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The Cryosphere Discussions

Interactive comment on "The Potsdam Parallel Ice Sheet Model (PISM-PIK) – Part 2: Dynamic equilibrium simulation of the Antarctic ice sheet" *by* M. A. Martin et al.

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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.

Specific comments

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

p. 1310, eq. 3: it is curious that the elevation dependence of the surface temperature parameterisation 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.

p. 1311: The rates of basal melting below the main ice shelves obtained with the parameterisation seem very modest (between 5 and 20 cm per year, i.e. generally less than surface accumulation). This parameterisation 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 parameterisation would be helpful.

p. 1312, line 12: 'exemple' should be 'example.

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 modelling of ice streams.

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.

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&Vieli and Huybrechts in Knight (2006): Glacier Science and Environmental Change and in Pollard&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.

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.

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.

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.

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.

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.

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.

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.

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.

Interactive comment on The Cryosphere Discuss., 4, 1307, 2010.