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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "How cold was Europe at the Last Glacial Maximum? A synthesis of the progress achieved since the first PMIP model-data comparison" *by* G. Ramstein et al.

G. Ramstein et al.

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Note to the Editor and Reviewers:

In the course of our manuscript revision, we have discovered an error in the sensitivity experiment to LGM vegetation described in section 3.1. We have therefore corrected the corresponding figure (Figure 3) and the text of section 3.1. The general conclusions of this section are not altered.

Response to reviewer 1

Comment: The large disagreement between model simulations and data reconstructions of the temperature in Europe at the LGM has been one of the big issues in PMIP,

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and the subject of this paper is therefore highly worthwhile. My main queries are: 1. What findings have already been published in Wu et al 2007? Unfortunately I could not find a published version of the paper at the time of writing this review. The main finding of the paper by Ramstein et al seems to be that the reconstructions from Wu et al are closer to the PMIP model simulations than the older reconstructions. If Wu et al make this point in their paper then I am not convinced that the Ramstein paper reaches substantially new conclusions.

Reply: The first referee could not get a version of Wu et al (2007). This paper is now accessible on the Climate Dynamics web site (http://www.springerlink.com/content/f3862x002258785w/). We provide it together with this response. Because the referee did not have the opportunity to read this paper, he wondered whether our own manuscript was not similar to it. As he may see by himself, the objectives of both papers are very different. In our paper our major issue is twofold: 1. Demonstrate that all sensitivity experiments performed by modellers (increase resolution, account for ocean dynamics, vegetation changes) failed to reproduce the large MTCO decrease at LGM over western Europe as depicted by published pollen-based reconstructions, and so did the attempt to take climate variability changes in vegetation modelling. 2. Show that the most recent reconstructions from Wu et al (2007), based on inverse vegetation modelling and accounting for the impact of a lower CO2 concentration on the vegetation, are in much better agreement with the recent model results (from the PMIP2 project).

The Wu et al (2007) paper describes reconstructions of different climatic parameters based on Inverse Vegetation Modelling (IVM). One major difference between the statistical and IVM approaches is that the statistical methods used in previous studies are calibrated for pollen originating from plants growing under modern climate space and modern levels of atmospheric CO2 whereas inverse modeling does not require such a calibration. The IVM solution can use a mechanistic approach that allows random climate generator to sample outside the modern observed climate space, and consider

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the interactions between plants and climate, atmospheric CO2 concentration in the past. However, the Wu et al (2007) paper does not include a model-data comparison, which is the subject of the present manuscript.

The conclusion of our paper is new in terms of modelling. For years modellers tried to simulate much lower MTCO, and this paper summarizes all that has been done at LSCE/CEREGE/LGGE. It seems that now that MTCO reconstructions account for low CO2, there is no conflict between model results and reconstructions anymore, at least for MTCO.

Comment 2: Figures 1, 4 and 7 show that one of the reasons for the models now being consistent with the new reconstructions is because the error bars have increased in the Wu et al data so that the model results lie within the error bars. This should be commented on in the paper. Figure 7 also shows that the PMIP1 model results are in better agreement with the data than the PMIP2 model results. Please suggest reasons why this might be.

Reply: The referee is right. In fact, Figure 7's error bars represented the 95% confidence interval while other error bars on figures 1 and 4 show the 90% confidence interval. In the revised version of the manuscript, we have used a consistent definition of the confidence intervals, at 90%. Even with a consistent definition, the confidence intervals for the IVM's reconstructions are much larger than for the PFT ones. In fact, even though the range of IVM reconstructed temperatures encompasses those estimated with the PFT method by Jost et al. (2005), the average IVM anomalies are warmer than the PFT ones by as much as 9°C (Fig.4 and 7). Both these aspects (changes in the mean and changes in the confidence interval) were rapidly discussed in the original version of the manuscript, but we have expanded the discussion in the revised version to state these ideas more clearly. (section 5)

Comment 3: Section 5 is the most important and relevant part of the paper, and should therefore be more detailed, and/or remove the earlier sections which don't contain

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substantial findings. The end of section 5, starting at line 5 on page 208, overrides the findings in the previous sections and is too brief, compared to the detail in the previous sections. The discussion should be expanded. Also, if MTWA and MAT are still to be commented on in the text at line 8 then a figure needs to be added to illustrate this result.

Reply: We agree that section 5 was too brief and we have substantially increased it. As suggested by the reviewer we have extended the end of this section which summarizes all the tests described in this paper to reconcile data and model results.

Besides, we have decided not to comment MTWA and MAT, since it would significantly lengthen the paper and the number of figures.

Specific comment, Section 2: - The authors argue that increasing model resolution should improve the representation of a given climate because certain features, e.g. mountain glaciers, and complex coastlines and topography, are not resolved in GCMs. Is a resolution of up to 50 km sufficient to represent the glaciers, or complex coastlines and topography? I'm not sure that it is, in which case, what does section 2 tell us, apart from that 50 km might not be high enough resolution.

Reply: Section 2 shows the results of 3 different models, using 3 different methods to increase their resolution over Europe. Compared to the resolution of the PMIP1 models (˜ 400 Km), these models use a substantially increased resolution (˜50km) without producing any further cooling of MTCO. We agree with the reviewer that an even higher resolution might be needed to capture the specificity of each site, and this has been added to the text. On the other hand, the comparison with the new reconstructions leads to a much better agreement and it might not be necessary to spend a lot of time running experiments at very high resolution for the purpose on which this manuscript focuses. Nevertheless, forcing a limited area model such as MM5 would certainly still be interesting for paleoclimate studies.

Specific comment: Do the authors think that the simulations presented in Jost et al

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are long enough for the models to have adjusted to the large LGM forcing (1 year for CCSR1 and LMDZHR, 4 years for HadRM), and are averaged over a long enough period (10 years for CCSR1 and LMDZHR, 5 years for HadRM) to account for interannual and interdecadal variability? If not then some caveats should be included, or this section should be removed.

Reply: The Jost et al paper is based on AGCMs and the forcing (in particular in terms of Sea surface temperatures and sea ice) is a climatological forcing, i.e. is repeated each year of the simulation. This duration for AGCM experiments is classical in the literature and has been chosen in the PMIP1 protocol. Besides, the high resolution experiments described here are time consuming because of the resolution. The reviewer is right to point out that changes in interannual variability could not be studied in these simulations. In a sense, this would also be the case in longer simulations because of the climatological forcing. In the present manuscript, we do not discuss such potential changes in interannual variability using these simulations. We have made those points clearer in the manuscript.

Specific comment: What about using another technique for downscaling from the GCM, for example a statistical technique to produce information at the individual pollen sites, rather than a gridbox? Reply: This is another solution indeed. We mention it in the perspectives of our work.

Specific comments, Section 3:

- Page 204, line 26: Figure 4 seems to show that the discrepancy for the PMIP2 simulations is actually worse, and not “unchanged” as stated in the text. - Line 28: this statement needs clarifying. How different are the PMIP2 AOGCM SSTs from the CLIMAP SSTs in this region? A few sentences summarising the differences would be useful, and perhaps include a figure to illustrate. Reply: Both comments are related to each other. We now refer the reader to earlier model-data comparisons performed for the North Atlantic sea surface temperatures (Kageyama et al, 2006). The PMIP2 AOGCM

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SSTs (and sea-ice cover) are very different from the CLIMAP SSTs: the temperatures are warmer in the North Atlantic, and the sea ice less extensive. These conditions make it more difficult to obtain a larger cooling over continental Europe, especially on its western part, near the Atlantic ocean. This is now better explained in the manuscript.

Specific comment, Section 4:

- Page 204, line 23: are these 5 sites included in the results shown in the other figures? If not, why not choose a subset from the sites used in the rest of this paper for consistency, or include these sites in the other figures. Reply : These 5 sites are indeed in the results shown on the other figures. We have selected these 5 sites because they are very well documented and dated for the LGM period under focus, and because even though four of them are located on the most southern region of Europe, they all record steppes and tundra during LGM. The fifth site (La grande Pile), located northward, outside the Mediterranean domain, is considered as a control site, being representative of the European continental climate in regions that were not covered by ice during LGM.

- I don't understand the rationale for the different experiments presented in this section. In particular, in the "third scenario" why choose a factor of 3? Is it chosen for a particular plausible reason? Similarly, in the "last experiment", why are mean T anomalies 1.25 colder, and P anomalies 3 times smaller? There needs to be some further explanation for the motivation for the design of the experiments.

Reply: Figure 5 shows the main steps of a larger simulation protocol developed to test the climate variability effect on vegetation composition and dynamics. We first needed to test the effect of a CO2 change from the present 345 ppm to the LGM value of 200 ppm. Indeed, as shown by Wu et al (2007), such a parameter plays an important role in the improvement of pollen-based reconstructions. Here we use LPJ, which is different from the model (BIOME4) that Wu et al (2007) use in their Inverse Vegetation Modelling method. We show that such a CO2 change (experiments 1 with present-day CO2 concentration and 2 with LGM CO2 concentrations) affect vegetation functioning

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by reducing the Leaf Area Index (LAI) values, but that it does not significantly affect the vegetation composition. Based on this first result we then focus on the climate variability effect on vegetation composition, this second parameter being hypothesised to be responsible for vegetation composition change from past steppes and tundra toward modern forest, as if vegetation composition reflected more the extreme events, such as minimum temperatures reached once in a while in the simulations, than the average temperatures fot the coldest month. We have used several temperature variability increase factors from modern variance to 3 times modern variance, which creates important value fluctuations in climate data and more particularly extrema (minima) temperatures, but such variability does not affect vegetation to the point that grasses would dominate everywhere (Experiment 3). In fact the current range of temperature variability (1xVariability) produces more grass development than under 3xVariability. From that point we have worked on testing temperature average changes. We tested several scenarii of average temperature anomalies from 0.5 to 3 times the average anomaly computed from IPSL simulations. The fact is that up to 1.25 anomalies, grass production increases (it dominates at La grande Pile see output from Experiment 4) whereas above 1.25xTemp. anomaly, vegetation from LaGrandePile disappears (not shown). We then tested the increase in temperature variability anomaly again, once the 1.25 Temper. Anomaly was fixed, but the increasing variability had no effect on vegetation change (not shown). In addition, we have also tested the effect of rainfall changes without changing temperature. Similarly to temperature, variability change had no effect on vegetation composition as compared to average changes, which produced grass dominance in Ioannina and Ghab sites with a reduction of average precipitation by a factor of 3 (Experiment 5). Finally, the best combination of temperature and precipitation changes to simulate steppes and tundra dominance using LPJ was reached when combining both results: 1.25 average anomaly on temperature and 0.3 average anomaly on precipitation (Experiment 6). We have therefore selected the most relevant experiments instead of showing all the results from our simulations. The important point is that the simulated vegetation is not very sensitive to changes in climatic

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variability, applied on the modern or the LGM climates. We have modified the text so that the rationale of the experiments and of Figure 5 is clearer.

Specific comment, Section 6, final sentence: add a few sentences to explain why these new models will lead to a better understanding of the reconstructions. Reply: Ok, done

Specific comment, Figures 1, 2, 3, 4 and 7 show model results up to 70N, but data only up to 48N. It would be very useful to show data at higher latitudes if possible. If not, then why show the model results at these higher latitudes? Reply: The figures have been redrawn.

Specific comment, The figure legend should explain what the diamonds and squares represent in figures 1, 2, 4, 7. Also, explain what the shapes mean in Figure 6. Reply: The symbols on figures 1, 2, 4 and 7 are associated with the type of reconstruction (Peyron et al 1998, Tarasov et al 1999, Jost et al 2005, or Wu et al 2007). This has been precised in the legends of the figures. For figure 6, the box plots indicate the differences between the results of the inverse vegetation model (IVM) and those obtained with plant functional type method (PFT, Tarasov et al., 1999b; Jost et al., 2004). Boxes indicate the interquartile intervals (25th and 75th percentiles), and the bars are 90% intervals (5th and 95th percentiles). Horizontal bar in the boxes is the median, and the colored symbol is the mean value. This has been added to the figure caption.

Response to reviewer 2

Comment: I concur with “Anonymous Referee #1” that this manuscript addresses a key issue for PMIP and the data-model community in general, and I share the frustration of not being able to evaluate the overall between this paper and the Wu et al. paper (in press).

Reply: The Wu et al (2007) paper is now available from Climate Dynamics, and we provide it along with this response for the reviewer’s information. As pointed out in our response to reviewer 1, the Wu te al (2007) paper present the new reconstructions based on Inverse Vegetation Modelling, while the goal here is to compare model results with data.

Comment: In addition, there are several parts of the manuscript that require attention before the reader can assess the validity of the conclusions. In particular, many of the figures are either missing information, have very small fonts and symbols, or are poorly explained.

Reply: We thank the reviewer for his comments and have attempted to take his advice into account while revising the manuscript and figures.

Comment 1: The regions north of 50_ N were largely covered by the ice sheet during the LGM. The simulations are apparently strongly by this ice sheet and there is no possibility of acquiring pollen data from this area. This is also the area of the greatest disagreements among the models, and the reader's eye is drawn to these discrepancies (which are not relevant to the subject of this manuscript). So why not truncate the figures at 50N?

Reply: The figures have been modified to only take into account continental areas free of ice.

Comment 2: For Figure 1, is there any meaning implied by switching from squares to triangles between the two panels?

Reply: Yes there is. The symbols refer to either the Peyron et al (1998) or the Tarasov et al(1999) reconstructions. This is now précised in the figure caption.

Comment: What are the error bars representing (2 standard deviations?).Are the error bars the same for all figures?

Reply: The error bars are the 90% confidence interval in all figures where they are given, except in figure 7 where they show the 95% confidence interval. In the new version of the manuscript, this has been standardized to the 90% confidence interval in all the figures.

Comment 3: For Figure 2, it would be helpful to specify the resolution of the model next to the model in the legend.

Reply: Ok, done

Comment 4: Figure 4 has the PMIP1 results in gray with PMIP2 results in color. The result is pretty confusing - I suggest not including the PMIP1 results on this figure.

Reply: We would like to keep the figure as it is for easier comparison.

Comment 5: I think that that figure 5 is a great idea, but it is not well executed. The text is too small and the color blocks for the legend are tiny. The text does not explain how well the simulated modern vegetation agrees with the observed - are there any systematic biases?

Reply: We have increased the size of the legend color blocks, as well as the size of the five histograms and the caption font. We have also modified the text related to Figure 5 to better explain the simulation scenarii and results.

Comment 6: Are the results from the inverse vegetation modelling methods in Figure 6 from Wu et al.? Reply: Yes they are.

Comment 6, continued: On this figure, what are the meanings of: a) the bars in the middle of the diamonds (median values?), b) the colored dots, c) the limits of the diamonds (1st and 3rd quartiles?), and d) the limits of the whiskers?

Reply: The meaning of the symbols is given in fig. 6 caption of the revised manuscript, as described in our response to reviewer 1.

Comment 7. What is the relation between the Inverse results presented in Figures 6 and 7? They do not appear to be the same and it is not clear why this is so.

Reply: This was the result of using the 90% confidence interval in the case of Figure 6 and the 95% confidence interval in the case of Figure 7. The regional temperature anomalies (e.g. West Europe) in Figure 6 were calculated from the data shown on

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figure 7.

Comment: I think that it is unfortunate decision to include Mean Annual Temperature (MAT) as a variable for reconstruction. Similar MAT values can result from either a maritime climate with a small range of temperature variations over the course of the year OR from a continental climate with hot summers and cold winters. Why not use growing degree days as a measure of the annual energy budget?

Reply: In the new version of the manuscript, we focus exclusively on MTCO and all references to other climatic variables have been removed.

Interactive comment on Clim. Past Discuss., 3, 197, 2007.