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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.

Anonymous Referee #2

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Comments on the manuscript entitled Williams, C. J. R., Guarino, M.-V., Capron, E., Malmierca-Vallet, I., Singarayer, J. S., Sime, L. C., Lunt, D. J., and Valdes, P. J.: The UK contribution to CMIP6/PMIP4: mid-Holocene and Last Interglacial experiments with HadGEM3, and comparison to the pre-industrial era and proxy data, Clim. Past Discuss., https://doi.org/10.5194/cp-2019-160, in review, 2020.

The paper describes results from two simulations using the latest version of the UK's physical climate model, HadGEM3-GC3.1; the mid-Holocene (∼6 ka) and Last Interglacial (∼127 ka) simulations, both conducted under the auspices of CMIP6/PMIP4.

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Based on three model experiments, this paper presents the response of several climatic variables, especially temperature and precipitation, to changes in insolation and greenhouse gases. Inter-model and model-proxy comparison are also included, but they are not in-depth. It is worth to do this effort as many modelling studies have already been made on the mid-Holocene and Last Interglacial. It is critical to present new findings and methods to make the paper more attractive. At the current stage, there are several major weaknesses from which the paper suffers:

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.

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.

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.

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

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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.

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).

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.

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

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?

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.

7. Fig. 4 and Fig. 5: Perform a Student's t-test to identify in which regions the anoma-

lies 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.

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

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

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

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

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.

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.

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%.

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.

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

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

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.

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.

20. Line 447: if => but

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

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.

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

References:

Myhre, Gunnar, et al. "New estimates of radiative forcing due to well mixed greenhouse gases." Geophysical research letters 25.14 (1998): 2715-2718.

Lohmann, Gerrit, et al. "A model-data comparison of the Holocene global sea surface temperature evolution." Climate of the Past 9 (2013): 1807-1839.

Pfeiffer, Madlene, and Gerrit Lohmann. "Greenland Ice Sheet influence on Last Interglacial climate: global sensitivity studies performed with an atmosphere–ocean general circulation model." Climate of the Past 12 (2016): 1313-1338.

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