We received three sets of reviewer comments and three short comments, all of which suggested helpful improvements to the manuscript. Our responses below have been color coded and formatted as follows:

> Reviewer/short comments are in gray and indented.

Our responses are in black and not indented.

> "Changes to the text are in quotation marks, in dark blue and indented"

Hopefully this formatting is helpful and keeps things clear.

Authors' responses to comments from Reviewer 1:

> This paper describes in detail 3 new model intercomparison projects, MISMIP+, ISOMIP+ and MISOMIP1 for marine ice sheet and regional ocean models. MISMIP+ is dedicated to the marine ice sheet models, ISOMIP+ to the ocean models incorporating an ice-shelf cavity and MISOMIP1 to the coupling of both type of models. This paper is well written, clear and it is an important contribution for the ice-sheet/ocean community. I have only one main concern regarding the diffusion of the setups through different canals (this paper and a web site) and few minor remarks listed below. I will mostly comment on the ice part of the experiments, being not an oceanographer.

Thank you for taking the time to review our manuscript. We are glad you found the paper to be clear and well written, and the experiments to be worthwhile for the community. We have endeavored to address your concerns as detailed below.

> My main concern is the existence of more than one place to find the description of the experiments, which might be confusing and source of errors for the participants. It should be stated clearly with which document participants should be working, both on the GMD paper and on the CLIC webpages hosting the description of the MISOMIP experiments. This is also true for supplement material, part of it being attached as a supplement of this paper and an other part being located on the webpage. I suggest that if this GMD paper is the reference for these experiments that all the needed material (input files, examples of model description, etc) is provided as a supplement of this paper.
> The GMD being the reference, it questions the way changes or updates (which might be necessary when participants will start running the experiments and find some ambiguities in the experiments description, because it is always difficult to think about all possible configurations in advance) will be provided to the community. The strategy for setup update after the GMD paper is accepted should be clearly stated in the GMD paper itself (a link to an update webpage on the MISOMIP website for example).

We agree that it is important not to have two versions of the experiments, one on the MISOMIP website and one in this manuscript. We plan to modify the MISOMIP website to make clearer that it is meant to provide a short summary of the experiments as well as some supporting material, and that the GMD manuscript is the definitive description of each MIP.

Since this manuscript has been granted an extended discussion period, this has allowed various groups to try out the MISMIP+, ISOMIP+ and MISOMIP1 experiments and to raise issues and concerns. We have attempted to address these concerns in the updated manuscript to the best of our abilities. We feel that it is important that the experiments be "finalized" so that researchers do not face a continually moving target. We realize that this will necessarily mean that the experiments remain imperfect. A web page with updates to the experiments could theoretically allow for further changes to the experiments over time but this would likely make it nearly impossible to know which version of the experiments each participant was performing and would make the results difficult to compare. For that reason, we will consider the experiments within this manuscript to be the final version without allowing for a process for making further changes. In practice, it will still be possible to make further changes by consensus among participants, as long as these deviations from the standard protocol are documented along with each participant's submission.

The fact that the supplementary material is supplied in three ways (as a zip file through GMD, a DOI for the ISOMIP+ topography data sets, and links to example results on the MISOMIP website) is unfortunate but necessary. We wished to supply the small scripts and example MISIMP+ data in the simplest and most easily accessible form as a supplement to the manuscript itself. This was not possible for the ISOMIP+ topography data sets because they are too large. We did not feel that it was appropriate to give a DOI to the example results for ISOMIP+ and MISOMIP1. For one thing, the simulations had to be re-run based on recent changes made to the experiments, meaning the results were not available early enough for a DOI to be possible. In addition, that POP2x and POPSICLES results will be included in the ISOMIP+ and MISOMIP1 analyses, respectively, and that the results will be archived with a DOI in that process. For the time being, inclusion on the MISOMIP website seems more appropriate. A description of each code and data set is now included in a new section called Code and Data Availability (Sect. 5).

> Other remarks
> page 9865, line 13: performed simulations used offline . . . → performed offline coupled simulations . . . (?)

Changed as requested.

> page 9869, first equations and all over in the paper. Some of the notations are not homogeneous through all the paper. For example, the bed (which is also the bathymetry) is written B here, z_b after Eq. (8) and (13). Then when a quantity is evaluated at the bed (in fact the bottom ice surface) it is noted b ($\tau_{nt}|_b$ in Eq. (6)). I would suggest to adopt the same notations as in the previous ISMIP and ISMIP3d experiments for the geometry: b for the bedrock (and bathymetry), z_s for the ice upper surface and z_b for the ice bottom surface, this latter being equal to b when the ice is grounded and describes the ice-ocean interface (ice draft for the ocean model) when ice is floating. The same apply for the coordinates which are sometime written using lowercase (ice part) and sometime uppercase (ocean part). Legend of figures will have to be updated accordingly.

Thank you for pointing out these discrepancies. As I'm sure you can appreciate, different communities have different conventions, and these were mixed more than they should have been in the previous draft.

The suggested notation (z_s, z_b and b) is not very intuitive from the ocean's perspective, where z_b would be the surface. We have opted for a somewhat more neutral notation throughout: z_s is the ice surface, z_d is the ice draft (lower surface of the ice/upper surface of the ocean) and z_b is the bed topography/bathymetry.

> page 9869, line 20: the fact that either A and β^2 (or both) should be modified is repeated at different places, but I would suggest to write it as a preamble of how the steady state is obtained. This is an important point and the strategy of doing it this way should be explained. Also, it should be stated more clearly if A and β^2 should only be one single scalar for the whole domain (or the whole bedrock) or if participants are free to have space evolving A and/or β^2 .

We have removed the redundancy. We feel that the results will not be strongly sensitive to the precise method used to adjust A and β^2, so we leave it up to participants to choose an optimization strategy that is appropriate for their model:

> "We prescribe that the steady-state grounding line should cross the centerline of the trough at $x = 450 \pm 10$ km, ensuring that all models start from similar initial states. Participants should adjust the grounding line position by modifying first the values of A and, if necessary, the value of β^2

beginning with the suggested values given in Table 1. We have adopted this approach for model initialization to be more consistent with the methods used to initialize models for real-world problems: unknown parameters or fields are determined by search or inversion techniques so that initial conditions are consistent with observations. The precise method used to adjust A and/or β^2 and for finding the steady state is left up to the participant. Some participants will spin up their models for tens of thousands of years with different parameter values until the grounding line lies within the desired position. Others might construct a more formal optimization problem and solve it with variational methods."

page 9870, line 1: The tangential component . . . → Where the ice is grounded, the tangential component . . .

We have made this change.

Eq. (7): I have two points here: the first is on how the Tsai friction law, and its dependency to water pressure, is written. The second is on the use of Tsai and others (2015) friction law instead of the C 1 Coulomb-type friction law proposed by Schoof (2005) and Gagliardini and others (2007).
First, I would suggest to really make the distinction between the friction law itself and the way the effective pressure entering the friction law is estimated. I would suggest to write the Tsai and others (2015) friction law as:
[see pdf] (1)
and then explain how the effective pressure is estimated: $N = -\sigma_{nn} - \rho_{sw}\, g\, z_b$ (assuming that sea level is 0). It should be stated clearly (more clearly than in the Tsai and others (2015) paper at least) that the water pressure is assumed over all the bed to be given by the ocean hydrostatic pressure, which can be seen as a zero order hydrology model assuming a perfect connection of all the bedrock interface to the ocean (which is certainly a good approximation in the close vicinity of the GL but might give too large water pressure far inland if bedrock elevation decrease again). Then you might want to explain that N can be expressed using the floatation thickness as $N = \rho_i\, g\, (h - h_f)$.

As you requested, we have separated out the effective pressure explicitly, making clear the assumption that N is determined with the assumption of connectivity to the ocean throughout the domain:

"We note that Eq. (8) is a zeroth-order hydrology model that assumes connectivity to the ocean throughout the domain and is likely only valid within a few tens of kilometers of the grounding line (Leguy et al. 2014). It is likely that simulations using more realistic topography would would require a more sophisticated hydrology model to produce results consistent with observations inland of the grounding line."

Second, I would suggest that the participants can choose between Tsai and others (2015) friction law and the most commonly used so far Coulomb-type friction proposed by Schoof (2005) and Gagliardini and others (2007):
[see pdf] (2)
which depends on the same number of parameter than the Tsai and others (2015) friction law. Moreover, if C = f and $A_s = (\beta^2)^{-m}$, both law are very similar, but the latter is C1 and always bijective, whereas the former might conduct to numerical difficulties when the plateau is reached. I would suggest that at least the participants have the choice between both effective pressure dependent friction laws.

We agree that there is no reason not to also allow the Schoof (2005) basal traction parameterization (a limiting case of that of Gagliardini et al. 2007). We have added the option to use this parameterization of the basal traction in the current draft. We would point out, however, that the Schoof (2005) basal friction is *not* C1 continuous at the grounding line, though it is C1 continuous within the ice sheet. It is also not clear in our experience that a C1 continuous basal traction has particular numerical advantages, whereas there are several studies (e.g. Gladstone et al. 2010, Leguy et al. 2014) point out the numerical advantages of C0 continuity in the basal traction at the grounding line.

> Eqs. (6) and (7): u should be u_t (only the tangential part to the bed of the velocity vector) and one should define the norm of the tangential velocity (noted u b above) instead of |u|.
> page 9870, line 18: computing basal melt by balancing . . . → computing basal melt bellow the ice-shelf by balancing . . .

We have adopted your suggested notation and made the suggested change (though we find it unlikely that modelers will expect basal melting under grounded ice in these experiments).

> page 9871, line 2: u_* is the friction velocity . . . → u_* is the ocean water friction velocity . . .

Changed as suggested.

> the elevation of the bedrock is already defined before. thickness, and where $u_{*,0}$ . . . → thickness, and $u_{*,0}$ . . .

Changed as suggested.

> page 9872, line 14: in units definition, "yr" should be written "a" all over the manuscript.

We think this is a matter of taste and style, as the year is not an SI unit, but we have made this change.

> page 9875, line 23: in a. pdf file . . . → in a pdf file . . .

Changed as suggested.

> page 9876, points 2 and 3: if A and/or β^2 are not uniform, how should they be given?

The experiments prescribe both A and β^2 as uniform constants. Some forms of the basal traction involve additional constants but they still involve β^2 as a constant. If participants did choose, for some reason, to use non-constant A and/or β^2, we think it is clear that these should be described here as well. It would be up to the participant to decide how these are given.

> page 9877, line 4: of ice, not water equivalent) . . . → of ice (not water equivalent) . . .

Changed as suggested.

> page 9877, line 9: what is expected here is the basal traction at the bottom interface, so using your notation it should be $\tau_{nt}|_{zd}$, but adopting what I have suggested it should write $\tau_{nt}|_{zb}$ (this should be corrected at many other places in the manuscript).

Changed to consistently use $\tau_{nt}|_{zd}$ throughout the manuscript.

> page 9879, line 15: Ocean2 has a fixed geometry?

Hopefully the following change clarifies this point: "...to accommodate the retreated ice-shelf topography used in Ocean2, which is also the most retreated state in Ocean3 and Ocean4."

> page 9879, line 27: the web site address is given at different places in the manuscript. I would suggest to give it once at a judicious place where you should also explain which document is the reference document for the experiences and what can be found in the MISOMIP website (see my main comment).

In this particular point in the text, we now cite a DOI for the topography data used in ISOMIP+. In place of other references to the MISOMIP website, we now include a single reference in the new "Code and Data Availability" section, and refer to that section instead of the website in other parts of the manuscript. We make clear in this new section that this manuscript, not the website, is the definitive description of the experiments.

> page 9880, line 12: why it is important should be stated more clearly? Also, are the ocean models supposed to account for the iceberg melting. I guess not, but it should be stated that when calved, iceberg are simply removed from the system and don't induce any fresh water flux to the ocean model.

We have added further clarification of why this is important:

> "...include the effects of a cliff-like calving front so that participating ocean models will be required to demonstrate their ability to handle advance and retreat of this jump in topography. We feel that this

is important because ocean models will require this capability to handle real-world problems with dynamic calving fronts."

We added the following text to address the second point:

"Calved ice is simply removed from the domain, and contributes no freshwater flux to the ocean. We feel this is justified partly because it keeps the problem as simple as possible and partly because an Antarctic iceberg would be transported out of the ISOMIP+ domain in a matter of months, meaning most meltwater would be deposited elsewhere in a real-world problem."

page 9881, Eqs. (15) and (16): would suggest to replace = . . . by ≈. Coordinates are now in uppercase. Should keep this homogeneous through the manuscript.

We have removed the ellipsis (...) and made clear that this is the time-rate of change of T and S due only to restoring. We're not sure what you mean by coordinates being uppercase. T and S are always written with uppercase letters, and x and z are lowercase here.

page 9881, line 14: T_{bot} should be T_b (using my notation suggestion), but then in Eqs. (21)-(24), it should be T_{zb} , S_{zb} and p_{zb} . . .

We agree that a consistent notation is helpful. However, T_{bot} is the temperature at $z=z_{b,deep}$, not a $z=z_b$, so we stick with the notation T_{bot}. It was never our intent that subscripts would necessarily indicate the z depth of a given quantity.

3.2 to 3.5: would suppress "experiment i": ISOMIP+ experiment 1 (Ocean1): . . . → ISOMIP+ Ocean1: . . . and only refer to this experiment by Ocean1 (not experiment 1 of ISOMIP+). Same for 4.1 and 4.2 for the IceOcean experiments.

Changed as suggested.

In part 3.2, both Ocean1 and Ocean2 are in fact described.

We have created a new section 3.2 called "Experiments," that includes Ocean0-4 as subsections. Text that applies to multiple experiments is now in the introduction to this section.

page 9893: for ISMIP+, a pdf file is asked. Should be the same? Also, I suggest to have also the required inputs in this file to be in a numbered list, as for ISMIP+.

Presumably this refers to MISMIP+? As you have realized later, we do describe a readme file that will include all these details. At your suggestion, we have made this readme a pdf file to allow for better formatting. We have add examples for the POP2x and POPSICLES results to the supplementary material.

page 9894, line 18: from Parallel Ocean Program version 2 extended (POP2x) . . . → from POP2x . . .

Changed as suggested.

page 9897, lines 18-20: I would suggest to remove these two sentences, which are a bit contradictory?

We can see how these sentences were confusing but we don't see them as contradictory. However, they have been removed in the updated manuscript. While we feel that it will be important to compare results with various stress approximations, MISMIP+ rather than MISOMIP1 is the appropriate place to explore this. We no longer suggest a particular stress approximation in MISOMIP1.

page 9898, line 12: website address already given.

Changed as suggested.

Table 1: ρ_{fw} not given (but may be too obvious to be ambiguous?)

ρ_{fw} constant has been added to Table 4. It is incorporated into Ω but is not needed anywhere else in MISMIP+, so is not needed in Table 1.

Table 2, caption: A list . . . → List . . .

Changed as suggested.

Figure 1: change notations if bedrock equations are modified

We maintain the functions B(x) and B(y) in constructing z_b(x,y), so there is no need to change the notation in the figure or caption.

Figure 2: axes label are a bit small

The figure has been replotted with larger labels.

Figure 3: the color bar should be the same height than the plot itself.

Changed as suggested.

Figure 8: replace "expt i" by "Oceani" in the legend.

Changed as suggested.

Figure 13a: the colorbar legend is too small and I would suggest to avoid a colored background.

We have replaced this figure with one showing several cross-sections of T (four per experiment), which also show the evolution of the ice-shelf geometry, as suggested by reviewer 2.

Authors' responses to comments from Reviewer 2:

> **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.

Thank you for reviewing our manuscript, and for your detailed response. We are glad that you find these MIPs to be worthwhile and, for the most part, well designed. We have done our best to address your remaining concerns, as detailed in what follows.

> **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.

We have described the possible approaches for initializing models and tuning parameters (A and/or β^2) in more detail:

> "Some participants will spin up their model for tens of thousands of years with different parameter values until the grounding line lies within the desired position. Others might construct a more formal optimization problem and solve it with variational methods"

> 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,...

We found this remark somewhat alarming. We have attempted to emphasize throughout the text that the results presented here are *not* to be treated as benchmarks. We do not wish in any way to include results that will be treated in this way, nor do we claim to have models that would be appropriate as benchmarks. We have added the following text to Sec. 2.2 to try to further emphasize this point:

"We emphasize that the example results shown in this figure are *not* intended as a benchmark for other simulations, but simply to demonstrate generally what type of behavior might be expected in each experiment."

In Sec 3.2, we added:

"We emphasize that we do *not* intend these results to be treated as a benchmark for other participants to try to match. Instead, the examples show that the simulations can be performed and that they achieve their intended purposes. They should give the participants a qualitative idea of what to expect. After all, the MIP is not to attempt to produce identical results with all models but rather to try to understand the differences that occur."

...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.

We fully intend to compare a variety of models, including Stokes models in the paper describing the MISMIP+ results. This manuscript is not the place for performing such a comparison, and the danger of the example results being treated as a benchmark would only be increased by including results from a variety of models here. We feel strongly that this paper is not the place for intercomparing results except where it motivates the choice of parameterizations and parameters (e.g. basal traction and viscosity).

Connected to spinnup 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.

These comments would seem to reveal at least two potential misunderstanding of the intent of these experiments, particularly the coupled experiments (MISOMIP1). We seem to have struck a nerve by inadvertently implying a preference for SSA models over other stress approximations. Our purpose in requesting results with a particular stress approximation was merely to allow us to control for stress approximation while allowing the other modeling choices to vary among models. Our feeling was that this would be more likely to allow us to understand the relative importance of differences in stress approximation compared to these other variations. Because SSA seems to be the stress approximation appropriate this marine ice sheet/shelf that was available in the largest number of models, we wished to solicit results from as many models as possible with the SSA *in addition to* other stress approximations. However, we decided that it was adequate to analyze results with various stress approximations in MISMIP+, and that there was no need to specifically request results with any particular stress approximation in MISOMIP1. Therefore, no specific stress approximation is requested under MISOMIP1 in the current version of the manuscript.

By "realistic simulations" we refer to simulations with realistic forcing and topography. We feel that this term is well understood in the community, and does not refer to any modeling approach (e.g. SSA vs. Stokes vs. Blatter-Pattyn).

We have attempted to remove inadvertent biases toward vertically integrated models elsewhere in the manuscript. As requested by Reviewer 1, the effective pressure within the sliding laws is now expressed in terms of σ_{nn} instead of the lithostatic pressure (ρ_i g h). We now state explicitly in Sec. 2.1, “As in the previous MISMIP experiments, MISMIP+ uses a symmetry boundary condition at the ice divide, ocean pressure (up to sea level) at the ice-ocean interface, and stress-free boundary conditions at the upper surface (see Pattyn et al. 2012, 2013 for details).” These boundary conditions should be appropriate for all stress approximations.

> • 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.

Reviewer 1 also felt that the Schoof/Gagliardini basal friction laws should be allowed. We have added the option to use this parameterization of the basal traction in the current version of the manuscript.

> 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.

We have attempted to allay this concern by stating a preference for adjusting A first and β^2 only if necessary in Sec. 2.1:

> “Participants should adjust the grounding line position by modifying first the values of A and, if necessary, the value of β^2 beginning with the suggested values given in Table 1. The precise method used to adjust A and/or β^2 is left up to the participant.”

It is likely true that one could arrive at the same grounding-line position with different choices of A and β^2 within the same model. However, the same grounding-line position implies (nearly) the same catchment and therefore, in steady state, the same volume flux. Even so, the point is well taken that the different choices of A and β^2 could affect the resulting dynamics. This is likely inevitable, and consistent with the variation and uncertainty that we introduce when we initialize models with realistic topography and forcing.

> 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?

We have added a new figure (Fig. 5) that indicates the sensitivity of the grounded area in the Ice1r experiment to the three friction laws that we allow. We include results with both SSA and SSA* approximations (which were shown to bound Stokes results in MISMIP3D). The figure shows little difference between these two stress approximations and little difference between Tsai and Schoof/Gagliardini friction laws. We do see significant sensitivity to the use of the Weertman sliding law, even if the value of A is tuned to match the steady state grounding-line position. We have added the following text describing the sensitivity results:

> “Figure 5 shows the sensitivity of the BISICLES Ice1r results to various choices of basal traction, stress approximation, and values of A. Results are nearly insensitive to the differences between the basal-traction parameterizations of Tsai et al. (2015) and Schoof (2005), and also to differences between two stress approximations, SSA and SSA* (Schoof and Hindmarsh, 2010). However, the simulations with the basal traction of Weertman (1974) show a significant difference in both the initial grounded area and the rate of retreat compared with the other parameterizations.

Furthermore, even when A is adjusted so that the initial grounding-line position (and therefore the grounded area) is in agreement with the other configurations, the rate of retreat remains significantly slower than for the other parameterizations."

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).

Regarding the surface mass balance, we now state, "A constant accumulation rate *a*, with the value given in Table 1, is applied over the entire ice surface." As mentioned earlier, we added in Sec. 2.1, "As in the previous MISMIP experiments, MISMIP+ uses a symmetry boundary condition at the ice divide, ocean pressure (up to sea level) at the ice-ocean interface, and stress-free boundary conditions at the upper surface (see Pattyn et al. 2012, 2013 for details)." This should make clear that the Stokes boundary conditions are to apply ocean pressure up to sea level, as opposed to an constant cryostatic pressure.

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).

We feel that it is up to the participant to determine where the grounding line is located in their model. Variations based on different methods for locating the grounding line are expected to be negligible compared to differences in the grounding-line location between models. We added the following text to section 2.3:

"It will be left to each participant to decide how to determine location of the grounding-line points (e.g. taking cell edges between grounded and floating regions or performing sub-grid-scale interpolation)."

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.

We have taken your suggestion for the IceOcean1 (and IceOcean2) results, and have displayed temperature snapshots at various points in time. We have adopted this approach for the ISOMIP+ experiments as well. These new figures have larger axes, so they should be more readable.

Good luck with this inter-comparison!

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.

We have added a footnote with the location, dates and URL for the workshop. We have changed “we decided...” to “the participants decided...”, as the coauthors of the paper and the workshop’s participants are distinct groups.

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

We now introduce three other previous MIPs:

“A number of previous MIPs not specifically focused on marine ice sheets have explored model physics (EISMINT: Payne et al. 2000), provided benchmarks (ISMIP-HOM: Pattyn et al. 2008) and demonstrated modes of internal variability (ISMIP-HEINO: Calov et al. 2010), improving our understanding of ice-sheet models.”

* page 9863, line 20:"topoography →topography"

This has been fixed.

* 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.

We have changed “and did not…” to “meaning it did not..”

* 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 $x = 450 \pm 10$km."
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.

We now give a more detailed description of the thinking behind adjusting an unknown parameter value (in this case A) to match an "observed" grounding-line position:

"We prescribe that the steady-state grounding line should cross the centerline of the trough at x = 450 ± 10 km, ensuring that all models start from similar initial states. Participants should adjust the grounding line position by modifying first the values of A and, if necessary, the value of β^2 beginning with the suggested values given in Table 1. We have adopted this approach for model initialization to be more consistent with the methods used to initialize models for real-world problems: unknown parameters or fields are determined by search or inversion techniques so that initial conditions are consistent with observations. The precise method used to adjust A and/or β^2 finding the steady state is left up to the participant. Some participants will spin up their models for tens of thousands of years with different parameter values until the grounding line lies within the desired position. Others might construct a more formal optimization problem and solve it with variational methods"

Also, as previously noted, we have added a new Fig. 5 that explores the sensitivity of the results to three different choices of the basal-friction law, two different values of A and two different stress approximations (SSA and SSA*, which have been shown to produce significantly different steady-state grounding-line positions in MISMIP3d). This figure suggests that the choice of basal friction law is likely to affect the dynamics more than the the choice of A (which moves the initial grounding line position, but appears to have relatively little impact on the amount of retreat) or the stress approximation.

We feel that a more detailed demonstration of the sensitivity (or lack thereof) to various parameters and in various models lies more in the scope of the analysis of MISMIP+ than in its description.

* 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.

As noted in our response to Reviewer 1, we now allow the Schoof/Gagliardini friction law. Figure 5 now shows that the example results are nearly indistinguishable from those with the Tsai law.

We have found that there are strong feelings in the community about which friction law was the "right" one to use, and the decision made at a splinter meeting before the IGS symposium in Cambridge last year was that multiple friction models should be allowed. Initially, we felt that the results would be simpler to analyze with just two choices, but we have found in our own tests (again see Fig. 5) that there is little difference between Tsai and Schoof/Gagliardini, so there seems little down side to allowing both.

Following Reviewer 1's suggestion, we have changed (7) to use the effective pressure N. We no longer assume the ice-sheet model uses the lithostatic approximation.

* 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.

A panel has been added to Fig. 3 with a sketch of the geometry, as you suggest.

* 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).

We have corrected this problem by changing the notation in what is now (15) (previously (12)) to also use $\max(z_0 - z_d, 0)$.

* page 9872, line 14: "The coefficient Ω has been given a value of 0.2 yr^{-1} , corresponding to a maximum ambient ocean temperature ~1.0◦C, which leads to a melt rate with a maximum value of $m_i \approx 75$ m yr^{-1} near the grounding line (see Fig. 2)."
First of all, is m_i ice or water equivalent? Secondly, I guess the 75 m yr^{-1} correspond to the steady state spun-up configuration of BISICLES in your setup. If so, then please explicitly mention this.

This has been fixed as suggested. We now show both m_i in m/a of ice and m_w in water equivalent in (12) ((9) in the previous draft) to highlight the distinction.

* 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.

Again, we feel that the way that participants choose to locate points on the grounding line is up to them and cannot usefully be prescribed.

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

This has been fixed.

* page 9876, line 1:"2. Englacial stresses: the stress model and coefficients (e.g. SSA, $A = 2.0\times10^{-17}$ Pa^{-3} yr^{-1})."
I would rather call it stress approximation model.

We have changed “stress model” to “stress approximation”.

* 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.

Conventional certainly was not intended to mean SSA. “Conventional models should” has been changed to “Typically, models should…” This is the typical way of demonstrating sufficient resolution, though we are open to other methods.

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

This has been fixed.

* 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.

We have added references to Gill (1982) and Pond and Pickard (1983), which are good general oceanography references. We specifically reference a chapter in each about the the f-plane (which assumes that the Coriolis parameter f does not vary with space).

* page 9880, line 17: "Ice thinner than H_{calve} = 100 m (equivalent to an ice draft above ~ -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.

We have added the following justification for this choice:

"This threshold was chosen to eliminate the thinnest ice on eastern and western flanks of the ice tongue while maintaining the tongue itself. A thicker threshold, more consistent with typical Antarctic ice shelves, would eliminate large portions of the ice shelf during retreat and making analysis of the evolving melt-rate field more challenging."

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

This has been fixed.

* 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.

We went through several iterations of naming conventions before settling on these names for the experiments. The numbering and punctuation already included in the MIP names made it impractical to use them as prefixes for the experiment names (e.g. MISMIP+1ra vs. MISOMIP1.1 was confusing). We have changed "Ice1r" here to "MISMIP+ Ice1r" and have done the same for the first mention of an experiment from another MIP in the other MIPs' sections. We feel it is too cumbersome to prefix the experiment name with the corresponding MIP each time.

* 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?

This has been added to the ice-sheet model description as well.

* 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.

True. This has been changed to:

"The simulation evolves for another 100 years, during the first decade of which the ocean should cool and the melt rate should be greatly reduced, similarly to Ocean2. The reduced melting should and allow ice to re-advance for the remainder of the simulation."

* 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 calve = 100 m (equivalent to an ice draft above ~ -90 m) or beyond x calve = 640 km should be calved → 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.

In this case, the threshold was chosen simply for consistency with ISOMIP+, as we now state.

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.

The ice draft (and ice surface elevation and floating mask) is already zero beyond x = 640 km in the topography data set supplied to the participants, so we do not feel it is necessary to mention this explicitly under ISOMIP+. We now mention the dynamic calving criterion and the fixed-front calving condition separately, so that it is clear that only the dynamic condition is adopted directly from ISOMIP+.

Minor detail: I would replace "calved" with "removed", as the first one suggests that this is according to a physical calving criterion.

This has been changed as you suggest.

* 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 coupling. 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 infor-

mation from variation of coupling intervals.

We appreciate your interest in seeing more results and analysis from the POPSICLES results. However, we feel strongly that this is not the right context for deeper analysis, including parameter studies. Other papers are in the works that will explore POPSICLES specifically, and the MISOMIP1 analysis paper will include POPSICLES results as well (possibly including several coupling intervals). We feel that the more emphasis that is placed on the POPSICLES results here, the more likely they will be to be treated as a benchmark for other results, which we adamantly wish to avoid.

To avoid confusion, we have removed the two uses of “conventional” from the paper (one referring to methods for determining convergence with resolution, one referring to basal-traction parameterizations) and use the word “typical” only to describe model configurations (as distinguished from the “common” configuration that prescribes more parameters and parameterizations). We do not refer to “typical” models. We do not feel that there are enough coupled ice sheet-ocean models in existence yet to have “typical” models, and do not wish to claim that POPSICLES is one.

> In my personal opinion – if
> the results seem to be just around the corner – it would have been bet-
> ter to wait to have also IceOcean2 achieved before pushing the paper to
> publication, to have a complete set of simulations.

We now include IceOcean2 results in the revised manuscript. We think they are actually quite a bit more interesting than IceOcean1 because even a simple calving criterion such as we have here can produce rather dynamic results and appears to have a more pronounced effect on buttressing. To accommodate these extra results as well as those from the new Ocean0 experiment, we have had to remove some other figures and do not have room to add any more, but we provide extensive output including animations through the MISOMIP website.

> 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).

These are both correct hyphenations (strange as they may seem) according to a website dedicated to this topic:
https://www.hyphenation24.com/word/recommend/
https://www.hyphenation24.com/word/coupling/
In any case, the hyphenation rules that Copernicus chooses to use are really up to them and not something we are interested attempting to change.

> * 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.

We have added a paragraph each of analysis of the example results from IceOcean1 and IceOcean2. We feel that more is not appropriate for this manuscript. The text quoted below may not be terribly useful without the associated figures to refer to.

> “The blue curves in Fig. 12 shows the mean melt rate and the grounded area and from an IceOcean1 simulation using the POPSICLES model (coupled POP2x and BISICLES). The top row of Fig. 13 shows the evolution of the ice draft and ocean temperature over the course of the simulation. The mean melt rate is initially relatively small, increasing by several orders of magnitude over the first decade as warm water reaches the cavity and initiating grounding-line retreat. Because of the ocean temperature profile, the melt rate is a strong function of the depth of the ice-ocean

interface. As the ice shelf thins, melting becomes concentrated over a steep region within the channel near the grounding line. As the grounding line retreats, the area of the cavity increases (no calving occurs except beyond x=640 km) while the total melt flux remains nearly constant, meaning that the mean melt rate gradually decreases. Between year 100 and about year 130, the melt rate decays by several orders of magnitude, reaching a nearly steady value for the remainder of the simulation as the ice shelf thickens and grounding line begin to re-advance."

Later on, describing the IceOcean2 results:

"Mean melt rates and grounded area from an example POPSICLES IceOcean2 simulation are shown in the green curves in Fig. 12, and the evolution of the ice draft and ocean temperature are shown in the bottom row of Fig. 13. The beginning of the retreat phase of IceOcean2 proceeds similarly to IceOcean1, with small differences resulting from the smaller, thinner ice shelf that results from the calving criterion. Starting at around year 30, dynamic calving removes significant portions of the ice shelf. Although the melt flux remains relatively steady, the mean melt rate increases as the ice-shelf area decreases. Just after year 60, a large iceberg breaks off from the ice shelf, leading to an abrupt increase in the mean melt rate. For the remainder of the retreat phase, the ice shelf exists only as a small remnant of its initial size close to the grounding line. The re-advance phase begins at year 100 when the far-field restoring is switched to the COLD profiles. As the ocean cools, the melt rate decreases by several orders of magnitude. The ice-shelf area remains much smaller than in IceOcean1ra while melt fluxes are similar, meaning that the mean melt rate is nearly an order of magnitude higher."

* 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. certainly available for a longer period – or would it be better to choose a certainly permanent URL to provide this data?

While we understand the concern about the persistence of the description and example results from the original ISOMIP experiment, it is not necessarily within the purview of this work of the MISOMIP target activity to take over the archival of ISOMIP.

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. "

These typos have been fixed.

(b) shows a cross section that to me seems only to be valid over the region of the trough (where threshold of −B max does not apply); here one could get the impression that it applies to the whole region.

$B_y(y)$ is not a cross section of the topography. Like $B_x(x)$, shown in blue in (a), it is a function that is used to define the bedrock topography. Over most of the domain, cross sections of the bedrock topography do look like (b) with a vertical offset. We are uncertain how to better clarify this. We assume that participants will graph the topography for themselves and become acquainted with it.

* 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.

The figure has been simplified and the size of the annotations has been increased. We felt that an additional figure with velocities, while potentially interesting, would be too much.

* 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.

We appreciate the comment. We tried various versions of these figures both with and without the box, and it was unclear both where sea level was and exactly how the topography related to flat surfaces (the top and bottom edges of the box) without having the box itself. What may seem look like unnecessary clutter actually appeared to provided useful context. However, to make room for other figures that have been added, we have decided to remove these figures entirely from the paper.

* 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.

These figures have been replaced by time series of temperature only. Animations of the time evolution of all the fields in the standard output are available on the MISOMIP website but fields other than temperature are not included in the manuscript. We have endeavored to make the fonts bigger in the figure. In the output that we provide on the MISOMIP website, we now mask the overturning and barotropic streamfunctions based on where there is ocean and where not. Masking streamfunctions is a slightly non-trivial operation because their computation involves both an integral over the domain and in our case two different transformations to staggered grids, so typically we don't bother. But it is true that the data is easier to interpret with the appropriate masking.

* 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.

We have replaced the figure as you suggested -- showing four cross sections of temperature at different times for each of the ISOMIP+ and MISOMIP1 experiments. The new figure should be a more acceptable size. The previous figure made for a better movie than a still image.

Authors' responses to comments from Dan Goldberg:

> I think this intercomparison is a great idea and i applaud the authors' efforts. I agree with the philosophy of the experiments and think it is the right way forward. I have no high-level objections with any part.

Dan, we are very happy to have your support on these MIPs.

> I would only like to make a few comments about Section 3, the ocean experiments.

> Section 3.1.1: If an ocean model is using a lat-long grid (rectangular in the lat-lon space), it might be tricky to conform to the cartesian dimensions you state.

Xylar says: I do not think this should be very difficult. I have done this with POP2x in the past. I don't want to detail this in the paper because I feel like it isn't really the point, but the following python snippet should produce lon/lat points that have the appropriate spacing and the correct latitude at the center:

```
import numpy
import matplotlib.pyplot as plt

rEarth = 6.371e3

(X,Y) = numpy.meshgrid(numpy.linspace(320., 800., 241),
                       numpy.linspace(0., 80., 41))

x0 = 0.
y0 = 40.

lat0 = -75.*numpy.pi/180.
lon0 = 0.

# Y - y0 = rEarth*(Lat - lat0)
Lat = (Y - y0)/rEarth + lat0

# X - x0 = -rEarth*sin(Lat)*(Lon-lon0)
Lon = -(X - x0)/(rEarth*numpy.sin(Lat)) + lon0
```

The grid cells you get this way aren't equally spaced in longitude, as you might be used to from ISOMIP but the ocean model should be perfectly happy with this.

> Section 3.1.2: I think some mention could be given to what is "allowed" for any model which attempts synchronous coupling, i.e. evolution of ice thickness at the ocean time step). An instantaneous removal of 100 m of ice over a grid cell may wreak havoc in the ocean. MITgcm, for instance, will need to interpolate in time from an ice-covered to ice-free state. I don't know if GFDL is participating but from offline conversation with Alon Stern it sounds as if their ocean model needs to do something similar to move around large icebergs.

Yep, that's a reasonable concern. There are bound to be things like this that we haven't thought of and that may be more specific to a given model. In general, I would think participants will need to figure out how to best handle these situations, as they presumably have done for the previous ISOMIP and any other test cases they may have run (e.g. the experiments from your 2012 papers). Here is what we have added to address this particular issue:

> "Models that do not support a sheer calving face or which update the ice topography at each time step will likely need to smooth the calving face over several horizontal grid cells and/or to relax to the new geometry gradually over time. In such cases, it is suggested that participants interpolate the

geometry in time, then apply the calving criterion, and finally apply whatever smoothing or relaxation is required. This way, the (smoothed) calving front is expected to move relatively continuously in the horizontal, rather than abruptly jumping to the new location each year as the ice between the old and new calving fronts thins to zero."

Also, MITgcm (without a rigid lid) and possibly other models specify surface pressure to control the elevation of the ice-ocean interface – would these models be allowed to specify ice mass per unit area instead (with a standard ice density)?

Good call. We added the following:

"Some participating ocean models require a surface pressure rather than the ice draft as the upper boundary condition. These models are free to compute the ice thickness from the ice surface elevation and ice draft provided in the input geometry, and multiply these by ρ_i g to get a pressure. Equivalently, the pressure can be derived from the ice draft as $p_{zd} = -\rho_{sw} g z_d$. The elevation of the ice-ocean interface in the model will differ slightly from the prescribed z_d because of the dynamic pressure and difference between the reference density of seawater and the local ocean density, but these differences are expected to contribute negligibly to the differences between model results."

Section 3.1.4: any mechanical bdry conditions on ice shelf front or other vertical parts of the ice shelf?

We added the following text that we hope will address this comment:

"Also we prescribe no melting or drag from vertical ice faces (e.g. the calving front) both for simplicity and because many models do not support melting on vertical faces."

Immediately after this, we state (as before), "Participants that use other boundary conditions should note this when they submit their results", which we hope makes clear that participants could use models with melting at vertical faces, as long as this is documented. Given that the area of the sloped ice draft is likely to be many orders of magnitude higher than the area of vertical faces, it may be that melting on vertical faces is negligible for this particular set of experiments. However, it would be interesting to show this with a model capable of doing so. POP2x, which we used for the example simulations, does not currently support melting at vertical faces.

Section 3.1.5: i think "digging" needs some explanation

We have removed the reference to "digging". The text now states:

"Models with other vertical coordinates may be less restricted, but some modification of the topography may be required to maintain a minimum ocean-column thickness. In locations where the ocean column is too thin, participants will need to decide for themselves whether it is more practical to modify the topography (ice draft, bathymetry or both) or to remove the column from the ocean (i.e. mark it as "land")."

Eq (3.1) – i really like this idea

Xylar says: Ironically, I tried this and it didn't quite work out, so I've updated the suggested procedure based on what worked for me in MPAS-Ocean (see response to Nicolas below). The essence of the original method is still there.

Section 3.1.8: i found the boundary layer a bit confusing, and there are quite a lot of cans of worms hidden here. for one thing, you refer to a boundary layer, which at first i thought meant the viscous sublayer, but later on i thought meant the layer over which the eddy size scales with distance from the wall (i don't know what this is called, but p9885 line 11) but later surmised you were referring to the mixed layer (eg p9885 l19), which is a bit larger and involves buoyancy and rotational effects.

The text that you found confusing has been significantly modified or removed. We how use the new Ocean0 experiment (see Yoshi's comment below) to calibrate the GammaT and GammaS coefficients, rather than relying on vague theoretical notions about how they should vary. We think the new approach will be less confusing and more consistent with how uncertain model coefficients are typically computed by inversion or "tuned" based on observations:

> "Because of differences in vertical resolution, vertical mixing and the method for computing u_w,T_w and S_w, appropriate values of the heat- and salt-transfer coefficients, GammaT and GammaS, are likely to vary significantly between models. In Sect. 3.2.1, we prescribe a procedure for tuning these coefficients to achieve a desired mean melt rate. With the exception of GammaT and GammaS, we prescribe values for the coefficients in these equations in Table 4."

> In this section there is mention of interpolating values (u,v,T,S) to 20m away from the interface. Should doing this not follow some prespecified theoretical boundary layer profile, as opposed to linear interpolation? My understanding is that this would be logarithmic in velocity, at least close to the wall (< 2m?); but I am not familiar enough with the theory to know what the "outer" parts of the profile are meant to be, nor do I know much about the theoretical T/S profiles.

This subsection is much less prescriptive in the current draft:

> "Methods for computing the 'far-field' potential temperature, salinity and velocity (T_w,S_w and u_w) differ across models. Some models sample these fields at a fixed distance below the ice draft (e.g. Kimura et al. 2013) while others average the fields over a prescribed thickness (e.g. Losch 2008). Participants are asked to describe how T_w,S_w and u_w are computed in the pdf included with their results."

> Furthermore i am of the opinion that for a synchronous approach with a dynamic grounding line (which ocean.3 and ocean.4 essentially are, from the view of the ocean) there should not be such a large minimum depth as 20m – but for column thicknesses below this range i would not imagine there are significant melt rates. How about rather than a minimum depth, it can be agreed that melting is shut off when column thickness is below this value?

Xylar says: The 20-m boundary layer is no longer prescribed, I think that will take care of your concern. I have found that it works fine to average over some fixed boundary-layer distance or the full ocean column, whichever is thinner. I don't think there is any need to explicitly shut off melting near the grounding line. This will tend to happen in some models because the flow is restricted and mixing is suppressed but there are no solid observational or theoretical reasons to require this. At this point, I think it's fine to let modelers do what they do.

Authors' responses to short comment from A. Kerkweg:

> In my role as Executive editor of GMD, I would like to bring to your attention our Editorial version 1.1:
> http://www.geosci-model-dev.net/8/3487/2015/gmd-8-3487-2015.html
> This highlights some requirements of papers published in GMD, which is also available on the GMD website in the 'Manuscript Types' section:
> http://www.geoscientific-model-development.net/submission/manuscript_types.html
>
> In particular, please note that for your paper, the following requirements have not been met in the Discussions paper:
> • "The main paper must give the model name and version number (or other unique identifier) in the title."
> • "All papers must include a section, at the end of the paper, entitled 'Code availability'. Here, either instructions for obtaining the code, or the reasons why the code is not available should be clearly stated. It is preferred for the code to be uploaded as a supplement or to be made available at a data repository with an associated DOI (digital object identifier) for the exact model version described in the paper. Alternatively, for established models, there may be an existing means of accessing the code through a particular system. In this case, there must exist a means of permanently accessing the precise model version described in the paper. In some cases, authors may prefer to put models on their own website, or to act as a point of contact for obtaining the code. Given the impermanence of websites and email addresses, this is not encouraged, and authors should consider improving the availability with a more permanent arrangement. After the paper is accepted the model archive should be updated to include a link to the GMD paper."
> With respect to MIPs the Editorial v1.1 explicitly states: "For model experiment description papers, similar version control criteria apply as to model description papers: the experiment protocol should be given a version number; boundary conditions should be given a version number and uploaded or made otherwise available; a data availability paragraph must be included in the manuscript; and links to the GMD paper should be included on the experiment website. Papers describing data sets designed for the support and evaluation of model simulations are within scope. These data sets may be syntheses of data which have been published elsewhere. The data sets must also be made available, and any code used to create the syntheses should also be made available. "
> Therefore I ask you to change the title in your revised submission to GMD naming the three MIP experiments explicitly including a version number.

We have revised the title to now include the names and version numbers of each of the 3 MIPs. The title is very cumbersome, which we had sought to avoid, but should meet GMD's requirements:
"Experimental design for three interrelated Marine Ice-Sheet and Ocean Model Intercomparison Projects: MISMIP v. 3 (MISMIP+), ISOMIP v. 2 (ISOMIP+) and MISOMIP v. 1 (MISOMIP1)"

> Additionally, please add a
> "Code and/or Data Availability" Section comprising access information to all data and code named in your article.

A section called "Code and Data Availability" has been added. There, we provide information for accessing the BISICLES code, the topography data required for the ISOMIP+ experiments and the example results.

We state that source code for POP2x and POPSICLES is not currently available to the public. Negotiating the public release of these codes is outside the scope of this work.

Authors' responses to short comment from Y. Nakayama:

> Authors summarize the experiments very well and descriptions of model experiments are easy to follow. I have one comment, which may make it easier for ocean modelers to compare their results without running experiments for 10-20 years.
>
> + I suggest adding 1-year ISOMIP experiments with warm initial condition and warm restoring. This way, at least in my setup, model melt rate converges within 4-6 months. I believe this makes it much easier for other ocean modelers to debug and test their code.

Yoshi, thank you for the suggestion. We have added a new Ocean0 experiment which we now use to specify a process for calibrating the coefficients Γ_T and Γ_S to achieve a desired melt rate. We agree that a shorter experiment that reaches quasi-equilibrium in a shorter time will be more practical for troubleshooting during model development (which is not to suggest that the example results should be used as a benchmark for model development!) and will also provide a fruitful starting point for parameter experiments in the future.

Authors' responses to short comment from N. Jourdain:

Thank you for your suggestions.

> I would just like to add a few minor comments that could improve the clarity of the manuscript:
>
> - Equation (20) : mention that T is potential temperature. Printer-friendly Version
> - It would be convenient to use the same value and notation for ρ_{ref} (Eq. 20) and ρ_{sw} (Eq. 21,23). Values are very close (1027.51 and 1028 kg m^{-3} respectively).

Hmm, we agree that this would have been convenient but at this point it seems like it isn't worth changing. It's convenient to have a relatively round number for ρ_{sw} since it's also used for flotation in MISMIP+. On the other hand, ρ_{ref} comes from a linearization of the equation of state (using TEOS10) with round values for T and S, which results in ρ_{ref} not being a very round number. We don't feel that the convenience (for participants) of having these two values be the same is worth the inconvenience (for us) of modifying and rerunning the experiments, given that the differences are likely not to be significant. We added the following text to further address the issue:

> "Any model that requires ρ_{ref} to be equal to ρ_{sw} should use ρ_{ref} for both values, and should note this difference along with their output."

> - Equation (26) and (29) : T_f should probably be T_b as in equation (21).

This has been changed.

> - About the artificial evaporative flux used to remove freshwater in models using volume fluxes : (i) this can be done on every time step, why using a 30-day characteristic time? (ii) it should be mentioned that this is evaporation with no associated latent heat (otherwise this will cool the surface).

The proposed scheme for maintaining sea level been modified significantly (see below). (We agree that freshwater could be removed at each time step to exactly counterbalance the melt flux, but this amounts to a global reduction at each time step that models might prefer to avoid. The original scheme was intended to remove excess mass based on only local quantities and without shocking the system too severely. None of this is particularly relevant, as the new scheme does not involve a characteristic time scale.) We now state:

> "Models using volume or mass fluxes will need a strategy for removing mass in the open ocean to compensate for the volume of melt water that enters the domain. Because of the small size of the

domain, without such a strategy, sea level would likely rise by hundreds of meters in simulations with large melt rates (Ocean1 and Ocean3). One possible approach is to impose an artificial evaporative flux in the restoring region (x > 790 km). Corresponding salt and heat fluxes will be needed to prevent the top cells from becoming cooler and saltier as mass leaves the cell:

$$\begin{aligned} F_e &= -\rho_{\mathrm{sw}} \langle m_w \rangle \frac{A_{\mathrm{shelf}}}{A_{\mathrm{res}}}, \\ F_{H,e} &= c_w T_0 F_e, \\ F_{S,e} &= S_0 F_e, \end{aligned}$$

where F_e, $F_{H,e}$ and $F_{S,e}$ are the evaporative mass, heat and salt flux, respectively, A_{res} is the area of the restoring region, T_0 and S_0 are the prescribed temperature and salinity at the ocean surface in the restoring profile, and $\langle m_w \rangle$ is the melt rate averaged over the area of the ice shelf A_{shelf} and over a suitable period of time (perhaps one month). Participants are welcome to use alternative strategies. They are asked to document whichever approach (if any) they use for removing excess mass in their description pdf."

- About u tidal (RMS velocity associated with tides): is it also used in the formulation of the bottom ocean drag?

No, it is only used in computing u* to be used in the melt formulation. We assume that it is largely irrelevant, at least to the types of problems that these MIPs explore, if there is additional drag due to tidal motion at very low velocities, since the velocity is small to begin with. As with any modeling choices, participants may choose to deviate from this prescription as long as they document this. We now state:

"The computation of top and bottom drag do not incorporate u_{tidal}."