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Climate of the Past Discussions

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 #1

Received and published: 30 January 2020

Review of manuscript by Williams et al. entitled "The UK contribution to CMIP6/PMIP4: mid-Holocene and Last Interglacial experiments with HadGEM3, and comparison to the pre-industrial era and proxy data"

The manuscript presents an extensive description of the new "past warm climate" simulations with the UK climate model and I applaud the effort to compare the results both to proxy-data as well as to previous versions of the same model. However,

in the current form the manuscript is difficult to read as it seems to presents a bit of everything, lacking a clear aim or structure. Moreover, there are several other issues that need to be addressed before I can recommend publication of the manuscript. Below I will detail my main concerns and list all minor and technical comments.

Main concerns:

As mentioned above, it appears to me that the manuscript presents various data sets and experiments that in its current form lack relevance or seem somewhat out of place. This makes that the manuscript lacks a clear common thread, making the manuscript difficult to read. Clarify the relevance of all the presented results and consistently mention all of them in the abstract, introduction, results and conclusion sections. Clarify why you present: a comparison to previous versions of the model; a comparison to CMIP/PMIP results; a comparison with proxy-data. A change to the overall structure of the manuscript as detailed in the following could also improve the flow of the manuscript.

The analysis of temperature on the one hand and precipitation on the other hand are very different. Temperatures are looked at globally, while the analysis of precipitation is solely on Africa. Clarify to the reader why this choice was made. The structure of the manuscript would potentially be improved if the analysis is firstly on global features (mostly temperature, but perhaps also precipitation considering the newly developed Last Interglacial precipitation reconstruction), and secondly zooms in on African precipitation changes.

An extensive description of the spin-up results is given in the manuscript. While in general I appreciate it that such potentially important modelling details are given, the relevance to the rest of the manuscript is not clear to me. Wouldn't it be sufficient

to provide the numbers of the spin-up results in a table and refer to that table in the method section? Is a whole results section for the spin-up results needed, given also that they are not referred to anymore in the remainder of the analysis?

The model-data comparison is limited to temperature proxies while recently a new compilation of precipitation reconstructions for the Last Interglacial has become available (Scussolini et al., 2019) which should be used here as well.

I find the whole analysis of African precipitation changes in the different simulations incomplete and too simplistic, leading to figures and results that are misleading. To improve this situation I suggest the following:
-> Remove the ocean grid cells from the domain over which the analysis is performed. Presenting zonal-mean figures is not appropriate if the results are clearly not zonally homogeneous as is the case around the equator in this analysis.
-> Why is the focus on JJA precipitation over Africa? The results in figure 12 are again annual mean. Be consistent and clarify your reasoning.
-> The authors seem to use the words 'monsoon' and 'ITCZ' interchangeably. While indeed they cannot easily be separated based solely on the analysis of precipitation, they are driven by fundamentally different processes and an attempt should be made to separate the two. An interesting read on this topic is by Nicholson (2009).
-> The results for African precipitation are rather different for the different periods that are considered and for the various versions of the climate model. Provide some analysis as to what drives these differences.

In many figures no measure of the robustness of the results is given. Provide measures to determine if your results are significant or if we are looking at internal variability of the climate system. A possibility would be to use long-term variability within the different simulations (PI, 6k, 127k) to deduce whether the depicted anomalies are outside of the range of this variability.

Minor comments:

Line 48: are surface (0 meter) temperatures discussed in the manuscript? Or do the authors mean SSTs here?

Line 82: these periods are not 'warm' everywhere and all the time. Please clarify.

Line 83: 129-116 is not the period that is discussed in this manuscript. Why not simply 127ka?

Lines 108-112: Clarify that this warming is mainly located at high latitudes.

Line 126: convection in the atmosphere or ocean or both?

Line 127: are the ocean and sea ice models completely new or have parts been updated?

Line 140-141: This division in two subsections (3.1 and 3.2) suggest to me that the two topics are of similar importance while in reality this is certainly not the case, with the results on the spin-up phase being only a small side topic. Consider changing this structure to better represent the importance of the different topics.

Line 141: For me the term productions runs is a little strange, perhaps it is CMIP kind of language, but in the context of a manuscript is doesn't mean much to me.

Line 214: Isn't precipitation impacted by ENSO?

Lines 248-252: I don't think such details (number of output variables) are relevant for a manuscript.

Line 273: what is your definition of 'summer'?

Line 276: You constructed this 127 ka time-slice of the Hoffman et al data? Do you provide this data for future work?

Line 306: Is a trend of 0.16 degrees per century small? Sounds significant to me. Please clarify.

Line 335: "the current two warm climate", what does that mean or refer to?

Line 335: Which newly-available proxy data are you referring to? Did you gather new data? Or do you mean the 127 ka time slice based on the Hoffman et al data?

Line 342: HadGEM3 warm climate simulations?

Line 364: 30 degrees east doesn't sound like west African to me. Please clarify why this domain was chosen, also in light of my main concern on this topic.

Line 369: The wind patterns to me show an increase at nearly all latitudes, is that typical for an ITCZ shift?

Lines 374-377: Do we see the same kind of pattern to the south of the equator, so the South African region?

Line 407: Do proxies suggest a global annual mean warming during the MH?

Line 439: "within the average uncertainty range'? Please clarify this statement.

Line 466: The model is seasonally dependent? What does that mean? Do you mean the comparison of models and data?

Lines 488-492: Why would you compare your results to results from previous model version to see if you get sufficient precipitation over the Sahara to promote vegetation growth?

Lines 515-529: These kind of detailed (small) differences make me wonder whether we are really discussing forced differences or if we are discussing internal variability of the system. Please show statistics to argue either way.

Lines 546-553: When you are talking about an 'improvement' this suggest that we know what 'good' means. What kind of data or proxies do you use to determine 'good' and what is the uncertainty of these estimates?

Line 555: Aren't the paragraphs before already discussing "rainfall across the Sahara"?

Lines 570-583: What is the relationship between vegetation in the Sahara (the topic of this paragraph) and the state of the equatorial Atlantic ('drying'?)? Please clarify.

Lines 570-583: Not only is a vegetation model missing to directly determine whether or not vegetation would grow with the simulated amount of precipitation, but also all vegetation related feedbacks on the climate are missing. Discuss the possible impact of these missing feedbacks on you results.

Lines 616-619: meltwater does not only yield a warming, it usually results in a spatially varying pattern with regions of warming and regions of cooling. Please clarify.

Lines 619-621: Is the length of the spin-up really a potentially important caveat? Do you have evidence to support this?

Line 628: Only MH or both MH and 127ka?

Table 2: If some values are for the full ocean depth and others for the top 1054 meter, can we still compare them? Isn't it comparing apples and oranges?

Table 4: I appreciate the attempt to provide a lot of information, but I find this table very confusing. Perhaps it can be split or rearranged?

Figure 1: Have calendar effects been taken into account when making this figure? Please apply corrections, following for instance the methodology outlined by Bartlein et al. (2019).

Figure 2: There seems to be a gap between the control data and the start of the 127k simulation, is this a real data gap or an error in the figure?

Figure 2: Are the temperatures in the left-hand figure surface or 1.5 meter temperatures?

Figure 2: Consider: 'b) TOA radiation balance'

Figure 2: This figure gives a good idea of the amount of internal variability in the system, which seems considerable in both the MH and lig127k simulations. Use this information to define which of your results are robust with respect to this internal variability. Is it true that variability is larger in the 'warm climates' than it is in de control?

Figure 3: For the control simulation the full depth is used instead of the top 1054 meters according to the main text, please clarify.

Figure 4: These figures show some well-know climate change features, including polar amplification. The mechanisms of such spatial temperature anomaly patterns are not discussed. Provide a discussion or refer to previous work on the topic.

Figure 6: Rainfall anomalies on y-axis?

Figure 6: x-axis values are not easy to read in this format.

Figure 6: consider showing absolute precipitation values because I think those give a much better idea of the width of the wet and dry regions as discussed in the main text.

Figure 6: Can't this figure be combined with figure 9?

Figure 8: Remove the ice core data points if the corresponding modeled surface temperature anomalies are not shown.

Figure 9: What does this figure add that is not already depicted in figures 10 and 11? Can't it be removed?

Figure 12: why are the grey dashes that show required rainfall for grassland growth only start from 16 degrees north?

Figure 12: Rainfall anomalies on y-axis?

Figure 12: Why are anomalies shown? Doesn't the threshold to support grassland depend on the absolute amount of precipitation?

Technical comments:

Line 41: are similar

Line 41: period, but

Line 53: generations of the same

Line 121: therefore in the

Line 146: consider removing "indeed"

Line 149: double space before "Full"?

Line 186: tuning of

Line 201: including a reduction of the temperature bias in many regions

Line 221: remove comma after 'design'

Line 244: Too many brackets

Line 272: annual mean surface

Line 298: radiation balance?

Line 384: in the core monsoon region?

Line 395: ‘recent’, what do you mean?

Line 399: what kind of uncertainty in simulated anomalies are you referring to, please clarify.

Line 400: remove ‘often’

Line 437: small number of reconstructions?

Line 449: remove double comma

Line 499: refer to figure 10?

Line 505: smaller northward displacement?

Line 590: ‘auspices’, not sure if that is the right word for it.

Line 590: replace comma by a dot.

Line 591: remove ‘time’?

Line 592: were assessed?

Line 603: ‘time’, are we talking seasons or different geological intervals?

Line 626: ‘necessity’ is perhaps a bit too strong in this context.

Line 1007: better not to use the ‘’ symbol.

Line 1008: for each

Line 1018: in this caption and some others the words ‘simulated gridded anomalies’ are used. This sounds a little double to me since nearly all climate models work on spatial grids so the output is per definition also gridded.

Lines 1024-1027: Is there no overlap between these two data sets? No single core was used in both of them?

Line 1033: erroneous bracket?

References:

Bartlein, P. J., and S. L. Shafer (2019), Paleo calendar-effect adjustments in time-slice

and transient climate-model simulations (PaleoCalAdjust v1.0): impact and strategies for data analysis, Geosci. Model Dev., 12(9), 3889-3913.
Sharon E. Nicholson (2009). A revised picture of the structure of the “monsoon” and land ITCZ over West Africa. Climate Dynamics volume 32.

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