

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-52

Anonymous Referee #2

Referee comment on "Antarctic surface climate and surface mass balance in the Community Earth System Model version 2 during the satellite era and into the future (1979-2100)" by Devon Dunmire et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-52-RC2>, 2022

Summary

This paper describes the performance of the CESM2 model in simulating factors relevant to the surface mass balance of the Antarctic ice sheet in the recent past and the next century. It is a useful exercise, providing documentation and detail for people who use CESM2 or its output to force ice sheet models, for other ESM groups as an example of good evaluation practice and context, and for cryosphere scientists who might not know what to expect of a global ESM simulation of AIS climate. As primarily a descriptive paper it doesn't contain revolutionary science conclusions, but it is still a very worthwhile contribution and I would consider it a TC paper rather than model development that might be more suited to eg Geoscientific Model Development. It is well written and organised with a good level of detail. I would recommend publishing it with minor revisions.

General comments

Some suggestions to consider for those minor revisions that would improve the utility of the paper:

Obviously this paper focuses mainly on factors local to the AIS surface, but would also be nice to see a little more assessment of the regional climate away from AIS (cf the metrics used in assessing GCM climate plausibility for AIS SMB in section 2.2 of Barthel et al. 2020, <https://doi.org/10.5194/tc-14-855-2020>) that will have a significant influence on eg the amount of precip that gets tracked in from the ocean, or warm air advection. It would be good to have an attempt at attributing improvements wrt CESM1 to either the large-scale CESM2 climate or the changes in surface modelling.

Whatever the cause, the improvements in match to the reference products at the end of the 20th century wrt CESM1 are very encouraging, and that - along with the potential for improving the sea-level projection capabilities in CMIP that are mentioned - should definitely be highlighted. The flipside though, whose impact I think could be discussed more and might carry as much importance, is what appears to be an very significant discrepancy in the sensitivity of the simulated precipitation to changes in 20th century climate. What credence should users of this model/simulation data put in the projections of future SMB on that evidence? I'm not criticising the simulation, just asking for a more prominent discussion of the implications. I've just read the paper on the PaleoCalibr version of CESM2 (Zhu et al 2021, <https://doi.org/10.1029/2021MS002776>) which claims to have a better/more physical tuning of nucleation around ice in clouds and affects high latitude cloud and general climate sensitivity significantly. Since these cloud microphysics are mentioned as important in this text, it might be nice to have a comment on what, if any, impact they think that tuning might have on their SMB simulation and sensitivity?

The title promises 1850-2100 but I think this is a little misleading:

1) the focus is mostly on evaluation against various high resolution or observationally-based products, so for understandable reasons there's virtually nothing about the situation before the 1970s. The only exception is figure B2, used to establish the change in precipitation trend in the model at the end of the 20th century, but that's a fairly scant use for 120+ years of simulation. There is presumably little that can be said other than presenting the differences with CESM1 for the earlier period - that might be interesting in itself, or it might be better to change the title.

2) everything after 2014, for all climate and surface variables across several different scenarios, is condensed into 1 side of text and 2 figures - the future feels pretty shortchanged as well. What is there is good but it would be nice to see much more detail. For instance, does the pattern of increase in precipitation just match surface temperatures and scale with Clausius-Clapeyron humidity arguments, or is there evidence of something dynamic eg more frequent/vigorous storm incursions. How do the areas of increased shelf melt compare to estimates of which shelves will be vulnerable to hydrofracture from surface melting in the future. eg Kuipers Munneke et al. 2017 (<https://doi.org/10.3189/2014JoG13J183>).

Specific Comments:

line1, abstract: could be more concise and to the point. I'm not sure the first or last 3 sentences are really needed here at all, since they provide general information and broad justification that could live in the Introduction. Some more numbers - integrated SMB, precipitation, runoff, percentage discrepancy from the reference products in the present, projections for the future - would be more useful. I would note the issue with the historical trends in CESM2 here too, as well as the marked improvement in the mean state wrt CESM1.

line23: "attributing to" - should be "accounting for", perhaps?

line40: the word "limit..." gets used a lot in these two sentences

line79, section2.1.2: are the CESM2 mec subgridscale ice elevation classes used for the AIS in either version? I think there were particular parameterisation tunings that were done for the calculation of Greenland SMB in CESM2, at least for the version of the model with interactive ice - eg adjustments to the phase of precipitation for certain land surface type (van Kampenhout et al, 2020 <https://doi.org/10.1029/2019JF005318>) - are they active here?

line106: I may have missed something, but is this approximation for SMB used consistently in the analysis of all the model simulations and evaluation products, or does it only apply to ERA? If this is the formal definition to be used in the paper, it then needs amending for the future results where runoff becomes very significant.

figure1: does panel(d) impart useful information that can't be got from panel(e)?

line137: can the CESM radiation components shown (and other surface energy fluxes) be compared with the RCMs/reanalysis?

line152: are the katabatic winds really well-resolved a 1degree model?

line169: these are the first of the area-integrated quantities, I think. It would be worth saying what the size of the AIS is in each of these different models/products. It's not obvious that a 1degree GCM will represent the AIS with exactly the same extent as a more detailed regional model, a factor which might bias results systematically high or low.

line182: "to too melt" - too /much/ melt?

line202, section 3.4: I know accumulation vastly outweighs other things in the current SMB balance, but we're not actually told a split between accumulation and ablation (whether or not that includes runoff) at any point here? Could something be shown on sublimation? Section 3.3 talks about surface melt, but no note of whether any of that runs off - the runoff and refreeze proportion becomes important in the future section later, so I think it should be mentioned for the present-day too, even if only briefly.

line231: it could be clearer whether these two sentences are talking about CESM2 or reality.

figure6b,c,d): it's not clear what's going on with the ice shelves. Are they excluded from the SMB analysis, and we're only talking about grounded ice here? Is it an issue with just this figure, or throughout the paper? Figure 5 shows melt on the shelves, so they're not excluded from all the analysis

line242, section 3.5: is there a general "future simulations with CESM2" paper that could be cited for more context (eg Meehl et al., <https://doi.org/10.1029/2020EA001296>)?

line246: "SSPx" is an incomplete reference to which emissions scenario is being talked about - eg SSP5-8.5

line247: "1990" is probably a typo

line257: is there any evidence from the 20th century simulation that the available pore space and refreezing in CESM2 are realistic to start with?

line315: "in the latest iteration of CMIP", perhaps - various EMICS, simpler models and even a CMIP6 ESM (Siahaan et al. 2021, <https://doi.org/10.5194/tc-2021-371>) have produced "coupled" estimates of future AIS contributions to sea level rise, for whatever they're worth

line 316: "will be used" - it already has been (Payne et al. <https://doi.org/10.1029/2020GL091741>)!

line 324: I don't think the repeat of the factors in parentheses is required.