Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-155-RC2, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Climate of the Past Discussions
Open Access

Interactive comment on "Evaluating seasonal sea-ice cover over the Southern Ocean from the Last Glacial Maximum" *by* Ryan A. Green et al.

Anonymous Referee #2

Received and published: 10 February 2021

General Comments

This study compares PMIP3-simulated sea ice cover in the Southern Ocean, as well as that from two LOVECLIM experiments, against an updated catalogue of sea ice paleo-proxy data. In that sense, it follows directly on Roche et al (2012) and Marzocchi and Jansen (2017). They focus on the summer season, as that season has had the fewest data constraints in previous studies. Additionally, they correlate the sea ice edge to SST and wind stress curl in the models.

Overall, I find the results of this study underwhelming. Although they provide a lot of results, I feel that not a lot of meaning is derived from them (i.e. Too much of a "figure tour" or "data tour"). Assessing the paleo-proxy data is outside my area of expertise, but the model analysis does not feel like a substantial contribution beyond

Marzocchi and Jansen (2017), who examined Southern Hemisphere sea ice controls in these same PMIP3 simulations. Indeed, I find it concerning that a serious flaw in one of those simulations reported in Marzocchi and Jansen (2017) (that of a bug-related absence of wind-stress feedback on sea ice in MRI-CGCM3) is not mentioned here, even though wind-stress curl is one of the foci of the study. The two additional LOVECLIM simulations do not appear to be used in any significant way to get at causal mechanisms, even though these simulations differ substantially in both the design of the experiments and complexity of the model.

Finally, the authors do not provide much discussion of their results in the context of the previously-mentioned two papers, and given their claim that "the multi-model mean of austral summer and winter sea ice cover seem to provide good estimates of LGM conditions" appears to be at odds with the results presented in those papers for earlier generation (Roche et al, 2012) and the same model (Marzocchi and Jansen, 2017) data, I would have liked a bigger exploration of this difference. I found it hard to interpret these differences on my own due to the small size and indistinguishable lines in the figures and vague descriptions of methodologies.

Specific Comments

intro raises connections w/ SAM – why not connect results to SAM?

Line 55: while Marzocchi and Jansen didn't focus on seasonality of PMIP3 simulations, they did plot the seasonal comparisons btw PI and LGM and discuss the characteristics of the seasonality in the models

Given zonal asymmetries in sea ice edge, usefulness of hemispheric, zonal averages is unclear

Line 67-69: Multiple simulations from same model were averaged over – what if they involved different components? How reflective is the ensemble mean of the behaviour of any one simulation? (look into GISS-E2-R)

Interactive comment

Printer-friendly version

Discussion paper

CC BY

The LOVECLIM simulations that were chosen are not representative of the PMIP3 models for a number of reasons, and the differences in their attributes, the reasons for their selection and the implications for the results are not discussed much here. As a result, the comparison feels artificial and not very instructive. Looking further into the simulations in Menviel et al (2017), I can make a guess as to why these ones were chosen, on the basis of the performance of their carbon cycle models. However, the fact that these simulations were performed with prognostic CO2 concentrations (via a carbon cycle model) rather than prescribed emissions/concentrations, and were spun up in a transient fashion from 35ka BP rather than just equilibrated to fixed LGM conditions as most of the PMIP3 simulations would have been, and had anomalous hosing applied in either or both of the Northern and Southern Hemispheres has bearing on the interpretation of the results. However, only the hosing is mentioned, and the implications of these design choices on the sea ice distributions are not discussed.

I am concerned about the fact that at least one lower-resolution model (e.g. LOVECLIM, whose ocean is nominally 3degx3deg) was interpolated onto a higher-resolution grid (1degx1deg) for plotting the results. This artificially inflates the apparent resolution of the results and thus encourages attempts to interpret changes at smaller spatial scales than the model provides as real. Whether the resolution of any PMIP3 models was artificially inflated in this way is not clear.

Lines 82-97 I am not a specialist in the interpretation of sea ice proxy records, so I found this section confusing. My main source of confusion lay in interpreting the uncertainties related to the apparently weak signals (differences between 1-3% in diatom assemblages), given the RMSEP values were 10%. I'm assuming the two percentages were not referring to the same quantities. I'm not expecting this paper to provide an overview of this method, assuming the references already provide that, but a sentence or two to make these results interpretable to non-specialists would be appreciated.

Why were two months selected to define sea ice maxima and minima? Was this based on prior knowledge, or analyses of the seasonal cycle in the models?

Printer-friendly version

Discussion paper

Lines 104-106 I'm not entirely clear on the methodology here. Firstly, were zonal averages performed in each ocean basin region over the latitudes of 15% sea ice concentrations for each longitude division (= 1 grid cell) or over sea ice concentrations in each latitude band (from which the 15% was then calculated)? And then, when defining an hemispheric sea ice latitude, am I correct in understanding that a zonal average over all longitudes was not calculated and instead, an average (weighted or unweighted?) was performed over the individual ocean basin regions? If this is correct, why was this method chosen?

Lines 118-119 Why does the multi-model mean lead to an asymmetric region of variance north and south of the mean lines? Are you suggesting that the simulations are distributed in a non-Gaussian way? Based on visual inspection, I wonder how the mean was calculated. I'm assuming the multi-model mean was calculated by averaging over the latitude of sea ice margin for each cell's longitude range from each run, but it doesn't seem to match what I see in the figure. For example, between 150 and 180degE, two, maybe 3, runs extend past the latitude of the outermost blue points. That leaves 6ish runs south of those points, but the multi-model mean lies north of the points. If the two runs were far north of the points, I would understand this, but that is not the case. Rereading the text, I wonder if the authors are suggesting they performed a multi-model average of sea ice concentration in every grid cell and then calculated the 15% concentration margin from the ensemble-mean distribution. If those more extensive runs had very high sea ice concentration up until their margins, it might explain the position of the mean, but it's not clear to me how the standard deviations of the 15% line could be derived from this calculation. Since assigning a cutoff to sea ice at the 15% concentration is a non-linear operation, I would expect to get a more Gaussian multi-model distribution for the latitudes of the 15% lines than performing the calculation on the ensemble means of the sea ice concentrations directly.

Given the zonal asymmetries in the summer data and their relative absence in the models (based on inspection of the multi-model mean in Figure 1 and the performance

Printer-friendly version

Discussion paper

of PMIP2 models in Roche et al, 2012), I'd like to see the analysis performed for Figure 2 calculated on a regional basis, rather than based on hemispheric averages. Such an analysis would be more likely to bring out discrepancies between the models and the data than the hemispheric average.

Technical Comments

Two different definitions for LGM time period provided in the abstract and text

I find it very difficult to distinguish between the colours of the different lines in Figure 1, because they are so thin. If thickening was not chosen because of confusion in the plot, I think the line labels can easily be dropped to simplify the figure. Also, without any latitudes and longitudes marked on the plot and only the southern tip of S. America included as a georeference, it takes a lot of work for someone who is not intimately familiar with Antarctic geography to translate the descriptions in the text (most of which seemed to be in longitudes) to their locations on the figure and compare the latitude values stated in the text with those in the figure.

It would be helpful for the boundaries of the ocean basin regions to be marked in one figure. Their names are clear, but precisely where the boundaries between the basins are drawn and their northern and southern extents would be helpful in interpreting the results.

Line 110: Sentence fragment "The proxy"

Line 214: Typo MRI-ESM-P should be MRI-CGCM3

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-155, 2020.

Printer-friendly version

Discussion paper