

Review of '*PARASO, a circum-Antarctic fully-coupled ice-sheet - ocean - sea-ice - atmosphere - land model involving f.ETISh1.7, NEMO3.6, LIM3.6, COSMO5.0 and CLM4.5*'
by Pelletier *et al.*

General remarks

This paper describes a new coupled model configuration for a regional Antarctic domain. The importance of atmosphere-ocean-ice sheet feedbacks, in particular via the interactions between the ocean and ice shelf cavities, has recently spurred development of more complete coupled modelling systems and PARASO represents a significant step in this direction. This manuscript and the model it describes is well suited for publication in GMD, with the following minor corrections.

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.

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.

Minor corrections and typographical issues follow:

L18/19: If we are discussing WAIS ice loss, surely there is a more up-to-date reference than Shepherd *et al.* 2012?

L38/39: "since they allow representing the feedbacks between each component" - please rephrase.

L47: Remove the colon.

L46-50: This sentence is very long, consider breaking it up (the reference to Kruezer *et al.* should be a new sentence).

L51: "including ice-sheet coupling, with cavities explicitly resolved" remove comma.

L60: "is that the ice-shelf melting is" awkwardly worded. Consider changing to 'sub-shelf melt is directly computed' or similar.

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?

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.

L114: “we use the version 5.0” remove ‘the’.

L136: “pursued” a strange choice of words, perhaps ‘continued’ would be more appropriate?

L158: “over 20 NEMO grid points (the full grid being \approx 600 nodes)” Is ‘grid point’ and ‘node’ being used interchangeably here? This is not clear.

L186: “would have arisen from a category-specific coupler and the net nonsolar heat flux is a first-order development” Should this not be ‘category-specific coupling’ ?

L187: “While the wind stress computations do not take into account surface velocities,” which velocities are being referred to here? Presumably the ocean, or the sea ice also?

L214: “Afterwards, NEMO awaits while f.ETISh” awhile should be while.

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

L223: ‘divider’ should be ‘divisor’, I believe.

L225: should be ‘ice shelf draft’. On this note, the authors are inconsistent in their hyphenating of ‘ice shelf’. Please select one and make it consistent throughout.

L226: Is there a reason for the selection of a three month coupling time step? To maintain seasonality? One sentence would be nice here.

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?

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.

L269: “For the sake of efficiency...” This implies a tradeoff - would there be a reason here not to do this?

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.

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?

L351: "The CLM model starts from initial conditions that are set internally in the code Oleson et al. (2013), implying that the land component is not in equilibrium with the atmosphere at the initial time."

Remove 'implying' and rephrase - without a spinup of any kind, the atmosphere-land system is by definition not in equilibrium. "At the initial time" is also awkwardly phrased.

L354: Given that the rest of the manuscript references temperatures in degrees C, why is Kelvin used here?

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?

L362: "snow depth would extent the spin-up phase" extend, not extent.

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

L364: Should be 'come' not 'comes'

L372: What is meant by a coupled field? Are these not static fields that are updated once every coupling interval? This should be rephrased for clarity.

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.

L408: "List of experiments specifically designed for this study and used in diagnostics" strangely phrased. Should be something like "and from which the presented diagnostics are derived".

L413: "coincide" is the wrong word here, I think. If the aim is to describe a similar geographical melt pattern, then rephrase. If the aim was to discuss the temporal coincidence of melt, then I'm not sure what is being described here.

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.

L414: "less saline Eastern Antarctic surface water in PARASO (see Figs. 6(g), 9 and 10)."

To my eye, East Antarctic coastal waters have a positive salt bias in PARASO, have I interpreted this incorrectly? Could the authors please reference which panels in Figs. 9 and 10 we should be looking at?

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

L415 (and elsewhere): What is meant here when saying melt rates have the same 'magnitude'? Could the authors please provide values for the two experiments, or at least a value describing the discrepancy between the two experiments (i.e. one is ___% larger than the other)? At the moment, it is a bit hand-wavy for my liking.

L419: "The under-estimated melt rates stem likely from increased ice shelf geometry changes" What does 'increased changes' mean physically? This is unclear to me.

L421: significantly, not significant.

L427: "display a similar behavior" remove 'a'.

L434: I disagree with the use of the term 'correlation' throughout the manuscript, as it represents a very specific mathematical relationship which is not intended here. Please rephrase. Something like 'relationship' would suffice.

L451: "East Antarctica ice sheet" - Antarctic*

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?

L502: "focus of developments in the forthcoming of the model" Poorly worded, please rephrase.

L517: "The comparison to AEROCLOUD over the Ross ice shelf, in contrast, provides similar results to ERA5." please rephrase, it is not entirely clear what is meant here.

L520: "known to be a considerable challenge in polar regions." this may be a naive comment, as I'm not an atmospheric expert, but would a reference be appropriate here?

L520: "definitely" should be "definitively".

L521: "this deficiency to one of the products mentioned above." Not clear what is meant here - what is the deficiency being referred to and what does 'products' mean in this context?

L536: "interseasonality" - change to 'interseasonal'.

L558: "fewer" should be "less"

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?

L572: "the biases observed in PARASO are a significant drawback," I would like the authors to highlight which biases they believe are most significant here, particularly in comparison to uncoupled models of a similar complexity/ resolution. This would be a nice synthesis of the results (while you discuss key biases below, a summary - just a list - here would be nice).

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.

Fig. A10: "extents" should be 'extent'.

L703: "coupling interfaces only pre-/post-treat 2D fields at relatively low temporal frequencies (monthly at worse)." 'treat' seems an odd choice of words - should this be 'process'? Similarly, 'worse' should be 'worst' but I would suggest something more appropriate like "monthly at most frequent".

L861: "overtly" is not the right word here...