Response to RC1 on manuscript gmd-2021-315

December 16, 2021

We thank the referee for their careful inspection of our manuscript and insightful comments, which led to significant improvements. Nearly all comments have been addressed in the new version of the manuscript. Justifications for not incorporating two specific comments are provided below. Moreover, on top of being included in the new version, some comments calling for a more elaborate answers are also directly answered in this letter, with lines referring to the track change file which will be uploaded upon editorial permission.

> *While the paper emphasises the novel nature of explicitly resolved ice shelf cavities in the coupled system, it does not do enough in the introduction to emphasise why this is so critical to achieve the scientific aims one might have when utilising such a model. A brief discussion on the impact of resolved vs. parameterised vs. absent cavities would be valuable.*

More detail on these alternatives (no cavity, parameterized cavities), and the reasons why they were discarded for PARASO, have been added from page 3, line 70 on.

> *Similarly, I would like the description of the chosen experiments to be placed in the main body of the text and slightly expanded upon. Experimental design should not be relegated to a table caption.*
>
> *L408: I do not like that the experiment configurations are described within the caption for Table 4. A separate paragraph should be included describing each experiment and why it was necessary to the outcomes of this paper. There is no motivation presented as to why these experiments, specifically, were chosen.*

Agreed. More detail are provided from page 21, line 510 on, in the main text body.

> *L69: A reference is given for v1.0, but v1.7 is used in this study - what are the differences between these versions and do they have an impact on the results presented?*

More detail has been given from page 4, line 89 on. Overall, on the relatively short time scales of PARASO, the combined influence of the coupling-induced novelties (including the static calving front) are negligible. In particular, even with a free calving front, the land mask would not be significantly different, and the magnitude of the feedback on the grounded ice would be comparable to numerical noise.

> *L84: While the key parameters for the ice shelves are noted in Table 1, it would be appropriate to do the same for key parameters in the ocean, or, at the very least, reference a publication where the same ocean configuration is used. The same is true for the description of the atmosphere.*

Tables A1 and A2 have been added and referred to in the main text body.

L215: Does f.ETISh also provide the heatflux of the meltwater, or is this calculated within NEMO?

It is calculated within NEMO, which then extracts/injects the corresponding heat from/in the ocean at each NEMO time step (not only during coupling). Strictly speaking, this is more of an ice-shelf cavity module aspect rather than a coupled one, so this has been stressed out for clarity at page 5, line 126 in Sect. 2.2.2.

L258: Does this mean that simulating melt-induced sea level change is not possible with this configuration? I would suspect not. Do the authors consider the inclusion of dynamic sea level change to be unimportant in such a coupled system, or simply something that is not technically possible at this time?

Simulating melt-induced sea-level change is possible with PARASO, and would even be possible without ocean – ice sheet coupling. As soon as ice-shelf cavities and melting are included in NEMO (which is possible *without* ice-sheet coupling), the mass flux related to subshelf melt is injected into the ocean, the corresponding increase of volume gets incorporated through a divergence term, and the mean sea-surface height increases. This happens as soon as ice-shelf cavities are included, regardless of whether ocean – ice-sheet coupling is included. Hence, this aspect is technically conveyed in PARASO.

The lack of conservation discussed in the manuscript is much more minor and purely related to the ocean – ice-sheet coupling:

1. A small amount of mass and heat is brought into (resp., taken out of) the ocean after a coupling episode when NEMO opens up (resp., closes up) an ocean cell in reaction to ice-sheet geometry changes. For example, upon NEMO cell opening, the 3D ocean domain will be slightly bigger, and the new cells have internal energy which were not present prior to the cell opening, so there is a slight increment of volume and heat in the system. This is only related to the cell opening and closing, not to the ice-shelf melting in itself, whose related mass flux and latent heat had already been accounted for by NEMO, at each model time step (so not only at coupling instances).

2. At each NEMO – f.ETISh coupling time step, a divergence correction is applied for numerical stability. This is equivalent to injecting/extracting water from the grounded ice-shelf base, which yields marginal mass leak/gain (and associated internal energy).

Both these conservation caveats are really minor at the scales we are looking at. NEMO has an option for enforcing conservation (by compensating for both these caveats), but since PARASO is not a global configuration (hence, our integrated ocean mass and heat are not conserved anyway), and is meant to be run over relatively short periods, we have not activated it. Thanks for raising this point, more detail has been given at page 5, line 126 and page 13, line 303.

L266: "CCLM 2 runs for one coupling time window, sending f.ETISh monthly time series of surface mass balance (SMB)". Make clear that this is offline. You say in the previous sentence that it is 'restart based' but it would be more effective if you define online and offline coupling earlier in the article, then use these consistently throughout so as not to confuse the reader with different terms that ostensibly describe the same thing.

Agreed. "Online" and "offline" have been defined once and for all (done at page 7, line 170) and then systematically used, which is clearer. Thank you.

L277: Section 4 feels out of place. Is there a reason the authors have chosen to describe the coupling process before the configuration of the model components? This would fit in far better when describing each individual model, in my mind.

Roughly speaking, Sect. 2 describes the models (pre-existing code), Sect. 3 the coupling interfaces (new code), and Sect. 4 the configuration (input data). We feel that the manuscript flows better in that way, also because the input datasets (described in Sect. 4) are interdependent, hence it makes sense to join them in specific subsections (for geometry, initialization and forcings). In our opinion, this order makes more sense with respect to the global manuscript coherence. We also thought that it would serve the readers who are mainly interested by the input dataset we use, so that they can directly go to Sect. 4.

L288: "The land ice extent is kept constant as the NEMO-COSMO interface has not been designed to deal with an evolving land-sea mask"

I think this is an important point which merits further discussion in the Discussion and Conclusions. What impact would this have for longer simulations where the grounding line and shelf retreats, but the shelf is forced to remain due to this constraint?

This indeed is an important limitation which was not further discussed. It is one key-point that would keep PARASO from being directly transposed to longer time scales (e.g. centennal). More detail on the implications has been given at page 15, line 341. We have found that this was the right spot for it, rather than the conclusion. The latest NEMO version does allow evolving surface masks, but making it compatible with the atmosphere coupling is still a considerable challenge. The ocean – atmosphere interpolation weights (computed by OASIS in PARASO's case) would then have to be updated at each ocean – ice-sheet coupling episode.

L362: "keeping a maximum snow depth of 1 m is also convenient to limit the spin-up phase of the snow pack to a decade." I'm a bit confused here - above it mentions that the land is initialised from hard-coded initial conditions, but here you note that the snow pack specifically is spun-up for at least a decade? Could you clarify?

The snow pack is not spun up in the PARASO runs, but it will certainly be in future work that we plan using this configuration. We decided to drop the first year of the simulation in Sect. 5 to account for that. We expect one year to be enough, because the interactions between the atmosphere and the snowpack mainly affect the snow state in the top few meters (on short time scales; a year or so). One year is also enough, because we started our simulation from a prescribed 1 m-SWE-thick snow pack. In reality, developing such a snow pack would require 10 years where the precipitation rate is 100 mm per year (Antarctic Plateau). This approach is considered OK for a 1m SWE snowpack, because the first snow meters do show an annual cycle in temperature representative of the atmospheric forcing conditions. Thicker snowpacks would need more time to adjust to the atmospheric forcing. Accordingly, a spin-up phase of a decade should be regarded as an upper limit for a 1 m SWE snow pack to be at equilibrium. The text has been modified accordingly at page 19, line 458.

L362: "but also the risk of simulating permanent snow cover (difficult to correct for thicker pack) in places" please rephrase sentence fragment in parentheses.

This sentence has been removed, as it was relevant only for thicker simulated snow packs which would require a spin-up time longer than the experiments presented in the manuscript.

Figure 6. - I question the value of panels b - e. As the patterns of melt are generally very similar, would it not be more useful to make these figures anomaly plots like panels f) and g)? I do not consider this essential and defer to the authors, but it would be my preference.

Agreed. Figure 6 has been redesigned: for the Ronne-Filchner and WAIS, it now features PARASO absolute melts (to show that the WAIS is melting fast, and the refreezing pattern properly occuring in Ronne-Filchner) and PARASO - PAROCE anomalies (to show the relatively minor impact of coupling).

L415: Figures 9 and 10 are being referenced before Figures 7 and 8. Please re-order your figures accordingly.

Figures 9 and 10 refer to ocean variables, hence they are contained within Sect. 5.2. We have kept the ordering as it was, since the early reference to Figs. 9 and 10 is simply a nod supporting more elaborated comments on Fig. 6, which is in Sect. 5.1. Figures 9 and 10 are then discussed in more detail later, in Sect. 5.2. Since the GMD author guidelines do not explicitly request the figures to be numbered in order of appearance, and since it made more sense to us, we have kept the ordering as it previously was.

L499: Is this the only source of Drake Passage transport weakening or just the only one that is clearly identifiable? What is the magnitude of this counter-current?

It is the only one that has been clearly identified, and it also corresponds to a challenge for ocean models, and NEMO in particular (see the DRAKKAR 2021 meeting report). The magnitude has been added and commented at page 30, line 626.

L568: "However, our results suggest that at the short timescales investigated for this technical paper, the practical impact of this particular coupling interface is minor, and that the main features of PARASO would be reproduced with a similar NEMO - CCLM 2 coupled configuration (i.e., excluding coupling with f.ETISh)." This of course raises a question: with the coupling having such a small impact on short time-scales and no way to judge its impact over longer time-scales (given we look at 2 years of results here), how can we judge the soundness of this coupling approach and PARASO's utility for longer time-scale simulations where the coupling provides important feedbacks?

As specified in the quoted sentence, this is a technical paper describing the configuration, its capacities and limitations. We believe that showing a two-year simulation, during which 8 ice-sheet coupling episodes occur, is enough for assessing the soundness of the coupling approach. A reference to the decadal time scale applications has been added in the introduction at page 3, line 61. Moreover, results from longer simulations (up to a decade) are also briefly presented in Fig. A10. Regarding the impact of the ice-sheet coupling, we have found that the ice-sheet model initialization is more determining than the presence of the coupling interface, at least at the decadal timescale. This is not a result we were happy with, but this has been further explicited out at page 33, line 698 for the sake of transparency. As already hinted at in a comment above, PARASO could probably not be used as such for longer simulations because of the constant land-sea mask constraint. Fixing this is a significant technical challenge beyond the scope of our study.

L576: "the objective was to check whether the biases were affected by the coupling interfaces themselves" I may have missed it, but is this stated previously in the manuscript? This ties back into my request for a more fleshed-out description of the chosen simulations for validation.

This point was added in the introduction, at page 4, line 81.