

Geosci. Model Dev. Discuss., referee comment RC2
<https://doi.org/10.5194/gmd-2022-21-RC2>, 2022
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Comment on gmd-2022-21

Anonymous Referee #2

Referee comment on "Evaluation of an emergent feature of sub-shelf melt oscillations from an idealized coupled ice sheet–ocean model using FISOC (v1.1) – ROMSIceShelf (v1.0) – Elmer/Ice (v9.0)" by Chen Zhao et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-21-RC2>, 2022

Summary

This paper describes the investigation of an interesting phenomenon seen in simulations carried out according to the protocol of the MISOMIP1 intercomparison project. Although only one model is used here, the phenomenon seems to occur in a number of the models that contributed to the MIP, and the factors investigated are technical ones common to many coupled ocean-ice systems, so the paper will be directly relevant to a number of groups working in this field. As such, it's a useful contribution and fits the scope of GMD well. It's generally clearly written and at an appropriate depth, and proof-reading for grammar aside, could be published basically as is.

General comments

In terms of the factors that have been tested the work is quite comprehensive, but because the oscillations are present in all the configurations tested and no clear sequence of physical or numerical features that lead to them can be isolated, the conclusions that are drawn in current framing of the paper (why do the oscillations arise) are rather limited. However, for me the most useful thing about this paper is the range of choices of coupling interval, resolution, timestepping etc that are tested and the effects of these choices that are shown up in the changes of amplitude and periodicity of the oscillation. Showing the impact of these choices - using the oscillatory behaviour as simply a useful test bed, rather than the main feature of the paper in itself - to others who are configuring their own ice-ocean coupled models is valuable I think, and I would like to have seen more made of these smaller effects, rather than simply stating that the existence of the oscillation is insensitive to the choice. It would be ideal to have the influence of each factor really broken down into numerical and physical factors, but that seems like an unreasonably huge amount of work, and I think the level of detail that is provided is still useful. Just frame what has already been said about the sensitivity to each factor with that in mind.

In general, the language throughout could do with native-speaker tweaking by someone familiar with the specialist vocabulary. There's nothing that inhibits comprehension of anything that has been done or explained, but the current text is peppered with minor grammatical issues that give it a slightly awkward feel.

Specific comments

line15: "response is insensitive". Although the basic presence of some oscillatory behaviour is a constant as all of these factors are varied, since this work shows the amplitude, phase and sub-cycle variability of the oscillation varying significantly across the different configurations, I think this sentence could easily be taken the wrong way by a reader so should be made more nuanced.

line35: further developments to NEMO that allow coupling to an ice sheet model (and the rest of an ESM) could be cited via Smith et al, JAMES 2021.
<https://doi.org/10.1029/2021MS002520>

line99: the sentence feels lost here - should it belong to either the preceding or succeeding paragraphs that describe the model rather than the experimental configuration?

line108: "Results indicate..." it's not clear to me if you mean previously published work or something you've done (which may or may not be described in this paper).

line119: it's not clear if this sentence indicated that simulations here were unstable and failed to complete, or if this is just general advice for other users?

line125: "The water speed" - this is just the speed as used in the three equation-melt parameterisation, not water velocities in the model in general.

figure2: the oscillations would be clearer if the vertical axes were scaled to show them better. Could the large, linear adjustment in the first 20 years be shown in a separate panel on different axes to allow this? I'm not sure what is meant by the last sentence of the caption - what is the "nonphysical noise" that must be removed? The Ocean3 experiment doesn't appear in the main table of experiments and is not explained until much later in the paper - I'm not sure it helps to have those panels appear here.

line162: Perhaps I should read the cited Gladstone'21 for the more details, but I'm unclear on what's allowed and what isn't in the passive thin cells. This sentence implies to me that there's potential for an ocean cell to unground because of changes in water pressure conditions in the thin layer due to the ocean, not the ice sheet. They're not just wet cells as a technical convenience in the model then...and there might be potential for some time dependence in the precise position of the grounding in this model in /all/ the ISOMIP+ experiments, not just the ones where a change in ice sheet geometry is prescribed?

line166: "following MISOMIP1": I think it would be helpful to outline the protocol a little more for those unfamiliar with it. Perhaps the paragraph at line 95 belongs better here, and with a little more detail? I assume that aside from Ocean3 (whose protocol I don't think is explained anywhere) you're following the first part of the IceOcean1 experiment as the ice is forced to retreat, but that should be said - along with more on what IceOcean1 did and did not involve (eg. surface forcing, ice calving) - explicitly.

line171: why (these) four points/focus on year 44? The oscillation at the blue dot does seem qualitatively different from that at the green. Some more information about why - or what the distance from grounding line, or the thickness of the water column at the points are (is the ice ungrounding just a little before regrounding, or a lot?) might be interesting.

line176: it would be helpful to have the compass directions rather than just x and y marked on figures 1 and 3 if you're going to use them in the description, especially since MISOMIP chose to use left-right x as north.

figure 3,4. The top row of figure 3 could be zoomed closer in to the grounding line to show more of the relevant detail. The corresponding top row of figure 4 seems redundant?

line179: I wasn't initially sure exactly what was meant by "discrete" here. I can see it's explained later, but saying "The discretisation of GL retreat" at the start might be less confusing.

line195: can you distinguish between the dependence on the gyre for its introduction of warmer water to the interface (eg a purely thermal effect) and its influence on the friction velocity used in the 3 equation melt parameterisation?

figure5: whilst all the intervals do show some oscillation, the variation in periodicity that results does seem like a really interesting result - a good example of why I think the paper would be more useful rephrased in terms of the variations that /have/ been found as the coupling mechanics vary, rather than the fact that some kind of oscillation is robust across all the techniques.

line199: do the ice or ocean model timesteps also vary with coupling interval?

line245: this is a nice result that I think might be worth making more of. Coarser resolution ocean models used for pan-Antarctic or global simulations that some climate modelling groups are starting to use may effectively be in the same regime as UstarIndep due to their viscosity and poor resolution of details of flow under the ice. These ROMS experiments are all at the MISOMIP 2km COM resolution - are there clearly weaker oscillations in any lower resolution TYP simulations submitted to MISOMIP?

line253: I'm not totally clear on exactly what is being done with the tracer values in these two different approaches as explained here. Melt from the shelves does imply additional mass being added to the ocean, so here are we only considering changes in ocean volume caused by dynamic changes to the ice shelf draft?

line266: this is another result I can see being really useful to others developing coupled ocean-ice models that it would be good to highlight more, rather than the simple persistence of the melt rate oscillation - although it would be even better if I understood exactly what was being done in the two different approaches that are being compared (see comment above).

line281: there do appear seem to be significant differences in the mean melt rate, amplitude and phase of the oscillation though. My take-home from the figure is that the value chosen to initialise dry cells /does/ matter somewhat to the system in general, opposed to what is in the text. Figure 9, as with the other timeseries plots, would show the important details more clearly I think if the large linear adjustment in the first 20 years were cut out and the y-axis scaled to show the detail of the more steady melt rate.

figure 10: should this come much earlier? We're first told about this result and the multi-model nature of the oscillation at line 46.

line311: I don't understand what is meant by this sentence.

line339: You've given some plausible reasons for why the moving geometry might amplify the melt rates and thus the amplitude of an oscillation, but it's not obvious why your amplifying factors don't simply result in a positive feedback and general increase in melt and retreat rate, rather than leading to the cyclic strengthening and weakening observed. Could you talk through a hypothesised cycle?

line362: The UKESM-ice paper (Smith et al, JAMES 2021) referred to above discusses this partitioning and shows that the non-conservation implications of their approach (which I

think is similar to "preserving absolute tracer values" here, although I may have misunderstood) in their more realistic domain is considerable.

line399: as a conclusion, I think that simply stating that "this oscillation pattern is not sensitive to ..." is a bit misleading and underplays a set of results that are potentially very interesting to others developing their coupling schemes in this field.

line409: the last conclusion refers to a rather technical point that has only been introduced very late in the paper via one paragraph in the discussion, with no supporting results shown. Is it really the best point to end on - or needed here at all?

figure A2: as stated earlier, I think the difference in the form of oscillation at the green and blue dots is interesting and would be nice to see explored a little.

figure A5: the caption and legend labels don't really explain the difference between the panels and lines in this figure.

line421: it might be nice to Acknowledge the general MISOMIP effort/community here, since this paper does require the existence of that protocol and the significance of the oscillation does partly rely on the fact that other contributors found it too - and you have a plot with their results (fig10)!