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

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This paper (Thoma at al, Geosci. Model Dev. Discuss., 6, 3289–3347, 2013) is a description of the RIMBAY numerical ice sheet model. RIMBAY has appeared in a few publications already Thoma et al 2010, Thoma et al 2010, and I think at least one manuscript in preparation (Determan et al 2013), so it is safe to say that the model is useful. Therefore a description of its capabilities and methods is suited to publication in GMDD.

RIMBAY seems to have some interesting features, for example the ability to treat some parts of the domain with a simple (SIA) approximation and others with more accurate (e.g Stokes) approximations. The paper begins with a description of the governing equations and numerical methods used to approximately solve them, and concludes with some examples and standard test. I couldn't tell from the manscript whether RIMBAY is a serial or parallel code - I'm guessing the first? Anyway, it could be an attractive model for paleo-climate work because it seems to be designed to work at low reolutions and can be coupled to climate/GIA/hydrology models.

Overall, I found the paper read well. There are a few scientific points regarding the presentation that I would like the authors to address, plus some typographical errors I spotted on the way (probably not an exhaustive list). I have chosen to call the revisions I suggest below 'major revisions' rather then 'minor revisions' because I would prefer to review them but they are modest in scope.

1 Issues related to scientific presentation

1. The title: does it make sense to refer to RIMBAY as a multi-physics model? That term usually seems to apply to applications the provide numerical methods useful across a wide range of physics, COMSOL multi-physics being a well-known example. I realise that RIMBAY includes a choice of approximation to the Stokes problem at its core, but that seems to be described by 'comprehensive'.
2. p 3290, line 26: 'Therefore, the imminent climate change will have profound impact on society'. Is this justified by the references? I'm not saying that this won't come to pass, but I don't think ice sheet modelling papers should contain speculative remarks like this.
3. p 3291, lines 16- : I agree with Helene Seroussi's comment, that there should be some more (a few lines) discussion of the capabilities of other ice sheet models.
4. p 3303, lines 14–17: Equations 10,14,16 are not linear. I think the authors know

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what they want to say (the equations are non-linear because of the form of the viscosity, and often the basal traction law too), but just need to remove the first 'linear'

5. p 3304, lines 1–5. Is convergence of the non-linear system really faster. If Newton's method were used, the (inner) linear systems would look like

$$\begin{pmatrix} A & A' \\ B' & B \end{pmatrix} \begin{pmatrix} u \\ v \end{pmatrix} = \begin{pmatrix} p \\ q \end{pmatrix} \tag{1}$$

   for the hydrostatic and SSA cases. RIMBAY (I think) is attempting to solve this linear system by solving

$$Au = p - A'v \tag{2}$$

   then

$$Bv = q - B'u \tag{3}$$

   once for each cycle of the outer iterations (or more times? – this needs to be clearer). Either way, this a quasi-Newton (Picard) method that is often outperformed. It is even surprising to me that the linear system (1) would converge more quickly by iteration of (2) and (3). I'd like to see some evidence of this, or a citation to such evidence.

6. Section 4.2 : Is the C-grid chosen to avoid checker-board pressure/velocity fields that appear in the A-grid? If so, could we see some solutions where the A-grid is problematic and the C-grid is not?

7. Section 4.3.1 and 4.3.3. Surely the central-difference scheme (eq 33) is unconditionally unstable for pure advection on both A and C grids (not just the A grid), just as it is even if the velocity is a known constant

8. p 3308–9. Useful as the first-order upwind scheme is, it does not avoid numerical diffusion - rather, it is notorious for it. The text suggests the opposite, presumably

inadvertently, because the phrase 'to overcome the restrictions involved with the numerical representation of Eq. (35)' which comes after noting the diffusion terms in eq 35/36.

9. p 3310 line 5, Gladstone 2010 is not a full Stokes model (it is SSA), It is also a flowline model - there are other adaptive models that treat the same sort of 3D problems as RIMBAY included in the MISMIP3D exercise (Pattyn et al 2013)

10. p 3311 line 5: I don't like the invented word 'schoofism'. I appreciate that the authors want to refer to the heuristic condition based on Schoof 2007 later, but I don't think that a phrase like 'imposing the heuristic condition outlined in section 4.4' is too unwieldy. Other authors that use this condition (Pollard and DeConto for example) don't introduce such a word.

11. section 6.2 : These are interesting examples, and I would like to see some discussion (with a graph of residual vs iteration) of RIMBAYs solver performance in these cases. Ice shelves are tougher than grounded ice, because the linear systems are poorly-conditioned when $\beta = 0$. The tabular iceberg example should be ill-posed, because any constant velocity can be added to a solution (u, v) and still satisfy the governing equations, unless the velocity is imposed at one point - is this the case?

12. section 6.3 : The text says 'In consideration of the approximations and the low horizontal resolution, RIMBAY was able to keep up with the other 16 numerical models'. I think the authors need to replace this with a more quantitative statement, comparing the RIMBAY results with the comparable models (ie those that use the heuristic, DPOx and VUBx, I think?). For example, does it produce the same steady-state configuration? Are the perturbed amplitudes and time-scales similar? Likewise, it would be interesting to know why RIMBAY produced less smooth grounding lines - is this something to do with the numerical treatment, e.g sub-grid interpolation or something like that.

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13. section 6.4 : It is interesting that the grounding line advances when HOM or FS is used in place of the heuristic condition – has it reached steady state ? If so, this wouldn't (or shouldn't) be the case for a straight grounding line, but it is harder to say what should happen in this case, especially if steady state has not been reached. For example, the MIMSIP3D perturbation caused the grounding line to advance at first from the starting point, but had the slippy spot remained in place for long enough, the GRL should eventually migrate upstream of the starting point. Softening the ice (by switching to HOM or FS) could lead to a similar transient, perhaps.

    If possible (and I know that the CPU cost might be large), I would like to see this experiment expanded to help readers decide whether the results are correct or an artefact of the numerical treatment. For example, spin the model up with the heuristic condition and a uniform basal traction coefficient $C = C'$ (as for the original MISMIP3D) , and report the position of the grounding line - does it agree with the formula in Schoof 2007? Then, switch to FS and/or HOM in a region around the grounding line. Does it retreat or advance at first? What is the steady state,if that can be computed in reasonable time, if not, does the grounding line ever change direction? Do the results change if the higher-order region is larger?

2 Typographical and minor grammatical errors

1. p 3290, line 11 : Full—Stokes, should be full Stokes
2. p 3291 line 27 : '[multiple items] has been improved', $\rightarrow$ 'have been improved'
3. p 3292 line 21 : 'fulfil this needs' $\rightarrow$ 'fulfil these needs'
4. p 3293: lines 17 : 'programmes' $\rightarrow$ 'programs'.

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5. eq 7: the strain rate is rendered as $\dot{e}$, but in the next lines is $\dot{\epsilon}$

6. p 3926: $\dot{\epsilon}_{xx}^2 + \dot{\epsilon}_{yy}^2 + \dot{\epsilon}_{zz}^2 \rightarrow \dot{\epsilon}_{xx} + \dot{\epsilon}_{yy} + \dot{\epsilon}_{zz}$ (the resulting expression, eq 9, is correct)

7. p 3303: 'rule of thump' $\rightarrow$ 'rule of thumb'

8. p 3304, footnote ; 'swopped' $\rightarrow$ swapped

9. p 3308, line 25 ; missing factors of $\frac{1}{2}$ in e.g $U_{i,j}^C = (U_{i,j} + U_{i,j+1})$ should be $U_{i,j}^C = \frac{1}{2}(U_{i,j} + U_{i,j+1})$

10. p 3308, line 3 ;'than less ice' $\rightarrow$ 'then less ice'

11. p 3312 line 13 'level of classification': I think this is just an English language oddity, but I haven't previously seen the organisation of C++ code into class hierarchies described as 'classification'

12. p 3312 line 20: 'It handles Makefiles and attends dependencies between different source (and header) files automatically.' sounds a bit awkward, though I know what is meant

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