

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 comments in order to improve the manuscript.

General comments

“Unfortunately, one of the models (MPI-UW) is missing here.”

We regret that in the conversion-process between different types of text-files this part has gone missing. It has been corrected.

“Then there is a description of the post processing of the results followed by the description of the forcings. I would suggest inverting these last two sections, i.e. first

forcings, second results.”

We agree that having the description of the forcings closer to the model description improves the flow of the manuscript. The sections are reversed, as suggested by referee #1.

“I regret that section 3 is so long, full of a lot of details about temperature, some of them being repeated in the discussion (section 4).”

The length of Section 3 has been reduced by only stating the most ‘robust’ findings and the according values.

“I was wondering whether it could be possible to combine both sections into a single section

that would include a sub section on temperature, one on temperature and forcings, and the subsections in the present section 4.”

Former Section 3 and 4 have been combined into one section “Results and discussion”. This section now consists of several subsections: ‘robust temperature evolution’, ‘temperature evolution and forcings’, ‘temperature evolution and model complexity’ and finally a number of subsections discussing the importance of a number of feedbacks (sea-ice, AMOC, monsoon, remnant ice-sheets; land-sea contrasts has been removed).

“The “conclusions” is a summary of the major points discussed in section 3 and 4.”

The former ‘conclusions’ section has been renamed to ‘summary’.

“I am missing a more quantitative approach.”

We agree with the referee that our manuscript could be made more quantitative. Accordingly, we have revised the manuscript in several ways.

Firstly, in Figure 2 and the according text, we have calculated if the simulated maximum

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LIG temperatures in the individual simulations are significantly different from the individual model mean (on the basis of individual standard deviations, calculated based on residuals of detrended temperature time-series). Only if this is the case the period of maximum temperatures is shown.

Secondly, the spread in each model's individual period of maximum temperatures has been used to construct and quantify an uncertainty range for the multi-model-mean (MMM) period of maximum warmth. Also this MMM period is only shown when significantly different from the mean MMM LIG temperature. The main results from Figure 2 have also been listed in a new table which shows for the two months and the 5 latitude bands the temperature and timing of simulated MMM maximum LIG warmth including uncertainty estimates.

Thirdly, in Figures 3 and 4 we have similarly taken into account if the local temperature maximum is actually significantly different from the LIG mean temperature and if not a local timing of maximum warmth is not depicted.

Fourthly, in Figure 4, locally the results are only shown when at least 50% of the models have a significant result.

By adding significance intervals, leaving out non-significant results and by listed these values in a table the approach in the manuscript has become more quantitative.

“Similarly several subsections ended with statement like ‘this need more investigation’ or ‘this is out of the scope of the paper’, which reinforces this general feeling of a very qualitative paper.”

We fully agree with this comment. We have tried to make it more quantitative as described above. Furthermore, we will clarify in the introduction of the manuscript that a detailed investigation of the feedbacks that affect the LIG temperature evolution is not the focus of this manuscript. An important objective of our manuscript is to provide a starting point giving directions to future studies that will be performed to investigate

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these feedbacks and the causes of the inter-model differences, including future reconstruction studies that shed light on the LIG temperature evolution. This will allow us to delete several of the statements as listed above by the referee.

Detailed comments

“Page 4669 – line 1 (Description of the Bern 3D model). “This model includes prescribed

changes in the extent of the Northern Hemisphere (NH) continental ice sheets”. However,

nothing is said relative to the ice volume and the location of this ice remnant. Are the Northern Hemisphere ice sheets prescribed (and adjusted) at each time step of the model?”

The location of the NH ice sheets in the simulation performed with Bern3D are over North America and Eurasia in accordance with reconstructions of the Last Glacial Maximum. The area covered by ice (area only, no elevation changes taken into account) is linearly related to the changes in global ice volume as reconstructed from benthic $\delta^{18}\text{O}$.

In line with the above explanation we have included a bit of explanation of the locations of the ice sheets in the Bern3D simulation. In our opinion, no more details are necessary for the reader to understand the relation between remnants ice-sheets and the simulated temperature evolution. Such details can however be found in the given references.

“Does sea level change in this simulation?”

In the Bern3D simulation the sea level (or actually the land-sea mask) does not change during the simulation. For clarity we have added a statement to the caption of Table 1 indicating that, unless stated otherwise, sea level is fixed in the different simulations. Note however, that a fixed sea level or land-sea mask does not necessarily imply that

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the model calculations are not based on a ‘free surface ocean’ including the according pressure gradients and dynamic features.

“Page 4671 – line 10. LOVECLIM is the only model for which the authors explicitly state the forcings, i.e. astronomical configuration and GHG. I suggest leaving it for the ‘forcings’ section.”

In order to make the model descriptions more uniform we have deleted this description in this part of the manuscript.

“The description of MPI-UW is missing.”

This has been corrected.

“Therefore, I like very much the idea of table 1 and I support the idea of transferring as much information as possible from the main text to the table.”

In line with the suggestion of the referee we have moved most of the technical information on the models to Table 1. The model description paragraphs now only give details on parts of the model or applied scenario and forcing if different from the other simulations and if highly relevant for the simulated temperature evolution. All details (such as information on the spin-up of the simulations) have been moved to Table 1.

“Page 4671 – line 21 (data processing). “These differences mean that the degree to which short time-scale climate variability is filtered out differs from sub-decadal (CCSM3 and KCM) to multi-decadal (all other models)”. Please explain.”

All results are presented as 50-year averages. This is true for the changes in the main forcings (orbital and GHG). However, to make long simulations feasible, most of the GCMs applied an acceleration technique (10-fold), meaning that they ‘only’ performed around 1500 model-years of simulation instead of 15000. The 50-year averages are therefore only 5-year averages if one looks from the perspective of the ‘model-years’. One of the consequences of this difference in the length of the averaging period is that less short time-scale climate variability is averaged (filtered) out.

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This part of the manuscript has been reformulated to make this point more clear.

“Page 4672 – line 21 “We will also identify : : :”. Do the authors mean : : : trends that are

directly connected to changes in insolation or GHG concentrations? Or do they mean trends that are directly connected to neither changes in insolation nor GHG concentrations?”

In the second subsection of the “Results and Discussion” we first investigate which part of the simulated temperature evolutions are clearly related to insolation changes and or changes in the GHG concentrations. All features not directly related to these forcings will then be discussed in terms of simulated climate feedbacks.

The line has been reformulated to make it clearer.

“Page 4672 – line 24. Is it annual mean insolation?”

In the lines following this line we specify the changes in both annual mean insolation as well as seasonal changes.

“Page 4672 (line 24) – Page 4673 (line 25). The very detailed description of lead and lag between insolation at different latitude and month can be ‘summarised’ into one reference, i.e. BERGER A., 2001. The role of CO₂, sea-level and vegetation during the Milankovitch-forced glacial-interglacial cycles. In : Å’n Geosphere-Biosphere Interactions and Climate Åÿz, Lennart O. Bengtsson and Claus U. Hammer (eds), pp. 119-146, Cambridge University Press, New York. It is stated on top of page 122 “: : : variations of the daily insolation depend mainly on precession : : : . As a consequence, for a given latitude there is a phase lag of about 2 kyr : : : between insolation of two consecutive months”.”

We agree that this part of the manuscript is too lengthy. We have summarized it and included a reference to Berger (2001).

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“Page 4676 line 20. ‘ : : : the magnitude of the overall trend’. I assume that it is the temperature trend for both mid and high-latitude in July. This should probably be mentioned

in the text. If my interpretation is correct, the authors indicate a trend in the mid and high-latitude but no trend for the whole hemisphere. What does that mean for the low latitudes? Is there a trend opposite to the trend in the higher latitudes that compensates each other?”

This part of the manuscript is not precise enough indeed. This text refers to the preceding line about the evolution of July temperatures in the mid and high-latitudes of the Southern Hemisphere. In our updated results and plots it is apparent that all the models that include changes in GHG concentrations according to the PMIP3 protocol indicate a July temperature maximum before ~125ka BP in the mid and high-latitudes of the Southern Hemisphere. The other simulations do not show a significant peak. We did not investigate how temperatures changed averaged over whole hemispheres.

In the revised version of this section we reformulated this part of the text to make it more precise.

“Page 4679 – lines13-14 and lines19. It seems slightly non coherent to say that “The relative importance of either the GHG forcing or the sea-ice feedback on the Arctic winter MWT is not easily determined” and that “the sea-ice feedback plays an important role in determining the LIG winter temperature evolution in the Arctic region”. Once more, I assume that this is related with the fact that the analysis is qualitative and not quantitative. There is no information about JJA. Is JJA temperature in favour or against an important role of the sea ice?”

Our main reasons to rule out changes in GHG concentrations as an important factor in driving January temperature changes in the Arctic region are that 1) the pattern of an early LIG temperature maximum is clearly confined to the sea-ice covered ar-

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eas of the Northern Hemisphere which contrasts with the global GHG forcing, and 2) some models not including GHG changes in accordance with the PMIP3 protocol (and therefore do not include the early LIG GHG peak) do show the early peak in January temperatures.

In the revised version of this section we reformulated this part of the text to make it more precise.

“Page 4680 – section 4.2. This section discusses in parallel and for each model three issues that does not seem to be fully related at first. There is the abrupt change in the AMOC, the abrupt temperature change in the Northern Hemisphere and the MWT anomaly. I can indeed see the correlation (again qualitatively speaking) between rapid changes in AMOC and in temperature. However, I do not understand how the timing of maximum warmth is related with these abrupt changes.”

We have no direct indication that the temperature anomalies under consideration are the result of changes in AMOC strength. This would require sensitivity experiments lacking these AMOC changes, allowing an analysis of the AMOC's impact on the temperature evolution. We take a more indirect approach, using two sorts of information: 1) the time-correlation between the described temperature changes and AMOC strength changes and 2) the fact that the regions in which the anomalous patterns are simulated agree with regions known to be the major regions with deep water formation in the different models. For instance, we use this combined information to argue that the peak in mid-latitude NH January temperatures in the FAMOUS simulation around 121ka BP is not directly related to changes in insolation or GHG concentrations, but the result of a feedback involving the AMOC. Furthermore, there are indications that the anomalous patterns in the timing of maximum January warmth in FAMOUS in the north-east Pacific, the Southern Ocean and parts of the North Atlantic are a consequence of these AMOC changes. The same procedure is applied to indicate in which regions the AMOC feedback might be important in other simulations.

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In the revised version of this section we will make clearer what our approach is, what we can conclude from it and also what the uncertainties are in this indirect approach.

“Page 4681 – line 3. I would suggest to use chronological order to discuss AMOC with FAMOUS.”

We agree that this section is not well structured and therefore difficult to read. We have changed this section to make the description more chronological and clearer.

“Page 4681 – line 16. Could the authors elaborate more on the changes they are discussing (“changes in the sea-ice cover and the dynamics of the Southern Ocean”)?”

The changes in the AMOC, deep convection and sea-ice in the LIG FAMOUS simulation are intriguing indeed. However, we did not perform a detailed analysis of all the changes and the underlying causes, as this was not considered feasible given the amount of time available. Such an analysis might be the topic of a future manuscript focused on the results of FAMOUS. We stress that our manuscript is focused on the common signal in the wide range of climate models involved, rather than on the anomalous result of an individual model that may be caused by a number of reasons.

In the manuscript we will remove the sentence indicated by the referee. Instead we will include a part to describe the difficulties in assigning specific features of the simulated temperature evolution to a specific feedbacks mechanism because of the many interconnections between them.

“Page 4681 – line 22. “-20-30%” Which is the reference? Percentage of what?”

We agree that this was not clear in the manuscript. The percentage mentioned in relation to the decrease in strength of the AMOC in the Bern3D simulation is compared to the state of the AMOC during the period in which no freshwater forcing is applied and the AMOC is seemingly stable (125-121ka BP).

A clarification has been added to the manuscript.

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“Page 4681 – line 27. “AMOC strengthening does not seem to have a clear impact on the simulated LIG temperature evolution”. As long as there isn’t a simulation without AMOC strengthening, it is hard to conclude that it does not have an impact.”

We agree with the referee that this should be formulated more cautiously. As with all discussed feedbacks in our manuscript, the indications are only indirect and sensitivity experiments excluding a particular feedback are needed before more firm conclusions can be drawn. What is meant to be said in this part of the manuscript is the following. In comparison with the examples discussed before (temperature-AMOC relation in for instance the FAMOUS and LOVECLIM simulations), the AMOC strengthening after ~125ka BP does not seem to have a clear impact on the Bern3D temperature evolution.

In the manuscript we will reformulate these sentences.

“Page 4684 – lines 5-6. “ the simulated MMM MWT over these regions is clearly later than the surrounding regions “. According to figure 3 and to what the authors write later (line 15 same page), this is the case only for three models. I would urge the authors not to draw general conclusion that they later minimise. Moreover, I would like to know to which extent the concluding lines of this section (Page 4685 – line 3) are valid for all the models or only for three of them.”

In the July MMM timing of maximum warmth it is apparent that the only regions of the NH where maximum warmth is simulated in the later part of the LIG is the Sahel and India; Both regions for which other studies have indicated that changes in insolation and GHG concentrations can have a strong impact on temperature via a monsoon-dynamics related feedback. This anomalous timing of maximum July warmth in these specific regions is however only simulated by KCM, CCSM3, MPI-UW and somewhat by FAMOUS. In the second part of this section we describe specific limitations of EMICs, clarifying why the monsoon-feedback is only found to be important in the GCMs in this model-intercomparison. Therefore, our results provide a strong indication that in regions characterized by a monsoon climate, maximum LIG warmth

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is strongly driven by feedbacks rather than a direct response to insolation and GHG changes. Moreover, it is important to make clear that in such regions, the results of EMICs must be treated with care.

In the manuscript we have reformulated this section to make more clear on what basis we identify the importance of the monsoon feedback and what consequences it has on the temperature evolution in these regions and the crucial differences between GCMs and EMICs.

“Page 4694. The reference for the insolation is missing.”

The missing reference has been added to the manuscript.

“Page 4696. A typo. 115 instead of 1115.”

We apologize for the typo. It has been changed.

“Page 4699. It is disturbing to have each time series drawn on its own scale while a comparison is conducted between the series.”

We agree that the differences in the y-axis-scale make a comparison difficult. We will improve the figure by giving January and July temperatures for the same latitude band and between the different latitude bands the same y-axis scaling. However, we want to make sure that the large amplitude of for instance simulated high-latitude winter temperatures does not cause the relatively small amplitude changes in the equatorial region to become unrecognizable.

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