Response to Review 1:

This study analyzed the simulated LGM climate relative to the pre-industrial climate in PMIP3 and PMIP4 models with a focus on Australia. The results show that although the models all simulate widespread cooling over this region, they are not consistent in simulating changes in precipitation large scale circulation patterns. Compared to the last version, this manuscript is much improved, with more insightful analysis and discussion on the mechanisms driving hydroclimate change in Australia during the LGM, as well as more robust statistical analyses. However, for the issues I listed below, I suggest that the manuscript undergo another round of revision.

The authors tried to perform a model and data comparison. However, it remains unclear which models agree better with the proxy data, which leads to a rather open conclusion at the end. In my opinion, perhaps the model-data comparison could be discussed in more detail. For example, in the paragraph of line 520, the authors suggest that the proxy-inferred moisture availability is not consistent with changes in P-E from the PMIP models. From Figures 11 and 12 we see that the models do not agree on the sign of P-E over much of Australia. Therefore, I expect some models might agree with the proxy data better than the others for these particular regions.

Response: Thank you for the comments, we have rewritten the discussion in Section 4.2 to include a more detailed model-data comparison at a regional scale.

Other comments:

20: This statement is a little odd. We can use the model-proxy data comparison approach to determine which simulations perform the best. Then by analyzing this set of simulations, we can determine the driver of circulation changes during the LGM.

Response: This last two sentences in the Abstract have been edited to reflect the possible role of model-proxy comparison in selecting the best performing model.

342: There are 12 models, but only 5 models are described for changes in westerlies. What about the other 7?

Response: This paragraph has been expanded to include more discussion of all the models used in the study.

364: Are there dynamic components that may cause the “drying” of the Australian mainland? e.g., shifting of the westerlies away from the region?

Response: This paragraph has now been expanded to include discussions relating to dynamic changes that may be associated with the precipitation patterns simulated over each region.

485: Here you show that the westerly wind and precipitation are weakly correlated, but

what about the meridional shifts in westerlies? You mentioned that some models show a southward shift while others show a northward shift. Do these shifts cause consistent changes in precipitation in the respective models?

Response: Thank you for the suggestion. We have added the plot of latitudinal shift of maximum westerly winds versus precipitation in JJA in models as Supplementary Figure S9b and added a comment in the text to note the weak correlation between westerly wind position and precipitation. We also note that there is no significant correlation at the 95% confidence level, new methods could be used to quantify the meridional shifts in SH westerlies at the LGM (e.g. as proposed by Gray et al., 2023).

Reference:

Gray, W. R., de Lavergne, C., Jnglin Wills, R. C., Menviel, L., Spence, P., Holzer, M., Kageyama, M., and Michel, E.: Poleward shift in the Southern Hemisphere westerly winds synchronous with the deglacial rise in CO_2, Paleoceanogr. Paleoclimatol., 38, e2023PA004666, https://doi.org/10.1029/2023PA004666, 2023.

Response to Review 2:

General comments

This manuscript by Du et al. has been much improved since first submitted, thanks to the diligent commitment of the authors to address the reviewers' comments. The paper reads easily and the parts which were rewritten have clarified the overall structure of the manuscript. I think that this modelling intercomparison study at the LGM is well suited for publication in Climate of the Past, providing an interesting focus on the Southern Hemisphere hydroclimate thanks to the case study on Australia.

I do have a few additional comments to hopefully guide further improvement. Some I have still classified as major.

Major comments

1. Knowledge gap:

(a) the knowledge gap outlined in the abstract (L7 : « The climate changes... remain uncertain. ») is extremely laconic. I would argue that the abstract's objective is also to convince the reader to continue reading, and thus to explicitly present him or her with a problem worth solving. So I would recommend elaborating a bit on the knowledge gap in the abstract as well. [If the authors are limited by abstract length requirements, I think that the relationship between CMIP5 and 6 models and PMIP phase 3 and 4 models is comparatively much less important (and could be explained in Section 1.2 only).]

Response: Thank you for the suggestion. We have expanded the explanation of the knowledge gaps and edited the Abstract as suggested.

(b) The introduction sentence of the knowledge gap on L170-171" The LGM is commonly recognised as a time of global cooling and lower sea levels, best estimates placing this at ca. 21 ka. However, ..." is extremely confusing until the temporal discrepancy is pointed out later in L.174. The knowledge gap is also underdeveloped, to my opinion. Could the authors clarify and elaborate on the knowledge gap, possibly describing the different temporality of the SH regarding the start of the deglaciation, the bipolar seesaw mechanism, etc...?

Response: The knowledge gap has been expanded now, and the discussion of the timing of the LGM in Australia has been removed from this section to avoid confusion.

2. Ending note (L20-22 and L1284-1289): I am not a fan of the 'further analysis is required' statements as it doesn't spell out clear directions of where the research should move forward to make progress on the still unresolved knowledge gaps of the paper. It is a bit of a shame to end the abstract and conclusion on an underwhelming note. Could you maybe provide clearer recommendations for modellers and for experimentalists, based on what we have learnt in this study? To better identify model biases – and better resolve mechanisms, what analysis are we lacking? Which sensitivity tests could be made? As for data, what do we need from data to better constrain models in the Australian regions?

Response: The abstract has been rewritten to more clearly identify the nature of future work required. The conclusions have also been expanded to address some of the comments of the reviewer. However, we feel that as this is a preliminary study, we are not able to fully map out future research directions.

3. L141-145 and Figure 1 / L191 : (a) Placement: The 'Australia case study' is brought to the reader's attention too soon, before it is even justified, and before starting describing the LGM overall climate again. In addition, Fig. 1 shows the different regions that are starting becoming relevant from Section 1.1 onwards. I would advise moving this figure to later in the text.

Response: The figure has been moved to a later position.

(b) Additional proxy information : while useful as it is, Fig. 1 could also be enriched with, e.g., the coring locations of all the proxies described in Section 1.1.

Response: We have added the main locations of the proxies into Figure 1, showing by dots with numbers. The other proxy records without specific indication of locations are described in the text in Section 1.1. As this paper does not focus on proxy records, more detailed discussion of proxy records of LGM climate in the SH can be found in Petherick et al. (2022), which we now cite.

Reference:

Petherick, L. M., Knight, J., Shulmeister, J., Bostock, H., Lorrey, A., Fitchett, J., Eaves, S., Vandergoes, M. J., Barrows, T. T., Barrell, D. J. A., Eze, P. N., Hesse, P., Jara, I. A., Mills, S., Newnham, R., Pedro, J., Ryan, M., Saunders, K. M., White, D., Rojas, M., and Turney, C.: An extended last glacial maximum in the Southern Hemisphere: A contribution to the SHeMax project, Earth Sci Rev., 231, 104090, https://doi.org/10.1016/j.earscirev.2022.104090, 2022.

(c) Justifying the regionalisation : finally, I would like to point out that the connection between the separation into 3 regions and the atmospheric circulation mechanisms explaining the existence of this specific regionalisation is not expicitly made (neither in the legend of Fig 1, in L191, or around L175-177), until the much too late Section 2.3.
Hence, I would suggest reorganizing things (Fig. 1 / Section 1.1 / Section 2.3) so that the relevancy of this regionalisation becomes apparent to the reader in a logical manner.

Response: This issue arises due to our responses to previous reviewer recommendations to combine figures and rearrange material. To resolve the issue, we have moved Figure 1 to the Data and Methods section 2.3 where it more clearly belongs. In Section 1.1 we now refer more generally to the proxy records within three climate zones, consistent with previous work (e.g. Reeves et al. 2013a; Petherick et al. 2013; Fitzsimmons et al. 2013). We think this is a more logical arrangement of the material.

4. The land-sea masks and their potential impacts. (a) It is a bit of the shame that Fig. 1 doesn't show the difference between PI and LGM land-sea masks with different contours.

Response: The LGM land mask contour has been added to Figure 1.

(b) The authors have indicated that they will show the LGM land-sea masks for individual models in SI. I would suggest it may also be relevant to show those with additional contours in Fig. 3 and 9 notably, for I have been constantly wondering about the impact of different coastlines on the simulated variables and their potential disagreements. When the 'maritime' continent is becoming less maritime, leading to less evaporation, how does that affect the precipitation patterns over the whole region? My point is that different models, of different resolution, and using different ice- sheet reconstructions for the LGM, are not likely to have implemented the exact same coastlines e.g. around the Sahul shelf, leading to possible model disagreement in this region (i.e. stippling where the coastlines differ). I would like for the reader to have a chance to examine this potential effect (and if some shows up, for the authors to also discuss this).

Response: The new figs 3 and 9 has been made with the black thick lines indicating the LGM coastlines prescribed in different modern, and the thin black line for modern coastlines. Corresponding discussion is also added in Sections 3.1 and 3.3.

Specific comments

- L12-13 « with a multimodel mean 2.9°C decrease in annual average surface air temperature over land at the LGM compared to the pre-industrial » is so packed with information that it is a bit difficult to read.

Response: This was rewritten.

- L13-14 « while models show consistent patterns of regional cooling » confused me at first as it felt like a repetition of L11-12. Can't both of these informations be presented all at once ?

Response: This was rewritten.

- L16-18 « [...] vary greatly between modes [...] shows little change [...] are also uncertain, with wide model disagreement » : I was confused by the 'also' L18 since a sentence describing surface moisture balance changes was placed between the two sentences pointing out the model disagreement.

Response: "Also" was deleted.

- L296: While the newly added summary paragraph works well, I would add here a transition sentence to the next section to justify the use of models to complete the picture formed with the proxy data, something along the lines of : "In this context, climate models could thus provide precious insights into the mechanisms responsible for this observed climate."

Response: A transition sentence was added.

- Table 1: While the use of the first 100 years of model outputs has been justified in the reviews and in the text, I would suggest avoiding the terms "run length" and "length of simulation" (e.g. in Table 1 or in Section 2.2 title) to refer to the length of the model outputs available on the ESGF. The authors could use something like "output length after spin-up" or some other equivalent so as not to confuse the reader. I would even argue that actually, the third column of Table 1 is irrelevant, for (1) the authors are using only the first 100 years anyway, and (2) only the spin-up duration can give the reader an idea of how well equilibrated the LGM simulations are. So the authors could consider replacing this column with the spin-up duration numbers, or removing it altogether.

Response: The title for Section 2.2 has been changed, and the third column in Table 1 has been deleted.

- Fig. 2: I admit to finding the he first occurence of the 70% stippling peculiar, for their is no stippling appearing in Fig. 2. The authors could either choose a higher standard (e.g. good agreement on the amplitude of the change?), or simply warn the reader with e.g. "A stippling indicating areas where less than 70% of ensemble members agree on the sign of the anomaly has been chosen, consistently with the following figures. As a result of the high agreement between models in terms of the sign of the temperature anomaly, no stippling is shown here."

Response: A sentence was added to clarify the confusion with no stippling in Fig 2 as suggested.

- Fig. 5: Please consider further commenting on Fig. 5 in the text. What do we see in terms of model disagreement? Do we see an increased seasonality at the LGM wrt. PI? The authors could also consider quantifying the model spread or commenting on the obvious two outliers (one PMIP3 and one PMIP4) in the global mean temperature plot.

Response: More discussion regarding Fig 5 was added in Section 3.1.

- L696: Please consider adding a transition sentence to connect the previous section with the one starting now.

Response: A transition sentence was added.

- Fig. 6: I am wondering whether showing wind **changes** as vectors is the best data vizualisation choice. The reader may assume that arrows stand for the winds themselves.

Response: For consistency with all other plots, we show the changes in winds rather than the actual winds. This is also consistent with other papers on the LGM simulations, e.g. Yan et al. (2018) plot wind vector changes.

- L793-795: Please also mention that some models do not show any significant change. Also, I would start with the change of westerly strength (L796) before mentioning the latitudinal shifts, as the latter is not the first metric that usually comes to mind.

Response: The entire paragraph was reframed.

- L829 "as noted in Section 3.2": It was not clearly spelled out in Section 3.2. I would suggest reformulating the transition, so that the writing can flow more in-between sections.

Response: This was a typo and should have referred back to Section 2.3 where the different climatic regions were first discussed. This has been reworded.

- L952: This mention to Table 3 may confuse the reader as to whether he should read Table 3 or Figure 12 first. I suggest it is not necessary here.

Response: This sentence referring to Table 3 has been removed.

- L1063 "While a decomposition of thermodynamic and dynamic drying components of precipitation change is not included in this study, it is evident that the thermodynamic drying response is dominant": (a) Why not? It would be interesting to see such an analysis. (b) On what basis can you say it is dominant (without doing the decomposition)? I may have missed the 'evident' fact which helps conclude this. Please justify, or nuance (for this is a strong statement, comparatively to e.g. L1059 "it is likely that...").

Response: The thermodynamic response would be drying due to cooler temperatures (consistent with Clausius-Clapeyron). The sentence was reframed to avoid confusion.

- L1230: (a) It feels like the winds subsection lacks a clear conclusion, along the lines of "This also reveals that much progress remains to be made with respect to...".
(b) Please also consider refering to this recent paper by Gray et al. (2023): https://doi.org/10.1029/2023PA004666, to elaborate on your discussion.

Response: The paper and the conclusion sentence were added.

- L1279-1280 "again suggesting that caution is required" is redundant with a previous statement, so it does not bring anything to the table. I suggest removing it.

Response: This has been removed.

Technical comments

- L139-140: Why not use 6.1 ± 0.4 °C, to provide uniformity with the previous figures?

Response: This has been modified.

- L305 "a slight increase" of what?

Response: This sentence has been modified.

- In L315 and a couple of other times, the poleward/equatorward shifts are refered to as southward /northward. I think it is best to stick to poleward/equatorward even if the study is focusing only on the SH.

Response: This was corrected.

- L316 equatorward shifts (reversed word order)

Response: This has been modified.

- L318 "more recent" repetition

Response: This has been modified.

- L358 "the" -> their

Response: This has been modified.

- L1142 "has evaluated" -> evaluates

Response: This has been modified.

- L1158 "the models may not have resolved" -> the models do not resolve

Response: This has been modified.

Response to Review 3:

The authors compared models simulations with existing proxy to investigate the climate changes at the LGM over the Australian region, in terms of temperature, precipitation, moisture balance and wind. Compared to the previous version, the current version of the manuscript by Du et al. has been improved and well written. Here are a few basic comments and reviews, mainly on the SH westerlies, for the authors' consideration.

Comments:

1. In Figure 8, the strength of westerly is calculated over all longitudes. But the regional westerly over Australia would be much better since the regional westerly is closely related to the precipitation over the southwest of Australia (as shown in figure S9).

Response: We have replotted Figure 8 to just calculate the strength of westerly winds over Australian longitudes defined in the study (110°E-160°E).

2. In section 4.3, there is no proxy or evidence to illustrate the behavior of the westerlies in LGM. Is there any proxy to indicate the strength or the position of the mid-latitude westerlies? To constraint the characteristics of westerlies in LGM, it would be useful to collect proxy or geological evidence in future, especially the direct evidence, such as the grain sizes of eolian sediments.

Response: We have added future suggestions in Section 4.3 for using the new method of calculating the SST front latitude in climate models to quantify shifts in the westerlies at the LGM, proposed by Gray et al. (2023).

3. For the southern westerlies change, the simulated sea ice is important to explain the westerlies over the Southern Hemisphere (Chavaillaz et al., 2013). Thus, the authors might consider to analyze the simulated sea ice to illustrate the diversity of the westerlies in LGM among CMIP models in future.

   Referrences:
   Chavaillaz, Y., Codron, F., and Kageyama, M.: Southern westerlies in LGM and future (RCP4.5) climates, 2013, Clim. Past, 9, 517–524,

Response: Thanks for the suggestion. We have added the discussion of this reference in Section 4.3 as suggestions for future research.

Minors:

1. In Figure 5c and 5d, the value of MMM of PMIP4 models is larger than each ensemble. Please check the calculation.

Response: Thanks for the reminder. We have checked the calculation, and they are correct.

2. Figures 6, 11 and 12, the texts for the title of each panel are too much and could be simplified. For example, ‘LGM-PI’ might be excluded.

Response: Thanks for the suggestion. We kept it there aiming to clarify that the figures are showing anomaly values, rather than LGM patterns.

3. Line 170, there are two ‘only’ here.

Response: This was corrected.