Reply to the anonymous reviewer 2

We would like to thank the reviewer for his/her very clear comments. The remarks have definitely improved the manuscript. Below you will find a point-by-point reply, with our reply given in blue. We hope that we have answered all questions sufficiently.

Major points

The major reproach is that the grounding line migration is almost not dealt which in the paper while many studies have demonstrated that it is a key issue. Solving shallow equations for both grounded ice and ice shelves is not sufficient to simulate grounding line migration. We know from the various MISMIP experiments that only a specific parameterization prescribing ice flux at the grounding line allow for a proper grounding line treatment at the time scales considered here. Some other crucial processes such as calving or basal drag are not detailed enough. I understand that this comparison was done with existing models and that no specific development were done, but at least the new version of the paper should:

Similar to the comment of the first review by David Pollard we have included a new paragraph in the 'Discussion' section that discusses the processes involved in grounding-line migration and its relation to our experiments.

Be careful that the models descriptions are more homogeneous and all include how are modeled the following processes:
-Grounding line. This is the most important. How is it done (flux or other specific parameterization, subgrid, or no special care), what is the resolution at the grounding line (if a subgrid is used).
- How is implemented basal melting right at the grounding line. It seems a technical aspect but it is a well know trick to make a grounding line retreat for the wrong reasons. It seems especially important to know for SICOPOLIS (see p. 5553) that has a specific value at the grounding line (high) and no treatment of the GL migration.
- calving, both in the case of retreat and advance (many calving schemes prevent ice shelves to grow).
- basal drag (or distribution of sliding coefficient). If it has been calibrated to give a steady state for present conditions, with which atmospheric fields?
- are all the models on a 40x40 km grid? (as are the projected forcing fields)

This could be done quite easily in table 3 with eventually details in the text (preferably more explicit than for instance the following description in AISM-VUB "coupled across a one grid-box wide transition zone").

- As follows from the suggestion by the reviewer, we have added an additional row in Table 3: 'treatment of the grounding line'. In the model description, Section 2.3.1-6, we provide now some more details on the treatment of the grounding line.

- If basal melting at the grounding line is applied right at the grounding line, details are also given in Section 2.3.1-6. Only for SICOPOLIS a specific regression is used for the grounding line.

- Calving details are given in the specific sections 2.3.1-6. Most models employ a simple scheme that terminates ice at the edges of the shelves.

- More information on the treatment of basal sliding is given where needed in sections 2.3.1-6.

- Yes all models are run on a 40x40km grid, this is now mentioned in the text.

In the results analysis, try as much as possible to make the distinction between surface mass balance effects and dynamical effects (likely driven by oceanic forcing). For instance, systematically discuss the extent versus the volume. How much does the extent explain differences between volume, can you explain the spread of the models as a function of grounding line retreat in figure 8 (and 7)

In the Section 3, Results, we now try to make a distinction between SMB effects and ice dynamics as much as possible. In Section 3.1, for the $Control_{HadCM3}$ experiments, the larger volume of PSU-ISM and AISM could be due to a slightly higher SMB, since the ice flux across the grounding line is large too. The smaller volume and extend of ANICE and SICOPOLIS is due to more basal melting relative to the other models. For the control experiments it basically holds that a large grounded area of the ice sheets corresponds to a larger volume. In Sections 3.2 and 3.3 we have added a couple of statements on differences between models. In Section 3.4 a new paragraph is added discussing the results of the Pliocene experiments, see also our comments to the next question.

A major finding in this intercomparison is that, for Pliocene climate, the initial ice sheet geometry plays an important role on the final (at steady state) ice sheet with always a larger ice sheet when starting from the present day (larger than PRISM3) geometry. Both Pliocene simulations were done with the same forcing fields (except for surface temperature that is corrected with a lapse rate). This raises questions that have to be discussed. Are there two branches depending on the initial condition and if so, are these branches numerical (due to grounding line modelling or is it an ill-posed problem) or are they physical (due to various feedbacks, and then which ones)? Is it a general behaviour for antarctic models if they were applied to other time slices / climate ? This could be tested by an additional experiment with present day climate and initial PRISM3 ice sheet.

Initial conditions are naturally of importance. Also reviewer 1 pointed this out and suggested to perform a new experiment. We believe this new experiment (explained below) adds sufficient discussion on how much the experiments performed here depend on the initial conditions. We have performed a new experiment, named $Pliocene_{PD-Ant}$. For the initial ice sheet for this experiment, as suggested by reviewer 1, we have used the equilibrium results of the $Control_{HadCM3}$ experiment (after 100 kyr of simulation time). This ice sheet is then forced, again for 100 kyr, with the climate of the HadAM3 climate model using PRISM3 BC, except a modern Antarctica. This new experiment is discussed in Section 3.2. How the ISMs are set up now (i.e. quite a course resolution and not all models have additional grounding-line physics included), the models are not capable of simulating a retreat of the ice-sheet from its modern grounding-line position. Grounding-line physics are now much more discussed in the light of our results in Sections 4, 4.1 and in the Conclusions, Section 5.

Detailed comments (from supplement)

p. 5541, line 17 "*We include an overview ….how specific model configurations influence the resulting Pliocene Antarctic ice sheet*". I do not think that this point has been fully addressed, especially concerning the grounding line migration.
More details on the grounding-line migration and (if used) calving are given in Section 2.3.1-6.

p. 5546, line 1. How does ERA40 compare with observations ?
As was shown in Bromwich and Fogt (2004) the comparison with (although sparse) observational data over Antarctica shows good skills since 1970 and improves even more after including satellite data in the reanalysis, since 1978. We use here the 1971-2000 climatological average and hence feel confident this is a good data set for comparison.

Reference

Bromwich, D. H. and Fogt, R. L., 2004. Strong Trends in the Skill of the ERA-40 and NCEP--NCAR Reanalyses in the High and Midlatitudes of the Southern Hemisphere, 1958-2001, *Journal of Climate*, **17**, 4603-4619.

p. 5546, line 10. Please add here the information that is given p. 5553, line 16. "*ocean temperatures are adjusted according with the depth of the bottom of the shelves*".

Done

p. 5547. line 21 " I*n general velocities are underestimated by the SIA and overestimated by the SSA*" It is not due to the approximation themselves SIA or SSA (BTW, Ma et al. 2010 paper was based on full Stokes experiments) but it is linked to the role of ice anisotropy acting differently in the case of shear stresses in the vertical plan (taken into account in SIA) and horizontal stretching (computed in SSA). So the objective to use different enhancement factors for SIA and SSA is a way, for a model with isotropic flow law, to approximate the effect of anisotropy and better compare with observed ice flow.

Absolutely. This is now added in the text.

p. 5548. line 1 "*...own parameter settings for the thermodynamics, mass balance and ice flow as would be used for regular paleoclimate simulations*". Basal boundary conditions are also crucial, please mention here that it is also part of the setup of a model and state how it was done in every model.
Where needed, in Section 2.3.1-6 we have included more information on how basal sliding is calculated.

p. 5550, line 17 "*Basal sliding is included as a Mohr–Coulomb plastic law*". Is the plastic law coefficient uniform over the ice whole continent?
No. A till parameter, the friction angle, is a function of bedrock elevation (see De Boer et al., 2013). This is now mentioned in the text.

p.5551 line 16 "*that is in a Surface melting*" something missing?
The part "Under a … that is in" is accidently placed here and now removed.

p.5551 line 24 "*such that shelves thinner than 250 m are automatically calved*". How does this method allow for a growing ice shelf?
In the simulations with PISM two calving criteria are used: firstly, we use the eigen calving approach of Levermann et al., (2012) which predicts calving losses according to horizontal spreading rates, and secondly we impose a thickness limitation, such that shelves thinner than 250 m are automatically calved. The latter is a tuned value found through experimentation to yield ice shelf extents of reasonable fit to observed geometries."

Reference

Levermann, A., Albrecht, T., Winkelmann, R., Martin, M. A., Haseloff, M. and Joughin, I., 2012. Kinematic first-order calving law implies potential for abrupt ice-shelf retreat, *The Cryosphere*, **6**, 273--286.

p. 5552, line 14 (Rimbay). "*RIMBAY is based on the 3-D ISM by Pattyn(2003) and a full description is given in Thoma et al.(2014). RIMBAY combines SIA and SSA velocities in a similar way as PISM and ANICE*"

I am a bit puzzled by this description because Pattyn (2003) solves a 3D system for ice flow and the second part of the sentence is contradictory because combining SIA and SSA is a vertically integrated approach. I understand that RIMBAY is a multi-approximation 3D model, but that only the vertically integrated part has been used here. This point should be better stated.
We now state that we use here the shallow-approximation version of RIMBAY.

Results

p. 5554. line 4 "*PSU-ISM (green), show a smaller initial increase in ice volume*". The small initial increase could be due to the initialisation procedure.

We added the sentence: "The small initial increase could be due to the initialisation procedure and the different basal-sliding parameterisation (Section 2.3.4)."

p. 5554, line 16. "*Nonetheless, the topography and the extent of the ice shelves are rather similar compared to the PD initial ice sheet"*

This is rather wrong for PSU that extent substantially in the Ross sector. Was this model calibrated with ERA-40?

All models were used with their standard setup, in the case of the PSU-ISM this gives a better volume for the ERA-40 simulation, not a specific calibration to ERA-40. For the $Control_{HadCM3}$ differences in ice thickness are comparable to the other models (new Fig. S3). In terms of the ice extent in the Ross sector, the grounding line for the PSU-ISM

We have changed the sentence and added a remark on the PSU-ISM extent of the Ross ice shelf: ".. are similar compared to the PD initial ice sheet for almost all models (Fig. 3). The PSU-ISM does exhibit a retreat of the Ross ice shelf, largely induced by substantial sub-shelf melting."

P 5556 , line 17 "*plotted ice sheet presence which shows how many of the six ISMs predict ice of any thickness in that particular grid box*" Is it for grounded ice or for any ice (grounded and floating ?)

This is done for both grounded and floating ice, we have added 'both grounded and floating ice' in the text and in the caption of Figure 7.

p. 5559. line 9 "*Differences between model is largely due to the variability in ice fluxes, whereas the average SMB for the six ISMs is 2113.3 ± 129.7 Gt yr−1 (Gt = 1012 kg) and the ice flux across the grounding line is 346.5 ± 147.8 Gt yr−1 at the final step of each 100 kyr simulation*"

I do not understand. The ice sheet should be in equilibrium by this time so why are the SMB and ice flux so different?

This is mainly because ice fluxes are calculated afterwards from final ice thickness and the vertically averaged velocity fields. This is now mentioned in the text, that it provides an indication of the mass budget of each model, and not exact numerical calculations.

Figure 1. It is difficult to really see the differences between simulated climates, would it be possible to have the anomalies for surface temperature and precipitation (eventually in the supplementary material).

Differences in climatology between the two figures are now shown in the new Figure S1.

Figure 8. It would be easier to follow if each profile kept its name A,B,C from figure 7 and eventually A1, A2 for Control and Pliocene (rather than a,b,c,d,e,f).
The letters of each profile are now placed after the names in the plots, and mentioned in the caption. Figure labels a-f are still in place for reference.

Figure 9. It would be better to have maps of differences between bedmap1 and bedmap2
We believe it is sufficient to use Figures 9 and 5 for a comparison of the two simulations with Bedmap1 and Bedmap2. Also the now included ice areas in Figure 10 (see comment below) provide additional information on the differences.

Figure 10. It would be good to have the grounded extent (2 more panels).
Yes, we have added the 2 panels with grounded ice area, and included some referencing in the text.