

Interactive comment on “A model comparison study for the Antarctic region: present and past” by M. N. A. Maris et al.

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This is an interesting and novel study which addresses a valuable question: the fidelity of various climate models in simulating the climate of Antarctica. The models which participated in PMIP2 are evaluated against the output of a regional climate for the present day, and against ice core data for the mid-Holocene and the Last Glacial Maximum.

Overall, this is a useful exercise in model intercomparison. The results are likely to be of interest to anyone studying past changes in the climate of Antarctica, and particularly to anyone who might be considering using the output of the PMIP2 models to drive a regional climate model or an offline ice sheet model. The scope of the manuscript is consistent with the scope of Climate of the Past.

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However, I have a considerable number of comments regarding the analysis, interpretation and presentation of the results, which are provided below. I recommend the manuscript for publication, subject to these comments being adequately addressed.

General comments

1. I am concerned at the inclusion of two of the models. According to Table 1, only 3 years of model output was used for ECHAM53-MPIOM127-LPJ and only 20 years for HadCM3M2. These averaging periods are much too short to derive a meaningful climatology from the output of a coupled general circulation model. Unless the authors are able to obtain output from these models that spans at least 50 years, they must be removed from the analysis.
2. The study lacks analysis of the uncertainty in the results. For the model output, confidence intervals should be calculated and provided. Without this information, it cannot be determined whether the temperature and precipitation anomalies provided in Tables 2 to 5 are statistically significant.
3. The fidelity of RACMO2/ANT must be discussed, particularly in regard to its accuracy in simulating temperature and precipitation. Although the authors do provide references, this information is of critical importance to the conclusions of the study and should be provided in the manuscript itself. If RACMO2/ANT has any known biases, the consequences of these biases for the current study must also be discussed.
4. More discussion of the physical and numerical reasons for any model discrepancies should be provided. I have made a number of specific suggestions below.
5. The authors should use terminology more carefully. In general, it would be preferable not to refer to the output of models as “data” (“output” or “simulations” is better, depending on the context). Also, the ice core data should not be referred to as “observations”, particularly not when referring to the interpretation of an isotopic ratio as a climatic variable (“reconstructions” is better).

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Specific comments

1. Title: I suggest that “intercomparison” might be more appropriate than “comparison”.
2. Abstract, line 11: It would be better to say “Antarctic Peninsula”, if this is what is meant.
3. p3586, lines 8-9: The methodologies used to calculate the correlation coefficients, biases and root mean square deviations should be provided.
4. p3586, lines 18-25: The authors should describe how temperature and precipitation are derived from the ice core data, and whether they have performed this themselves or whether the conversion was performed in the studies cited.
5. p3587, line 8: It would be useful to provide the temperature correlation coefficients. This information could be added to one of the existing figures or tables.
6. p3587, lines 24-26: Representation of topography within the models is likely to influence the simulation of temperatures over the ice sheet. This issue should be discussed.
7. p3588, lines 5-6: The rationale for the choice of these four models should be provided.
8. p3588, lines 10-11: This indicates deficiencies in the simulated meridional temperature gradient. This could be discussed.
9. p3588, line 15: Again, the rationale for the choice of models should be provided.
10. p3588, lines 18-20: If all the GCMs exhibit the same bias, this suggests a bias in the output of RACMO. This issue needs to be discussed. Is RACMO consistent with observations in this region?
11. p3588, lines 23-27: This paragraph could be reworded to make it clearer that the “timing problem” is a general issue that affects all the models, rather than just MRI-

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fa. Also this issue could be discussed within the context of the use of “time slice” experiments, which assume that the climate system is in equilibrium with any external forcings.

12. p3588, lines 25: It would be better to say “excessively warm” than “very high”.
13. p3589, line 1: Spatial patterns should not be shown just because they are “interesting”. The aim of this study is to evaluate the fidelity of the models. Are these spatial patterns relevant to this aim? If so, why?
14. p3589, lines 12-13: This sentence is a little speculative. Does it lead to more realistic temperatures or not, according to the results presented in the manuscript?
15. p3589, lines 22-23: Why is the inland location of Vostok significant here?
16. p3590, line 1: “nondeductible” is a poor choice of word; “indistinguishable” perhaps?
17. p3590, lines 8-9: Again, the representation of the topography within the models could be mentioned here.
18. p3590, lines 22-23: Which other estimates?
19. p3591, lines 1-2: This could be expressed better as “suggesting that the representation of cyclonic systems is deficient”.
20. p3591, lines 4-7: The atmospheric component of the CNRM-CM3 global coupled model is a spectral general circulation model. “Speckling” of precipitation can be observed in spectral models, depending on the scheme used for moisture advection. The authors should determine which scheme is used by the CNRM model, and make an appropriate comment.
21. p3591, line 9 and lines 10-11: It would be better to say “Antarctic Peninsula”.
22. p3592, lines 4-6: This is more likely to arise from deficiencies in the meridional tem-

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perature gradient, arising from deficiencies in the simulated meridional heat transport. GCMs do not have a closed energy balance.

23. p3592, lines 7-8: Figure 2 shows that four models underestimate precipitation over the ice sheet, so this statement appears to be incorrect.

24. p3592, lines 12-13: This statement seems very subjective. What criteria is it based on?

25. p3592, lines 14-19. This conclusion is worthy of further analysis. It would be valuable to show scatter plots of MH and LGM biases against present-day bias.

26. p3592, lines 24-28: I agree that changes in elevation could not be taken into account. However, the magnitude of the potential discrepancy that these changes introduce should be discussed. Is it of order 0.1 K, 1K, 10 K?

27. p3593, lines 5 and 8: Again, the criteria used to determine these judgements should be provided.

28. Figures 4 and 7: A different colour palette should be used. The current palette has identical colours for both the positive and negative extremes, which makes the figures hard to interpret.

Technical corrections

1. "last glacial maximum" should be capitalised throughout.

2. p3584, line 23: I think "(near-surface)" should be "near-surface".

3. p3589, line 6: "squares" not "sqares".

4. p3589, line 8: "modelled" not "modeled".

5. Table 2, In the caption, "three" should be "five".

6. Table 4: In the caption, "five" should be "six".

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7. Figures: These should be re-plotted to avoid the gaps at the Greenwich Meridian.

Interactive comment on Clim. Past Discuss., 7, 3583, 2011.

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