Geosci. Model Dev. Discuss., 2, S17–S22, 2009
www.geosci-model-dev-discuss.net/2/S17/2009/

Geoscientific
Model Development
Discussions

Interactive comment on "Derivation of a numerical solution of the 3D coupled velocity field for an ice sheet – ice shelf system, incorporating both full and approximate stress solutions" *by* T. J. Reerink et al.

Anonymous Referee #3

Received and published: 6 March 2009

General comments:

This paper presents a higher order model for the 3D coupled velocity field for an ice sheet-ice shelf system, from its assumptions to a compact vector format for the sparse matrix. Ice-sheet - ice-shelf systems such as Antarctica and Greenland are known to play a major role in the climate system. To model their response to the present climate change and their contribution to sea level rise is one of the big actual challenges in glaciology. It has been shown recently that to model accurately the flow of such systems, a proper inclusion of the grounding line and the shelves is required. Presently, large models that are used to model Antarctica, Greenland or paleo ice-sheets are based on two different assumptions for the sheet and the shelves, which are treated separately with a fictive boundary at the grounding line. The development of a unique model for both the sheet and the shelf is then a crucial point to improve ice flow models. The goal of the authors is to present a higher order model that holds both for the sheet and the shelf and that allow comparisons with several stress approximations.

There are 2 ideas in this paper:

1) To develop a higher order model that holds both for the sheet and the shelf. But the model presented here is not new, and, as said by the authors in Discussion, with a higher order model the only differences between the sheet and the shelf are the boundary conditions. The authors give a lot of details for basic assumptions and give no details for some crucial assumptions for the boundary conditions. Because this model leads to a huge system the authors let the numerical solution for future work and no applications are presented.

2) To develop a model that holds for different stress approximations just by adding coefficients that can be put to 0 or 1. But because with the different approximations, the number of coupled equations and the type of boundary conditions are different, it is hard to believe that the system can be solved in a unique way for the different approximations and again an application is really needed.

I found the paper poorly referenced, at least in the Introduction where a detailed description of the state of the art is needed to understand the aim of this paper, especially the distinction between full or higher-order models and approximate models. I understand that a lot of appendixes are needed and very useful in this highly technical paper, but most of the appendixes are not cited in the text, making the paper very hard for reading. I really appreciate all the efforts made by the authors to give all the numerical details, to make the model really easy to implement.

After this general comments, I will not recommend this paper for a publication in GMD without an application showing the applicability of the model.

In the following I address more specific comments to help the author to improve their paper for future submission.

Specific comments:

Title:

-*"incorporating both full and approximate".* Everywhere else in the paper the model is referred as a higher-order or "almost-full-stress (p91.l6)" model.

1. Introduction:

-*p83.l20 - p84.l16:* This part is confusing. You should clearly highlight the state of the art to model ice-sheet-ice-shelves systems and especially the differences between full-Stokes (or higher-order) models and approximate solutions. With a full-Stokes model the only differences between the sheet and the shelf are the boundary conditions, but for the moment these models are too much time and memory consuming to be applied to large ice-sheets. Approximate solutions have been developed as you say both for the sheet and the shelf but are incompatible and need to be treated separately (provide more references to existing models: Mac Ayeal, Ritz...).

-*P83.l29-p84.l3:* This sentence here in the middle is very confusing as A_{flow} is not defined and the main non linearity in the equations is caused by the second invariant of the strain-rate in the flow law. I suggest to add a sentence at the end of this part saying that the flow of ice is also strongly dependant of the temperature, adding a non linearity and that you can solve the heat equation in ice but it is not the topic of the paper.

-*p84.l26:* your type of model is referred as LMLb in Hindmarsh (JGR,2004) (whereas the Pattyn (2003) model is referred as LMLa). The conclusion is that LMLb is usually

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very good but can become unstable. Taking a LMLa type model should allow you to solve your system as the equations for the horizontal components of the velocity and the vertical component uncouple, as you say in the Discussion, making the system lighter to handle. Moreover the gain in precision from LMLa to LMLb seems very small and maybe doesn't justify the (small) computation time gained from the full Stokes solution. Many LMLa type models have been applied in glaciology (Pattyn et al., The Cryosphere, 2008). And as you say, your model reduces to a LMLa type model by taking $f_0 = 0$. I suggest to start with this simplification to provide an application.

-P85.l8-l9: The flow law you are using relates the deviatoric stresses to the strain-rates, via a second invariant of the strain-rates but is not *"between the second invariants only"*.

3. Basic Equations and Appendix A:

-eq. 3.1: the notation is unclear, I suggest something like

$$H_s = \begin{cases} H + Hb & \text{for } \frac{\rho_{ice}}{\rho_w} H > S - H_b \quad \text{(grounded)} \\ S + (1 - \frac{\rho_{ice}}{\rho_w}) H & \text{for } \frac{\rho_{ice}}{\rho_w} H < S - H_b \quad \text{(floating)} \end{cases}$$

Moreover this equation implicitly assumes the hydrostatic assumption used later in your model. With a full-Stokes model, the proper condition would be that the normal stress at the base of the shelf is balanced by the water pressure.

3.1 Continuity equation for ice

- eq. 3.2 : this equation supposes incompressibility of ice (your second and fifth assumptions) because this is the mass balance equation (where ρ is assumed constant) integrated along the vertical (cf Pattyn (JGR, 2003)).

3.3 The Deviatoric Stress equations

-P91.l1 "If the vertical stresses are relative important65533;" this sentence makes no sense for me.

-P91.l4 "normal stress". Do you mean vertical stress? Moreover, this is confusing with your appendix A2 where you give the full hydrostatic approximation and don't use it.

-p91 l23-25 and Appendix A: "incompressibility assumption". Your assumption $tr(\tau)' = 0$ derives from the definition of the deviatoric stresses and is not the incompressibility assumption. The incompressibility assumption is $\rho =$constante which lead to $div(u) = 0$ for the mass conservation equation (add this equation in your text, as the Stokes system is the mass conservation and equilibrium eq 3.8-3,10).

-I suggest to remove appendixes A1 to A3, and to define the deviatoric stresses (A2) in the text.

3.6 The 3D Boundary equations.

As you say in your Discussion, with a higher order model the boundary conditions are the only differences between an ice-sheet and a shelf. This section is too short and full of approximations that are not justified.

-Eq 3.27 eq 3.28 : What are the horizontal domain boundaries? I understand that it is the ice-shelf fronts in the x and y direction? In that case, why are you using a stress free condition and not the condition that the normal stress is balanced by the water pressure (e.g. Paterson 1994)?

- The usual free surface condition is no tangential stress. I don't find where it is said in Deponti et al. (2006) that your condition is similar to a free surface condition. Please justify. See for example Pattyn (2003) for a detailled expression of the free surface condtion for the ice surface (and shelf base). Formally it is a stress free surface for the ice surface and a basal drag free surface for the ice shelf base, but beacause of your hydrostatic hypothesis they effectively reduce to the same kind of expression.

-Give references to models used to compute u and v sliding, as in your Discussion you say that *"the friction depends on a temperature dependant treshold for sliding"*.

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4. A formulation of the general velocity equations65533;

I really appreciate the efforts made to explain which terms correspond to which approximation etc65533; But as I said in my general comments I am doubtful that a unique procedure will allow you to solve the system for different approximations, as the equations are uncoupled for some approximations and not for the others.

5. Numerical scheme.

Please refer to appendix B and C somewhere in the text with a short sentence giving the main purpose of these appendixes and when to report to them.

6 Discussion

p105.l15-21: the community even for 3D applications now uses many higher order models. To solve simple synthetic applications with few nodes should be possible and straightforward as all the details are given to handle the matrix. Using the 4.5 and 4.6 approximations by taking $f_0 = 0$, allow to uncouple the horizontal velocity and the vertical velocity leading to a smaller system which should be easier to solve.

Interactive comment on Geosci. Model Dev. Discuss., 2, 81, 2009.