Response to referee comments on
'The Potsdam Parallel Ice Sheet Model (PISM-PIK) - Part1: Model Description'
by Winkelmann 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: 13 October 2010

General comments:

Winkelmann and coauthors describe the three-dimensional thermomechanical ice sheet model PISM-PIK, which can be applied for simulating large-scale marine ice sheets. The model is largely based on an existing ice sheet model (PISM) with some further development specific for simulating marine ice sheets. The paper is generally well written and structured. In its present form though, it lacks substance and detail, since most of the novel aspects of the model are described in other (unpublished) documents. It is furthermore questionable whether the described model development represents an improvement over the state of the art of existing large-scale marine ice sheet models. In particular, the document lacks a discussion of how the model deals with grounding line migration, one of the key issues for simulating marine ice sheet evolution. Therefore, I suggest major revisions to be made. For a model description paper, I would expect all relevant aspects of the model to be described in detail, including the subgrid parametrisation (2.4) and the calving law (2.6). These parts are in the present form rough summaries of presently unpublished material. Even if these additional documents were published, the reader is asked to consult three additional publications to get a full model description, which I find unacceptable. I suggest to substantially expand sections 2.4 and 2.6 comparable to the review of relevant formulas in 2.1.

We understand the reviewers frustration with a publication that heavily relies on other publications. We are however constrained by the page limit of "The Cryosphere" which expects research articles to be between 6 and 12 journal pages long. For this reason we have divided the paper into two parts: One describing the model set-up and the other describing its performance under present-day boundary conditions. In order to confine the paper to a readable length, we have only described the parts which are new in comparison with the base code PISM which has been described separately. We were aiming at providing sufficient information for all new model parts. Some parts like the ones on the sub-grid-scale calving front motion parameterization and the calving law have been described with all

necessary detail to explain the concept and provide the basic equations. We find that it is not possible to evaluate the respective performance of the parameterization in only one publication alongside the other aspects of the model. The numerical performance of the sub-grid scale parameterization has in the meanwhile been published in The Cryosphere Discussions (Albrecht *et al.*, 2010). For the calving rate law the necessary equation is given while its performance is being evaluated in an upcoming paper.
Here, we are not aiming at giving a detailed analysis or validation of certain aspects of the model, this would go beyond the scope of this paper. The present paper is meant to provide a reference for future publications. Such approach allows to considerably reduce overall publication size since future publications do not need to repeat the model set-up. While we are grateful for the reviewers' detailed comments and are planning to incorporate them all, we hope that we can convince referee and editor on this general point that the presented manuscript in principle suffices this goal of providing the general model set-up for future reference.

As opposed to conclusion 1291, l24-25 no details about the grounding line problem can be found in the manuscript. Attempts have been made in the past to improve the representation of grounding line migration in large-scale ice sheet models e.g. by including the Schoof boundary condition (Pollard and Deconto, Nature 2009). Although the authors seem to be aware of this work, it remains unclear how this issue is treated in the presented model. It should be clearly discussed which mechanisms control the grounding line movement in the model. The validation experiment (MISMIP) does not take into account lateral shearing and buttressing and is thus not a complete test case for grounding line migration. Further (idealized) experiments are needed to show the transient response of the grounding line relevant for simulating the evolution of whole marine ice sheets as motivated in the introduction.

We are truly grateful for this comment and appologize for a number of misunderstandings our manuscript caused.
First, our formulation 'major modifications for ice shelf and grounding line dynamics' is misleading. In fact, the grounding line migration is only implicitly modeled, since it results directly from the floatation criterion. The modifications meant here concern the computation of the ice velocity in sheet and shelves which implicitly cause the motion of the grounding line. Secondly, since it is beyond the scope of the current paper to prove that the treatment of the transition zone is indeed a good approximation of the full Stokes problem, we should not have stated this. Consequently we have removed all references to this statement from the text. What we are showing in the second part of the paper is that the approach indeed leads to a velocity field which is continuous across the grounding line. With respect to the reviewer's request for transient experiments, we will provide time-dependent simulations in the second part of the paper which show that the grounding line is not fixed by any means and can move freely. It results from the dynamics of the flow and the floatation criterion.

In addition it should be noted that the Pollard & Deconto (2009) treatment of the grounding line is a somewhat heuristic grid-wise, 2D interpretation of the precise 1D (flowline) grounding-line-motion parameterization rigorously derived by Schoof (2007a) and does not represent a conclusive solution to the general grounding line motion problem.
I would suggest to reorder the document in order to separate review and changes of the original PISM model (2.1, 2.2, 2.5) from aspects specific for shelf configuration and marine ice sheet simulation (2.3, 2.4, 2.6).

We thank the referee for this suggestion, we think that the reordering is a very good idea and will lead to a clearer outline of the paper. We will change the manuscript accordingly.

It should be made clear in 2.2 why a different approach is used compared to the original PISM. Is the new approach considered better and why?

As stated above we made a mistake here. It is a beyond the scope of this paper to prove that the superposition of SIA and SSA within the entire ice domain is a good approximation of the Stokes equations. The paper by Schoof & Hindmarsh (2010) points into this direction. We are ourselves planning a publication on this. However, we have eliminated all reference to this kind of statement in this paper. What we can state is that the superposition yields smooth velocity fields. The use of the weighting function in PISM is not justified by any physical process. It is an additional degree of freedom which is not constrained by observations nor does it provide any other proven advantage. Since we cannot prove in this paper that it is better, we confine ourselves to the above justification for its removal.

Detailed comments:
Abstract

1278, l19-20: I suggest to remove "and is used for a dynamic equilibrium simulation ..." from the abtsract. This sentence is misleading, since it is not the second part of this paper, but rather another paper where this simulation is described.
We suggest the following reformulation:
'The model is tested within the Marine Ice Sheet Model Intercomparison Project (MISMIP) and is used for a dynamic equilibrium simulation of Antarctica under present-day conditions in Martin *et al.* (2010).'

1 Introduction

1278, l25 - 1279, l7: The given comparison between flow-line models, full-Stokes and higher-order models is misleading, since also flow-line problems can be solved with a higher-order approach.

We thank the referee for the comment, we will state this clearer in the manuscript.

1279, l14-15: SIA-based models also provide good approximations for regions with sliding, which seems to be excluded here.
We did not intend to exclude this, we will change the manuscript accordingly.

1279, l19: The use of the SSA arises from another stress regime (without basal friction, as has been written) and has initially nothing to do with the velocity of the flow.
We suggest the following reformulation:
'By contrast, the SSA has been used to simulate the flow in ice shelves which have no basal friction and thus a different stress regime'.

1280, l2: sheet in singular "regimes and speeds throughout sheet, streams ..."
We will change this in the manuscript.

1281, l2: It is not clear what "mode" refers to here
'Mode' means the model setup for the dynamic equilibrium simulation of Antarctica described in Martin *et al.* (2010). We will reformulate this in the manuscript.

2.1 Field equations and shallow approximations

1281, l25: Remove "in each time step", or clarify
We will remove this - all equations are solved in each time step.

1282, l4-6: Reformulate - heat is advected, not temperature - the velocity field does not advect
We suggest the following reformulation:
'The following shallow equation of conservation of energy includes advection and vertical conduction of heat and a strain dissipation heating term.'

1282, l9: add "heat" in "... and the geothermal heat flux ..."
The word 'heat' is redundant in 'geo*thermal* heat flux'.

1282, l13: replace "from" by "following"
We will change this in the manuscript.

1282, l19: remove flow in "... for a viscous fluid ..."
We will change this in the manuscript.

1283, l5: Reformulate - what is meant by "influenced by distant ice"
What is meant is that the SSA stress balance is non-local. When solving for the velocity

at a certain grid point, the solution will depend on the whole spatially-distributed field of stress inputs (driving and basal). We will clarify this in the manuscript.

1283, l13: Remove "re-" in "... can be expressed in terms ..."
We will change this in the manuscript.

1283, l17: Replace "together" by "combined"
We will change this in the manuscript.

1283, l20ff: Reformulate - shearing cannot dominate over stresses
We will reformulate this in the manuscript.

1284, l6: Reformulate - what are dragging ice shelves?
We suggest the following reformulation:
'Thus ice streams are conceptualized as regions which experience basal resistance but have a flow-regime similar to the one in ice shelves.'
The phrase "dragging ice shelves" appears also in Ritz *et al.* (2001).

1284, l10: Replace "the" by "that" in "... assumes that till ..." Reformulate - stresses cannot reach a stress
We will change this in the manuscript.

2.2 Velocity combination and sliding

1284, l16-17: This sentence needs a citation
We will change this in the manuscript.

1284, l19: Replace by "... were to depend on the local driving stress ..."
We will change this in the manuscript.

1285, l5: Reformulate - "SSA-as-a-sliding-law" is slang
We propose the following reformulation:
'In order to obtain a smooth transition from regions where the driving stress is fully balanced by vertical shearing (see Fig. 1; *ice sheet*) and those with significant sliding, a different approach is used, following the basic idea of superposition of SIA and SSA which we call "SSA-as-a-sliding-law" (Bueler & Brown, 2009).'

1285, l6: The equation order should be reversed to be in line with the description $v_b = v_{SSA}$
We will change this in the manuscript.

1285, l10: I wonder if "superpose" is a good expression here. Maybe "add" would be more appropriate. Appears also in other places in the text.
Yes, we will change this accordingly.

1285, l9-14: Schoof and Hindmarsch (2010) are referred to show good transition between sheet and shelf, but in this context the discussion is about transition between frozen and sliding grounded ice. Could the authors clarify?
Paragraph 1285,l2-8 deals with the transition between frozen and sliding grounded ice whereas paragraph 1285,l9-14 deals with the transition between sheet and shelf. We will try to make the distinction of these two issues clearer in the manuscript.
As stated in our answer to the general comments we will remove the statement that the superposition is a good approximation of the transition between sheet and shelves and confine ourselves to showing that it yields smooth velocity fields.

1285, l16-22: Reformulate, compare same section in Martin et al. as an example - what is a column contribution?
We will reformulate this in the manuscript. With 'column contribution' we mean the contribution of the vertical average of the SIA velocity.

1285, l23ff: Please clarify. Is SIA contribution taken into account or not? l23 says so, but l25-26 denies it.
Sorry for the confusion on this point. Both SSA and SIA velocities are found over the whole domain as written in l23. What is meant by l25-26 is that the SIA contribution on the shelves is so small that the dynamics there are fully dominated by the SSA contribution.

2.3 Calving front stress boundary condition

1286, l9: Remove "itself" in "... in PISM there is no ..."
We will change this in the manuscript.

1286, l11: Reformulate - what is meant with "choice of its strength"
We suggest the following reformulation:
'The choice of the ice thickness and viscosity used for this extension and the effect on the rest of the modeled ice are inadequately understood.'

1287, l16ff: Reformulate This whole paragraph remains unclear for me. What are "simulated icebergs"?
What we call 'simulated icebergs' are pieces of ice that break off of ice shelves due to calving, and which are no longer attached to the main ice body. Ice dynamics models for slow ice flow, including ours, will not simulate the dynamics of these zero-basal resistance floating

pieces. In fact Schoof (2006b) shows the SSA has no unique solution for the velocity of these un-attached shelves. In any case these pieces cover at most a few cells in our simulations, and we identify and eliminate them at each time step. We add their volume to that reported as lost through calving.

Proposed reformulation:
Due to the dynamic calving law, shelf tongues can break off the ice shelf and form icebergs of the size of a few grid cells. For these floating pieces with zero total basal resistance the SSA does not have a unique velocity solution (Schoof, 2006b). Therefore they are identified and eliminated, and their volume is reported as loss through calving.

2.4 Continuous ice shelf advance and retreat through subgrid parametrization

1288, l13-15: Reformulate - mass cannot account for a calving rate
1288, l16: Reformulate - calving cannot advance into cells
We propose the following reformulation:
'A retreat of the calving front can be modeled in the same manner: First, the calving rate is determined dynamically through the calving law described in Sect. 2.6. The amount of mass lost through transport by this calving velocity is removed by changing the ice volume in the partially-filled cells at the ice front, and, as needed, by making adjacent filled cells into partially-filled cells.'

2.5 Discretization scheme for mass transport

1288, l27: What is multiplied by what here?
We propose the following reformulation:
'The sum of the SIA and SSA velocities on the staggered grid is the total velocity which is used in a mass-conserving upwind finite difference scheme for the mass continuity equation (1):'

1289, l11: Halfar solution needs a reference
Meant is the similarity solution described in Halfar (1983) and the references therein. We will add this reference in the manuscript.

1289, l14: Better compared to what?
We propose the following reformulation:
'For the SSA it can be shown that the analytical solution for the flow line case from (Van der Veen, 1983) for an ice shelf in equilibrium is better approximated with this alternative mass transport scheme than with the scheme described in (Bueler & Brown, 2009).'

3 MISMIP intercomparison

1290, l9: "MISMIP intercomparison" is not a good section title, since no intercomparison is presented here. I suggest "Validation experiments"
We reserve the term 'validation' for comparison with observations. We propose to reformulate the title as "Experiments from the MISMIP intercomparison".

1290, l14-16: It is not clear why and in what aspect these two solutions differ. Does the representation of the grounding line change with the new boundary condition? Why is that the case? Should be analysed and discussed in detail.
The two solutions differ because the PISM base scheme, and all of the finite difference schemes described in MacAyeal et al. (1996) as well, impose the wrong Neumann condition at the calving front because they replace that condition with a shelf extension. This shelf extension method generates a less-accurate backstress on the whole ice body. Therefore this influences the grounding line position. Introducing the calving front boundary condition in a principled way leads to a better representation of the stresses in the ice shelf.

1291, l10: The resolution is not indicated on the figure. Would be nice to be able to compare.
Yes, we will show the resolution instead of the number of grid points on the x-axis.

1291, l24: As stated above, "grounding line dynamics" should be further explored and needs a thorough discussion.
As stated in our answer to the general remarks, this formulation is misleading. The modifications meant here concern the computation of the ice velocity in sheet and shelves which implicitly cause the motion of the grounding line.

1292, l13: This is a validation rather than an application.
As stated in our answer to comment *1290, l9*, we reserve the term 'validation' for comparison with observations. An alternative to the term 'application' could be 'test'.

1292, l17: It is not the second part of this paper, but rather another paper
We will change the manuscript accordingly.

1300, Fig. 1 caption: It should be mentioned what the red arrows stand for
The red arrows stand for the velocity contribution of the SSA, we will denote that in the figure part 'ice stream'.

1302, Fig. 2 caption: I would add "arrows" in "(grey arrows)", "(black arrows)" since there is a lot of other black and grey in the figure
We will change this in the manuscript.

Anonymous Referee 2
Received and published: 25 October 2010

General comments:

Our aim for this paper is to provide a reference for future publications in which all basic information to understand the main parts of PISM-PIK is given. As a description paper, it is not meant to give an in-depth analysis or validation of specific model features or judge its performance against other models. We hope we can provide a first insight into the model's performance in the companion paper (Martin *et al.*, 2010) and show further experiments in future publications based on this description paper.

This paper introduces a 'new' continental ice sheet model (PISM-PIK). The paper is divided into two main parts. First, a description of the flow physics in particular the model used to simulate the distribution of stress and velocity within an ice mass, as well as information on a new subgrid parameterization of ice shelf advance and retreat, finite difference discretisation of the mass (thickness) balance equation and calving law. Second, the application of the model to the MISMIP inter-comparison experiments and analysis of the results.
I found the paper disappointing in two ways: first, most of the material is not particular new or novel and, second, the amount of detail of the more novel aspects is very sparse so that it is very difficult to judge the contribution. The area of grounding-line migration is clearly of great importance and warrants the great amounts of attention that it is currently receiving. The bulk of the literature in this area points towards the difficulties of producing meaningful simulations of this process (indeed the paper references some key publications in this area); progress in the area is therefore only possible if results models are clearly described and adequately tested. I do not believe that this is the case with the present draft. Key issues that need to be addressed include the following.
First, a better, clearer account of how the two stress models (SSA and SIA) are implemented and, in particular, the effects of the crude way in which the models are combined (the reference to Schoof and Hindmarsh as justifying this approach is a bit too tenuous). The present version is vague and a little confusing when details are presented; it relies on the reader knowing their way around an ice sheet model and reads very much like extracts from a larger work (where concepts, terminology etc appear without due introduction). The ad hoc addition of the two resulting velocities appears crude and very little effort is made to justify this or to assess the errors introduced.

This points in the direction of a general remark also made by Anonymous Referee 1. As stated in our previous answer, we did not mean to prove that the superposition of SIA and SSA within the entire ice domain is a good approximation of the Stokes equations. What we hope we can do is clarify our approach and illustrate how the addition of the velocities leads to a smooth transition from sheet over streams to shelves - we have tried to answer the questions in the detailed comments regarding this matter.

Second, the remainder of Section 2 (calving front stress boundary condition, advance/retreat parameterization, mass transport discretisation and calving law) appears to be a standard reworking of material that has been published several times elsewhere or is presented in a superficial manner. In both cases, it is hard to see what the real advance in understanding is.

We understand the role of the presented manuscript as a reference for future publications which provides the model equations and general set-up. We do not intend to value our model against other approaches. A first application of the model is the dynamic equilibrium simulation given in (Martin *et al.*, 2010) which shows a comparison of our model results to observations. We disagree with the reviewer that our work is a standard reworking of published model descriptions, for which the referee does not provide further reference.

Finally, the presentation and analysis of the MISMIP results are, again, superficial. If the intention of the paper is to develop some level of trust in the model's predictions so that it can be used in full Antarctic simulations, then far greater attention needs to paid to explaining how grounding-line migration is simulated; it is hardly mentioned while the less relevant and fairly standard implementation of shelf boundary conditions, etc etc is given a great deal of attention.

As explained in our answer to Anonymous Referee 1, grounding line migration is modeled implicitly, it results directly from the floatation criterion. Since the same evolution equations are solved on the whole ice domain, the grounding line position is in fact fully determined by the dynamics described in manuscript - especially the shelf dynamics play an important role because they determine the backstress onto the sheet. We understand the need for further experiments to illustrate the performance of PISM-PIK and will provide time-dependent simulations in the second part of the paper which show for instance that the grounding line can advance and retreat freely.

Detailed comments:

1278.10. 'naturally emerge' what is meant by this statement? The discussion thus far is limited to models used to solve for stress/velocity, which are purely diagnostic (i.e., no time dependence). The term 'emerge' implies time dependence so make no sense in this context. Clarification required.

The term 'emerge' will be removed. There should be no implication of a time-dependence, as the SSA computation itself is time-independent. The intended sense of 'emerge' is precise, however. That is, the locations of streaming flow are determined by solving the free boundary problem which is shown to be well-posed in Schoof (2006). The text will be rewritten accordingly.

1278.11. 'membrane stresses' define what is meant here.
Membrane stresses mean the stresses held by viscous deformation. Membrane stresses and the basal shear stress balance the driving stress in the SSA stress balance (Hindmarsh, 2006). We will explain the term in the manuscript.

1278.25 onwards. This discussion mixes dimensionality with physics and is therefore very confusing. 1-d. models could have Stokes physics and (perhaps) be computationally cheap. The key issue is dimensionality which is omitted from the second half of the paragraph.
The referee is right, we will make a clearer distinction between these two lines of argumentation.

1279-1 why do they fail with ice shelves - this is not inherent to 1-d. models but to a certain type of 1-d. model!
The referee is right, 1-d models can of course investigate the buttressing effect of a shelf onto a sheet by prescribing a certain buttressing strength. What is meant here is that 1-d models cannot compute a buttressing field for any given embayment. We will clarify this in the manuscript.

1279-16. More correct to talk about gravitational driving stress here rather than height and surface slope.
Yes, we will reformulate this in the manuscript.

1279.20. Need further explanation: combining SSA and SIA is exactly the current paper does so why are the Ritz etc papers different? Clearly, some may be aware of these issues from prior knowledge but this cannot be relied on by the paper; a great deal more effort is required to make the point here.
Yes, the referee is right, we should state this clearer: Although all of the models mentioned here combine SIA and SSA, we would here like to make the distinction between "horizontal hybrid" models that solve either the either the SIA or the SSA in distinct map-plane regions and "vertical hybrid" models which solve the SIA and SSA everywhere (at least all grounded points) and combine the velocity or stress fields in a more-or-less heuristic manner (Bueler and Brown, 2009; Pollard and DeConto, 2007). PISM-PIK is in the latter category.

1280.1. This paragraph reads as if it were the introduction and has little connection to the previous discussion.
The previous discussion introduces different types of ice dynamics models. This paragraph refers to the one before in that it points out one difficulty faced by all of them, namely the large difference in flow regimes and speeds throughout sheets, streams and shelves.

1284.10. Equation has pw. (water pressure) where is its determination discussed? This is key because it determines what the relative contributions of SSA and SIA will be to horizontal velocity, and hence the location of fast flow etc etc.
We will add further information on the porewater pressure in the manuscript. As explained in (Martin *et al.*, 2010), the porewater pressure is computed using the following parameterization:

$$p_{\mathrm{w}} = 0.96 \rho_{\mathrm{i}} g H \lambda \tag{1}$$

where

$$\lambda(x,y) = \begin{cases} 1\,, & b(x,y) - z_{\mathrm{sl}} \leq 0\,, \\ \left(1 - \frac{b(x,y)-z_{\mathrm{sl}}}{1000}\right) & 0 < b(x,y) - z_{\mathrm{sl}} < 1000\,, \\ 0\,, & 1000 \leq b(x,y) - z_{\mathrm{sl}}\,, \end{cases} \tag{2}$$

For maximal λ, i.e. for ice resting on bedrock at or below sea level, the porewater pressure is limited to a maximum of 96% of the overburden pressure $\rho_{\mathrm{i}} g H$.
In PISM, the water content is computed thermodynamically from the basal melt rate. Our parameterization of course decouples thermodynamics from basal sliding, but the resulting basal melt-water distribution is very similar to the thermodynamically computed one. In particular, our experiments with PISM have shown that for almost all marine areas of the ice sheet maximal melt-water content was computed, which is also true for our parameterization, so the bottom friction at the critical locations of the onset of fast sliding is not altered.

1285.1 onwards. I assume that both the SSA and SIA are found over the whole domain (i.e., that the SIA is used in both fast and slow flow, as is the SSA even though the assumptions on which it is based a clearly violated). It would be worth mentioning this, at present it is not immediately obvious why this approach removes the abrupt changes mentioned on 1285.1.
Both SIA and SSA velocities are found on the whole model domain enabling a smooth transition from sheet to shelves. However, the SIA contribution on the shelves is so small that the dynamics there are fully dominated by the SSA. The same holds for the SSA contribution on the part of the sheet where bottom friction is high enough for vertical shear to dominate over horizontal shear. The transition region of grounded ice where the velocity derived from the SSA has a significant influence is what we define as streams. This is a purely diagnostic definition.
The smooth transition from sheet over stream to shelves is illustrated in Martin *et al.* (2010), Fig. 2 (see also answer to comment *1285.15*).
An abrupt velocity transition in grounded areas is avoided by solving the SSA. Schoof (2006b) shows that the velocity solution of the SSA is smooth even if the basal strength field is discontinuous.

1285.10. Schoof and Hindmarsh (2010) do indeed present a vertically-integrated model but it is a little more subtle the addition of velocities derived from the SSA and SIA. At least to my

reading of the S&H paper, the suggestion that 'this combination of velocities ... yields a good shallow approximation' is a misrepresentation of their work.
As stated in our answer to the general comments we will remove this statement from the manuscript and confine ourselves to showing that the superposition of SIA and SSA within the entire ice domain yields smooth velocity fields.

1285.10. superpose do you mean add?
We will change this in the manuscript.

1285.15. I do not see how the claim can be made that the two end-member flow regimes naturally emerge: they have simply been added together in an ad hoc manner. Supporting this claim with a reference to a cartoon illustrating different types of flow adds nothing.
To support this claim, we should have included Fig. 2 from (Martin *et al.*, 2010). In that figure the schematic diagram from Fig. 1 is shown and additionaly an example cut through the sheet-stream-shelf transition of the Lambert Glacier and Amery Ice Shelf from the dynamic equilibrium simulation further described and analyzed in (Martin *et al.*, 2010). This figure illustrates how the two velocity contributions supersede each other and the modeled velocity undergoes a qualitative change from sheet to shelf.

1285.20. Why is this diagnostic required? Aren't horizontal velocities being found as the sum of SSA and SIA over the whole domain irrespective of being ice stream or not? Is this used for display purposes?
Streams are only identified from the computed velocities using the purely diagnostic condition given in Eqn. (16), see also answer to *1285.1 onwards.*

1286-1. The application of stress boundary conditions on the SSA is fairly standard and I do not see how the description in 2.3 adds anything to the literature on the use of SSA.
We included the section on the calving front stress boundary condition in order to give a full overview of the model equations. It is one of the major changes compared to PISM where periodic boundary conditions are used and as such worth to be summarized in the manuscript, we believe. Also, we have independently developed a scheme to apply the boundary condition directly at the calving front. This allows for computing the stresses at calving fronts of any given shape. Existing finite difference models for ice shelves apply a "shelf extension" technique beyond the modeled calving front (MacAyeal *et al.*, 1996, Ritz *et al.*, 2001), and so a demonstrated finite difference implementation of this stress boundary condition, as sketched here and in Appendix A, is an appropriate use of journal space.

1288-1. This section is too vague to be of any use. It is not at all clear how the partial cell methodology is implemented or how this affects model dynamics. The parameterization is a 'precondition' for the application of a 'continuous calving law': what does this mean? Is a

calving used? What is 'just the right amount' of mass loss
1288-14? The idea of dealing with calving using a partial cell technique is interesting but this section needs a great deal more information before it becomes useful. Is a velocity found in these partial cells; 1288-5 suggests not but how can a mass budget then be determined?
The subgrid scheme is further described in Albrecht *et al.* (2010), now published. The text will be changed to reflect that.
The subgrid parameterization is a precondition for the application of the calving law in two respects:
First, it is necessary to have a technical mechanism for removing only part of the mass of a certain grid cell, namely the amount computed from the calving law. Second, it is essential to use the proper ice thickness for computing the stress field within a shelf and thus for the computation of a calving rate. Ice shelf fronts are observed to typically have an almost vertical cliff-like shape. Without the subgrid scheme, the ice flux into a newly occupied grid cell is spread out over the entire horizontal domain of that cell, possibly resulting in grid cells of only a few meters ice thickness or less. This leads to an unphysical extension of the ice shelf onto the ocean.
Velocities are not computed in the partially-filled cells - we determine the mass budget as follows: The flow into these partially-filles cells is computed using the ice thickness at the front and the velocities on the staggered grid (see answer to *1288-25*). The flow out of the partially-filled cells is computed through the calving rate.

1288-25. What staggered grid is being employed here? We have not been told how the variables are arranged on the FD grid.
We will add information on the grid in the manuscript.
The variables are arranged on the finite difference grid as follows: Both the ice thickness and the SSA velocities are computed on the regular grid. The SIA velocities however are computed on a staggered grid, i.e., on a grid which is shifted about half a grid length compared to the regular grid. For the discretization of the mass transport, the SSA velocities are transferred onto that same staggered grid by averaging over the SSA velocities from the adjacent grid cells.

1289-3. Equation 25 have left hand side something like Delta Q / Delta x. This is a first-order upwind scheme isn't it?
Yes, it is a first-order upwind scheme. We give it here in detail in order to clearly show how it differs from the discretization scheme used in PISM.

References

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