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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Last interglacial temperature evolution – a model inter-comparison" *by* P. Bakker et al.

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We would like to thank the referee for thoroughly reviewing this manuscript and providing a large number of instructive comments to improve the manuscript.

General comments

"As the consistency between the models is rather poor, the potential contribution of this study to advancing the field would be by providing a better understanding of the causes of the different transient temperature evolutions simulated. This should therefore be given due attention in a revised manuscript and form the basis of the main conclusions and summary of the paper. Similarly the seasonal, latitudinal and depth (not included) dependent response of simulated temperatures to the forcing is interesting and highly

relevant for the interpretation of proxy based temperature reconstructions. Clear conclusions and a summary of this topic should be included, making its relevance clear to the proxy community."

We agree with the referee that many aspects of the transient LIG climate simulations which are highly interesting and potentially beneficial to the palaeoclimate community are not included in this manuscript.

However, for the following reasons we deem that the results presented in this manuscript are an important contribution to advancing the field:

1) This is the first time that transient simulations of the LIG climate are available, performed with a suite of models, differing in complexity. It should be appreciated that it took a considerable effort to obtain these results.

2) This manuscript forms an important first step to assess the common temperature signal that is present in this set of simulations, despite all differences in model complexity, model setup and forcings. This common signal is likely to indicate the first order response of climate system to the main driving forces. Therefore we have chosen to focus in this paper on this common, consistent signal, rather than on derivations from this signal in individual models that may have been caused by a number of reasons. We consider this highly relevant, even more so because we think, in contrast to the referee, that the results do show considerable consistency between the different simulations despite the large differences between the simulations.

Moreover, there are several good practical reasons not to include an in-depth investigation of the causes of the inter-model differences:

1) The data which is needed to thoroughly investigate the causes of inter-model differences in sea ice, Atlantic Meridional Overturning Circulation - AMOC, remnants of continental ice, change in monsoons (all highly interesting indeed!), is not easily available.

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2) To conduct such an in-depth investigation would require going deep into the physics and dynamics of all 7 climate models, a humongous task indeed. Within the Past4Future project parts of this task have and will however be taken up.

In the updated version of the manuscript we have clarified our main objective: Investigating the common, consistent signal in the LIG temperature evolution in 7 different climate models and the main driving forces behind it. Furthermore, we discuss what could potentially cause the inter-model differences to make clear what work is needed to progress our understanding the LIG temperature evolution (i.e., what sensitivity experiments and what type of proxy-based constraints).

Specific comments

“1) Abstract: the abstract is quite extensive. However, details of the temperature maxima

for the different periods is given too much focus and is not as valuable to the reader as the mechanisms behind the inter-model differences. These mechanisms are briefly listed (sea ice, Atlantic Meridional Overturning Circulation - AMOC, remnants of continental ice, change in monsoons), however, a clear description of their relevance is lacking. As an example, statements such as “for the Atlantic region, the Southern Ocean and the North Pacific, possible changes in the characteristics of the Atlantic meridional overturning circulation are critical” do not give much information.”

In accordance with the comment of the referee we have decreased the length of the abstract. Furthermore we have made it clearer what our aim is, what the results are and how we see that the results provide information on what experiments should be performed in the future and what proxy-data is needed in order to make further progress in modelling the LIG climate.

“2) Model simulations: the number of years used for the spin-up of the initial (equilibrium)

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state should be stated for all models. This is important theassess the possible influence of initial model drift."

We agree with the referee that this information is lacking. We have therefore included information about the forcings, length and nature (transient or equilibrium) of the spin-up period in table 1.

"3) Data processing: it is stated that the effect of the acceleration technique in CCSM3 and KCM on the ocean is of minor importance. However, when using this approach it is important to note that possible changes in deep ocean hydrography as well as overturning circulation will not be captured correctly. Therefore, one can only consider changes in the atmosphere and in the ocean mixed layer. Analyzing changes in AMOC, as done in this manuscript should be avoided, as it will not be consistent with the slow orbital forcing. See also Lorenz & Lohmann (2004)."

We agree with the referee that we should be more careful in interpreting changes in the overturning circulation in the accelerated simulations (CCSM3 and KCM). Furthermore we have to make it clearer to the reader what the impact on the results could be. However, as these simulations are 1500 model-years long, this is about the same order of magnitude as the time-scales of the overturning circulation (hundreds of years to several thousands of years). For many models it has been shown (for instance Stouffer et al. J. Climate, 2006) that the time it takes for the overturning circulation to adjust to changes in the boundary conditions is less than 1500 years. We therefore argue that the apparent stability of the overturning circulation in the simulations performed with CCSM3 and KCM could result from either the applied acceleration technique or be an actual characteristic of the simulated climate.

We have reformulated the discussion on the overturning circulation to make this uncertainty more clear.

"4) Evolution of the main climatic forcing..: in figures 2, 3 and 4 monthly anomalies of simulated last interglacial (LIG) temperatures relative to pre-industrial are analyzed.

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However, as shown by Joussaume & Braconnot (1997) the choice of calendar used by the model can significantly impact these results. In particular for the last interglacial where changes in the length of the seasons are large relative to today. Even with a fixed vernal equinox there will be biases when comparing monthly model output between a given period of the LIG and pre-industrial. The effect of this should be assessed and if large a different approach should be used in the analysis of the results (e.g. use astronomical positions). Also, make sure that all models use the same date for vernal equinox."

As proposed by the referee, we have included information about how the different simulations deal with the issue of the choice of a calendar. Furthermore, we agree that a more thorough discussion should be included about the possible impacts as discussed by Joussaume & Braconnot (1997). Ideally one would like to recalculate the monthly temperatures with the changes in season length taken into account. This is however not feasible for most of the simulations included in this inter-comparison.

We would like to stress that since all simulations use the same fixed-calendar the inter-comparison of the different simulations is not biased. It is clear, however, that the possible impact should be discussed when in the future a comparison is made with proxy-data.

We do not think that performing additional sensitivity experiments investigating the possible impact would yield new insights in addition to the study of Joussaume & Braconnot (1997).

"5) Results (page 4675): It is stated that simulated January temperatures are consistent with December insolation for the mid-latitudes, however there is no robust trend for winter temperatures at high latitudes between the models. To clarify this and to make the difference between models forced solely by insolation compared to insolation and greenhouse gases (GHG) it would be beneficial to make a separate section focusing on the impact of including transient GHG forcing (in the Discussion section). It is possible

that the GHG forcing will help explain winter temperatures at high latitudes (where the insolation forcing is weak or absent). See also General Note on change in focus of the manuscript."

The statements about the Northern Hemisphere winter temperature evolution at high latitudes are indeed not formulated correctly. This has been changed in the manuscript.

For the high latitudes we find that winter temperatures tend to differ largely among the models. Our reasoning is that this is caused by the lack of a strong insolation forcing which makes that both the included GHG forcing and feedbacks become relatively more important. This is clearly visible for the Southern hemisphere high latitudes where winter temperatures only show a clear early LIG peak if GHG changes in accordance to the PMIP3 protocol are included.

In the newly included subsection "Temperature evolution and forcings" we elaborate on this finding.

"6) Sea-ice and LIG temperature: the observed early Arctic winter warming is interesting and should be elaborated and included in the summary and conclusions. However, additional analysis and a figure showing seasonal change in sea ice should be included. In particular, it is important to investigate the transient evolution of the sea ice thickness, to assess the impact of summer insolation."

Indeed the general agreement among the models on an early Arctic winter warming is intriguing. In the revised manuscript we will include a more extensive description of the likely mechanism connecting summer sea-ice changes and the evolution of winter temperatures. However, in line with the focus of this manuscript, we dot think that an in-depth investigation on the reasons for this warming and the differences between the models is within the scope of this manuscript. We do agree that we should stress the need for a more thorough investigation of sea-ice changes and its relation to summer insolation. A working group within the Past4Future project is currently working on such an investigation.

“7) The AMOC and the LIG: it is stated that the AMOC has a large impact on oceanic heat transport and the exchange of heat to the atmosphere. This is misleading, as most of the heat transport and exchange with the atmosphere occurs in the horizontal gyre circulation of the ocean. If this heat exchange is key to the conclusions, an analysis of the gyre circulation and its contribution should be included. The statement “it seems that changes in sea-ice cover and dynamics of the Southern Ocean play an important role in the simulated climatic changes around 121 ka BP” is speculative and should be clearly documented. A key issue which must be discussed is the reasons behind the very different response of the AMOC in the models analyzed. E.g. why do the EMICS show large (and abrupt) changes in AMOC whereas the GCMs are stable? Why do LOVECLIM and FAMOUS show the opposite response?”

We agree with the referee that we should be more precise in detailing the role that the overturning circulation plays in transporting heat. In the manuscript we have made it clearer that our aim and main result is to identify feedbacks like the AMOC as being potentially important in determining the LIG temperature evolution. Furthermore we will include a more thorough explanation about the model-dependency of the evolution of the AMOC in the LIG simulations and that there is a strong need for more proxy-based constraints in order to select those simulations which are likely to have a better resemblance with proxy-based temperature reconstructions.

“8) The monsoon and the simulated LIG: it should be made clear if the anomalous pattern in the Sahel region and India is only a feature observed in models which can resolve the monsoon. If so, these should form the basis for further discussion (e.g. are these models consistent).”

Indeed it seems that the anomalous patterns in the Sahel and Indian regions are only observed in the models of higher complexity. We discuss in the revised manuscript that care should be taken when interpreting simulated temperatures in monsoon regions from models of low complexity.

"9) Land-Sea contrast: to facilitate a comparison of land-sea contrast requires a plot illustrating the seasonal evolution of the mean ocean and land temperatures through time."

In line with comments of referee #1 we have come to the conclusion that the findings related to land-sea contrasts in the simulated temperature evolution are not novel or highly important and have therefore deleted this paragraph.

"10) Conclusions: a summary of the different specific temperature responses of the different seasons is not a significant contribution to advancing the field. Rather the conclusions should include a summary of clear conclusions of each of the main results discussed in the manuscript and the mechanisms behind. E.g. why are the models different, where do they agree, what is the impact of sea ice, AMOC, remnant ice, GHG, monsoons, land-sea contrast. These results are essential and would constitute a valuable contribution from this paper if given due attention."

The main findings of this manuscript are that in a number of regions in January and or July, the simulated LIG temperature evolution based on 7 very different models (different in complexity, resolution, forcings, spin-up and included components of the climate system) show strong consistency. In the summary section we therefore shortly list in which regions this is the case and which periods of maximum warmth with according temperature anomaly ranges have been found. If a future model-data comparison would show that these findings are not in agreement with reconstructed LIG temperatures we see our results as a strong indication that the main forcings of the LIG climate in those regions were apparently not the changes in insolation and or GHG concentrations.

Secondly, the summary of this manuscript lists the regions for which we have found strong indications that climate feedbacks are potentially highly important in determining the temperature evolution. This highlights some very interesting and necessary lines of future research.

"11) Figure 2: the figure is very hard to read and should be separated into two separate figures - one with all the individual models (summer and winter) and one with the multi-model mean and variability, together with the insolation anomalies. At present it is not possible to assess the multi model mean and its variability, and the chosen temperature range does not give a clear separation of the different models presented in the limit space. the different integration lengths of the model will impact the multi-model mean and variability. Therefore, this analysis should only be done for the period where all models overlap in time (126-115ka)."

We agree with the referee that the readability of Figure 2 is greatly enhanced when it is split into more subpanels. Furthermore we have, in line with the comments of referee #1, included more information in Figure 2 on the significance of the periods of maximum warmth.

We agree that the multi-model-mean and variability are affected by the changing number of models between 130, 128 and 126ka BP. However, there are more characteristics of the simulations which affect these properties: at very high latitudes there are no results from the Bern3D model, the MPI-UW results have been interpolated from 200-year averages into 50-year averages and the CCSM3 and KCM simulations are performed with an acceleration of the forcings. Since all these aspects impact on the multi-model-mean and variability we have chosen to ignore them all together.

We do agree with the referee that in the text and the figure caption we should point out these difficulties to the reader.

"12) Figure 3: This figure would benefit from removing the data which is not significant (high STDEV)"

We have calculated the significance of the simulated period of maximum warmth compared to the LIG temperature average and removed the data from the plot if not significant.

“Figure 4: Should reduced this to JJA and DJF, not necessary to include all 6 months as this does not add essential information. In particular, as manuscript discusses JJA in general (e.g. section 4.4).”

We agree with the referee that including 6 months does not add much and is not in line with the discussion of the results in the main text. We have therefore removed all except January and July.

TECHNICAL COMMENTS (page.line)

“4672.7: an ->a”

We apologize. A correction has been inserted.

“4682.18: what is a “limit-cycle”, explain.”

The term limit-cycle is used to describe the stability characteristics of an oscillating system. We agree that this term is overcomplicated and unnecessary in this context and it is replaced by a more simple explanation.

“4683.15: the seasonal impact of remnant ice and weakening AMOC should be quite different, and should be addressed in this context.”

We agree that the impact should be different. However, regrettably it seems that our simulations and their results do not allow for this distinction to be made. We have included a comment pointing this out to the reader.

“4684.5: the anomalous pattern is not confined to the Sahel. Please specify more clearly.”

We agree that the pattern is not only visible in the Sahel. Therefore we have changes throughout the manuscript this description into ‘centred on the Sahel’.

“4697.fig.2: According ->corresponding.”

This mistake has been corrected.

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