

Interactive comment on “Description of a hybrid ice sheet-shelf model, and application to Antarctica” by D. Pollard and R. M. DeConto

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Received and published: 29 June 2012

This paper gives a detailed description of the different components that form the Pollard DeConto model that has been used over the last decade, with a clear reference to the different published applications and to what particular boundary conditions conditions were used and how in those papers. This is definitely the way forward how models should be reported and documented for future reference. It is a highly informative paper that steers the reader through the structure of the model via a series of building blocks.

Such building-block structure makes the paper highly readably if one wants to reproduce a numerical model and apply directly the equations in its numerical form. However, it becomes more difficult to see the link with the physical model and what ap-

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proximations are applied at what stage in the derivation of the model from the basic conservation equations (mass, momentum, energy). I presume this is a deliberate choice, but some reference is necessary to better situate this model in comparison to other work. For instance how it relates to the Bueller and Brown model (2009) where SSA is used as a sliding law to SIA and the Schoof and Hindmarsh model (2010), which also includes shear softening terms in the effective viscosity. An overview of approximations to the Stokes flow can be found in Hindmarsh 2004. I therefore propose that before diving into the first equation (a bit out of the blue, both eqs 1 and 2) an introduction is written that better situates the hybrid SIA/SSA application with reference to other models and with respect to the physical approximations to the Stokes flow. This will definitely give an added value to the paper.

I have a number of minor remarks listed below.

Abstract: if similar results are obtained with an earlier model version (PD07?), what is the benefit of a newer version? Is this older version one that has the heuristic as well? Has it different physics? Either it should be stated, or better left out, certainly in an abstract.

1081 L24: shearing and stretching are not equivalent with $\frac{\partial u}{\partial z}$ and $\frac{\partial u}{\partial x}$, respectively.

1082 L3: Can be stated more directly: results are verified for the flowline version (Pattyn, 2012), and currently tested in the MISIP3d intercomparison (Pattyn et al., in prep. or reference to website/EGU abstract).

1082 L8: what is meant by shear-softening term in the 'other's' equations. Which equations?

1082 L16: more formal representation could improve the manuscript; u representing u or v should be done differently, for instance using a bold character v for the velocity in both horizontal planes: $\mathbf{v} = (u, v)$

1082 L25: stresses should be defined.

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I find this whole section quite confusing, as we see only the derived equations, without knowing from what they are derived (velocity gradients). This also means that if vertical strain rates incorporate longitudinal stress components via the effective viscosity, it is a Schoof/Hindmarsh type of model, or not? See my general remark.

1082 L9: does this mean that u_b is the velocity produced by SSA and u_i the one by SIA? if that is the case, it should be stated more explicitly.

1083 L10: reference to MacAyeal should be 1989 instead of 1996 i think.

1084: there is some circularity here which needs to be solved using an iterative procedure. This should be mentioned right away.

1085: the solution on the half time-step, is this a a direct consequence of the ADI scheme? this should be stated, because it is not clear at that stage why the half-time update is necessary.

1085 top: Definition of S is lacking. S is defined as sea level on 1097 and 1102, where its definition should be removed and put here.

1088 L3: what is meant by spatial gradients parallel to the grounding line? in velocity or in surface slope?

1088: what is the major difference between grids coarser than 1 km and coarser than a few km? Both are coarser than 1 km.

1088 L14: space between 'then' and ' u_g '; idem line 15 between 'the' and 'grounding'; i guess it is a result from the fact that this paragraph is in fact an equation (but one in words).

1091: The use of the f_g term is very interesting, but its effectiveness has not been shown. Buttressing is a serious issue in current ice sheet modelling and I find the authors going light over this type of parametrization. Furthermore, how does this effect compete with Eq (8), where buttressing is parameterized differently and f_g should be

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somehow incorporated in the C_s factor? This should be made clear.

1092: if a scaling through ice thickness is performed by setting $z' = (h_s - z)/h$, then additional terms should be included to correct the horizontal derivatives as well (see for instance Pattyn 2003). Why are they neglected here? Furthermore, shouldn't the vertical advection term have $-w'/h$ instead of w' , as the second derivative includes h^2 in the denominator as well? Idem for first line on page 1093. h in the denominator does appear on line 5 page 1093 for the vertical conduction at the base.

1094: Concerning the different enhancement factors for ice sheet and ice shelf flow, it should briefly be mentioned why this is necessary.

1106: Heat flows: as used in Pattyn 2010, there is also the Pollard et al. 2005 heat flow parameterization? Why is it not mentioned/used here? In reality, heat flow for a large part of EAIS is much lower than the 1.30 HFU proposed, and this is identified in temperature measurement near the bedrock.

1109 L5: this is rather puzzling, as other heat flow datasets are quite contrasting. Geothermal heat flow remains a great unknown, and as shown in Pattyn 2010, needs a lot of correction to put it in line with observed patterns of melting (i.e. subglacial lake distribution). However, it is probably ice thickness that is the most decisive parameter keeping bedrock warm in thick places and cold over subglacial mountain ranges. This should be mentioned.

1110 L10: the slower flow of Kamb: could this be resolved using a different parameterization of C? or to use the inversion method to do so?

1118: Pattyn et al 2012 is now published in TC.

Interactive comment on Geosci. Model Dev. Discuss., 5, 1077, 2012.

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