

Interactive comment on “The UK contribution to CMIP6/PMIP4: mid-Holocene and Last Interglacial experiments with HadGEM3, and comparison to the pre-industrial era and proxy data” by Charles J. R. Williams et al.

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To whom it may concern,

Re: Response letter to Reviewer 2, after submission of manuscript “The UK contribution to CMIP6/PMIP4: mid-Holocene and Last Interglacial experiments with HadGEM3, and comparison to the pre-industrial era and proxy data” by CJR Williams et al. to Climate of the Past (PMIP4 Special Edition).

I extend my sincere appreciation to the Reviewer 2 for their thorough examination of

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my manuscript, and their detailed and highly constructive comments. I propose to address all of their concerns, both minor and major, so please see attached for a revised manuscript, still with the Track Changes included, to show my proposed modifications. I also attach a tidy version of this manuscript. Please note that the line numbers shown here relate to the Track Changes version.

Here, I address the reviewer's suggestions, comment-by-comment. I have also attached a PDF version of this letter, where the reviewer's comments are italicised and in a smaller font, and my corresponding response follows in a standard font.

I very much hope that my responses will satisfy the reviewer.

Yours faithfully,

Dr Charles JR Williams, and co-authors

— REVIEWER 2

MAJOR COMMENTS:

1. It shall be elaborated what is new in this paper in terms of method, result and conclusion as compared to previous studies. Data model comparison in SST data and the question of seasonality could be more elaborated. It is not understandable that the SST comparison has not been performed on the MH experiment, although the data quality is higher and especially the dating uncertainty is much lower. Uncertainty is mentioned quite often, but not really elaborated. For the LIG, one could follow ideas outlined in Pfeiffer and Lohmann (CP) dealing with seasons and dating. For the MH, several data sets are available (e.g. Alkenone and Mg/Ca), again with uncertainties in the season or recorder depth.

Sentences have been added throughout the manuscript, including the abstract, introduction, methodology and conclusion, to elaborate and emphasise the novelty of this study. In short, we explain that although older versions of the UK model have been included in previous iterations of CMIP, and although present-day and future simula-

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tions from this model are included in CMIP6, this study is completely new because it is the first time this version of the model has been used to conduct any paleoclimate simulations. Give that these paleoclimate periods are out-of-sample in that they were not used in any way to tune or develop this model, these simulations provide a critical independent evaluation of the model's strengths and weaknesses.

Regarding the comment about including a model-data comparison with mid-Holocene SST data, we note that the manuscript already contains 5 separate datasets: i) land-surface temperature from the mid-Holocene, ii) land-surface precipitation from the mid-Holocene, iii) SST from the Last Interglacial (from 2 separate sources), and iv) precipitation from the Last Interglacial. The manuscript is already quite long, and we feel that the addition of more mid-Holocene SST data would not bring added information to the study. Moreover, another study (involving many of the co-authors here) is currently under review, looking specifically at the large-scale features during the mid-Holocene from CMIP6 models, including ours: Brierley et al. (2020). 'Large-scale features and evaluation of the PMIP4-CMIP6 midHolocene simulations'. Clim. Past. Under review. That study includes a significant model-data comparison section, including Holocene SSTs, and therefore we propose not to add any more model-data comparison in this paper, but rather to direct the reader on (lines 352-354).

2. The paper is too descriptive and focuses only on simulated temperature and precipitation. As a special contribution to CMIP6/PMIP4 is based on a single model, I would expect more comprehensive analysis, like the atmospheric and oceanic circulation, ocean states, and the potential relationship or mechanisms between different components. With such I believe the paper will meet the high standard of CP.

An example of atmospheric circulation changes (Figure 5) was already included in the original version of the manuscript, but this has now been elaborated in the text (lines 546-565). Moreover, in agreement with the reviewer, a measure of oceanic circulation has now been added (Figure 4), namely an example of the meridional overturning circulation. We find that there is almost no change in ocean circulation between the PI,

mid-Holocene and LIG simulations.

3. The authors show precipitation only for Africa. As a paper contributing to the CMIP6/PMIP4, it shall show the model behavior on global rather than regional scale.

This has now been done, showing both precipitation and temperature at both global and Africa-wide scales

MORE SPECIFIC COMMENTS:

1. Lines 94-104: This paragraph describes the previous studies on the modeled and observed MH and LIG states, which I find is too brief. As there are so many modelling studies and proxy papers, and this is directly linked to the present manuscript, thus I suggest to make more complete references. It is suggested to split the texts into two paragraphs, one describing the previous simulation results, the other the proxy issues.

This has now been addressed, with this section being expanded to include a number of other studies, as well as being divided into firstly a paragraph on the proxy data, and then a paragraph on modelling studies (lines 213-155)

2. Lines 106-108: The authors mention that the past warming are indeed different from future warming, as they are driven by quite different thermal forcing mechanisms, orbital parameters and greenhouse gases. I suggest to also mention that, i) the orbital forcing is shortwave and greenhouse gases are related to mainly the longwave radiation flux, ii) difference in orbital parameters leads to uneven horizontal and seasonal changes, but greenhouse gases can cause more uniform anomalies. Furthermore: iii) It is helpful to know the changes of greenhouse gases between MH/LIG and PI are equal to how much radiation flux anomalies? How to calculate such anomalies based on CO₂ changes can be found in some papers (e.g., Myhre, et al. 1998, GRL).

The first two elements have now been incorporated into the text (lines 163-165). Regarding the 3rd point, we feel that providing the precise calculation of the radiative forcing due to changes in MH and LIG greenhouse gases is beyond the scope of this study,

and would not provide a great deal of added information. However, this has been acknowledged and clarified in the text, with a reference to the Gunnar et al. (1998) study (lines 165-169).

3. Lines 161-203 Too detailed information in terms of the changes in model version is give here. I would recommend to simplify the text and to show what aspect/process can be improved in the newest model version. Details could be provided as supplementary material.

This has now been done, with much of the text being transferred to the Supplementary Material

4. Lines 205-209: The sensitivity and control experiments are performed on different platforms. I worry about how different the simulated climate can be. If possible, one shall show in the supplement the anomalies of surface temperature based on the same experiment

This issue is discussed in section 2.1.2 (lines 291-300), where a previous study (Guarino et al. 2020) compares simulations across different platforms and finds that the various climate variables discussed in this paper are not significantly different across platforms. Please see Figure 6 in Guarino et al. (2020) for an example of this.

5. Table 2 and Fig. 2a, the 1.5 m air temperature of LIG still show significant trend in the final years. Could you please show a trend map to check where such trend mainly occurs? Does it happen in the region of interest?

This has now been addressed, with a 1.5 m air temperature trend map for both climate simulations being shown in the supplementary material (SM3) and discussed in section 2.4 (lines 405-408)

6. Lines 297-323: I think it is not so necessary to describe the spin-up in such a detail. Just show the tables, and I also recommend to put Fig. 2 and Fig. 3 into the supplement.

As part of other changes, this has already been done (see above comments to Reviewer 1)

7. Fig. 4 and Fig. 5: Perform a Student's t-test to identify in which regions the anomalies are significant and which regions related to internal variabilities. Given the relatively short length of the MH and LIG experiments, it is very important to do so.

This has now been done to the new version of Figures 4 and 5

8. Line 334 'and'=>, and'

As part of other changes, this has now been removed

9. Lines 336-337: 'in order to' => 'to'

This has been corrected

10. Line 342: Title is confusing. The CMIP6 HadGEM3 simulations include the PI, right?

This has been corrected

11. Line 351: 'central' => 'Central'

This has been corrected

12. Line 359 and a lot of other places in the paper: please make the experiment name consistent throughout the paper, for example, use either MH or midHolocene, the same for LIG and lig127k, piControl and PI.

A new section detailing the terminology has now been added (section 2.1.1), to clarify exactly what term refers to either the simulations or the geological intervals (lines 216-226)

13. Line 371: greater land-sea contrast... Is it also the same case in your model? I would recommend to check the moist static energy instead of surface temperature, to also include the aspect of moisture.

As part of other changes, this has now been removed

14. Lines 374-377: the small anomalies... Again please use Student's t-test. Results discussed in the texts should have a significance level above 95%.

As above, this has now been included in the new versions of Figures 2 and 3, showing the 99% significant levels

15. Lines 373-374: Comparing Fig. 5a and 5b, I observe no obvious shift in ITCZ, only stronger monsoon rainfall in LIG compared to MH.

As part of other changes, this sentence has now been modified and clarified

16. caption of Fig. 6, 9, and 12: Generally West Africa should be within 20W-15E. Why take 20W-30E?

This has now been corrected, such that all zonal mean plots go from 20°W-15°E

17. Lines 398-400: Please explain where the large uncertainty in proxy comes from.

This has now been corrected

18. Lines 422-424: Can this underestimation of the warming be used to explain the “Holocene temperature conundrum”? Or, might the “Holocene temperature conundrum” be caused by the fact that most of the proxy locate in regions with positive temperature anomalies? The proxy data represent seasonal or annual mean value? It might be helpful to discuss these issues. See, e.g. Lohmann et al. (2013, CP) for a comprehensive comparison for SST changes during the Holocene.

We agree with the reviewer that this term is ambiguous, and it has therefore been removed

19. Line 396: It would be better to clarify here the threshold of RMSE (is there any?) for a reasonable simulation result, in terms of surface temperature, precipitation and sst.

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We do not use a threshold of RMSE, but we have clarified this in the text (lines 614)

20. Line 447: if => but

As part of other changes, this has now been removed

21. Fig. 10 and 11: Again, please show significance (t-test).

As part of other changes, these figures have now been removed

22. Line 557. The model used prescribed vegetation, and does not consider dust. Please discuss the influence of the lack of interactive vegetation and dust on the Africa monsoon rainfall.

This has now been addressed (lines 933-935)

23. Optional: I encourage the author to make a separate discussion section.

As part of other changes, the summary and conclusions have now been restructured and rewritten, and further discussion has been added throughout the results section

Please also note the supplement to this comment:

<https://www.clim-past-discuss.net/cp-2019-160/cp-2019-160-AC2-supplement.pdf>

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-160>, 2020.

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