Geosci. Model Dev. Discuss., 6, C938–C945, 2013
www.geosci-model-dev-discuss.net/6/C938/2013/

Geoscientific Model Development Discussions
Open Access

GMDD
6, C938–C945, 2013

Interactive comment on "RIMBAY – a multi-physics 3-D ice-dynamics model for comprehensive applications: model-description and examples" *by* M. Thoma et al.

T Zwinger (Referee)

thomas.zwinger@csc.fi

Received and published: 4 July 2013

1 General Comments

The paper presents a description of the ice sheet model (in all facets of the terminus *model* defined in the context) RIMBAY and by this matches the scope of GMD(D). But by being a descriptive paper the obligation to present more details on the algorithm used for parts of the code and on its computing abilities (serial, multi-threaded, parallel message passing or vectorized) comes with it. And at the moment the paper misses out on this. I will explain what I would see necessary to fill in in detail later. Also, in my

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

opinion, the presentation of the equations in large parts is inexact. Additionally, I also have to support the comment given by Helene Seroussi, that the work should be put in better context to existing similar codes. For instance, coupling lower order to HOM or FS models is not a new exercise - you might want to cite Seroussi et al. (2012). Additionally, let me point you to a very similar article currently under review in this very journal: Gagliardini et al. 2013 (see references at end).

I would see it necessary to work over these main points:

1. The paper discusses in detail the shallow shelf approximation (SSA) and shallow ice approximation (SIA) discretization and solution algorithms. The higher order model (HOM) and full Stokes (FS) solution are referred to in section 6.4 solely by references to Pattyn (2003) and Pattyn (2008). There seems to be some explanation on how the HOM system is solved in these papers (including a sparsely commented graphics in this publication), nevertheless, I am missing a detailed explanation for FS. I have doubts that exactly the same algorithm which is applied to solve the HOM equations also works for FS, where the horizontal directions of the conservation of linear momentum cannot any longer be decoupled from the other directions and unknowns, as the hydrostatic assumption has to be dropped. Additionally, by adding the pressure as a variable, the saddle-point problem demands special treatment to avoid the null-space of the solution (i.e. checker-board of the pressure field) - how is this achieved in the case of the A-grid and is the staggered grid formulation (C-grid) sufficient to cope with this? Hence, elaborate the differences between SSA/SIA and HOM in comparison to the FS solution procedure or else explain why it works.

2. Your equations (tensor and vectors and their products) are a strange mix of different notations that – at least to me – at some places do not make sense. Please, unify and correct your notation. I will explain it later on a case-by-case basis.

3. Some of your equations, in particular basal conditions, are uncleanly formulated

GMDD

6, C938–C945, 2013

Interactive Comment

at least in the aspect to what approximations they are being applied. If you apply different conditions between SIA/HOM/FS then spell it out. I will explain issues case-by-case below.

4. You completely drop the information on computational performance aspects of the code. Interesting questions the reader is left without any answers are:
   - Is this a parallel and/or multi-threaded code?
   - In case of a positive answer of the question above: How is the weak and how is the strong scaling of the code?
   - What typical sizes in terms of degrees of freedom are able to be computed in which wall-clock times?
   - Especially as you claim the multi-approximation approach to be necessary to do climate-relevant simulations: What are the savings compared to a complete FS simulation if using hybrid FS/SIA/SSA approach instead?
5. Under what license your code is published and what licenses are demanded for the external programs? This might be interesting for users who are in search of an ice-sheet code and do not know whether they have access to it.

2 Detailed Comments (sorted by their occurrence)

- page 3290, line 8: either drop the *multi-physics* or explain what in particular this argument is based on
- page 3293, equation (1): if $\boldsymbol{v}_i$ should denote the i-th component of the velocity vector, then I would not use bold-font for the symbol. Additionally, you then should also change the divergence into index notation. Suggested alternatives: $\frac{\partial(\rho v_i)}{\partial x_i}$ or, as often used, $(\rho\, v_i)_{,i}$. Same counts for equations (2), (3) and (4) on page 3294

- page 3294, equation (2): you write the acceleration term on the l.h.s. of (1), which – by a small Reynolds/Froude-number is negligible and, silently, in equation (6) on page 3295 is dropped. Suggestion: Either start directly from Stokes (and not Navier-Stokes) or explain why those terms have disappeared.

- page 3294, equation (3): the heat transfer equation actually is a balance for the specific internal energy , u. The caloric equation of state leads to $\frac{du}{dt} = c(T)\frac{dT}{dt}$ (see e.g. Greve, 1997), which makes the l.h.s. of (3) to be $\rho\frac{du}{dt} = \rho\, c\, \frac{dT}{dt}$, even if we have a non-constant capacity – consequently, your statement before equation (26) (page 3302, line 7) is incorrect. Additionally, a distinction between isobaric c_p (I guess that is what you mean with this symbol?)and isochoric c_V heat capacity in combination with incompressible fluids does not make sense.

- page 3295, equation (7): later the strain rate is denoted with $\dot{\epsilon}_{ij}$, here with $\dot{e}_{ij}$

- page 3295, equation (8): in equation (5) on page 3294 you define the deviatoric stress tensor as τ'_{ij}, here you define your rheological law to link the Cauchy stress tensor τ_{ij} to the strain-rate tensor, but it should be the deviatoric stress tensor τ'_{ij}

- page 3296, line 3: the incompressibility condition is defined as $tr\dot{\epsilon} = 0$ (where tr is the trace of the tensor), which would read as $\dot{\epsilon}_{xx} + \dot{\epsilon}_{yy} + \dot{\epsilon}_{zz} = 0$, i.e., the first invariant is zero. I would not immediately see why $\dot{\epsilon}^2_{xx} + \dot{\epsilon}^2_{yy} + \dot{\epsilon}^2_{zz}$ should vanish, despite the exceptional case when all diagonal components are identically zero. In that context you might add the definition of the effective strain rate, which usually is the square root of the second invariant divided by two (else (9) wouldn't work out), i.e., $\dot{\epsilon} = \sqrt{\left(1/2tr\dot{\epsilon^2}\right)}$

- page 3298, line 17: reache → reach

- page 3298, line 17: the unit of β^2 in SI should be Pa s m^{-1} - so you missing the time unit.

GMDD
6, C938–C945, 2013

- page 3298, line 18: thump → thumb
- page 3300, equation (19): stress tensor has full indexes, normal vector not. In my view, if sticking to index notation, one should write

$$\tau_{ij}\, n_{s\,j} = 0_i$$

- page 3300, equation (20): I think it would add to the understanding, if you could relate τ_{bi} to the stress vector, $t_{bi} = \tau_{ij} n_{bj}$. As far as I understand it, you have

$$\tau_{bi} = \left(t_{bi} - n_{bi}\left(t_{bj} n_{bj}\right)\right)$$

  page 3300, equation (21): these equations are not exact. After van der Veen and Whillans (1989) the second terms in the r.h.s. should contain a R_{zz} in the bracket, which – as you correctly point out in equation (12) – is neglected in HOM and lower approximations, but to my knowledge not in the FS. I want to have a justification – in particular with respect to the marine ice sheet problem – if the same condition is applied to your FS system.

- page 3301, line 8: should your free slip condition rather read like

$$\nabla_i \left(v_j - n_j (v_k n_k)\right) n_i = 0_j,$$

  hence a vanishing projection of the gradient of the tangential component of the velocity to the surface normal?

- page 3301, line 16: underline your argument of the common use of stretched coordinates by references (I know codes that use unaltered vertical direction)
- page 3302, line 7: As mentioned before, equation (26) is equally valid for non-constant heat capacity

GMDD

6, C938–C945, 2013

Interactive Comment

- page 3302, equation (26): again, the same issue with sloppy index notation: the internal heat source definitely is not a vector (or is the i so unfortunately chosen that it is an subscript rather than an index?). Secondly, the index in your advection term has to be summed over another i at the gradient of the temperature.

- page 3302, equation (27): the last term (deformation heat production), like the rest of the equation, has to be divided by $\rho\, c$

- page 3303, line 1: you have not defined the vector components t_{bx} and t_{by}. If they are according to my definition given earlier the components of the Cauchy stress tensor ($\tau_{ij} n_{bj}$), then your expression for the (absolute value of the) basal shear stress is only valid for horizontally aligned surfaces.

- page 3304, line3: *. . . and an equal stability solution.* Do you mean *an equally stable solution*?

- page 3304, footnote: swopped to swapped? And, actually, if you come up with such a detailed information, could you perhaps give a stronger reason than history for this? Is it because of the arrangement of array entries implied by the programming language?

- page 3305, line 22: inbetween → in between or in-between

- page 3308, line 20: *. . . this approach has discussed in some . . .* → *. . . this approach has <u>been</u> discussed in some . . .*

- page 3309, line 6: *The solution of a coupled ice sheet-ice shelf system is numerically complicated, if not solved with a high-resolution FS approach.* From my own experience I can tell you that the FS solution of an marine ice sheet indeed is numerically complicated (if one wants to use this expression) – the difference: it is mechanically correct.

GMDD

6, C938–C945, 2013

Interactive Comment

- page 3310, line 4: *Either a finite element discretisation (as in the Elmer/Ice model, e.g. Zwinger et al., 2007) or adaptive grids (Gladstone et al., 2010) with varying grid sizes are necessary to implement the FS approach in a reasonable way.* One could also for instance think of a finite volume model. Additionally, I do not think that Gladstone et al. used a FS model.

- page 3310, equation (39): again, we have an index i on the l.h.s for the flux Q^s that is not matched at the r.h.s.. Perhaps you want to add the unit directional vector to the r.h.s.?

- page 3311, line 11: *The RIMBAY-code is mainly written in C ++ . . . .* I interpret the *mainly* as $<$ 100%. So, in what programming language the rest of the code is formulated?

- page 3312, line 7: *The well established netcdf-output format of RIMBAY ensures that the computed results can subsequently post-processed with the desired software packages, if the supplied GMT-bash scripts (Wessel and Smith, 1998) (included in the RIMBAY-monotone database) should not be sufficient.* There is a verb missing in this sentence. Perhaps *. . . can subsequently be post-processed . . . .*

- page 3312, line 11: test-suit $\rightarrow$ test suite (at least I never saw it written the other way)

- page 3312, line 15: *The coupling of SIA and SSA at the grounding line, for instance, is realised by using the estimated velocities from the SIA solver as a Dirichlet boundary condition for the SSA solver.* How do you manage to couple a single, depth averaged value (SSA) to a vertical profile (SIA)? Are you depth integrating the SIA profile?

- page 3317, line 26: *With RIMBAY we provide a scalable open-source ice dynamics model to the scientific community.* This sentence contains vague hints on two main largely undiscussed issues I indicated above. Scalability is something I understand to apply to parallel codes (decrease of wall clock time by increasing the number of processes). Additionally, if it is open source, it would be good to mention the license scheme, as there are already quite some (GPL, BSD, copyleft, . . .).

References

Gagliardini, O., Zwinger, T., Gillet-Chaulet, F., Durand, G., Favier, L., de Fleurian, B., Greve, R., Malinen, M., Martín, C., Råback, P., Ruokolainen, J., Sacchettini, M., Schäfer, M., Seddik, H., and Thies, J.: Capabilities and performance of Elmer/Ice, a new generation ice-sheet model, Geosci. Model Dev. Discuss., 6, 1689-1741, doi:10.5194/gmdd-6-1689-2013, 2013.

Greve, R.: A continuum-mechanical formulation for shallow polythermal ice sheetsPhil. Trans. R. Soc. Lond. A, 355, 921–974, 1997

Seroussi, H., Ben Dhia, H., Morlighem, M., Larour, E., Rignot, E. and Aubry, D.: Coupling ice flow models of varying orders of complexity with the Tiling method, J. Glaciol., 58, 210, 776–786 , doi:10.3189/2012JoG11j195, 2012

Van der Veen, C.J. and Whillans, L.M.: Force Budget: I. Theory and numerical methods, J. Glaciol., 35, 119, 1989.