Review of: Experimental design for three interrelated Marine Ice-Sheet and Ocean Model Intercomparison Projects by Asay-Davis et al.

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1 General comments

This article introduces the interrelated inter-comparison suites MISMIP+ (for marine ice sheets in contact with ocean), ISOMIP+ (for coastal ocean in contact with ice shelves) and MISOMIP+ (for interaction of marine ice sheet and ocean models). I support the idea to publish the needed steps to participate in a paper alongside the necessary web-page. Inter-comparison projects have become very popular during the last 10 years and in my opinion sometimes the threshold of launching new initiatives should be set to a higher level. Nevertheless, I think in view of the challenges in improving on the prognostic modeling approach to sea level rise this is a necessary and useful setup. I am quite sure that there will be issues in the yet unexplored effects introduced not only by the separate components (i.e., ice sheet and ocean circulation models) but mainly the to date largely unexplored coupling effects coming up and that this will not be the last MISOMIP (or however future projects will be named) activity. I think it is a good crystallization point for development of coupled ice sheet/ocean models and by this a welcome initiative. Restricting myself to my field of expertise, i.e., ice-sheet models, I can say that by previous work from Gudmundsson et al. (2012) the glaciological part of the inter-comparison experiments is a good choice. I have little to contribute on the ocean model side concerning this aspect.

The article fulfills its major purpose, namely, to describe the setups of the experiments, the preliminary model requirements and the expected output. The outline is clear and structured and, by reading through the paper I - at least for the ice-sheet and coupling part - find the instructions to be generally clear (I elaborate on those parts which I think still need improvements). I have the main issues summarized in the following section. If these are addressed I would recommend the article to be published in GMD.

2 Main points of criticism

My first point of concern is the for me missing conclusive argumentation concerning the method to reach the **spinup-state**, also in connection with the **choice of the ice-sheet model**. As you correctly pointed out, the MISMIP3d exercises revealed a discrepancy between the steady state position of full-Stokes and higher order (from full-Stokes perspective: lower order) models. As you choose the latter to act as the reference, which, despite your later statement, has potential also to become the quasi benchmark, it actually would have been nice to analyze how much the initial state varies between different SSA models and (if possible) a full-Stokes and a SSA model - I know that one full-Stokes model was used in the Gudmundsson et al. (2012) paper, which tells me that in principle initial MISMIP+ tests would have been possible. Connected to spinup and model choice I have the following points that need clarification in this context:

- You refer to "realistic simulation", which I understand means realistic not in terms of physics but in terms of model approach (e.g., SSA). Do you basically render any Blatter-Pattyn or full-Stokes approach as unrealistic? If so, you should state more clearly that this is SSA-business, only (and not later add that "other models are welcome"). The clear preference of depth averaged models continues within the choice of the sliding law (page 9870), which assumes hydrostatic pressure distribution at the base and the not further specified calving front boundary condition.
- You let modelers choose to freely change viscosity as well as basal sliding parameter and/or sliding law (but therein restricted to Tsai and power law – why not the law proposed by Schoof/Gagliardini (Gagliardini et al. 2007)?) to achieve the initial spinup geometry. I do not claim that this is a bad choice, but I have the suspicion that even for each single model this is not unique, i.e., you can have a multitude of parameter combinations that produce you initial conditions with grounding lines intersecting a certain single point, but showing different volume fluxes. I would expect a justification, e.g. some proof of (non)-sensitivity of the spinup-state to variations of the freely chosen parameters within the chosen sliding law and rate-factor from Tab. 1. Could it be that this in some aspect might already exist either in form of a journal article or in form of the (page 9868) two models from MISMIP3d that were applied to the spinup state? For the latter, could you please elaborate which physics (approximation to Stokes) and which sliding/viscosity parameters these two models had in order to increase or relate the information to the reader/future MISMIP+ participant?

Secondly, – and that was mentioned already in my first point – I could not find any hint on two boundary conditions for the ice sheet model within the text: What I am missing is the **free-surface accumulation pattern** (or rather constant value) needed to grow the ice-sheet into a steady state, neither if one

has to apply accumulation/ablation on the free surface during the experiments. Even if this is trivial (as I conclude from Tab. 1), I think you should to provide that information in the text – from earlier experience with MISMIP3d I also can recommend to double-check its value before people start doing simulations.
Also the **dynamic condition at the artificial calving front** is missing. For depth-averaged (SSA-ish) models it is one and the same, but for full-Stokes you have to specify whether you apply sea water pressure (up to sea level) or a cryostatic pressure distribution over the full thickness (mimicking an infinite long ice shelf).
Thirdly, could you please elaborate on how the **output of the grounding line** should work. In particular, models based on SSA often apply sub-grid schemes for grounding line dynamics and hence a clearer rule on whether you demand output of properties on such an interpolated line (leading hence to interpolated values) or at the last grounded/first floating mesh-point would be at place (NB: with a full-Stokes model this would not be an issue).
Finally, I think section 4.5 on the **results of the coupled simulation** in my opinion is a little bit thin. I have the feeling that you could do better on displaying the IceOcean1 result (rather perhaps stick to 2D cuts, where one actually can see something). Also some **figures** displayed on MISMIP+ and ISOMIP+ results reveal their information only beyond a zoom-factor of 200% (which is not acceptable on the printouts); I elaborate this case-by-case in the next section where I think that there would be space for improvements.

Good luck with this inter-comparison!

3 Technicalities (sorted by their occurrence)

Line numbers refer to the printer-friendly version of the document that is to be found under `http://www.geosci-model-dev-discuss.net/8/9859/2015/gmdd-8-9859-2015-print.pdf`. I further include here also manifestation of the main points of critics of the previous section:

* page 9862, line 2: "At the first MISOMIP workshop held at New York University, Abu Dhabi in October 2014, participants decided that intercomparisons of ice sheet-ocean dynamics in realistic configurations would be more credible if it was preceded by a more idealized intercomparison and evaluation process for the standalone components and coupled models involved."
  Is there some official document or maintained URL available on this workshop, where one could see what type of workshop (invited or open) that was and who of the community participated? This is interesting if the author list is a sub-set of the participation list, simply, because expressions like "we decided" occur frequently in this paper.

* page 9863, line 6: "The second marine ice-sheet MIP, MISMIP3d (Pattyn et al., 2013), aimed at exploring grounding-line dynamics on centen-

nial timescales in a configuration that varied in two horizontal dimensions (2HD)."

This is just a suggestion, but as there are multiple non-marine ice sheet MIPs, it might be a good idea to be explicit on that.

* page 9863, line 20:"topoography $\rightarrow$topography"

* page 9868, line8: "First, it started from a steady state that was invariant in the cross-flow direction – that is, 1HD – and did not involve significant lateral stresses."

  Minor issue, but, if you refer to the initial state, the fact that it was effectively 1HD tells us that the initial state involved no lateral stresses. Else, add an "... during the applied perturbations" at the end of the sentence.

* page 9869, line 19: "A suggested value for A is given in Table 1, but participants should modify this value (and/or the coefficient β^2 that appears in the basal traction below) so that the steady state grounding line crosses the center of the trough at x = 450 $\pm$ 10km."

  That is in particular the sentence I referred to in the previous section. I would say that as a minimum you have to provide a good argument why you think that this is {a good enough, the best of all, the only possible choice} to obtain comparable spinup. At the best, you demonstrate the non-sensitivity of both, the spinup as well as the perturbation phase with simulations obtained by one or (preferred) multiple model(s) with respect to variation of A, β^2 as well as the chosen sliding law.

* page 9870, line 1 - 17: That paragraph links to the question why – if allowing for choices in the type of the sliding law – you confine it to (6) and (7)? First of all, I think (6) is not a good choice, as it is not bounded, secondly, the sliding law introduced in detail by Gagliardini et. al. (2007) would at least be an equally good alternative to (7). The free choice between two sliding laws introducing further model variety should be justified. Explain also what a model that does not include the assumption of a hydrostatic pressure distribution should do with (7) - I guess the answer will be, *effective pressure at the base*.

* page 9871, equations (10)-(13): This is just a suggestion, but I think a simple2D flow-line sketch instead or in addition to the 3D pictures showing all the relevant geometrical parameters, such as z_d, z_0 and H_c would be good for readers not being that familiar with marine ice sheet setups.

* page 9872, equation (14): I am aware that you explain your cut-off value before, but, the statement that (14) is a result of (10)-(13) with a lumped coefficient does not apply, as $|z_d - z_0|$ and $\max(z_0 - z_d, 0)$ simply are not the same. My suggestion would be to either rewrite (12) or postpone the explanation of this cut-off to after (14).

* page 9872, line 14: "The coefficient Ω has been given a value of 0.2yr^{-1}, corresponding to a maximum ambient ocean temperature $\sim$1.0°C, which

leads to a melt rate with a maximum value of $m_i \approx 75\text{myr}^{-1}$ near the grounding line (see Fig. 2)."
First of all, is m_i ice or water equivalent? Secondly, I guess the 75myr^{-1} correspond to the steady state spun-up configuration of BISICLES in your setup. If so, then please explicitly mention this.

* page 9875, line 4: "xGL(nPointGL,nTime), yGL(nPointGL,nTime) [m]. The x and y coordinates of a given point on the grounding line."
  As mentioned in my general comments, this would need more detailed rules for models using sub-grid representation of the grounding line.

* page 9875, line 23:"..., in a. pdf file, ..."
  There is an orphan dot in this line.

* page 9876, line 1:"2. Englacial stresses: the stress model and coefficients (e.g. SSA, $A = 2.0 \times 10^{-17}$ $\text{Pa}^{-3}\text{yr}^{-1}$)."
  I would rather call it stress approximation model.

* page 9876, line 13: Conventional models should simply carry out a convergence study of experiment Ice1r and Ice1ra, showing that the grounding line shape and positions at the start and end of Ice1r and the volume-above-flotation curves throughout the experiments converge with mesh refinement and differ by a fraction at the finer resolutions.
  Conventional = SSA? Please explain what you mean by conventional.

* page 9879, line 11: ...of most Antarctic ice shelves...

* page 9879, line 18: "We prescribe an f plane configuration ..."
  Like there was a reference for the SSA in the part prescribing the MISMIP+ experiments, I think – in order to keep symmetry between the level of explaining typical approximations in glaciology and oceanography – it would also be appropriate to introduce the standard literature for glaciologists that want to invest into reading about the nature and consequences of such approximations in ocean models.

* page 9880, line 17: "Ice thinner than $H_{\text{calve}} = 100$ m (equivalent to an ice draft above $\sim$ -90 m) is considered to have calved and the ice draft is set to zero."
  It is clear from the text why you want to have a calving front within the ocean model domain. But could you provide some motivation (if there is) why exactly at 100 m and not another number? For instance, something along the line that there are no shelf thicknesses below that threshold observed in Amundsen sea area, or 100 m gave a nice ratio between shelf-covered and open ocean in the model setup.

* page 9887, line 26: " The WARM profiles was chosen to ..."
  Either profile was or (more likely) profiles were.

* page 9888, line 25: "...retreat from Ice1r."
This is just my personal view, so, leave or change it: If referring to other experiments from other sub-MIPs, you could put the corresponding MIP identifier in front, such as MISMIP+ Ice1r – easiest would have just been to use MISMIP+/ISOMIP+/MISOMIP in the experiment names (this is also just a personal opinion), but you might not want to change this at this stage.

* page 9893, line 26: "- A link to the repository where the model can be downloaded (if public) and specific tag, branch or revision (if available)."
Why such a demand is confined to the ocean models – should similar information not have been demanded from the ice-sheet model in MISMIP+ and MISOMIP suite?

* page 9896, line 6: "This should greatly reduce melting within a decade, similarly to Ocean2, and allow ice to re-advance for the remaining 100 years of simulation."
Minor detail: If there is a retarded signal in lowering the melting from the ocean side that lasts a decade, the there are no remaining 100 years for re-advance.

* page 9896, line 22: "Whereas the MISMIP+ experiments do not include a dynamic calving front, IceOcean2 prescribes the same simple calving criterion used in ISOMIP+: ice thinner than $H_{\mathrm{calve}} = 100$ m (equivalent to an ice draft above $\sim$ -90 m) or beyond $x_{\mathrm{calve}} = 640$ km should be calved $\rightarrow$ removed and the ice thickness set to zero." Two issues here: First, as stated already in a comment to section 3.1.2, you should provide motivation (either by physics or model setup) why you chose this values. Secondly, you did not mention the x_{calve}-criterion in connection to ISOMIP+ section 3.1.2. This should be explicitly mentioned also there, if it was applied. Minor detail: I would replace "calved" with "removed", as the first one suggests that this is according to a physical calving criterion.

* page 9897, line 9 + page 9898 1st paragraph: "Coupled ice sheet-ocean models are not well enough established to have typical resolutions and parameters. Therefore, we invite participants to submit several sets of results with parameter choices at their discretion in addition to the COM run and ensure these are well documented in the `readme` file.

  The coupling interval for the model is left to each participant to decide. We rec-ommend based on experience with the POPSICLES (coupled POP2x and BISICLES) model that participants use a coupling interval of six months or less if they are able, as results with yearly coupling diverged significantly from those with more frequent cou-pling. We ask participants who are able to do so to provide multiple sets of results using different coupling intervals."
  Here in particular I would have liked to see a slightly more in-depth analysis of POPCYCLES results on IceOcean1 included in this article. You

often use the terms ”conventional” and ”typical” within the text, so I think the participants of this inter-comparison would benefit from obtaining more information on the settings of a ”typical” coupled setup, such as POPCYCLES. What, for instance, was the range of coupling timestep-sizes, what where the timestep-sizes in the sub-models, what computational load and wall-clock-times did those runs produce? From the next paragraph on page 9888 I conclude that you actually have more information from variation of coupling intervals. In my personal opinion – if the results seem to be just around the corner – it would have been better to wait to have also IceOcean2 achieved before pushing the paper to publication, to have a complete set of simulations.

Something completely different: I have the feeling that there is something strange with hyphenations (but that might be more a typesetting issue from Copernicus). I found two hyphenations that in my view are not correct and marked those in red - please check (in case with the GMD typesetter).

* page 9889, section 4.5: As mentioned in the previous item and the main points of criticism, I think that you should reveal more information within this section.

* page 9990, line 3: "**The Supplement related to this article is available online at doi:10.5194/gmdd-8-9859-2015-supplement.** "
This is useful.

References

From the ice-sheet point of view I have no complaints. One point I would like to have cleared and I think that also links to the editorial comment given in `http://www.geosci-model-dev-discuss.net/8/C3046/2015/gmdd-8-C3046-2015.pdf`: Is the link given in
Hunter, J. R.: Specification for test models of ice shelf cavities, Tech. Rep. June, Antarctic Climate and Ecosystems Cooperative Research Centre, available at: http://staff.acecrc.org. au/ johunter/isomip/test_cavities.pdf (last access: 7 November 2015), 2006. 9864, 9885 certainly available for a longer period – or would it be better to choose a certainly permanent URL to provide this data?

Figures

* page 9912, Fig 1: "Figure 1. The bedrock topography for the three MIPs as defined by Eqs. (1)–(4). (a) $B_x(x)$, the variability of the bedrock topogrpahy → topography in the x direction. The topography through the central trough is shown in blue and on the side walls is shown in red. (b) $B_y(y)$, the bedrock topography in the y driection → direction relative

to that at the center of the trough. (c) The topography in 3-D at 1 km resolution. Sea level is shown in translucent blue. "

(b) shows a cross section that to me seems only to be valid over the region of the trough (where threshold of $-B_{\mathrm{max}}$ does not apply); here one could get the impression that it applies to the whole region.

* page 9913, Fig. 2: This figure is way to small, in particular in terms of annotated text. I think a plot of the corresponding velocities also would be interesting.

* page 9916 + 9917, Fig. 5+6 : This is just my personal opinion, but I think that the boundaries of the ocean model are trivially clear (its a box outside the shelf) and leaving out the dark-blue side-walls would lower the complexity and improve the visualization of the essential information.

* page 9920 - 9822, Fig. 9-11 : Again, too small if read in 1:1 size. Why do all the stream-functions extend beyond the ocean region? I guess they are derived quantities and take zero values there, but could you, for convenience, mask the areas where there is grounded ice? If using ParaView, the Threshold-filter might come handy. Same applies to Fig. 12 on page 9923.

* page 9924, Fig. 13: Too small, again. Upper picture does not really convey a lot of information. You could think of using cross-sections to show the change in shape of the ice shelf as well as display the temperature distribution in the ocean.

References

Gagliardini, O., Cohen, D., Råback, P., and Zwinger, T.: *Finite-element modeling of subglacial cavities and related friction law*, J. Geophys. Res., 112, F0227, doi:10.1029/2006JF000576, 2007

Gudmundsson, G. H., Krug, J., Durand, G., Favier, L., and Gagliardini, O.: *The stability of grounding lines on retrograde slopes*, The Cryosphere, 6, 1497-1505, doi:10.5194/tc-6-1497-2012, 2012