Referred readers to original references, we do not quantify the difference between TOPO1 and TOPO3/4 (i.e. surface area) due solely to thermal subsidence and plate movements as it is not given in the original references.

5706. 5-6: skip sentence, as it repeats sentence before – Removed repetition

5706.16: what is area of TOPO2? – Included area of TOPO2.

5707.2-8: move to a new section “Simulation set-up” or to end of 2.3. – Moved to end of section 2.3

5708.20: not all paleo-climate are so uncertain as the Eocene/Oligocene. Rewrite. – Agreed, have now clarified that we are referring to the Eocene.

5708.21-22: “100 member” – which parameters are perturbed? Make clear that the one selected out of this 100 is the one that shows the best agreement to the proxy data. – Changed to: “The FAMOUS simulation differs from the other simulations as it was selected from a 100 member parameter ensemble (varying 10 parameters, see Sagoo et al., 2013) based on closest agreement with early Eocene proxy data.

5709.1: All sections 3 include discussion, so call this “Results and discussion” and change 4.1 to 3.6. – Changed

5709.10-13: And where does ice nucleate for CESM1.0 forcing? – Now included CESM1.0 in description.

5709.15-20: That sounds plausible, but GENESIS has even lower climate sensitivity and in contrast has an even larger ice difference between 2 and 4xPIC. Please explain. – Added this point.

5710.3-5: Change sentence. 4xPIC HadCM3L has no ice. – The 4x PIC HadCM3L simulation has minimal ice so this sentence does make sense.

5710.21: Change “To” to “The” – Changed, thanks for spotting

5711.13-19: Rewrite scaling explanation, is not clear. You linearly scale between 2 and 4xPIC, and extrapolate beyond that range, right? – Rewritten this to make clearer: “In these transient CO_2

experiments, we scale between 6× and 0.5× PIC over 1.5 Myr using the climate data from HadCM3L, CCSM3 H, CESM1.0 and GENESIS. We interpolate between the simulations at 4× and 2× PIC and then extrapolate for CO_2 values outside this range (for 6–4× and 2 – 0.5× PIC).

Include info from Supplement in paper. You can show the table and discuss the other info. The table would be clearer if you just show the extrapolated values, and in brackets the difference to the "control" values. – We have not included the figures in the main paper but described the main point of this figure, that this is a relatively small source of error; including the figures in the main paper would add length to what is already quite a long section. We have made the suggested changes to table S1.

5712.18: Change to "Sensitivity to lapse rate adjustment and topography" 5713.17: - Mention default value at the start of the section. – Changed title and added default value at start of section.

- You repeat the simulations with CCSM3_H and GENESIS forcing, not the GCM simulations itself, right? Clarify. – Correct, now clarified.

5713.29-5714.4: Did they allow for changes in (land) albedo? – Added clarification that we are referring to GCM simulations, which include albedo feedback, rather than ISM simulations.

5714.26: ". . . topography (Fig. 7). . ." – Now included reference to figure.

Section 3.4: Please rewrite this section; it is too vague & long – Re-written and removed 2 paragraphs from this section to shorten.

5715.13-14: Change to "It is not immediately clear why there is such variation in ice volume caused by the different climate forcing" – Now changed.

5716.24-25: move explanation temperature half range up to 5715.20 – Moved

5715.27-28: Change to "The lack of ice in the simulations using HadCM3L (and FA- MOUS) is a result of ablation exceeding precipitation." – Changed

5716.1: Mention variables – Changed

5716.22-23: Change to: "simulations which cause the main restriction in ice growth." – Changed

5717.1-2: Change to "lowest annual mean air temperature, but at the same time also the lowest precipitation and the highest annual air temperature half range." – Changed

5717: You mention that you'll determine whether the low precipitation or high seasonality causes the HadCM3L results, but then you continue with comparing annual mean vs half range temperatures. Bit confusing, please rewrite. – Removed this section when shortened.

Fig. 8: Would be much clearer if you do not just show snowmelt, but the difference between snowmelt and snow accumulation, so that it is directly clear is ice can be sustained or not. – This figure was intended to show how the 3 variables passed from the climate model are related to the mass balance, and therefore which of the 3 causes the lack of accumulation. Figures 2 and 3 already show whether there is accumulation.

5718.15-17: rewrite to "... other simulations, and nucleates on Victoria and Wilkes Land instead of Queen Maud Land and the Gamburtsev Mountains." – Changed

5718: "variability from this mean". Please explain. – Changed to: "As these values vary spatially, ice can grow for simulations where the mean annual precipitation is lower than the potential snowmelt."

5718: last section. Can you redo Fig. 8, taking into account elevation lapse rate adjustment? – This figure does take into account the lapse rate adjustment, stated in the figure caption.

Section 3.5: - This section could fit better in the beginning of the Results section, or even in 2.3. – We would prefer to leave this section here, as it could be confusing to the reader to discuss the GCM seasonality without the motivation provided by the ISM results.

- Also, clarify that this is the global picture, not just Antarctica. – Clarified that we mention global seasonality.

2719.6: What do you mean with "annual temperature range"? The seasonal cycle, max/min temperatures, monthly mean or daily? Or transient annual mean temperature variations (guess not)? – Clarified that we are referring to the difference between max/min monthly means.

5720.14: "significant": how did you define this as significant? – removed.

Fig. 9&10: You can reduce the number of panels by plotting the North and South views in one map. – We chose this projection as it shows the poles more clearly, however we have re-plotted them as you suggest.

5720.14-15: FAMOUS has a different seasonality over Antarctica. – Clarified that FAMOUS has a high seasonality over West Antarctica for the modern control simulation.

5720.16-20: What is so different in the HadCM3L boundary conditions, compared to the other EoMIP models? – Unfortunately we have not yet diagnosed why this is the case, hence the discussion at the end of 3.5.

5722.1-5: "replacing the East Antarctic ice sheet"? I thought there was no Antarctic ice sheet included in these GCM simulations? – Rewritten this so that it is clearer.

Change 4.1 to 3.6 – Changed.

5721.15: Change to ". . . glacial CO_2 threshold is . . ." - Changed

5731.18: Mention also the model results plotted in Fig. 11. - Changed

5722.10-11: Delete - Done

5722.11: Change "coupled" to "compared" – changed to 'combined'

5724.11-26: The isotopic composition of the Antarctic ice sheet is not constant, but depends on the size and climate (maybe Fig.7 in Langebroek et al. (2010) is useful). How does that change your computation? – Recalculated based on the values in Langebroek et al. 2010.

5725.6: "less than 1xPIC", be more precise - Changed to "but is not crossed for HadCM3L"

5725.27: add ". . . are correct." - Changed

5726.5: Change "recent" to "previous" – Changed

Table 2: Maybe you can include the number of PDD for each model. I guess HadCM3L has many. – We show the potential snowmelt instead of the number of PDDs as it is easier to relate to precipitation.

Fig. 1B: Why not show the higher resolution bedrock that is actually used? Fig. 2: - Include total ice volume values.
- "Bedrock/bathymetry scale as in Fig. 1" – Now included ice volumes on Figures and added statement to figure caption.

Fig. 3: Include total ice volume values. – Now included on Figures

Fig. 4: Colour the symbols same colour as the corresponding lines. – Now changed

Fig. 6: Enlarge legend. Change titles to “HadCM3L – ISM” etc. – Made the legend larger. These are not anomaly plots but absolute GCM elevations.

Fig. 11: - Change time direction for paleo scientists. - Check colours (is PD2005 brown and pink?) - Include (if possible, at least for GENESIS), uncertainties due to different orbital settings. – Have now changed the time direction and corrected the colours.

Reviewer 2 - Anonymous

General comments:

The authors have shown clearly the CO_2 threshold for Antarctic glaciation depends on 1) lapse rate, 2) Antarctic bedrock topographies, 3) climate model. But the authors concluded in the last sentence of the abstract that 560-920 ppmv is the threshold for Antarctic glaciation. I assume this threshold is derived from the summary figure, Fig. 4, which is only based on single lapse rate of 7 K/km (Fig. 5) and single topography (TOPO1, Fig. 7). I suggest the authors recalculate the CO_2 threshold for Antarctic glaciation with all three uncertainties (lapse rate, Antarctic bedrock topographies, climate model). – We have now removed this sentence from the abstract, the conclusions mention the range in atmospheric CO_2 threshold resulting from a change in the lapse rate.

Minor comments:

5702. 19-20: growth of an intermediate sized ice sheet (> 25 m sea level equivalent) occurs with for atmospheric CO_2 concentrations in the range of 560–920 ppmv, which is consistent with previous studies. - Changed

5703. 13. earlier than the this event, during the Eocene (Miller et al., 2008). - Changed

5703. 19. to investigate the model dependence of the Antarctic atmospheric CO_2 threshold for Antarctic glaciation. - Changed

5704. 10. We use the Glimmer ISM in this paper, and the mechanics of this model are documented in Rutt et al. (2009). - Changed

5705. 1. The ISM has a spatial resolution of 20 km x 20 km, and all simulations are initiated from ice-free conditions. - Changed

5705. 10-13. Previous modelling studies suggest that Antarctic glaciation generates a number of feedbacks on the climate system, such as changes in surface albedo, sea- ice and cloud cover (e.g. DeConto et al., 2007; Goldner et al., 2013)., Wwe
acknowledge the limitations of our methodology in representing these feedbacks. - Changed

5705. 18-19. The mass balance scheme adopted is the widely used positive-degree day (PDD) method (Reeh, 1991). Is the PDD method the state-of-art mass balance scheme, or there is more advanced mass balance schemes? Please discuss the potential uncertainty associated with different mass balance schemes. – Now mention alternative energy balance model, which is not used as it is not included in the Glimmer ISM, we mention that using a different surface mass balance model could affect our results in the conclusions.

5710. 21-22. to The model checks for potential negative values for precipitation resulting from this scaling and resets these to zero. - Changed

5720. 12-18. ... This suggests that the strong HadCM3L seasonality is caused by the change to early Eocene boundary conditions, although it is interesting that a similar change does not affect the other GCMs.

I don't agree. Clearly HadCM3L has the strongest seasonality in modern control simulations among all GCMs in Fig. 10. – Having looked at means over Antarctica, HadCM3L has a slightly greater seasonality than the multi-model mean (~3C), which is now acknowledged in the text. This remains much smaller than the bias HadCM3L exhibits in the Eocene.

5721. 14-15. The offline simulations undertaken in this paper suggest that the modelled glacial CO_2 threshold for Antarctic glaciation is highly climate- model dependent. - Changed

5726. 1. than that some other mechanism prevented glaciation. – Changed