Response to RC2 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. All comments but two have been addressed in the new version of the manuscript. Justifications for not incorporating two 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.

> *Introduction: you make a good point about the need to simulate the Antarctic and Southern Ocean climate in a coupled model to capture the various complex interactions. I would recommend to also mention the typical spatial resolution which is needed to capture characteristics of the climate (e.g. global climate models usually do not provide a decent Antarctic surface mass balance). This could serve to highlight the potential of PARASO.*

Agreed. This has been added in the introduction, at page 3, line 61.

> *P6, ll. 140ff: please add some more information such as: "...provides NEMO with updated ice information about the geometry of the ice shelves. " Also it could be mentioned here already that the coupling allows ice shelves to change in thickness but not in extent.*

Slightly more detail has been added at page 7, line 176, but we have not specified that ice shelves can change in thickness but not in extent. Since grounding lines can move, technically, ice-shelf extents can change (but the total ice-sheet extent cannot, since the ice-shelf front shape has to remain constant). We estimated that this as too detailed for this brief introduction.

> *P.15, l. 342: This is a bit confusing, especially, since so far the experiment PARASO and its forcing was not yet introduced. Maybe: "...NEMO stand-alone, using consistent forcing with the subsequent coupled experiment. Specifically, this is ORAS5 forcing at its lateral boundaries and an ERA5 forcing which has been processed by the NEMO-CCLM2 coupling interface." And another question came to my mind here: is this forcing which is coming out of the coupling interface identical with the forcing which is used in the coupled experiments outside of the CCLM2 domain? From P17 and the discussion I take that this is not the case- so is the coupling interface also used outside of the CCLM2 domain for the spin-up? Maybe it would be interesting to show the difference between ERA5 derived surface forcing fluxes from the coupling interface and from the CORE bulk formula.*

> *P17, l. 375: also here: is the CORE bulk formula producing fluxes different to what the coupling interface would produce?*

Thank you for pointing that out, since this is an important point which was not clear enough in the first version of the manuscript. For the spin-up, the fluxes are not strictly speaking equivalent. ERA5 input data is used for both of them, but the COSMO interface (used for coupling) leads to fluxes distinct from the CORE bulk formula used for the spinup, even with the same input dataset (in that case, ERA5). This was already briefly hinted in the conclusion of the first submission (e.g. page 33, line 727), but probably too lightly, as the above concern testifies.

The COSMO surface scheme is very specific. It requires atmospheric inputs which are quite model-dependent (e.g. the TKE on the lowest 2 atmospheric levels), and thus not available in reference products such as ERA5. Implementing it within NEMO would have required using these inputs, which are not available over the uncoupled part of the NEMO domain. Two alternative options were then considered and subsequently discarded. First, we considered forcing NEMO with fluxes coming from the COSMO surface scheme, feeding this scheme with ERA5 input variables for classical, available fields (e.g., radiation, air-temperature, winds, etc.), and COSMO-stand-alone outputs for the "COSMO-specific" ones (TKE, etc.). This was discarded as this would have:

(a) required running a stand-alone COSMO simulation over a larger domain than PARASO's COSMO domain (to cover the full NEMO domain);

(b) led to an hybrid forcing dataset, featuring inputs from both ERA5 and COSMO-stand-alone, whose coherence would be questionable.

The second considered option was to force NEMO with offline COSMO fluxes. This would have solved (b), but not (a). Moreover, with this second option the NEMO surface properties (temperatures, sea-ice concentration, albedo) would have been completely ignored, which is not ideal either, especially over sea ice. As a result, forcing NEMO stand-alone with fluxes derived from the COSMO surface scheme was not achieved and considered beyond the scope of the paper.

Since no easy solution arose, we eventually settled with the one implemented in the paper - computing the fluxes from the same input datasets, with different bulk routines. It is a clear limitation, and this has been further stressed out in this revision, e.g. at page 17, line 384, page 18, line 441 and page 19, line 444.

> *P18ff., Results: I wonder why there is no PARCLIM experiment (a coupled atmosphere-ocean experiment)? This could provide the forcing for the PARCRYO experiments, as it seems that the model drift is to larger parts related to the atmosphere-ocean interface, which makes it difficult to compare the PARCRYO and PARASOL experiment.*

This is a good suggestion. However, while PARASO is not *that* demanding in terms of CPU requirements, running an additional nearly-fully coupled experiment within the delays of this minor review process is difficult to achieve for practical reasons (lack of computational resources). However, our guess is similar to yours: if we did run such a PARCLIM experiment and provided PARCRYO with meltrates and SMB coming from it, then the ice-sheet results would be very similar to PARASO's.

> *P19, section 5.1: this section is hard to read. I think it could be more clear and better structured if the purpose of the different experiments and comparisons would be formulated before respective paragraphs (eg.: comparison to observations, identifying the drift of ice thickness after ice shelves are allowed to evolve , illustrating the effect of SMB coupling, illustrating the effect of ocean coupling, illustrating synergy in the fully coupled system).*

More explanations on the PARCRYO experimental designs and their motivations have been provided from page 21, line 510, and Sect. 5.1 has been clarified.

> *P21, Fig. 7j: please discuss this figure a bit more. The integrated surface mass balance anomaly is almost constant in the first 2.5 months- while VAF is slightly negative- was the SMB forcing of the preceding model year of opposite sign? What is the interannual variability of SMB from ERA5 or VAF by comparison?*

More detail has been added from page 24, line 555 on. Due to the shortness of the experiments, an evaluation of the interannual variability would not be very sound and is therefore omitted.