The Cryosphere Discussions

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Comment on tc-2021-371

Anonymous Referee #2

Referee comment on "The Antarctic contribution to 21st-century sea-level rise predicted by the UK Earth System Model with an interactive ice sheet" by Antony Siahaan et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-371-RC2, 2022

The manuscript “The Antarctic contribution to 21st century sea-level rise predicted by the UK Earth System Model with an interactive ice sheet” by Siahaan et al. presents perhaps the first results of a climate model coupled to an ice-sheet model of the Antarctic Ice Sheet. The paper presents a protocol for offline data coupling of UKESM and BISICLES and explains the initialization process for this complex configuration. After this, results are presented for small ensembles of coupled simulations following both SSP1-1.9 and SSP5-8.5 greenhouse gas emission scenarios. SSP1-1.9 shows small changes from initial conditions, while SSP5-8.5 leads to ice-shelf basal melt regime change beneath Ross and Filchner-Ronne ice shelves, and associated ice-shelf thinning and acceleration. The manuscript discusses the major terms of the ice-sheet mass balance and considers unique aspects and limitations of the modeling approach presented.

The manuscripts presents a significant achievement of running coupled climate and ice-sheet models for Antarctica. This has been a community goal for many years, and these results are the first to achieve it that I am aware of, even given the “offline” coupling method employed. On the other hand, the methodology for achieving it includes a number of questionable choices with an unclear impact on the resulting simulation. Most significantly, the initialization procedure for both the climate model and ice the ice-sheet model appears a bit ad hoc, with a number of details of the protocol not clearly described. It appears there may be significant artifacts and model drift associated with the ice-sheet model initialization that have not been quantified. The authors do a fair job of noting shortcomings, but more work could be done in quantifying the impacts of those choices.

On the whole, this is an impressive modeling achievement, but the scientific utility of the results is questionable without further substantiation. The authors themselves note these limitations, and I wonder if this paper would not be better suited for a journal like GMD. Below I focus on two major areas that require more work - description of the coupling and description and analysis of the initialization procedure. I then list a number of smaller issues that require addressing. Even after significant additional analysis, it may be that the initialization and coupling procedure leave the scientific intepretation of the results ambiguous. The authors may wish to consider if a model description journal would be a more appropriate venue for this work, where it would be a significant contribution.

MAJOR CONCERNS

1. Better description of coupling.

I recognize that the coupling methodology was previously described in the Smith et al. (2021) JAMES paper, but given the importance of the coupling to the science results, inclusion of more information here is warranted. Some specific suggestions are:

142-6: The description of the coupling protocol is vague. Given this is a significant novelty of this work and coupled model results can be very sensitive to the coupling procedure, it should be described in much more detail.

Please provide evidence that the results are not sensitive to the chosen coupling interval.

130: This sentence is unclear - please clarify or expand on this. Also, please add a description of how retreat of the ice-sheet grounding line is handled by the ocean model.

126-146: Please acknowledge in this section that the calving front restriction and the bilinear remapping prevent the system from conserving mass and heat, though these errors are not likely to be significant for the experiments being conducted.

2. Model initialization and its impacts.

Initialization of coupled climate and ice-sheet models is a very challenging problem. Still, the procedure described here appears ad hoc, missing key information, and does not include an assessment of key initialization choices on the results. The specific point below detail these concerns:

179: This paragraph is confusing and is missing key information.

182: 45 years seems like a short ocean spin-up time. Can you provide justification that the most important transient behavior had reduced to small levels prior to creating the branch runs?

179-194: After reading the full paragraph, I am more confused. Replace the opening paragraph by clearly stating the initialization process is based on a hybrid state of the ice cavities from UKESM1.0-ice stitched onto the global ocean state of UKESM1.0. Also, a schematic of the various runs and processing, would help communicate this process.

186: Emphasize for the reader that UKESM1.0 does not include ice shelves, e.g. add a parenthetical "(without ice shelves)" after "UKESM1.0".

187: Which years were chosen? Can you elaborate on what "a range of variability" means? How many ensemble members were in the UKESM1.0 CMIP6 historical ensemble? Also, do you have a reference for that ensemble, or a reference to the dataset on ESGF? What does "end of the 20th century" mean? What specific years were used?

189: What does "short" mean? And what does "balance" mean on line 190? Please show evidence of balance or behavior that is close to steady state, either in the form of a plot or some statistics.

196: Please explain why the ocean-sea ice simulations are regarded as beginning in 2000 if they were branched from specific times of UKESM and included a cavity state from perpetual 1970.

197: Are the atmospheric fluxes from the same runs that each was branched from? Or one common set of atmospheric fluxes?

195-202: So the final initialization step is 1) not fully coupled to the atmosphere and 2) has temperature and salinity restoring applied. Given that, it is unlikely that you can branch into a fully coupled projection without some shock and drift. Please justify this choice.

195-202: Throughout this section, please clarify if every time you refer to UKESM1.0-ice, prognostic ice-shelf basal melt fluxes are on and what is happening with iceberg fluxes (if anything).

214: What is the thickness output of the inversion procedure? In the previous sentence it is only said that basal drag coefficients and viscosity are adjusted.

216: Ice-shelf melting from what year(s) of each standalone run?

221: Referring to this ‘spike’ is vague, as is “about 20 years”. Please report statistics or, better, show a time series of this RMS metric.

204-223: It is entirely clear what year the ice sheet initial condition represents. In line 212, it is stated the ice-sheet state starting the procedure represents “early 21st century”, but then it is relaxed for “about 20 years”. How does the final state of each ensemble member compare to recent observed thinning rates, which have been highly variable in time?

Figure 3 and associated text (286-292): The very large amount of noise (presumably transient flux divergence) in the ice-sheet elevation/thickness change deserves a few sentences of explanation. While this is a well known and common challenge for ice-sheet models, the amount of spurious thickness change after 15 years of integration and 20 years of relaxation (if I followed the protocol correctly - it was confusing - see above) seems unacceptably large. Also, what is the purpose of panel d? It shows nearly the same information as panel b.

297: It is not obvious to me why ice shelves would slow so significantly in the first year just due to one year of thickness change coming from surface and basal mass balance, especially if further adjustment after one year is small. Is it possible grounding line positions have shifted or the ice temperature field changed or something else is going on? A more thorough explanation is warranted.

Section 3.4: It would be easier to interpret the changes presented if there was also a standalone BISICLES control run that had constant 2015 forcing. Presumably the speckly pattern of thinning and thickening in Fig. 13 panels a and b is due to unrealistic transient behavior in the initial condition. That is a common problem in ice-sheet models, so it does not necessarily invalidate the results, but it should be clearly identified. I would prefer to see additional results for an unforced control run. Without it, it is difficult to assess what aspect of these results are an effect of the ice-sheet model initialization procedure and what is due to the climate forcing coming from UKESM.

Other Comments:

Abstract: Mentioning Greenland in the abstract is slightly misleading, because the Greenland results are not part of this paper.

47: Also cite the only paper that demonstrates this for the observational period that this sentence discusses:

Gudmundsson, G.H., Paolo, F.S., Adusumilli, S., Fricker, H.A., 2019. Instantaneous Antarctic ice sheet mass loss driven by thinning ice shelves. Geophys. Res. Lett. 46, 13903–13909. doi:10.1029/2019GL085027

48-55: There are a lot of other important studies that would be appropriate to reference here, e.g.:

Spence, P., Holmes, R.M., Hogg, A.M., Griffies, S.M., Stewart, K.D., England, M.H., 2017. Localized rapid warming of West Antarctic subsurface waters by remote winds. Nat. Clim. Chang. 7, 595–603. doi:10.1038/nclimate3335

65: CMIP5 and CMIP6

68: One CMIP-class ESM recently published (since this manuscript was submitted) Antarctic subglacial melt rates (but those simulations were not part of CMIP6):

Comeau, D., Asay-Davis, X. S., Begeman, C., Hoffman, M. J., Lin, W., Petersen, M. R., et al. (2022). The DOE E3SM v1.2 Cryosphere Configuration: Description and Simulated Antarctic Ice-Shelf Basal Melting. Journal of Advances in Modeling Earth Systems, 14, e2021MS002468. https://doi.org/10.1029/2021MS002468

69-74: There also is recently published (since this manuscript was submitted) regional model that includes all physical climate components (atmosphere, ocean, sea ice, land, ice sheet):

Pelletier, C., Fichefet, T., Goosse, H., Haubner, K., Helsen, S., Huot, P.-V., Kittel, C., Klein, F., Le clec'h, S., van Lipzig, N.P.M., Marchi, S., Massonnet, F., Mathiot, P., Moravveji, E., Moreno-Chamarro, E., Ortega, P., Pattyn, F., Souverijns, N., Van Achter, G., Vanden Broucke, S., Vanhulle, A., Verfaillie, D., Zipf, L., 2022. 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. Geosci. Model Dev. 15, 553–594. doi:10.5194/gmd-15-553-2022

58-81: Somewhere in here you should also acknowledge the fully coupled configuration of CESM with the Greenland Ice Sheet.

104: Can you report the approximate horizontal resolution of the 1 degree ocean grid at the typical latitude of Antarctic ice shelves?

111: It is worth pointing out that this adaptivity is dynamic in time.

117: Can you briefly summarize the impact of choosing 2 km as your finest resolution instead of 1 km or 0.5 km as is sometimes used for BISICLES?

162: I would say most ice-sheet models follow this practice, but it is not 'typically' the case - there are a number of ice-sheet models that do a paleo or steady state spinup.

169-171: These comments make me wonder if this manuscript would be more appropriate for GMD or JAMES.

Figure 1a,b and associated text in 3.1: It is rather awkward to compare these plots to referenced observational data without showing those observations or model biases relative to them. As presented, these comparisons are not useful.

264: Would not surface restoring bring properties closer to observations?

249-271: This discussion of water mass properties, especially at depth, based on mixed layer depth is obtuse. It would be much better to show T&S diagrams for the regions of interest compared to observations. Many global ocean models struggle with the formation of Dense Shelf Water, even with realistic mixed layer depths, so that in and of itself is not a guarantee of good water mass properties. It would also be quite important to see maps of ocean bottom temperature and salinity, as those matter more for ice-shelf basal melting than surface properties.

271-2: Please provide evidence for this statement (e.g. the bias value for UKESM and other CMIP models). Are you basing this statement off of the version of UKESM in Heuze

(2020) or the simulations presented here?

278: Do you mean *near-shore* fresh bias here? Over most of the Southern Ocean, Fig. 1 shows a saline bias.

Figure 4: Similar to figure 1, this figure should include the observational references fields (or show an anomaly). Simply saying "shows a similar spatial pattern to present day observations (Rignot et al., 2013; Adusumilli et al., 2020)" and expecting a reader to pull those up and make comparisons across different colorbars is not sufficient. Also the linear colorbar is inadequate for showing the high melt rates in the Amundsen Sea - presumably the ice shelves in that entire sector are well above 5 m/yr. Similarly, the colorbar in panel e and f saturates in areas of interest (e.g., Ross and Larsen ice shelves).

Table 1: Presumably in your model analysis you can separate Ross and FRIS into the two halves that the observational data uses.

309: While the modeled melt might be within the range, I suspect a t-test would indicate a significant difference. That is not necessarily unacceptable, but please report a more careful comparison.

Figure 5: Please also show present-day observational estimates for reference.

Section 3.2.1: This section demonstrates the melt regime change at FRIS very clearly, but the causal mechanism is left only hinted at. There is a plot and mention of declining sea ice volume and its possible relation to declining density. There also is a mention of increasing freshwater flux. This is already a long, dense paper, but if it were possible to tease out the mechanism(s) leading to WDW increase, that would be a valuable contribution. Have you looked at changes in wind stress? The previous papers you cite also discuss that as a potential mechanism for the WDW intrusion at FRIS.

424-6: From Figure 9, it looks like a missing piece of this explanation is that Dense Shelf Water (cold and saline) on the continental shelf is present at the start of the simulation, but becomes significantly fresher by 2060 (Fig. 9e). This is consistent with the sea-ice decline mentioned and shown in Fig. 8b to become more substantial around that time. Similar to the FRIS case, the reduction in the continental shelf density barrier facilitates the intrusion of mCDW. This series of events is alluded to in this paragraph, but the sentences at 426-7 implies that the driving mechanism is warming of the mCDW, which is not apparent in Figure 9. Maybe this just requires some rewording.

432: As you say, I think the relative model fresh bias in each of these regions is critical.

To further illustrate that point, could you follow up with a quantitative metric of the salinity bias in each region at the start of the simulation? (e.g. averaged over the region or at the shelf break analysis point used in each region.

440: You haven't shown that the regional climate is warming during this period. Maybe reword to "While the changing climate".

441: Remove "is".

Fig. 10: What year and simulation are these draft values from?

Section 3.2.3: Initially I was skeptical of even discussing results from an ice shelf represented by 11 grid cells, but I appreciate the honest assessment of what is happening in the model here, given the importance of this region. Better to acknowledge the limitations of interpreting these results than ignore it and risk readers reading their own interpretation into it.

491: Similar to previous comments, simply stating your results look similar to your observational reference is insufficient. Please include a panel in the figure showing the reference dataset (or the difference from it).

Table 2: Please also include a present-day estimate (e.g. from RACMO).

525: Another relevant reference here: Trusel, L.D., Frey, K.E., Das, S.B., Karnauskas, K.B., Munneke, P.K., Meijgaard, E. Van, Broeke, M.R. Van Den, 2015. Divergent trajectories of Antarctic surface melt under two twenty-first-century climate scenarios. doi:10.1038/NGEO2563

541: The Thwaites and Pine Island inland thinning goes away when you difference the two scenarios, and there is in fact less thinning in the SSP5 scenario. Please discuss this. Having a control run for context (previous comment) would likely help here.

Figure 14: Typo in 'cumulative' in title above panel b.

574: Consider rewording this sentence to avoid the possible interpretation that the thinning of Ross Ice Shelf has a direct impact on MAF.

580: This goes back to my earlier comment about what Thwaites and Pine Island are doing in the control run.

Figure 15: Consider using the same color scheme for the two scenarios here as in the previous figure.

642. 655-8: Maybe GMD/JAMES is a better fit?

Section 4.2: A short comparison of the results to those of ISMIP6-AIS is warranted, as that set of experiments is perhaps the closest point of reference to this work. In addition to considering the overall behavior of each region, it would be interesting to look at the threshold for surface hydrofracture employed by ISMIP6 and if/when that is passed in your simulations. Similarly, comparing your simulated basal melt rates to the parameterization they employ might help explain differences in response.

ISMIP6: Seroussi, H., Nowicki, S., Payne, A.J., Goelzer, H., Lipscomb, W.H., Abe-Ouchi, A., Agosta, C., Albrecht, T., Asay-Davis, X., Barthel, A., Calov, R., Cullather, R., Dumas, C., Galton-Fenzi, B.K., Gladstone, R., Golledge, N.R., Gregory, J.M., Greve, R., Hattermann, T., Hoffman, M.J., Humbert, A., Huybrechts, P., Jourdain, N.C., Kleiner, T., Larour, E., Leguy, G.R., Lowry, D.P., Little, C.M., Morlighem, M., Pattyn, F., Pelle, T., Price, S.F., Quiquet, A., Reese, R., Schlegel, N.-J., Shepherd, A., Simon, E., Smith, R.S., Straneo, F., Sun, S., Trusel, L.D., Van Breedam, J., van de Wal, R.S.W., Winkelmann, R., Zhao, C., Zhang, T., Zwinger, T., 2020. ISMIP6 Antarctica: a multi-model ensemble of the Antarctic ice sheet evolution over the 21st century. Cryosph. 14, 3033–3070. doi:10.5194/tc-14-3033-2020

705-708: This is a very speculative statement. The water in warm cavities can certainly get warmer, as it is modified CDW and not unadulterated CDW. Please remove or rephrase this statement with supporting information.

Section 4.3: A major limitation not mentioned is the lack of iceberg calving and dynamic calving front position. Other missing physical processes that might be important are subglacial hydrology/basal physics and the impact on ice rheology of fractures and damage.

Section 5: The conclusion would benefit from an additional couple sentences about the technical achievements and limitations of the model.