Response to referee comments on 'The Potsdam Parallel Ice Sheet Model (PISM-PIK) - Part 2: Dynamic Equilibrium Simulation' by Martin et al.

We would like to thank both referees for reading the manuscript and providing their criticism. Following replies to their respective general criticism, we provide detailed responses to each specific point. We here present suggestions for changes in the manuscripts which we will conduct after the editors decision on the necessary revisions.

Anonymous Referee 1
Received and published: 22 October 2010

A nice piece of work. The authors have taken a well-written model (PISM) and extended it in ways that are absolutely necessary to study fast modes of change in the Antarctic Ice Sheet. This manuscript represents the next logical step after to model development - a whole-continent steady state experiment and comparison with observed fields. The use of simplified parameterizations of complicated process provides a useful way to assess the "minimum" of what is needed to capture a "snapshot" of Antarctica as it is today. (I would caution, though, that this is not necessarily the same as the "minimum" needed to project future change, as e.g. it presupposes where subglacial water may exist.) Many of the most relevant values have been compared between model and observations, I feel, and I look forward to seeing results from future experiments performed with the model.

I have a few comments and questions which I would like to see addressed.

1. Regarding ocean melting, it was nice to see that some attempt at realistic melt rates was made, but I have some questions about that:

i. What was the basis for the choice of γ in the melt parameterization? In HJ99 this is related to mixed layer speed (and possibly a few other things) - a rationalization in the text would be nice.
Holland & Jenkins (1999) cite Hellmer & Olbers (1989) with a value of $\gamma = 10^{-4}$, which we took as a starting point. We have however observed that in the Antarctica simulation this choice results in strong grounding line retreat. This is the reason for the introduction of the model parameter F_{melt}, with a value of $F_{melt} = 5 \times 10^{-3}$, determined by tuning of the model to fit the present state of Antarctica. Hence a more careful choice of γ would suggest a relevance for model results that is not given due to F_{melt}.
We propose to give a similar reasoning in the manuscript, to clear up our motivation for the choice.

ii. The amount of heat available for melting at the ice shelf base depends on the heat flux

into the ice shelf. This does not seem to be accounted for. How did k dT/dz|z=b (heat flux into the shelf) compare with the flux from the ocean?
Please see response to next remark (iii).

iii. Why would the basal Dirichlet temperature condition not be the freezing point? Temperature should be continuous at the interface. Seems especially strange because this would make the ice temperature even warmer than the water, which should not be true anywhere in the ice shelf (given that the air temp. at the upper surface is even colder).
We would like to answer the upper two remarks in one, because they relate to the temperature boundary condition at the base of the shelf. The core of the problem is that we do not resolve the (turbulent) oceanic mixed layer underneath the ice shelf in which the H_2O phase changes from frozen without significant salt content to liquid with a salinity above 35 psu occur.
We solve the problem in a standard way providing boundary conditions both for the temperature field as well as a sink/source term for the ice thickness equation which represents basal melting and refreezing.
First, let us consider the temperature equation: Since we are only interested in modeling the dynamics of a full body of ice for which the continuum field equations are valid, we define the frontier of this domain as ice which is at pressure melting temperature. Consequently, the bottom boundary condition of the ice has to be given by the pressure melting temperature T_{pm} which is determined by the ice thickness only. This yields the Dirichlet boundary condition for the temperature field in PISM-PIK.
Second, let us consider the mass loss/gain at the base of the shelf: A warm, but more importantly saline ocean does result in melting of the ice at the base of the ice shelf. The melt rate is computed through a heat flux which is equivalent (via a constant) to a temperature difference. In order to compute this melt rate we have several possibilities. Following (e.g., Beckmann & Goosse, 2003, Holland & Jenkins, 1999, Hellmer & Olbers, 1989) we compute a virtual temperature T_f which is the freezing temperature of water at the pressure as present below the shelf and with a salinity of 35 psu. This transition occurs in the oceanic mixed layer which we can not resolve in our approach. The equivalent heat flux (which represents the melting effect of the ocean onto the ice through BOTH temperature and salt) is then assumed to be proportional to the temperature difference between T_f and the oceanic temperature T_o which we set to be constant.
This way we provide a Dirichlet boundary condition for the temperature field and a source/sink term for the ice thickness equation. It is important to note that this approach is not new but has been adopted from previous work. We will try to improve the explanation of this aspect in a revised manuscript.

iv. I was not a referee on Bueler and Brown 2009 or the companion to this paper, so I must ask this here. I scanned those papers and could not get a clear answer - is the temperature *equation solved in z-coordinates or sigma (terrain-following) coordinates? If it is the former, I think your scheme for temperature evolution needs to be explained in detail, either in this paper or its companion. Keeping a Dirichlet condition on a moving (?) boundary in a z-coordinate frame is not straightforward.*

The coordinate system is not sigma-type. The z-coordinate used in PISM and PISM-PIK measures the vertical distance above the bedrock (grounded ice) or above the ice shelf base (floating ice), so the zero level for z is always the base of the ice. The coordinate is not sigma-type because the coordinate is not scaled with ice thickness. This grid choice, and thermomechanical verification results from PISM using this grid choice, are documented in (Bueler *et al.*, 2007). The reason a sigma coordinate is not used is because the scaled conservation of energy equation has singular conductivity at an ice sheet margin, a bad consequence of the sigma coordinate system.

We propose to clarify this in the companion paper.

v. Did you calculate the Peclet numbers related to the melt rates? Was the resolution you used sufficient to capture the associated boundary layer?

This is a reasonable question which probably deserves further attention in the ice sheet modeling literature, but which is beyond the scope of the current paper. We assume the referee refers to what we would call the "*mesh* Peclet number". In the context this is the ratio $X = \rho_i c_i |v_3| \Delta z / k_i$ which compares the transport effects of the vertical energy advection ($\rho_i c_i |v_3|$) to the diffusive transport by vertical conduction ($k_i/\Delta z$) over the same distance in a given time step. X is large when vertical velocities v_3 are large compared to conduction. Large vertical velocities at the base of the ice are indeed significantly related to high basal melt rates, though this is not the only source of high vertical velocities in the ice column. A specific analysis using X to determine the basal grid spacing, necessary to resolve the boundary layer using a classical centered-space explicit-time scheme for the problem, has *not* been carried out for PISM. Instead, an implicit upwind-when-needed vertical scheme has been implemented in PISM to address the same issue. A theorem is proven in the PISM documentation on the stability of this adaptive scheme; see this page in the online PISM documentation:

`http://www.pism-docs.org/doxy/html/bombproofenth.html`

The theorem in question specifically uses the dynamically-computed mesh Peclet number to determine if the implicit centered scheme should be swapped for implicit upwinding in the given column of ice. This swap reduces order of the scheme but maintains a stable approximation of the boundary layer, regardless of the user-adjustable choice of vertical grid. Automatic choice of an equally (or unequally) spaced grid to achieve a certain error tolerance, and not just stable approximation, would be a worthy addition to PISM.

2. page 1314, line 12: how similar is your P_{eff} distribution to the thermodynamically defined one? can you show a figure?

In a revised manuscript we will provide the figure for the referee, since we already have 15 figures in the paper, we propose not to put it into the manuscript unless editor and reviewer insist.

3. page 1316, line 10: is this volume of fluctuation of floating ice due to variability in floating front extent or in ice shelf thickness? Or something else?
This is caused by calving events, hence it is indeed the floating front extent.

4. page 1318, line 11-12: how does this happen if a CFBC and subgrid ice front migration is not implemented for ice cliffs and marine fronts (as you say later)? If ice that moves past a cliff or a front is "calving off in the same timestep", then within that timestep you presumably assess the principal stresses and add or remove mass from those floating partial-cells. How/where are the principal stresses evaluated? This process needs to be addressed in this manuscript or one of its companions.
We do apply a CFBC at the ice cliffs and marine fronts, only the subgrid interpolation is not implemented for these types of ice fronts. So within one timestep, first the stresses from the current ice geometry are evaluated and velocities are calculated accordingly. Secondly these velocities are used to update the ice geometry, i.e., ice is transported from one box to the other. E.g. a small amount of ice is transported from a grid cell at a marine front into the adjacent ocean grid cell, forming a shelf of the length of one grid cell and of a very small thickness, typically only a few meters. Thirdly calving happens, in the case of these new small shelves, the whole shelf (i.e. the grid box with the very thin shelf) calves off. Only then a new timestep begins, again without such a very thin and small shelf. Hence we never evaluate stresses for a partially filled cell.
Generally at the fronts of larger shelves we apply the CFBC at the last 'full' cell. We add and remove mass from the partially filled cells until they become either 'full' or 'empty', i.e., transformed into a shelf cell or an ocean cell.
We propose to try to explain this in more detail in the manuscript, also in the companion paper.

5. Has the "dynamical core" (i.e. the isothermal velocity solver) or PISM-PIK (or PISM) been tested against the results of the ISMIP-HOM intercomparison? I am curious to see how it compares with Blatter/Pattyn. Looking at Fig. 12, it looks like velocity is underestimated in a few ice streams where, as far as i know, there is a nonnegligible vertical shearing component. One difference in the velocity solve between PISM-PIK and other "hybrid" solvers, e.g. Pollard and DeConto 2009, Schoof and Hindmarsh 2010, and Goldberg 2010 (in press) is that in PISM-PIK, the strain rates from one lowerorder balance are not accounted for in the viscosity of the other. I don't know how large a factor this is.. could be that the SIA enhancement factor more than compensates. I wouldn't expect this final comment to be accounted for in the manuscript, but I would just like to open it up for discussion.

The PISM developers are indeed interested in comparing the hybrid in use here, from Bueler & Brown (2009) but with PISM-PIK improvements, to the other hybrids identified by the referee, and to Blatter-Pattyn, and to full Stokes. The ISMIP-HOM intercomparison would be a worthwhile exercise for a PISM user perhaps, but, for the reasons described in the Cryosphere-published discussion of the ISMIP-HOM paper, the result of running PISM on ISMIP-HOM would probably be that the results "look pretty good" and that few further precise conclusions could be drawn. That is the nature of intercomparisons.
Two SSA verification tests already included in the PISM source, one of which is the Schoof (2006b) exact solution already addressed in Bueler & Brown (2009). These tests address the question of whether the velocities computed from given basal resistances by the harder SSA portion of the hybrid are correct. An additional large number of SIA tests are implemented there. The referees question about the relative quality of the hybrid remains. Note that the hybrid was built, and Bueler & Brown (2009) was published, in part to address the issues raised in ISMIP-HEINO, which required whole ice sheet thermomechanical modeling, not the small scale models suitable for ISMIP-HOM.
The direction currently being pursued by the PISM developers is to build trusted Blatter-Pattyn and full Stokes *flow-line* solvers with the ability to scale to the same fine grids already achieved in PISM by the SIA+SSA hybrid implementation, and compare solutions using these relatively-precise tools. These tools can be made precise because there are unexploited exact solutions in the literature, and further constructable exact solutions. Indeed verification of full Stokes solvers is by no means out of the question (e.g. from Ladyzhenkaya, 1963 to A. Sargent and J. L. Fastook, 2010. Manufactured analytical solutions for isothermal full-Stokes ice sheet models, The Cryosphere, 4, 285-311). To use the referee's phrase, this will remain "up for discussion" for some time.

Anonymous Referee 2
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General comments
Martin et al. present first results from an equilibrium simulation of the present-day Antarctic ice sheet with the Potsdam version of the PISM code. The reader is confronted with a limited selection of rather fragmentary model output compared with observations that are used to support a number of important claims. These include that the model is able to reproduce large-scale dynamic features of the Antarctic ice sheet such as ice streams, grounding zones, and calving fronts, and that this can be attributed to their new hybrid scheme superimposing SIA velocities on a SSA solution, as well as to their novel treatment for calving fronts. Despite this potential promise I believe the paper is problematic in several respects. The discussion lacks focus and too many statements have to be taken on face value. Even when consulted together with the companion paper of Winkelmann et al. (2010) many details of the model remain unclear. Both papers touch on many different aspects of Antarctic ice sheet

dynamics, however none are discussed in sufficient detail to properly assess the merit of the new approaches with respect to previous 3-D Antarctic model results. Notably, this paper, as well as its companion paper, remains very vague on the most crucial aspect of Antarctic ice dynamics, namely the interaction between ice-sheet and ice-shelf flow and the associated issue of grounding-line motion. Also the role of ice thermodynamics, while supposedly being part of the model, is underexposed in the discussion. My feeling is that the modeling has not matured enough yet to convincingly support the main conclusions. The paper could be much improved by clearly demonstrating what is new with respect to previous work, and how new approaches really make a difference with respect to 'classical' SIA models. Additionally, the model would gain credibility when results from more stringent validation experiments would be shown. As the paper stands now, little additional insight is given in the dynamical properties of the PIK-PISM Antarctic model and on whether the new approaches really make a difference with respect to long-standing model practices.

In this paper we present the results of an equilibrium simulation of the Antarctic Ice Sheet, aiming at reproducing the presently observed ice sheet. The intention is to test the additional model components that have been added to the base code of PISM, which is described in Bueler & Brown (2009). Those new components (described in the companion paper, Winkelmann *et al.*, 2010) were necessary to prepare the model to simulate the Antarctic sheet-shelf system, especially the shelf dynamics and hence the buttressing effects (see below) of the big ice shelves on a marine ice sheet like West Antarctica.
The general intention of this kind of paper is to provide a citable model presentation that can be referred to in later publications that will explore in greater detail PISM-PIK's ability or lack of it to model marine ice sheets.
What we provide here hence represents a new marine ice sheet model in several aspects of its approach to model stress transmission and shelf dynamics. The fact that we are able to simulate several flow regimes and fast as well as slow processes is contributed to the SIA/SSA hybrid scheme, but we do not and should not claim that there is no other way to achieve that, especially compared to earlier 'classical' SIA models that employ a sliding law. We propose to state that clearer in the manuscript and specifically ensure not to make statements that claim a strong difference in resulting dynamics over earlier models. Especially the question whether the dual approximation is indeed a valid approximation of the Stokes equations in the transition zone has to be deferred to other publications, because it is beyond the scope of this publication. We do however believe that the fact that the SSA velocities are computed simultaneously for the shelf and as a sliding velocity for the sheet provides a good alternative to boundary conditions to be imposed at the grounding line. Since SSA velocities are computed non-locally they provide a natural way to take into account buttressing effects, and stress transmission across the grounding line is guaranteed. We will make an effort to make that point clearer in the manuscript as well.
The computation of thermodynamics in PISM-PIK has not changed compared to PISM as

described in detail in Bueler & Brown (2009), Bueler *et al.* (2007, 2005), which is why we prefer not to repeat it in this paper or its companion. However, the role of thermodynamics in determining the sliding has changed (see answer to comments on p. 1312, lines 18-19 and on p. 1314, lines 15-17), and we propose to stress this aspect stronger at the respective places in the manuscript. Also the fact that the steady state we present is in dynamic thermal equilibrium in addition to the equilibrium with respect to geometry and velocity field will be stressed.
We thank the referee for pointing out the importance of the question of stress transmission across the grounding line and its migration. We propose – in order to better and adequately address the interaction of ice-sheet and ice-shelf flow – to add a new paragraph to the manuscript which will cover this question in more detail. This new paragraph will contain a clarification of the fact that although the position of the grounding line in PISM-PIK is determined merely by the floatation criterion, we do provide a new treatment by allowing for stress transmission across the grounding line via the non-local computation of SSA velocities and thereby are able to simulate buttressing effects of ice shelves on the ice sheet upstream. Concerning the validation of this implicit grounding line treatment we agree that in its present form the manuscript lacks a demonstration of its usefulness. There is however a MISMIP-based comparison in Part 1. The more basic, mathematical task of model verification is carried out in Bueler & Brown (2009) and Bueler *et al.* (2007) and references therein. Although the results of the MISMIP experiments presented in the companion paper (Winkelmann *et al.*, 2010) demonstrate that grounding line migration is reversible on a sloping bed in a flow-line setup, we agree that the situation is different for a 3-D setup with an ice shelf that provides buttressing from lateral friction or pinning points. We therefore propose additional transient experiments with climate forcing to demonstrate the ability of the model to simulate the according shifts in grounding line position (see further details in the answer to the first comment on page 1316, below).
Regarding validation of the whole Antarctic model, figures 12, 13, 14, and 15 each compare modeled surface velocities to observed values on a whole ice sheet scale. The modeled surface velocities are from a dynamic equilibrium simulation in which the surface was not held fixed. By comparison, early work (e.g. Ritz *et al.*, 2001) do not evaluate the model by the quality of surface velocities. The well-known recent model by Pollard & Deconto (2009) shows modeled surface velocities but makes no quantitative comparison to observations of the same quantity.
Only regional models in which the surface is not allowed to evolve (e.g. Joughin *et al.*, 2009) have, so far, included careful comparison to observed surface velocities (or inversion thereof). Indeed, PISM-PIK is setting a new standard for validation for whole ice sheet models in this area.

Specific comments
p. 1310, line 9: presumably 'bed' topography is meant here.

Yes, we will clear that up.

p. 1310, eq. 3: it is curious that the elevation dependence of the surface temperature parameterization is dropped over ice shelves. Ice shelves are flat with elevations between 20 and 100 m and so the elevation term in eq. 2 would only account for 0.5 K over ice shelves anyway, presumably well below the uncertainty introduced by the other terms of the parameterization.
We thank the referee for pointing that out, we agree with his/her argumentation. We propose to alter the parameterization in dropping the artificial splitting in regions with surface elevation above and below 100 m, and instead use a slightly altered version of equation 2, namely $T_\mathrm{s} = 273.15 + 30 - 0.0075h - 0.6878|\Phi|$. This results — for the presently observed ice sheet — in a surface temperature field that agrees with observations similarly well as the old parameterization.

p. 1311: The rates of basal melting below the main ice shelves obtained with the parameterization seem very modest (between 5 and 20 cm per year, i.e. generally less than surface accumulation). This parameterization seems to be a new aspect of the modeling. Are the magnitudes and patterns displayed in Figure 1 in any way supported by inferences from observations and by more comprehensive models of subshelf water circulation? A more thorough discussion of the merits of the parameterization would be helpful.
The idea behind the choice of this parameterization was to use a very simple way to compute subglacial melt rates for the ice shelves, that are in their pattern supported by the work of Beckmann & Goosse (2003). The fact that the melt rate is — via the shelf bottom temperature — closely related to ice-shelf thickness and hence results in the pattern shown in Figure 1, was supposed to open up the possibility to influence modeled ice shelf thickness in turn. During the tuning procedure however we had to recognize that the use of the model parameter F_{melt} was necessary to decrease the melt rate to the very low magnitude shown in Figure 1 to avoid strong grounding line retreat.
We propose to explain this in more detail in the manuscript.

p. 1312, line 12: 'exemple' should be 'example.
We thank the referee, we will of course correct that.

p. 1312, Fig. 2: the authors contribute the existence of a transition zone from ice sheet to ice shelf flow ('onset of an ice stream') to their specific SSA/SIA treatment. I doubt that is really the case. Any classical SIA model that includes basal sliding of some sort finds basal sliding to be dominant in the flow near the margin. Have the authors ever compared their SSA basal sliding results with the SIA version of their equation 9, i.e. without the inclusion of horizontal velocity gradients? Both approaches are expected to be mainly controlled by the treatment of basal shear stress (eq. 9) and may well be virtually identical. It would help

the discussion if the authors could demonstrate that their SSA results differ from an SIA treatment to substantiate their claim that the hybrid SSA/SIA approach is decisive for a more realistic modeling of ice streams.
The referee proposes an SIA-only model with a sliding law, and presumably a thermocoupled model is meant (as PISM-PIK is also). Serious consideration of the results from such SIA-only sliding models suggest great caution. ISMIP-HEINO (**?**), in particular, generally demonstrated the difficulty such models experience with dynamically-evolving basal resistance fields. In fact SIA-only thermomechanically-coupled models which allow sliding using evolving basal resistance fields are generally subject to the failures described qualitatively and quantitatively in Appendix B of Bueler & Brown (2009). These are reasons why it is not natural to "demonstrate that their SSA results differ from an SIA treatment" as a standard for quality of an Antarctic model generally. For fast sliding the SIA treatment is presumptively wrong anyway, and the natural standard is the one identified by Referee 1, who asks about comparison to Blatter-Pattyn models and ISMIP-HOM. This latter standard is agreed by all to be a critical topic of current research, but it is beyond the scope of the current paper.

p. 1312, lines 18-19: 'the model is capable of capturing this observed transition to rapid flow upstream of an ice shelf without artificially prescribing any internal boundary conditions within the ice': what is meant here, do the authors refer to the cold/warm base transition here or is something else implied here? Please be more specific.
The text will be clarified on this point.
The PISM-PIK model is capable of modeling the onset of ice stream-like flow without artificially prescribing internal boundaries in the following sense: It solves the free boundary problem analyzed in Schoof (2006b), in which both locations of sliding and the sliding velocity are solved-for simultaneously as parts of the whole ice sheet application of the SSA stress balance. This whole ice sheet SSA computation is done at every (major) time step of the dynamic equilibrium model. The onset of sliding frequently arises from a transition from cold to pressure-melting-temperature base, but no sliding onset is imposed there or elsewhere. Rather, the basal material yield stress τ_c evolves according to the Mohr-Coulomb equation (10), the driving stress evolves according to the mass continuity equation ((1) in Part 1), and the solution of the SSA itself determines whether the (regularized) plastic basal material fails and sliding occurs.
We however do — in the presented Antarctica simulation — also not take the cold/warm transition directly into account. Rather the parameterized meltwater content (equation 13) along with the other equations governing basal friction (equations 9-12) is decisive. As we explain on page 1314, lines 10 ff, in the PISM base version this melt-water content is computed thermodynamically, which connects sliding conditions to the basal temperatures as well, but in a new manner using the SSA as a sliding law. We have decided to employ the parameterization in equation 13 instead. Our experiments with the PISM model for Antarctica have shown that practically for all marine areas of the ice sheet maximal melt-water

content was computed, and hence our parameterization, giving the same in marine areas, does not alter the bottom friction an the critical locations of the onset of streams.

p. 1312, and beyond: throughout the paper it is claimed that the model is able to simulate the fast flow in ice streams. However, the flow patterns in Figs 9-11 look very similar to results obtained with existing models as published widely in the 1990s and reproduced more recently in a number of textbooks (e.g. in book chapters by Payne and Vieli and Huybrechts in Knight (2006): Glacier Science and Environmental Change and in Pollard and Deconto (2009)). The fact of the matter is that none of those models, including the PIK-PISM model are very good at simulating the ice streams as they really are: distinct linear features with velocities in excess of several km per year, often sharply separated from almost stagnant ice at their lateral boundaries by shear zones. Arguably the role of model resolution is far more important in defining real ice streams rather than the inclusion of longitudinal stress gradients.

We are exploring the model's ability right now to simulate streams as the referee describes them above better with high resolutions. For the present simulation we should and will restrict ourselves to describing these flow patterns for their shape and velocity range, expressing our hope that for better resolutions they might indeed come closer the simulation of actual ice streams.

However, the referee says "flow patterns", while the paper under review mostly shows surface velocities, comparing model results (figures 9, 12b, 13, 14b, 15) to observation (12a, 13, 14a, 15). The appearance of wide-coverage surface velocity observations is essentially new in the recent decade (e.g. based on the techniques in **?** and Jezek *et al.* (2003), for example). They did not appear in figures in the late 1990s.

We believe the referee is referring to *balance* velocities instead. Such balance velocities are neither an analysis (through numerical modeling) of the stresses within the ice as a function of boundary inputs (as here in PISM-PIK), and nor are they observations of actual ice motion. Rather they are a reprocessed form of three particular data sets which were available earlier, namely ice thickness, accumulation rates, and surface elevations (for computation of surface gradient). A 1990s reference in which such a "flow pattern" figure appeared is figure 1 in **?**, an example of such balance velocity (or flux) results.

The referee is completely correct that horizontal resolution is important in resolving ice streams. This point is made at some length, with the role of longitudinal stresses shown to be critically important in generating convergence to credible continuum results, by Bueler & Brown (2009). The "parallel" in the title PISM-PIK is relevant on this point, in terms of potential abilities of the model.

p. 1314, lines 15-17: although ice temperature seems to be part of the calculation, the basal temperature does not seem to be used to distinguish between slip and no slip at the base, i.e. basal sliding occurs everywhere although substantial parts of the Antarctic ice sheet is frozen

to bedrock. If so, the implications need to be discussed in more detail.
It is indeed the case that basal sliding occurs everywhere beneath the simulated Antarctic ice sheet. This however represents no conflict with the fact that — as the referee points out — substantial parts of the Antarctic ice sheet is frozen to bedrock. As described on page 1313, lines 14 ff, in areas with stresses lower than the yield stress computed sliding is negligibly small. Those areas can be associated with those where the ice is frozen to the bedrock. As explained in our answer concerning the referee's remark to p. 1312, lines 18-19, above, in the PISM base version these areas with high basal resistance are those with very small or no basal melt-water content, hence the cold areas. The fact that we chose to use a parameterization for melt-water content instead of course decouples thermodynamics from basal sliding, but since the resulting basal melt-water distribution is very similar to the thermodynamically computed one, we think that for a equilibrium simulation this is of no disadvantage.

p. 1316: the setup of the dynamic equilibrium simulation is only a very weak test of the model. Starting from the present-day ice sheet to find that the grounded ice domain is very similar to the observations does not really validate the grounding-line treatment which is at the core of Antarctic ice-sheet dynamics. It may equally mean that the grounding line is not very responsive to the forcing in their model. Unfortunately, the companion paper of Winkelmann et al. is not very informative either. In that paper, they refer to the MISMIP tests but those have not been published and the PIK group did not participate in them either. The main objective of the MISMIP tests was to investigate the reversibility of grounding line migration on a sloping bed, and as it turned out, this could only be achieved well in models that introduced an additional flux boundary condition at the grounding line (the 'Schoof' boundary condition). However, the Winkelmann et al. manuscript does not show such experiments and is very vague about their specific treatment. A much more credible validation experiment would be to impose a glacial-interglacial shift in model forcing during the spinup period to investigate whether the present-day grounding-line position could also be obtained when following another trajectory.
We agree that from a reader's point of view it must seem not very surprising that the grounding line position at the end of an equilibrium position starting from present conditions resembles those present conditions. We however have experienced a lot of grounding line variability during our work with PISM-PIK, e.g. for basal melt rates higher than the ones used in this simulation. In order to illustrate this to the readers, we propose to apply a sinusoidal climate forcing to the steady state presented here, to show that this results in changes in grounding line position oscillating around its steady state position.
In asking the question this way the referee probably assumes that the present Antarctic ice sheet state is a rather well-behaved function of its history. That is, the referee assumes that agreement with present observed grounding line, geometry and surface velocity would all be

the natural result of roughly-approximating the climate through a "glacial-interglacial shift". Note that the best-quality constraints on prior climate, like temperatures and accumulation rates extracted from ice cores, could still only generate a very rough approximation of the boundary inputs to the ice flow model, for example the time-dependent surface temperature and accumulation maps. Such maps would be the actual inputs into such paleo-climatic spinups. A reasonable expectation is that a *perfect* ice flow model might generate quite a different sheet from the present observed sheet, because of these poor constraints on prior climate. (Such constraints are definitely "poor" relative to the constraints on present climate.) The referee's intuition may be right, but the detailed properties of the Antarctic ice sheet are not yet proven to be a highly-stable function of prior climate, to our knowledge.
Concerning the MISMIP experiments that have been performed with PISM-PIK, we show in the companion paper (Part 1, Winkelmann *et al.*, 2010) that grounding line migration is reversible on a sloping bed (page 1291, lines 3 ff and figure 5 therein). This has been achieved by PISM-PIK without applying the 'Schoof' boundary condition. Whether it has been achieved 'well' will be seen in the comparison with other models participating in that intercomparison. It is planned to submit the PISM-PIK results to MISMIP soon.

p. 1316: ice thermodynamics are equally a crucial component of model spinup because of the thermal inertia, but very few details are provided. A map of the basal temperature obtained for the dynamic equilibrium state would equally be very helpful.
We will provide a map of basal temperature obtained in this simulation as a new part (b) in Figures 3.
We have on page 1315, lines 26 ff, described the thermodynamical spinup procedure. We propose to add in the manuscript a remark that we have ensured that at the end the first part of the spinup procedure the temperatures within the ice has reached an equilibrium. Also after the second part of the procedure, during which also the geometry of the ice sheet is allowed to evolve, the 3-D temperature distribution in the ice has reached a new equilibrium, i.e., the ice sheet is in geometric and thermal equilibrium.

p. 1316 and beyond: a novel feature of the PIK-PISM model seems to be the treatment of an 'ice cliff' as a separate lateral boundary. The reader can only guess which criteria are used to obtain such a boundary? Is it prescribed, and if so, how? Since the model does not consider surface ablation it is difficult to understand how the ice sheet margin could end on land.
The notion 'cliff' might be misleading. Indeed, ice sheet margins ending inland to not occur in our simulation. By 'cliff' we mean ice resting on bedrock above sea level, adjacent to the ocean. This is the case where the dark blue areas in figure 7 meet the white areas, i.e. the ocean. A lateral-view finite-differences visualization of this looks like ice resting on a cliff above the ocean. It may either really be the approximation of ice resting on a cliff above the ocean, or as well of a tidewater-type glacier, where the ice sheet margin exactly meets the

point at the coast where the elevation of the land drops down to sea-level.
We propose to clarify that on page 1316, line 20.

p. 1317, Fig. 6: the comparison of modelled with observed ice thickness seems to share much of the features seen in older SIA models of grounded ice flow. Also in the PIK-PISM model the ice is too thick upstream of grounding lines, most strongly so in the Siple Coast area of WAIS. The new SSA treatment of basal sliding apparently does not seem to remedy the situation, and that deserves to be discussed further. Besides, the comparison in Fig. 6 is compromised by the fact that the observed ice sheet is not necessarily in steady state, but that is not discussed further.
We will better emphasize the restrictions of comparability of modeled equilibrium fields to observed data, given that the ice sheet is not assumed to be in actual steady state at any time. We state that the ice is too thick upstream of grounding lines on page 1317, lines 22f. We do not intend to claim that we have resolved this problem. See answers to general comments at the beginning.

p. 1318, lines 1-4: one cannot claim that marine portions of the EAIS and WAIS are subject to the marine ice-sheet instability solely because of their bedrock slope. The marine ice-sheet instability is a feature of simple models but in reality 3D effects play such as buttressing from ice shelves in embayments. This should be mentioned.
Yes, we agree. We will clarify this in the manuscript. We thank the referee for bringing this misleading formulation to our attention.

pp. 1319-1320: Much emphasis in the manuscript is laid on a comparison between modelled and observed surface velocities to validate the steady state dynamics of the model. The fact that a point-by-point scatterplot shows a more or less straight line for grounded ice is however not a very strong test that the model gets the ice dynamics right. Since the modelled thickness generally deviates by less than 10% from the observations it merely states that the model is mass-conserving and that the accumulation distribution of Van de Berg is representative for the longer-term average. The overestimation of ice shelf velocities is likely due to the underestimation of basal melting but that is not discussed further.
This is an interesting point, that will be discussed in more detail. However the situation is difficult, because in spite of our very low subglacial melt rate, the ice shelves are too thin compared to observations instead of too thick.

p. 1320: Unfortunately, the most interesting parts for model validation of the flow field are missing from the observations, i.e. near to the margin where the flow concentrates. A comparison of modelled velocities with balance velocities obtained with the same datasets is likely to be much more conclusive to properly judge the dynamic quality of the PIK-PISM model.

We agree with the referee that an important aspect of model validation is to look in detail near the margins where the flow concentrates. This is why Figures 1, 4, 9, and 11 appear in the current paper. No claim is made that we have resolved all issues, but these figures allow the reader to consider the dynamical quality, and imperfections, of the PISM-PIK model. However, if reproduction of details near margins is the standard for validation then even the best balance velocity results would be rather undesirable source for a comparison. Such balance velocities give unreasonable detailed features for ice stream-like flow. In summary this is because they force all flow down the surface gradient even in locations where there is very little driving stress generated by the small surface slopes, so that at such points the actual flow is not following the exact (negative) surface gradient. For the same reasons the balance velocities are sensitive to observational error in the digital elevation model, which is necessarily differentiated to generate the balance velocities. Indeed figure 1 of the well-known paper Bamber *et al.* (2000) shows the substantial differences which arise from comparing balance velocities to observed surface velocities in the Siple Coast area, which is the part of Antarctica where such observations were first compiled. When judging the dynamic quality of the PISM-PIK model we prefer to work with the observations which are available (surface velocities) rather than introducing a second, highly-simplified model for ice flow which is unconstrained by conservation of energy or conservation of momentum (namely, the balance velocities), and comparing to that.

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