Rimbay – a multi-physics 3-D ice-dynamics model for comprehensive applications: model-description and examples

1 Answer to Reviewer Thomas Zwinger

Many thanks for the review and the suggestions to improve the manuscript. In particular for the hard-to-spot errors in the equations. We answered all items below in detail.

1.1 General Comments

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.
   In section 6.4 we describe that the HOM is explained in detail in Pattyn (2003), the FS-model is described in detail in Pattyn (2008). Additionally in Thoma (2010) it is explained, that a 3D-viscosity filtering suppresses the instability problems. Therefore we don't repeat this in this manuscript, which has a slightly different focus. In section 4.2 we explain, that the C-Grid solver for the momentum is *only* available for the SIA- and SSA-solver, and not for the HOM or FS. We are therefore not sure which additional information the reviewer requests.

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.
   We completely agree with Thomas Zwinger, that a mixed vector–index notation is not good. Originally, we preferred the index notation, but this seems to be in conflict with the GMD-rules, which prefers the **bold-**vector and **bold-**matrix notation. The mixture was introduced during the typesetting process and accepted from us because of carelessness. In accordance to the GMD-rules for typesetting, we adjusted the notation throughout the manuscript as follows:

- For vectors we use $\vec{v}$ rather than v_i

- For the stress tensors and the strain tensor use $\boldsymbol{\tau}$, $\boldsymbol{\tau}'$, and $\dot{\boldsymbol{\epsilon}}$, respectively.

3. Some of your equations, in particular basal conditions, are uncleanly formulated 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.
We explicitly thank the reviewer for his diligent review of the equations which surely helped a lot to improve this manuscript. His comments are answered in detail 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?

The reviewer is correct, we didn't mention this so far. In fact RIMBAY is (apart from LIS) a serial code, we added this information to section 7. Therefore we don't give any information about scaling. To our knowledge the expression *degrees of freedom* is typical for finite-element modelling, but not so common for finite difference modelling. Basically, if the geometric model fits into the memory it can be solved with RIMBAY. We also don't want to publish wall-clock times, as they change with each new CPU-generation.

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.
We added some information about this to the discussion section.

1.2 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
As the first reviewer, Stephen Cornford, suggested, we removed the expression *multi-physics* from the manuscript.

- page 3293, equation (1): if v_i should denote the i-th component of the velocity vector, then I would not use bold-font for the symbol.
Neither would I ;-), but see below 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

$(v_i)_{,i}$. Same counts for equations (2), (3) and (4) on page 3294
See our comment on 2., above.

- 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.
  We added a sentence just before (6) to explain this.

- 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.
  Corrected, many thanks to the reviewer to point this out (we unmindfully cited
  Eq. (3) of Pattyn(2003) without correcting it). We also changed c_p to c as sug-
  gested.

- page 3295, equation (7): later the strain rate is denoted with $\dot{\epsilon}_{ij}$, here with $\dot{e}_{ij}$
  The first reviewer also spotted this error, corrected.

- 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}
  The reviewer is correct, that the the deviatoric stress tensor has to be used in
  the rheological law. Also there was a sign error in Eq (5).

- page 3296, line 3: the incompressibility condition is defined as $tr\epsilon = 0$ (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 $\dot{\epsilon}^2_{xx} + \dot{\epsilon}^2_{yy} + \dot{\epsilon}^2_{zz} = 0$ 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., $\epsilon = \sqrt{(1/2 tr\dot{\epsilon}^2)}$
  Reviewer Stephen Cornford also spotted this error, the ^{2} has to be removed, (9)
  is correct.

- page 3298, line 17: reache $\rightarrow$ reach
  Corrected

- page 3298, line 17: the unit of β^2 in SI should be Pa s m^1 - so you missing the
  time unit
  The time unit is given in years here: Pa **yr** m^{-1}

- page 3298, line 18: thump $\rightarrow$ thumb
  Corrected

3

- page 3300, equation (19): stress tensor has full indexes, normal vector not. In my view, if sticking to index notation, one should write $\tau_{ij}n_{sj} = 0$
  As mentioned above we had to drop the index notation, we changed the notation to $\boldsymbol{\tau_s} \cdot n_s = 0$.

- page 3300, equation (20): I think it would add to the understanding, if you could relate τ_{bi} to the stress vector, $\tau_{bi} = \tau_{ij}n_{bj}$. As far as I understand it, you have $\tau_{bi} = (\tau_{bi} - n_{bi}(\tau_{bj}n_{bj}))$
  We dropped the index notation as suggested by the reviewers and slightly reformulated this passage.

- 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.
  The reviewer is right, we added R_{zz} to the equation and we can confirm, that R_{zz} is also applied in the numerical code for the FS-model.

- page 3301, line 8: should your free slip condition rather read like $\nabla(v_j - n_j(v_k n_k))n_i = 0$ hence a vanishing projection of the gradient of the tangential component of the velocity to the surface normal?
  We reformulated this part and dropped the index notation.

- page 3301, line 16: underline your argument of the common use of stretched coordinates by references (I know codes that use unaltered vertical direction)
  We dropped this argument.

- page 3302, line 7: As mentioned before, equation (26) is equally valid for non-constant heat capacity
  Reformulated

- 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.
  Both sloppiness are corrected now.

- page 3302, equation (27): the last term (deformation heat production), like the rest of the equation, has to be divided ρc
  Corrected

- 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.
  t_{bx} and t_{by} were wrong, correct are: τ_{bx} and τ_{by}, which are defined by Eq.(21).

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

- 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?
  Swapped is corrected. Of course, the reviewer is correct, when he assumes that the *history* has to do with the *row-major order* C uses to arranges arrays. However, we think that this is to much detail. The mentioning of the swapped indices, however is useful to everyone who looks into the code for the first time.

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

- page 3308, line 20: ... this approach has discussed in some ... → ... this approach has been discussed in some ...
  Corrected

- 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.
  Reformulated

- 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.
  Reformulated

- 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.?
  We removed the index notation and also adjusted Eq. (40) accordingly.

- 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?
  We clarified this by adding a footnote: *A few parts of* RIMBAY *still base on the original code of Pattyn (2003,2008), which was written in C and not C++; also the implemented solver libraries (NR and LIS), and the netcdf-interface are written in C.*

- 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 ... .
Corrected

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

- 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?
  Reformulated

- 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, ...).
  We added additional information to clarify this.