Response to Review 1:

General response: We thank the reviewer for their detailed and thoughtful comments. We have revised the text in response to the review comments, as outlined below.

The knowledge of the characteristics and mechanism for the climate change over Australian regions in LGM is still not enough. This study investigated the climate changes at the LGM over the Australian region, in terms of temperature, precipitation, moisture balance and wind, based on the output from PMIP3 and PMIP4 simulations. The work might contribute to our understanding of the hydrological change of Australia in ice ages. The following are my comments and reviews for the authors' consideration.

Comments:

1. The uncertainties of model simulations and reconstructions are important information for model-data comparison. It would be better to evaluate the model consistence since there was large model spread. Specifically, the authors could further provide the percentage of model ensembles consistent on of the signal of their multiple model ensembles mean value.

Response: Stippling has been added to all MMM temperature and precipitation anomaly figures (Figure 2, 4, 10, 11, 12) to show model agreement. All other figures show individual models which allows model consistency to be evaluated by the reader.

For the reconstruction, the background information of proxy used here and their uncertainty could be listed in a table. The information of the LGM climate getting wetter or drier and in which parts of Australia based on proxies is still not clear, even though the authors cited others' work in line 459-463. It would be easier to read and make comparison if were there reconstructed data mapped on the plots of model results.

Response: We emphasize that this study is not concerned specifically with detailed data-model comparison of LGM hydroclimate in Australia. Our principal reason for avoiding a quantitative data-model comparison with proxy climate reconstructions of the Australian LGM is that the Australian proxy-based LGM palaeoclimate literature typically has provided qualitative reconstructions, ('drier', 'much drier', 'somewhat drier') and that, where quantitative reconstructions have been provided, they have uniformly been based on comparison of LGM plant distributions with modern plant distributions, or comparison of the LGM occurrence of mobile sand dunes with their modern distribution, respectively. These comparisons, however, have ignored the plant physiological effects of low atmospheric CO_2, which is increasingly recognised as a problem that should not be ignored (Scheff et al., 2017; Prentice et al., 2022). In brief, C3 plants perceive an LGM world with low (180 ppm) CO_2, as much 'drier' than today, so that model simulations with dynamic vegetation typically show widespread forest reduction, even when holding temperature and precipitation at modern values. Moreover, it is increasingly suspected (e.g. Scheff et al., 2017; Roderick et al., 2015) that greater LGM dustiness and dune-mobilisation are secondary effects from reduced plant productivity, via landscape destabilisation.

For these reasons, we believe it is misleading to describe or list the published Australian LGM hydroclimatic reconstructions in detail. We believe it is clearer, to explain in summary form the recently-recognised problems associated with understanding LGM hydroclimate, as described above; and treat the published reconstructions collectively, at the level of a literature, rather than as individual reconstructions.

1. Usually, modeling community use surface air temperature (SAT) instead of surface temperature (ts) to investigate the temperature change, and to explain the related change of circulation and/or precipitation.

Response: We have redone all temperature analysis for surface air temperature (tas) instead of surface temperature (ts).

2. The climate proxy from 28 to 18 ka is compared with the LGM simulations at 21ka (Line 55-56). This may also contribute to the model-data inconsistence considering the extended date of proxy. For example, the variability of climate proxy during 28 and 18 ka may switch between drier or wetter condition than pre-industry and thus make the complexity of model-data comparison. This point could discussed further when necessary

Response: We have checked the dates of the proxy records, this range can be narrowed to 24-18 ka, which is consistent with the definition of the LGM in many previous studies (e.g. Clark et al., 2009).

3. As pointed out by the authors, the sst gradients and related circulation change could explain the precipitation change (Line 309-311). Thus the analysis of sst change (which roughly equals to the ts value over ocean) and model-data comparison of sst could improve the knowledge of the LGM climate change over Australia.

Response: We have added Supplementary Figure S8 showing the surface temperature change for each model over the northern part of the domain. We include both land and ocean surface temperature to show any changes in the land-ocean temperature gradient. The link between these surface temperature changes, precipitation and circulation changes is now discussed in Section 3.3.2.

Line by line comments

Line 42. “Many regions”, could be pointed in details.

Response: This sentence was rewritten to provide more detail of vegetation changes based on proxies and biome models, with citation of Prentice et al. (2011) as the appropriate source. It has been changed to: “There was a large reduction in area covered by boreal and temperate forests in northern mid- to high latitudes, expanded lowland tundra in Eurasia, expansion of savanna and grasslands at the margin of Amazon tropical forests and replacement of some areas of tropical forest in Africa, China and Southeast Asia with savanna, woodland and grassland (Prentice et al. 2011).”

<u>Reference:</u>

Prentice, I. C., Harrison, S. P., & Bartlein, P. J. (2011). Global vegetation and terrestrial carbon cycle changes after the last ice age. *New Phytologist*, 189(4), 988-998. https://doi.org/10.1111/j.1469-8137.2010.03620.x.

Line 92-100. The reconstructed evidence of moisture or hydrocliamte could be compared with model simulations in the section of discussions.

Response: The comparison with hydroclimate proxies included in Section 4.2 is now expanded.

Line 155-157. There were three different ice sheet reconstructions. Thus it's necessary to clarify the information of ice sheet configuration of the four models from PMIP4.

Response: An expanded version of Table 1 has been updated adding one more column listing the ice-sheet reconstructions in individual models. The four PMIP4 models analysed in this study used the ICE-6G_C ice sheet configuration and the eight PMIP3 models used PMIP3 ice-sheet reconstruction (differences between the two LGM ice-sheet reconstructions in Kageyama et al., 2017). This is now mentioned in Section 2.1.

Table 2. In term of vegetation of PMIP4, were there any model using the dynamic vegetation? Please check and make it clear.

Response: Only the CMIP6 AWI-ESM-1-1-LR model is using dynamic vegetation. This is now noted in the text in Section 2.1.

Line 180-182. Usually models use the last 100 years, instead of the first 100 years, to do analysis. Were there any big differences between those two choices?

Response: We apologise for the confusion with the wording of this sentence. We now clarify why we chose the first 100 years of the model run in Section 2.2. We also provide a brief explanation below.

Most climate models used in this study only have 100-year length of simulation based on the number of years of data available on the ESGF (Earth System Grid Federation). According to Kageyama et al. (2017), the models have all been spun-up until equilibrium following the PMIP protocols (refer to Kageyama et al. (2017) for details of the spin-up requirements). At least 100-year data from the equilibrium part of the simulation is required to store on the ESGF (Kageyama et al., 2017). Therefore, the data stored on the ESGF has already reached equilibrium and it is reasonable to analyse the first 100 years (there should be no significant drift, as per PMIP4 protocols).

Line 213-214. The difference between the analyses in the paper with Kageyama et al. may lies in the choice of ts, instead of SAT. Please check.

Response: Thank you for the suggestion. As you note, Kageyama et al. (2021) used surface air temperature instead of ts. We have changed all of our temperature analysis to surface air temperature (tas) to allow a clearer comparison between our study and previous work.

<u>Response to Reviewer 2:</u>

General response: We thank the reviewer for their comprehensive and constructive comments on the manuscript. We have revised the text in response to the review comments, as outlined below.

General comments

In this study by Du et al., the authors propose a modelling intercomparison study of the simulated surface temperature, precipitation, moisture balance and winds at the LGM, focusing on the Australian region.

Such a study would fit well within the scope of Climate of the Past. The case study of Australia is interesting for several reasons such as the impact of sea level change on coastlines, the specific location (SH, in proximity to the Maritime Continent and Southern Ocean), and the overall disagreement of models and proxies especially concerning the latitudinal shift and variation of strength of the westerly winds, which seem both poorly represented in PMIP models (Chavaillaz et al., 2013) and poorly constrained by conflicting paleodata records (Kohfeld et al., 2013). The manuscript is also clear and an easy read.

However, as such (and as is often the case with intercomparison studies), the paper reads as very descriptive and superficial, so we are struggling to learn something new. Knowledge gaps, uncertainties in both model and data and processes/mechanisms are either barely mentioned or simply not highlighted enough so when we reach the end, the impression is underwhelming.

I am providing more concrete illustrations as to what could be improved and how with points 1-3. I also have suggestions related to methodology in points 4-8. So I would like to recommend a number of improvements before publication, in the hope of helping give this study more weight.

1. Please elaborate on the reasons why a case study of the past climatic changes over Australia is interesting. It would be great to mention climate processes that are key in this region. An example since dust transport is mentioned (L93): does this aridity have the potential to significantly enhance iron fertilisation in the ocean?

Response: In the Introduction (Section 1), we have now added more discussion of why this case study is interesting with reference to key climate processes in the region. While the issue of dust transport and iron fertilisation is beyond the scope of the present study, we now include discussion of the major changes in Southern Hemisphere climate and circulation which may have influenced Australia during the LGM and also the relevance to interpretation of records of human occupation and changes in biogeography at this time.

2. Consider adding one or two last sentences to the abstract and a paragraph in the discussion/conclusion to give the reader a broader perspective and hindsight on what we have learned and how significant are these new findings. A few questions to help brainstorm: So what? In the basis of the existing literature and these new findings, have we achieved a better understanding of the processes which influenced the past Australian regional climate? If not, what are we missing? Do we understand the model response to LGM forcings? What does it entail?

Response: Thank you for your useful suggestions. We have substantially edited the Abstract and Discussion to include more discussion of the significance of the findings.

3. Please underline the knowledge gaps in the introduction. As such, the introduction is very descriptive (not impactful), with a structure (global changes / changes in different regions of Australia) that doesn't help guide the reader very logically towards understanding the knowledge gaps, their importance, the scientific question and the methodology used in this study. If the authors would like to keep this regional description structure, then it would be welcome to also point out the contrasts between these different regions, and also with the global climate, with a few short sentences to conclude this subsection.

Response: The Introduction has been substantially rewritten to more clearly explain the significance of the study in relation to existing knowledge gaps. The proxy section (Section 1.1) retains the structure of northern/southern/central Australia as this follows many previous proxy-based studies examining regional changes in Australian climate during the LGM (e.g. Reeves et al., 2013; Fitzsimmons et al., 2013; Petherick et al., 2013, etc). We have now added a summary paragraph to Section 1.1 explaining the agreement and disagreement between regions and records.

Still, I feel like the sometimes conflicting proxy records and the 'uncertainty about the drivers of the LGM climate changes' (L74-75), the 'ambiguous results' of models (L117) should be arguments brought to the reader's attention in a more convincing order to justify the need of this particular intercomparison study and its methods. For exemple, it is not clear what is the advantage of using an intercomparison method, nor for which reasons modellers simulate the LGM period (L102-107). It is also not clear why the authors chose to examine these three specific climate variables. I believe there are ways to reinforce the visibility of the scientific reasoning behind this approach.

Response: We note that a model intercomparison approach is widely used in numerous studies of PMIP LGM and other PMIP simulations. We examine the most common variables required to characterise climate (temperature, precipitation, wind). We now more clearly explain the specific motivation for the study in Section 1 and the design of the study in Section 2.

4. Only PMIP4 outputs available on the ESGF were included. This is a bit of a shame because it limits comparison with the Kageyama et al. (2021) results, and the model ensemble size (and therefore the robustness of the results). CESM2 is also excluded for very good reasons (L143-144), but I believe the authors have found the source of the exagerated cooling in a cloud microphysics parameterization and run a corrected simulation. This is of course up to the authors, but they could consider contacting both the Kageyama et al. (2021) and Zhu et al. (2021) authors to request the model outputs. This would also enable the authors to compare individual model versions (CMIP5 vs CMIP6) and discuss potential improvements between the two generations. For now, only MPI-ESM is in the two ensembles.

Response: Thanks for your comments. We decided to make use of only those models which are publicly available via the ESGF to ensure our results were readily reproducible. We follow the standard approach in CMIP climate model studies which typically only use publicly archived simulations. Many other PMIP-based studies have also used this approach, e.g. a recent paper on LGM ITCZ changes from PMIP3/4 models used a similar set of models (Wang et al., 2023).

Regarding CESM2 models, we had some trouble linking the available CESM2 model simulations on ESGF with documentation and relevant publications. We now include the CESM2-WACCM-FV2 model as this model has pr, tas and ua, va variables available on ESGF

(CESM2- FV2 did not have the "pr" variable). All figures and tables have been updated to include this extra model.

The large-scale comparison between CMIP5 and CMIP6 models was provided in Kageyama et al. (2021) so we do not need to repeat this analysis. We do not assume that the two generations of a particular climate model (e.g. MPI-ESM) will produce a similar simulation of LGM climate, and we do not wish to focus on comparing generations of specific models – this is typically done by the relevant modelling groups.

The focus of the present study is to summarise the simulation of LGM climate of Australia from available PMIP3 and PMIP4 models. This information is of interest to the Australian palaeoclimate community and will provide a basis for comparison with numerous proxy-based reconstructions of standard climate variables such as surface temperature and precipitation, as outlined in the Introduction.

Reference:

Wang, T., Wang, N., & Jiang, D. (2023). Last glacial maximum ITCZ changes from PMIP3/4 simulations. Journal of Geophysical Research: Atmospheres, 128, e2022JD038103. https://doi. org/10.1029/2022JD038103

5. The authors mentions that all model outputs are regridded (L170), and it seems that all the following analysis use these regridded outputs. Of course, this is necessary to compute and plot the multimodel mean, but I am wondering whether it would be worth extending the use of the model native grids when plotting the individual maps and wind profiles. Is the latitudinal shift of westerly winds affected by the model resolution? Are there differences in the land-sea mask of each model which could impact the simulated temperature and precipitation patterns? Please consider plotting the PI or LGM land-sea mask (e.g. as a grey or dashed contour) on maps.

Response: Thanks for the suggestion. We have recalculated boundary lines between SH easterlies and westerlies on the native model grids (old Table 4), however, we decided to remove it from the paper. This is because the results differ quite a lot on the different grids, and we think they are not robust enough to be included. The whole Section 3.2 has been rewritten, including adding Figure 6 showing MMM seasonal wind changes and Figure 7 showing JJA wind changes for each model. This allows a more general discussion of changes in winds without relying on metrics of the position of the westerlies.

Regarding the influences from the land-sea masks in different models, we now plot the LGM land masks for individual models in Supplementary Figure S1 to show the different LGM land areas configured in each model. We decided not to plot the land masks over the maps as this would make it difficult to see the details of the variables plotted.

6. On maps, the authors should also represent the multimodel agreement significance as hashes (when >90%) and the proxy data as scatter points, whenever quantitative reconstructions can be found. I consider important that the reader is able to compare visually the performance of the models with the available proxy data, especially as the authors conclude that there is a general good agreement between model and data.

Response: We have now added stippling to all MMM figures to show areas of model agreement. As noted in our response to reviewer 1, we believe that the sign of the change from many hydroclimate proxy records is uncertain, especially when taking into account the

CO_2 effect on vegetation records, and therefore we refer to the literature but don't include proxy records in our plots.

7. The authors mention using the first 100 years of simulation for the analysis. Why not the last 100 years? Please check that the simulations are in equilibrium, e.g. by computing the drifts.

Response: As for Reviewer 1, most climate models used in this study only have 100-year length of simulation based on the number of years of data available on the ESGF (Earth System Grid Federation). According to Kageyama et al. (2017), the models have been spun up until equilibrium following the PMIP protocols (refer to Kageyama et al. (2017) for details of the spin-up). At least 100-year data from the equilibrium part of the simulation is required to store on the ESGF (Kageyama et al., 2017). Therefore, the data stored on the ESGF is already in equilibrium. We do not believe it is necessary to calculate model drifts ourselves as this has presumably been done by the PMIP modelling groups.

8. The paper would deserve more quantifications. An example is in L273: 'Some models show weakening and other model show strengthening but there are other instances (e.g. L372). It would be great to provide precise figures, e.g. phrasings like '5 models out of 12 show a weakening of at least 20%...'

Response: To provide a quantitative comparison as suggested, we would need to calculate the change in winds over a particular domain. The spatial pattern of wind changes as shown in new Figure 7 is quite heterogenous, with some models showing regions of both increased and decreased zonal winds over southern Australia and the adjacent ocean. Given this complex spatial response, we do not want to provide a subjective "area average" wind change, but simply to point out the lack of model agreement for changes in zonal wind strength over this domain. Therefore, we think the qualitative description is adequate.

Specific comments

L2, L12 and L13: 'changes at the LGM', 'to cool by 2.6 at the LGM' and ,'decreased'. Unlike in the rest of the paper, it is sometimes unclear in the abstract that we are mentioning changes relative to the PI. This has to be indicated in some way (e.g. LGM-PI anomaly, with respect to PI…) or else the verbs are inconsistent with the time direction.

Response: The wording has been changed to avoid confusion.

L13-14: Why are the changes in temperature and precipitation indicated over two different defined regions?

Response: The calculation for MMM precipitation changes was over a smaller domain aiming to exclude the land areas between 0 to 10°S to allow comparison with previous studies focused on northern Australian rainfall. The has been removed from the Abstract to make way for other material.

L18-19: I find the sentence explaining the potential reasons for model-data disagreement to be rather vague and could be reformulated.

Response: The Abstract, including this sentence, has been rewritten to improve clarity as suggested.

L23: The time window proposed for the LGM is unusual and only justified later in the text.

Response: We have changed the LGM time to "~21,000 years ago" here to avoid confusion and later introduce the extended LGM concept.

L31: Why such a gap between the Annan et al. (2022) and the Tierney et al. (2020a) estimates?

Response: This disagreement is due to the "choice of prior", i.e. the particular climate model used in the Tierney et al. (2020) study, as discussed by Annan et al. (2022). We now briefly note the dependence on method.

Fig 1 and 2: These figures do not bring a lot of information to the table. The authors could consider combining them, combining Fig 1 with e.g. Fig 3 (the land-sea mask could be indicated on another map with a grey contour), or enriching them with more information (e.g. the Sunda and Sahul shelves mentioned in L34 could be annoted on the map to show the reader their location). As for Fig 2, consider using a different style (than the red contour, e.g. hashes) for the southwest box.

Response: Thanks for the suggestion. We have combined Figure 1 and Figure 2 to make a new Figure 1 with the LGM land mask for CCSM4 model as background shading. The southwest box is plotted in hashes.

L85: The authors could elaborate on the reasons why they used the word 'possibly'.

Response: According to Kohfeld et al. (2013), "If a single cause related to the southern westerlies is sought for all the evidence presented, then an equatorward displacement or strengthening of the winds would be consistent with the largest proportion of the observations." This is now rewritten as "perhaps consistent with" to summarise the findings of the Kohfeld et al. (2013) study.

L110: 'not fundamentally different' could be true for the variables examined in the study and not others. Please check if this is the case for all variables (including ocean circulation).

Response: This paragraph is now deleted. However, in relation to combining the PMIP3/CMIP5 and PMIP4/CMIP6 models into a single ensemble, we feel that this is a reasonable approach given that the sample spread of PMIP3 and PMIP4 models overlaps for all the variables of interest in this study (except for CESM2-WACCM-FV2 temperature). This same approach is used in numerous studies, e.g. Brown et al. (2020); Wang et al. (2023). The alternative option would be to examine PMIP3 and PMIP4 LGM simulations separately, which we do not think would provide greater insight and which would result in two very small ensembles.

Table 2 does not feel very necessary. The authors could consider moving it to SI to save some space.

Response: Table 2 summarises model boundary condition information from Kageyama et al. (2017) for PMIP4 models and from the PMIP3 website for PMIP3 models and therefore we feel that it is worthwhile to include.

L168: Why use both ts and tas?

Response: We have now changed all temperature analysis to "tas" rather than "ts" to allow a clearer comparison between this study and previous work (following Reviewer 1 recommendation).

Fig 4 / Table 3: What is the GMST simulated by these models? Are the models which are cold on a global scale also the ones simulating cold temperatures over Australia?

Response: This is a good question. We have added the GMST values to new Figure 5 (reproduced from Table 3) to allow comparison between global temperatures and Australian temperatures and also comment on this relationship in the text in Section 3.1.

Table 3: Tables are not great to visualize data (also true for Table 4 and 5). The authors could consider a different type of plot to show the reader the large intermodel differences in the seasonal amplitude (not commented in the text?) in a single glance.

Response: Thanks for the suggestion. We have changed Table 3 to a scatter plot (new Figure 5) which is similar to Figure 1b from Kageyama et al. (2021) to compare the seasonal and global changes in individual models. Table 4 has been removed. We retain Table 5 (new Table 3) because that it is easier to compare each column in the table to identify differences between precipitation changes and P-E changes and to see values averaged over land only.

L250/L269: Please consider using transitions between subsections (here as in other instances). It is the opportunity to remind the reader of your scientific reasoning (e.g. how these variables are linked).

Response: Thank you for the suggestion. We have tried to add transitions between subsections where appropriate.

L272: I am wondering whether Fig S1 which shows very large model disagreements should not be part of the main text. Also, please explain why you chose to plot the JJA season specifically.

Response: The JJA season is when the westerlies bring rainfall to southern Australia, as explained in the text. We therefore wanted to see whether there was a shift in the westerlies in this season in order to help understand changes to Australian climate.

Old Figure S1 has been added to the main text (new Figure 7 in Section 3.2), and the whole section has been rewritten.

L304-305: 'In JJA the SH westerlies shift equatorward'. Would it be worth it to also investigate the seasonal shift of the westerlies in Sect. 3.2?

Response: We explore the shift in the position and intensity of the westerlies in Section 3.2 with a focus on the SH winter (JJA) season. We have also added a new figure (new Figure 6) which shows the MMM change in 850 hPa winds and convergence for each season.

L397-398: Does this correspondance hold for all models?

Response: There is higher model agreement over northern Australian precipitation change for MAM than for other seasons – this is now indicated with stippling in the relevant figures. The drying occurs over the region of cooler land, particularly the exposed Sahul shelf in a large majority of models. It is not possible to include seasonal plots for all models for all variables or the paper and Supplement would be unreasonably long.

L412-413: Does this relationship hold if you use the change in strength of westerly winds over the same region?

Response: This Section has been rewritten and the scatter plot removed as the relationship was not statistically significant.

L463-465: I will make a subjective comment here. While this is a valid reason to criticize the proxy records (well-explained in introduction), I feel like modellers should maybe not be too critical of proxy uncertainties when such large intermodel differences are observed. The primary reason why we are observing this model-data disagreement might be that, well, models are wrong. I will also point out here that the discussion and especially the conclusion seem lenient with models. I would expect the large intermodel difference observed to reflect a poorly-represented process.

Response: Thanks for this important comment. As you point out, given the model disagreement, it is indeed reasonable to argue that at least some models are wrong. We are interested in finding areas of model-proxy agreement, and thus focus on areas where models show robust agreement. However, we now also expand the discussion of possible model biases and uncertainties.

L467-470: Could you discuss the potential reasons why you can find a displacement of the boundary line but no consistent latitudinal shift in westerly winds? Do you have any idea?

Response: Section 3.2 and Section 3.3.2.1 (winds) has been rewritten. As noted earlier, the table of boundary lines was removed (old Table 4), instead, a new figure showing the MMM LGM seasonal 850 hPa wind change (new Figure 6) and the old Figure S1 showing individual model 850 hPa wind change in JJA (new Figure 7) were added into Section 3.2.

Fig. 11: Consider different marker styles or colors for individual models or model generation (CMIP5/6).

Response: Section 3.3.2.1 (winds) has been rewritten and the old Figure 11 has been removed from the text to the new Supplementary Figure S9.

Fig. 6: It would make sense to put MPI-ESM-P (CMIP5) and MPI-ESM-LR (CMIP6) in the same column on Fig 6 so that it is easier to compare the two generations visually.

Response: As discussed earlier, we do not assume that the two generations of a particular climate model will produce a similar simulation of LGM climate, and we do not focus on comparing generations of specific models in this study. Therefore, we prefer to arrange models alphabetically within CMIP generations.

Technical corrections

L16-17: ‘many regions‘ is unclear

Response: This sentence was rewritten to provide more detail of vegetation changes based on proxies and biome models, with citation of Prentice et al. (2011) as the appropriate source. It has been changed to: “There was a large reduction in area covered by boreal and temperate forests in northern mid- to high latitudes, expanded lowland tundra in Eurasia, expansion of savanna and grasslands at the margin of Amazon tropical forests and replacement of some areas of tropical forest in Africa, China and Southeast Asia with savanna, woodland and grassland (Prentice et al. 2011).”

Reference:

Prentice, I. C., Harrison, S. P., & Bartlein, P. J. (2011). Global vegetation and terrestrial carbon cycle changes after the last ice age. *New Phytologist*, 189(4), 988-998. https://doi.org/10.1111/j.1469-8137.2010.03620.x.

L26: ‘glaciers‘ instead of ‘glaciation‘

Response: Thanks. This was corrected.

L43: ‘reflect‘ or ‘are associated with‘?

Response: We have changed the wording as suggested.

L44: Consider replacing ‘related to combinations‘ with something like ‘caused by a combination‘

Response: We have changed the wording as suggested.

L45: 190 ppm in Table 2

[Response]: Thanks for pointing it out. The 180 ppm in L45 are summarised from proxy records, whereas Table 2 shows the IPCC configuration values for the LGM experiment.

L110: ‘drier conditions‘ would work but not ‘drier changes‘

Response: This sentence was deleted.

L121: ‘differences‘ or ‘gaps‘ would fit better than ‘variations‘

Response: We changed the wording to “disagreements”.

L124: ‘more recent‘ instead of ‘newer‘

Response: Changed as suggested.

L144-145: ‘Furthermore, the PMIP4 protocol highlighted…‘

Response: (Line 155) Changed as suggested.

L170: 10 m

Response: (Line 172) Changed as suggested.

L328: What is the Top-End region?

Response: We have changed this to “central northern Australia” as the term “Top End” is only known in Australia.

Reviewer#3:

General response: We thank the reviewer for their helpful comments on the manuscript. We have revised the text in response to the review comments, as outlined below.

General comments

Climate change in the Southern Hemisphere is poorly understood, and large model biases are known to exist. Studying how climate has changed at the LGM may provide unique insights into the climate dynamics of this region. This manuscript investigates changes in temperature, precipitation, and wind over Australia at the LGM using a subset of PMIP3 and PMIP4 models and compares these changes to existing proxy data. Such a study could be helpful in improving our understanding of Australian climate.

The analysis is generally okay: the authors looked at the climate response in individual models, ensemble mean, and seasonality. However, I think the authors could have added some more in-depth analysis or discussion. One thing they can do is to expand the inter-model agreement (hatching the maps of ensemble mean could be helpful), and consider how model disagreement may affect the ensemble mean values.

Response: Thank you for your suggestions. Stippling has been added to all MMM temperature and precipitation anomaly figures (Figure 2, 4, 10, 11, 12) to show model agreement. We also expand the discussion of model disagreement in Sections 3 and 4.

I also think that the mechanisms for changes in temperature, precipitation, and wind are not adequately discussed. Please see my specific comments.

Response: Thank you for your comment. We agree that there were inadequate discussions of the changes in temperature, precipitation and winds. We have substantially rewritten these sections, and added new figures showing seasonal changes in winds and wind convergence which expand the discussion of mechanisms. We have responded to your specific comments below.

In addition, I think the authors should do their due diligence to acquire model output from all PMIP4 models.

Response: Thanks for your comments. As noted to reviewer 2, we decided to make use of only those models which are publicly available via the ESGF to ensure our results were readily reproducible. We follow the standard approach in CMIP climate model studies which typically only use publicly archived simulations. Many other PMIP-based studies have also used this approach. We have now added CESM-WACCM-FV2 to the CMIP6 set of models and revised the figures and discussion accordingly.

In terms of presentation, the manuscript is structured logically. But the color scales for showing hydroclimatic anomalies could be improved such that the map colors are not overwhelmed by the changes at the coast to make it easier to see changes over the continent. And a better integration of data-model comparison could be achieved by showing the proxy-reconstructed changes in the map of simulated changes.

Response: Thank you for the suggestion. We have tried to modify the color scales of hydroclimate plots by changing to a smaller range of colorbar so that the values over land are more easily seen. However, it is difficult choose a color scale for hydroclimate figures which allows all areas to be clearly seen. We include the old Table 5 (new Table 3) to show average changes over land for this reason.

Regarding the proxy-model comparison, as noted for reviewer 1, we have noted already that the sign of the change is uncertain, especially when taking into account the CO_2 effect on vegetation records, and therefore we refer to the literature but don't include any proxy records in our plots.

Specific comments

The Abstract ends abruptly by describing changes in winds, whereas here it should provide the readers with some key implications or take-home message of this paper.

Response: The Abstract has been rewritten to more clearly summarise the key results of the paper.

47: Ujvari et al 2018 is not an appropriate reference, as it does not talk about changes in dust at the LGM.

Response: This reference has been removed.

61: Many of these referenced papers did not use PMIP4.

Response: This sentence has been corrected to refer to PMIP4 studies only.

66: You did not mark these regions discussed here in Figure 2. Maybe use consistent terminology here as the rest of the paper.

Response: Thanks for the suggestion. We have changed the naming in Section 1.1 to follow the same terminology as in the Figure and the rest of the paper.

74: Reference for the fire study?

Response: The information comes from Rowe et al. (2020) who examined microcharcoal in the Girraween Lake sediment record as an indicator of landscape fire. This sentence follows the previous sentence summarising results from Rowe et al. (2020) but we now add a second citation of the paper in this sentence.

77: You cited a wrong Denniston et al (2013) paper. The correct one is:

Denniston, R. F., Wyrwoll, K. H., Asmerom, Y., Polyak, V. J., Humphreys, W. F., Cugley, J., ... & Greaves, E. (2013). North Atlantic forcing of millennial-scale Indo-Australian monsoon dynamics during the Last Glacial period. Quaternary Science Reviews, 72, 159-168.

Note that in the paper you cited, the C126 speleothem shows more positive d18O and d13C values at LGM than the late Holocene, which might suggest drier glacial conditions.

Response: Apologies. The correct reference is now be provided, and the sentence modified to better reflect the information shown in the speleothem study.

143: This statement is incorrect: Zhu et al. (2021) only assessed CESM2-CAM6, the "low top" version of CESM2, not the WACCM version.

Response: We apologise for the incorrect statement. We had some trouble linking the available CESM2 model simulations on ESGF with documentation and relevant publications. We now include the CESM2-WACCM-FV2 model as we now understand this model does not have an unrealistic climate sensitivity.

156: Do these different ice sheet configurations affect the Australian climate at LGM? Did you use them in your study?

Response: A new Table has been added which gives information on ice-sheet reconstructions for individual models. The PMIP3 models used PMIP3 ice-sheet configurations and the four PMIP4 models used in this study all used the "ICE-6G_C" ice-sheet reconstruction. There may be influences on the simulated LGM climate due to the different ice sheet reconstructions used, although this is not likely to be large given that Australia is remote from the regions of expanded ice sheets. We therefore do not focus on comparing the role of difference ice sheet reconstructions.

180: Why do you choose the first 100 years? Models need time to reach new climate equilibrations in response to external forcings. I would use the last 100 years if possible at all.

Response: Thanks for the suggestion. This has been justified for reviewer 1 and 2 as well. We are using the first 100 years due to the reason that the simulations public on ESGF are already in equilibrium so there will be no significant differences for whether it is the first or the last 100 years. In many cases, only 100 years were available from ESGF. We now explain this more clearly in Section 2.2.

185: specify it is austral summer/winter. I also think this is where you can describe the regional climate systems in more detail. i.e., winter precipitation in the south is associated with the westerlies, summer precipitation in the north is associated with the monsoon.

Response: Thanks for the suggestion. We have expanded the description of the regional climate systems and specified austral summer/winter.

241: If "land areas warm more than surrounding oceans" during DJF and SON is the case, why DJF and SON show opposite signs in temperature change over Sahul? Are there other mechanisms that could cause this change in temperature?

Response: The paragraphs discussing seasonal temperature change have been rewritten to clarify the results. There were a number of points which required better explanation.

245-250: How do these analyses relate to your results in Figure 5? If there is enhanced cooling in SON and reduced cooling in MAM, why Fig 5 shows more cooling in MAM and less cooling in SON?

Response: This paragraph has been deleted as the insolation anomalies are not helpful in explaining the seasonal temperature anomalies.

311: What is this "SST gradient"?

Response: This sentence was deleted.

395-396: This statement does not make sense. Fig 5 shows DJF cooling and SON warming over northern Australia, why does it case wetting in both seasons? What is the "response to changes in seasonal heating" and "changes in atmospheric circulation" here?

Response: The discussion of drivers of change in rainfall has been rewritten to clarify. New figures showing changes in 850 hPa winds in Section 3.2 (new Figures 6 and 7) assist to show links with changes in offshore/onshore circulation and wind convergence.

414: p = 0.082 suggests that the correlation is not significant or "moderate" – it is insignificant. By the way, I wonder how do changes in precipitation and the northward displacement of easterly-westerly boundary correlate.

According to your findings, what is the mechanism for changes in winds?

Response: Thank you for this comment. We have now rewritten this section to indicate that the correlation is not statistically significant at the 95% confidence level. We also add a broader discussion of changes in westerlies and Southern Australian precipitation.

Technical corrections

268: You don't need a 3.2.1 subsection here

Response: This was removed as suggested.

323: Figure S4 is MMM seasonal anomalies for LGM - PI evapotranspiration, not precipitation.

Response: This was corrected.

397: to the => to the

Response: This was corrected.

403: should be 3.3.2.2

Response: This was corrected.