Responses to Reviewer #1

Review of “Evaluation of an emergent feature of sub-shelf melt oscillations from an idealised coupled ice-sheet/ocean model using FISOC(v1.1)-ROMSIceShelf(v1.0)-Elmer/Ice(v9.0)” by Chen Zhao, Rupert Gladstone, Ben Galton-Fenzi, David Gwyther and Tore Hattermann.

Recommendation: minor revision

Sub-shelf melt oscillations emerge from coupled ocean–ice-sheet simulations of the Marine Ice Sheet Intercomparision Project (MISOMIP), and this paper investigates the causes of these oscillations. This is a useful study for the ocean–ice-sheet modelling community. The paper is well written and the sensitivity tests make sense and are clearly analysed. I only have minor comments and I suggest to accept the manuscript once they have been considered.

We thank the reviewer Dr. Nicolas Jourdain for the time and efforts spent in reviewing this piece of work. The detailed comments are very much appreciated and will be of great help to improve the quality of this study. We will address all points raised below as part of our revisions. Note that all the line numbers and section numbers in blue refer to the modified manuscript.

Specific comments:

Abstract: it should be reminded that there is no external (atmosphere or sea ice) forcing in the MISOMIP experiments. This would help understand that it is somewhat surprising that an ocean oscillation emerges.

Thanks for the suggestion. But we don’t think it is necessary. We already describe the oscillation as an emergent feature, which already implies that it is not forced externally. We make it clearer in the Sec. 2.2 (Line 86). “No external forcing is applied at the surface of the open ocean, which means there is no atmospheric or sea-ice fluxes. A “WARM” forcing, as the only forcing, is applied within a 10 km restoring region near the ocean's northern boundary”. We choose to leave discussion about external forcing to the main paper and not the abstract.

L. 53, 152 and at other places: I am not a native speaker, but “couple” should probably be “coupled” (or “coupling” for some occurrences).

Thanks for pointing it out. We have corrected all the words “couple” into “coupled” or “coupling”.

L. 97: Weddell and Ross cavities are usually classified as cold, not warm.

Thanks for pointing it out. We corrected it into “…with the warm ice shelf cavities in Amundsen and Bellingshausen Seas”.

L. 128: “This parameterization” is a bit unclear.

We mean the three-equation parameterisation here and modified it into “The three-equation parameterisation is typically applied between the top model layer and the ice…”

L. 145-146 (and caption of Tab. 1): It is not clear to me what is the difference between “conserving the volume integrals of tracer values (temperature and salt)” and “preserve the absolute values, (e.g. heat or freshwater)” as, for example, the volume integral of temperature directly gives the heat content when multiplied by ρcp. Furthermore, how exactly is imposed the conservation: additional flux at the surface? uniform T,S correction? Without this information, it is difficult to understand section 3.5.

Thanks for the comment. We don’t impose a physical flux. For the basal melting we impose salt/heat fluxes on the ocean model (Galton-Fenzi et al., 2012). For the ice draft change we simply change the volume of the water column without adding any fluxes as such. When melting occurs and freshwater should be added, we remove salt. That’s why we have this decision about how to handle tracer properties.

To clarify this, we add a couple of lines here “Changes in water column thickness due to ice shelf thinning would be maintained through increased horizontal convergence/divergence in the ocean circulation in response to mass/volume changes. ROMS effectively introduces a source/sink term imposed by adding or removing heat or salt at the ice/ocean boundary. For example, when the ice shelf melts, the model removes salt/heat rather than adding freshwater volume. The circulation change in this case is a result from density changes rather than volume changes. The approach using a source/sink term of heat/salt transfer imposes a choice upon the ocean model: either conserving the volume integrals of tracer values (temperature and salt) or preserving the absolute values, (e.g., heat or freshwater). Here we will explore the effect of both options on the ocean circulation in a coupled system in Sec. 3.5”.

We also added a sentence at the end of Sec. 3.5 “Note that the handling of tracer properties through ice draft change is separate from the way in which basal melting is implemented, and the latter is imposed on the ocean model through salt/heat fluxes (Galton-Fenzi et al., 2012). In response to the ice draft change, we simply change the volume of the water column without adding any fluxes.”

Please provide more details on the CTRL and Ocean3 experiments somewhere in section 2 or 3.1 (initial state, temperature and salinity restoring near the northern boundary, coupled models or ocean model with ice draft evolution, etc).

Thanks for the comment. We added one section “Sec. 2.2 Experiment design” for more details about MISOMIP1 and Ocean3.

“Each coupled model experiment in this study was run for 100 years, following Experiment IceOcean1r of MISOMIP1 (Asay-Davis et al., 2016). Like in IceOcean1r, experiments in this study does not include a dynamic calving, in which ice thickness is allowed to be zero without calving. Various configuration in each experiment can be seen in Table 1 and corresponding sections in Sec. 3.

We build our coupled model following the ISOMIP+ projects for stand-alone ocean models with ice-shelf cavities and the MISMIP+ projects for ice sheet models. Result of ISOMIP+ Ocean3 from Asay-Davis et al. (2016) using the same ocean model will be used as a comparison to the control experiment in this study (CTRL in Table 1).

The ocean model in the coupled system is initialised with a steady-state ice geometry from the ice sheet model and a ``COLD'' initial condition following Asay-Davis et al. (2016). No external forcing is applied at the surface of the open ocean, which means there is no atmospheric or sea-ice fluxes. A ``WARM'' forcing, as the only forcing, is applied within a 10 km restoring region near the ocean's northern boundary (yellow area in Fig. 2a), which is consistent with the warm ice shelf cavities in Amundsen and Bellingshausen Seas. The warm water is expected to reach the ice-shelf cavity within the first two decades and induce strong basal melting and subsequent rapid GL retreat.

In Ocean3, the stand-alone ocean model uses the same steady-state ice topography with the initial state of the coupled system, and is run for 100 years with an annually prescribed evolving ice geometry. The ocean is initialized with the WARM profiles, forced with the WARM profile in the same restoring region with CTRL and strong melting is expected to begin immediately as the sub-shelf circulation spins up. More details about MISMIP+ and ISOMIP+ can be seen in Asay-Davis et al. (2016)."

Fig. 2: is the maximum of the barotropic stream function calculated under the ice shelf or all over the MISOMIP domain?

We calculated the maximum of the barotropic stream function under the whole MISOMIP domain. To make it clearer, we modified the related text into "The highest correlation coefficient between the basal melting and the maximum of the barotropic stream function under the whole domain (Fig. 3b) is 0.99 without a lag within both the 30 days and 1 day outputs". Note the previous Fig. 2 is now Fig. 3 in the revised draft.

Fig. 4: it would be easier to see the signal if the plots were showing anomalies with respect to the mean between year 63 and year 70.

Thanks for the suggestion. But we don't think it is necessary. The significant difference in basal melting across one cycle only occurred in one or two rows of cells where the GL retreated. That's why it looked nearly the same for the basal melting. We modified the color scale to make it look better. See the new figure below.

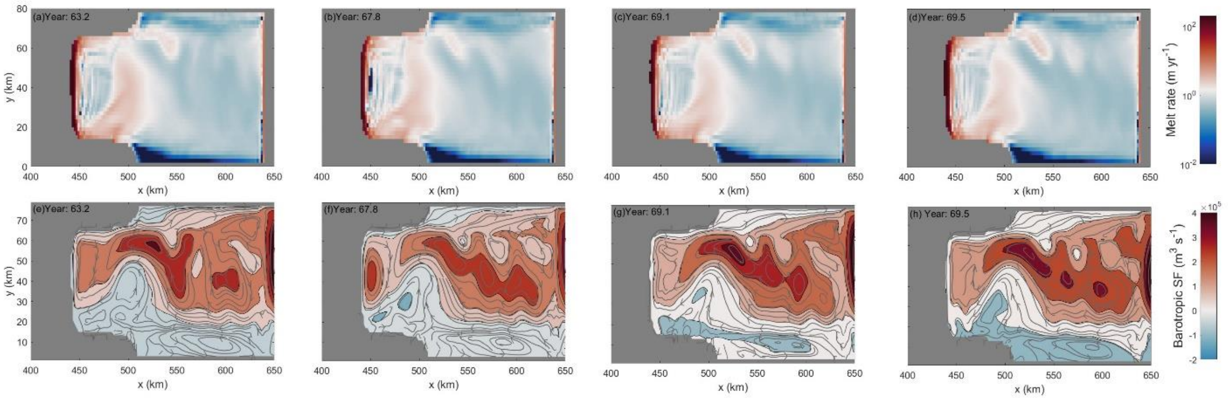

Figure 5

L. 185-186: what gyre are the authors referring to? Are these the gyres near the northern boundary or the gyre circulation within the ice shelf cavity?

We mean the gyre circulation within the ice shelf cavity. To make it clearer, we modified it into "2) the gyre circulation within the ice shelf cavity calculated as the strength of the barotropic streamfunction."

L. 193-195: I do not understand what the authors want to show with the barotropic circulation: any melt variation is associated with a change in barotropic circulation due to the modified horizontal density gradient and its role in the geostrophic balance (see Jourdain et al., JGR, 2017).

Thanks for the comments. We want to say that the basal melting is very much correlated with the process that we already highly suspected is driving the melt. We modified this sentence into "There is a high correlation (0.99) with no lag between the gyre circulation and basal melting (see Fig. 3b)".

L. 199-204: it is not so much the melt rate that is insensitive to the coupling period (it is actually smoothed for 6-mont h and 12-month coupling periods in Favier et al. 2019), it is the ice-sheet dynamics. Fig. 5 should therefore include another panel to show the ice sheet response (e.g. volume above floatation).

The only way the ocean impacts on the ice dynamics is through basal melting. So if melting is consistent across runs it is reasonable to assume ice dynamic behavior will be too. In this sentence, we made a statement about the sensitivity of general trend in basal melting to the choice of coupling interval rather than talking about the oscillation features. After this statement, we mentioned that CDT90 shows a smoothed oscillation pattern. To make it clearer, we added another sentence at the end “The simulated mean melt rates (Fig. 6a) and the ice volume above floatation (Fig. 6ab) indicate very little sensitivity to the coupling interval between 0.5 days and 3 months in the general trend. This is consistent with sensitivity tests with coupling periods ranging between 1 month and 1 year using NEMO-Elmer/Ice (Favier et al., 2019), in which the mean cavity melt rate seen by Elmer/Ice shows very little sensitivity to the coupling period. However, experiment CDT90 does not show an obvious oscillation pattern compared with the other experiments, which implies that using a coarse coupling interval may lead to the loss of temporal detail in the coupled ice sheet/ocean response. It can also be seen in the tests with 6-month and 12-month coupling periods in Favier et al. (2019), in which the oscillation feature was obviously smoothed. Additionally, mild variations in periodicity and magnitudes are found as the coupling interval varies. Tests with coupling interval of 5 days or less show more consistency, while tests with coupling intervals of 15, 30, 90 days show differences in magnitudes and phases. CDT30 is closer than CTRL (15 days) to the shorter coupling intervals, suggesting that there might be some cancelling effects in CDT30. Further study to understand the causes and nature of the impact of coupling intervals greater than 5 days would be of benefit to the coupled ice - ocean modelling community.”

We added another panel to show the ice sheet response and see new Fig. 6 below.

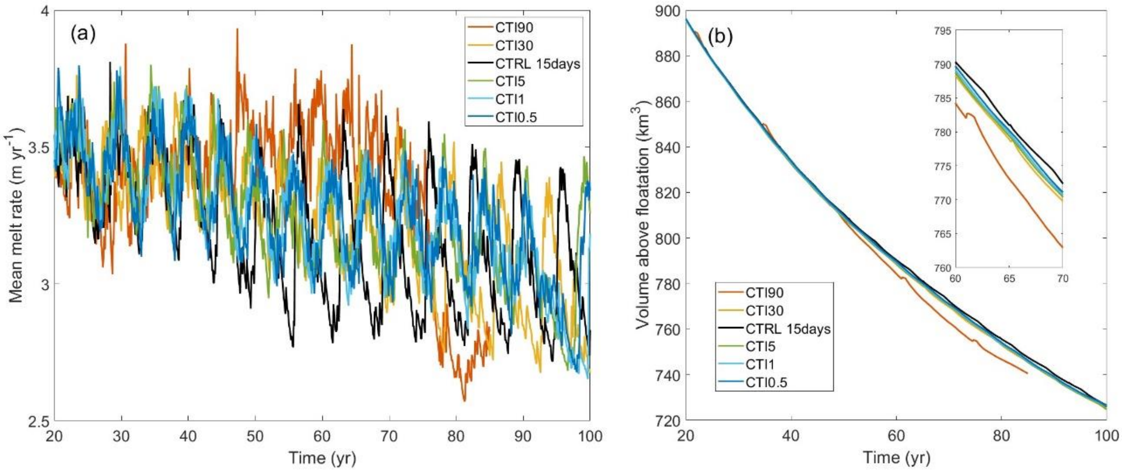

Figure 6 (a) Simulated mean melt rates and (b) ice volume above floatation with different coupling interval. The inset box in (b) is the zoomed in period between year 60 to year 70.

L. 201-202: While I appreciate that FISOC is flexible, this sentence comes out of the blue and I would remove it.

Removed.

Fig. 6: The vertical resolution seems to have an effect on the melt oscillation period (e.g. compare orange to black curves).

Yes, similar with other tests with different coupling interval, different initialisation of tracer properties of the dry cells, or the dependency of friction velocities to the vertical resolution, they all affect the amplitude and period of the melt oscillation at different degrees. We have mentioned it in Sec. 3.3 (Line 246) “A similar oscillation pattern existed in all of the experiments related with vertical resolution, but showed different frequencies and amplitudes. The outcomes of these experiments demonstrate that emergence of the basal melt oscillation does not depend on the vertical resolution of the ocean model.”.

L. 238: fu* should be u*

Modified.

L. 239: if melt is independent from u*, what equivalent constant u* value is applied?

In the ‘three-equation parameterization’ equation, the exchange velocity can be either assumed constant or assigned a functional dependence on the friction velocity u*. In UstarIndep, we adopted a constant Υ_T (thermal exchange velocity at the ice-ocean interface) and Υ_S (salinity exchange velocity at the ice-ocean interface) to remove the dependence of exchange velocity to u*. To make it clearer, I added the following sentences in Line 263:

“2) UstarIndep, in which we used constant values of thermal and salinity exchange velocities at the ice-ocean interface (T = 1 × 10^{-4} m s^{-1}, S = 5.05 × 10^{-7} m s^{-1}). The chosen values match those used by Hellmer and Olbers (1989), and are approximately equivalent to a constant friction velocity of ~0.01 m s^{-1}”.

Section 4.1: more information is needed: do all these models have the same ocean and/or ice-sheet resolution?

We added one sentence in Line 328 to make it clear. “All the contributing ocean models used the same horizontal resolution of 2 km while the ice modes used different horizontal resolution near the grounding line ranging from 200 m to 1 km.”

Fig. 11: anomalies with respect to the entire period would be better.

Thanks for the suggestion. See modified figure below.

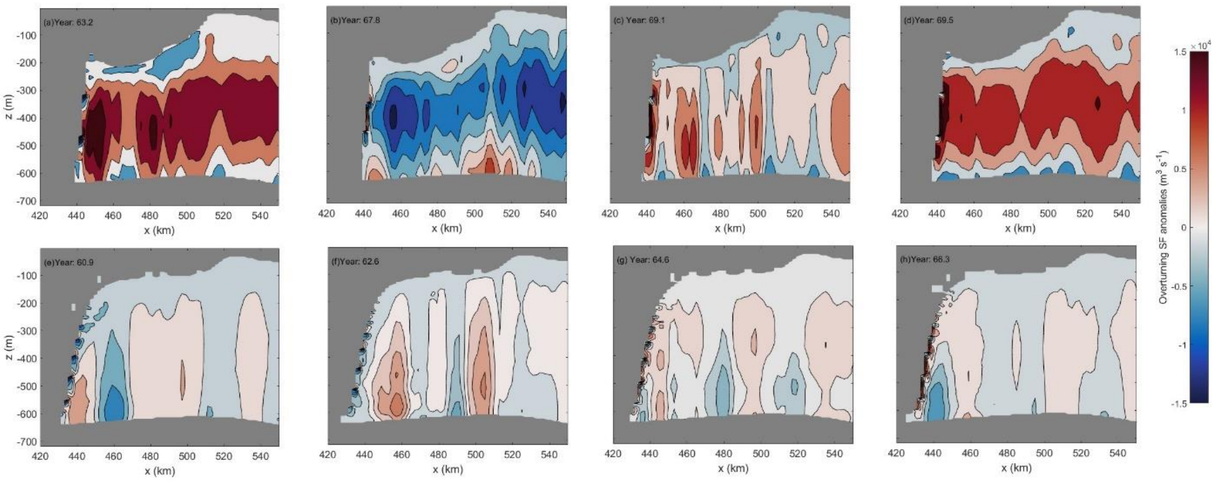

Figure 12 XZ sections of anomalies of overturning streamfunction near the grounding line from CTRL (top row) and Ocean3 (bottom row) around one oscillation cycle. Anomalies are calculated with respect to the whole cycle. The chosen time points are

shown with red points in Fig. 3.

L. 307 and 349-342 and 401: I do not see why the grid direction would matter, the issue of having discrete grounding line retreat will remain whatever the grid direction. I don't pretend that it won't make any difference, but I do not see why it would make oscillations disappear (for example, the ice slopes will still be affected by the grounding line motions). Instead of rotating the grid, I would suggest increasing the ocean resolution.

Thanks for the comment. We don't agree that the rotation of the grid would not remove the oscillations. The oscillations feature a correlation between the ungrounding of a row of grid cells and enhanced melting and circulation strength. The orientation of the grid and the design of the experiment (such that the central part of the GL is aligned with the grid) allow this ungrounding of a whole row of grid cells to occur approximately together. If the grid were rotated, the experiment design would not encourage the ungrounding of a whole row of cells. Instead, it could be that cells unground one at a time. We do not know whether the melt oscillations would then occur the same as in the current set up, with reduced strength, or not at all. Reduced strength seems most likely since smaller scale discrete ungrounding would still occur. A grid rotated to about 45 degrees would potentially allow a different pattern of ungrounding to appear. We acknowledge that an increased ocean model resolution may reduce this effect, but only if it can resolve a more complex grounding line geometry which is no longer aligned with the model grid.

To make it clearer, we modified those texts as below:

L307 "The fact that they occur only in simulations in which the GL moves, together with the close relation between GL retreat and mean melt, strongly suggests that the melt oscillations are driven by the discretized ungrounding that occurs on a structured grid that is aligned with the GL. The grid orientation and the experiment design in this study guarantee the central part of the GL aligned with the grid, which allows the ungrounding of a whole row of grid cells to occur approximately together. A grid rotated to about 45 degrees would potentially allow a different pattern of ungrounding to appear. If the grid were rotated to about 45 degrees, the experiment design would not encourage the ungrounding of a whole row of cells and cells may unground one at a time instead. We do not know whether the melt oscillations would then occur the same as in the current set up, with reduced strength, or not at all. Reduced strength seems most likely since smaller scale discrete ungrounding would still occur. A further test with a rotated grid in the ocean model might help to diagnose the potential numerical issues associated with coupled grounding line retreat processes."

L339-342 "Our results however also suggest that the pattern of ungrounding is controlled by the discretisation of the coupled system (primarily the ocean grid) and future work should investigate the use of a grid rotated to about 45 degrees to test the sensitivity. In a real-world simulation, in which the GL is not aligned with the model grid, do these melt oscillations still occur in the similar way? We also recommend future studies by employing finer resolution near the GL in the ocean model and quantifying the impacts of finer resolution and grid rotation to determine whether the time-mean melt in the current study is affected by numerical artefact."

L401: We think it is fine to say "Future studies with a higher horizontal resolution and a rotated ocean model grid will help further quantify the impact on this oscillation feature, and determine whether the melt oscillation is a numerical model artefact."

L. 323: buoyant plume speed…. and speed associated with the horizontal density gradient.

Thanks for the suggestion. We modified "buoyant plume speed" into "speed associated with the horizontal density gradient".

L. 399: "not sensitive" -> "not very sensitive".

Modified the sentence into "the existence of this oscillation pattern was insensitive to the choice of …"

References

Asay-Davis, X. S., Cornford, S. L., Durand, G., Galton-Fenzi, B. K., Gladstone, R. M., Gudmundsson, G. H., Hattermann, T., Holland, D. M., Holland, D., Holland, P. R., Martin, D. F., Mathiot, P., Pattyn, F., and Seroussi, H.: 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), Geosci. Model Dev., 9, 2471-2497, 2016.

Galton-Fenzi, B. K., Hunter, J. R., Coleman, R., Marsland, S. J., and Warner, R. C.: Modeling the basal melting and marine ice accretion of the Amery Ice Shelf, Journal of Geophysical Research: Oceans, 117, 2012.

Hellmer, H. H. and Olbers, D. J.: A two-dimensional model for the thermohaline circulation under an ice shelf, Antarctic Science, 1, 325-336, 1989.