

# ***Interactive comment on “Sea ice led to poleward-shifted winds at the Last Glacial Maximum: the influence of state dependency on CMIP5 and PMIP3 models” by Louise C. Sime et al.***

## **Anonymous Referee #3**

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Review of "Sea ice led to poleward-shifted winds at the Last Glacial Maximum: the influence of state dependency on CMIP5 and PMIP3 models" by Louise Sime et al

General comments:

This manuscript investigates the Last Glacial Maximum (LGM) southern westerlies, a subject of high interest due to their possible control of the atmospheric CO<sub>2</sub>, whose LGM level is still not fully understood, but also due to their importance in atmosphere-ocean exchanges and ocean circulation. The southern westerlies are predicted to shift poleward under climate warming, and would therefore be expected to shift equatorward during the cooler-than-present LGM. Previous work has shown that this is not the case. This manuscript further analyses the possible implication of sea ice changes in LGM

southern jet changes. It is based on the PMIP3-CMIP5 data, which is the most recent data available from several state-of-the-art models run with the same boundary conditions. For sea-ice, this data is compared to the reconstructed data set of Gersonde et al (2005). The authors show that models which are sufficiently accurate for the present climate in their representation of the jet stream, i.e. for which the position of the jet stream maximum is not too much biased northward, and simulate a realistic sea-ice extension from present to LGM, can show a poleward shift of the jet stream for the LGM compared to present. Although the southern westerlies had been investigated in previous publications, this work goes one step further by 1/ clearly showing the impact on present day biases on the LGM – present changes and 2/ showing the role of sea-ice for those models which simulate realistic westerlies for the present situation. The manuscript is clearly written and illustrated, I am therefore in favour for its publication in *Climate of the Past*, after the corrections and improvements listed below.

Suggested improvements:

1. Since there are not so many models which have provided the complete data set for this analysis (in particular sea ice concentration) the authors may want to encourage the corresponding modelling groups to submit their data to the CMIP5-PMIP3 data base. This might increase the numbers used to infer the relationship between the jet shift and sea-ice edge or SSTs on Fig. 7.
2. Another way to increase the number of models, by using PMIP2 data, could also help, but these models being of an older generation might show even stronger biases?
3. Finally, would the study of the yearly fields help and quantify the uncertainty of the results shown on Fig. 7 (regression between jet position and sea ice edge)? e.g. by using bootstrapping to add information on the variability of the results?
4. One aspect of the manuscript I am very confused with is the seasonal/annual averaging of the data. On the one hand, section 2.1 specifies that “jet diagnostics are calculated for the climatological maximum in the Southern Hemisphere 850 hPa wind

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component” but the season (or month?) is not specified. On the other hand, Figures 2, 3 and 4 show annual means of winds and sea ice. Given the large seasonal march of both the southern westerlies and sea ice, I wonder if it wouldn't be better to either concentrate on a season, or investigate the seasons of minimum/maximum wind/sea ice. Further, when comparing simulated sea ice to reconstructions, this distinction could also be made, as done in Roche et al 2012 using the same sea ice reconstructions and the PMIP2 data. This aspect does need to be at least clarified before publication.

5. The introduction is clearly written, especially on the westerlies, but missed the above reference on previous work on LGM sea ice model-data comparison.

Minor:

- Section 3.1 first paragraph, discussion of Table 2. This discussion mentions the mean and the median of jet changes, but the table itself doesn't give the median. It might actually be easier for the reader to present these results with graphics rather than a table.
- Section 3.3, line 182: are the figures of 7 and 10° in latitude for the Atlantic and Pacific sectors?
- Section 4, line 307: “increase in U”. “U” could be replaced by its full meaning for this conclusion section.
- Figure 1, 2nd line, l.h.s. panel: what is the vertical line at latitude ~-56°S? The information on which time averaging (annual mean?) is also missing (and is missing in the captions of Fig 4, 5, 6).

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