

## ***Interactive comment on “Coupled simulations of the mid-Holocene and Last Glacial Maximum: new results from PMIP2” by P. Braconnot et al.***

**P. Braconnot et al.**

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First, we would like to thank the reviewers and the editor for their useful comments that helped a lot to improve the manuscript and the presentation of the results. All the comments converged to suggest that additional information was needed either on the description of the PMIP2 protocol, or on the discussion of the results. Reviewer 2 and Julia Hargreaves also had concerns with the lack of focus of the manuscript in several places. To address this point we decided to follow the suggestion of Julia Hargreaves and to split the paper into two parts. Thanks to this we have been able to improve the presentation of the PMIP2 experimental protocol, and to add figures on model-data comparisons in the first part. The second part is better focused on the location of the ITCZ and the role of feedbacks from the ocean, vegetation, snow and ice. It was however not possible, given the difficulty of the subject and the present

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state of the PMIP2 database, in which too many fields are still missing, to fully address the questions on the vegetation feedback and the relationship between the relative changes in precipitation in Africa and the precipitation of the control simulations. We have therefore decided to better present the point and to open the discussion. In addition all figures have been redrawn, and it is now possible to identify the different models. The two manuscripts are now : Results of PMIP2 coupled simulations of the mid-Holocene and Last Glacial Maximum. Part 1: large-scale features

Results of PMIP2 coupled simulations of the mid-Holocene and Last Glacial Maximum. Part 2: feedbacks with emphasis on the location of the ITCZ and mid- and high latitudes heat budget.

We respond to each of the reviewers below, highlighting his/her original question in italic first, before each answer or comment is given in normal font.

Referee 1

.Interestingly, it appears that models which have a wet bias in North Africa for present-day climate yield only a moderate change from present-day to 6k climate in this region. This seems to be a peculiar feature of the PMIP2 models. Earlier results do not corroborate this statement. For example, the ECHAM3-BIOME1 model has wet bias in present-day climate, but yields a considerable greening of the Sahara for 6k climate. The LMD5-BIOME1 predicts only marginal greening for 6k climate, albeit the present-day climate appears to be reasonably realistic at the first glance. In this respect, it is not really appropriate, when the authors state that in PMIP2, the Atmosphere-vegetation feedback appears to be less important than in experiments by Claussen and Gayler (1997) and Texier et al. (1997). The latter studies reveal quite a different biogeophysical amplification of the African summer monsoon - basically as a result of differences in simulated subtropical atmospheric circulation - as analysed by deNoblet-Ducoudré et al. (Climate Dynamics, 16, 2000).

We do agree with this comment of the referee. There are two different points in this

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remark. The first one concerns the relationship between the relative change in precipitation in West Africa and the precipitation of the control experiment. This fact is new and only appears in the simulations with interactive ocean. The Claussen and Gayler (1997) and Texier et al (1997) AV simulations were run with prescribed SST. We now raise the point in the text that the reason may be found in the coupling with the ocean, even though we are not able to provide the answer to the question at this stage. We have added this paragraph in the text of part 2. “Future investigation would tell if this reflects the fact that in coupled experiments the large-scale circulation and the local changes in deep convection over Africa are consistent, so that for a given model there is a relationship between the amount of moisture advected over the Sahel region, convection and precipitation. This is not the case in PMIP1 simulations, since in that case the prescribed SST may be incompatible with the heat fluxes and the wind stresses simulated by the atmospheric model. More investigation is needed to test this hypothesis.”

The second part of the remarks refers to the role of the interactive vegetation. This is true that previous studies lead to a different amplification of the African monsoon by the interactive vegetation. But, even in the Texier et al, the change in monsoon was substantial, and even larger than what Braconnot et al 1999 got from OA simulations. This is a reason why the combination of feedbacks is strong in Braconnot et al. 1999. The point here is that two of the models produce less change with interactive vegetation than with interactive ocean. We enlarged the discussion on this point in the second part of the paper. We added a figure showing the change in temperature and precipitation for the 3 models for which OA and OAV simulations are available in the PMIP2 database, and also discuss the differences between the control simulations. It is outside the scope of this paper to make a full analysis of the differences between these simulations.

.The authors mention that feedbacks other than the biogeophysical feedback could play a role. I think, it would be instructive to present the albedo values used in the different

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model configuration as much of the atmosphere-vegetation feedback in West Africa can be attributed to changing albedo values.

We found that this was a very good suggestion and we added a figure and a paragraph, showing the range of surface albedo in the PMIP2 control simulation compared to PMIP1 and the loose relationship between the albedo and the location of the ITCZ in the control simulation.

.The analysis of the snow and sea-ice albedo feedback confirms earlier studies which highlight the importance of the migration of boreal forests. The authors focus on the radiative impact of snow and sea-ice cover on climate change. It would be useful, if the authors could give some consideration to changes in atmospheric circulation. Wintertime warming in 6k climate at high northern latitudes could be either due to changes in the Arctic Oscillation or due to changes in coupled sea-ice albedo feedback and snow-albedo vegetation feedback of both. We have not included an additional figure on the dynamics for 6ka. The reason is that the warming is due to the feedback from ice, snow and temperature. In addition Gladstone et al. 2005 show that for most models there was nearly no change in the Arctic oscillation nor in the NAO at the annual or interannual time scale in the PMIP2 OA simulations. However we have added a figure on storm tracks in part one of the new version of the manuscripts to better show the link with sea-ice and the sst gradient for PMIP2 simulations.

.The authors confirm that the difference between LGM and present-day climate can be attributed to changes in inland ice and hence, in surface albedo, while changes in atmospheric CO2 concentration are less important (contribute only 50% of the albedo effect). This has been stated earlier by Berger et al. (1996, a report of the Institut d'Astronomie in Louvain-la-Neuve) or Berger (2001) in Geosphere-Biosphere Interaction (one of the co-authors, MC, should have the references) or Jahn et al. (Climate of the Past, 2005). Hence it would be interesting to reassess the old results in the light of PMIP2.

We added references to several papers in the conclusion, showing that what we found is consistent with previous studies. s (Hansen et al., 1984; Hewitt and Mitchell, 1997; Shin et al., 2003; Jahn et al., 2005; Otto-Bliesner et al., 2006).

Minor comments: 1) Page 1297, line 20 ff.: Not only PMIP2 simulations are used to study feedbacks in the climate system. Actually, the assessment of feedbacks in the climate system was the focus of a number of experiments using EMICs ranging from the early papers by André Berger's group to the EMIC intercomparison projects.

The sentence is not limited to GCM, since EMICS also participate to the PMIP project. The objective of this paragraph is to make a rapid history of the PMIP project, not of the analyses of feedbacks. .

2) Page 1299, line 22: "Thus, the role of vegetation and feedbacks due to vegetation ca be analyzed." This statement contrasts with a latter statement on page 1313, line 15, that a strict analysis of vegetation feedbacks is hampered by the fact that OA and OAV experiments for 6 ka do not share the same control experiment. Indeed, a careful analysis of feedbacks and amplification of feedbacks by additional feedbacks would require 2n independent experiment which presents a huge effort.

The part has been rewritten and additional comments have been introduced about the control simulations between OA and OAV experiments. We hope that now, there is no more ambiguity between the different sections of the papers.

3) Page 1302, line 10 and subsequent paragraphs: Could one not summarise the Results listed in this section in a table - just for the readers' convenience. All the numbers can be found on the figures. The figures have been redrawn and we hope are now easier to read.

4) Page 1303, line 6: ice-5G or ICE-5G ? Corrected

5) Page 1307, line 5/6: I guess, it should read Braconnot et al. Thank you, this is a pb with the formatting of the reference

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- 6) Page 1309, line 1: event or even? It should be even, corrected
- 7) Page 1313, line 8: Here, we have AOV experiments, instead of OAV experiments. Oceanographer seem to prefer OAV models, meteorologists, however, AOV. Never mind, but it should be used consistently. Here we normally adopted OA and OAV everywhere. Consistency has been checked.
- 8) Page 1318, line 16: Is there a plural of albedo? Perhaps, “albedo values” is better than “albedoes”. This came from a native English<sup>Œ</sup> the section was rewritten
- 9) “-”, same line: 3/3 (?) or £ to the feedback ? It is a typo error ¿ is the right ratio
- 10) “-”, last line: W/m2 instead of W/m<sup>2</sup>. The units should be written properly throughout the document now.
- 11) Page 1322, line 13/15: a relationship is found &#711;E between the ratio of the precipitation change and modern precipitation ? a word is missing :the ratio of the precipitation change over modern precipitation
- 12) Page 1324, line 15: Who is R.W.b.e. al.? Again, this is a problem with the automatic formatting of the references.
- 13) Figure 5, Caption, last sentence: Results from PMIP2 OA E&#711; ; , but the header to figure 5 says PMIP2 OA OAV. ? The figure has been redrawn and the caption corrected.
- 14) Generally, almost all figures are hard to read (in the print version of CPD). Some information, for example in the head lines of figures 1 and 3 (is this information really necessary?) and the labels in figure 10b cannot be read at all. What about the head lines in figure 5 and 7? Would it not be sensible to simple have labels a), b), &#711;E like in the other figures. By the way, these labels a), b), &#711;E are missing in figure 6. Hence all figures should be redrawn thereby providing readable labels etc. All figures have been redrawn and models can be better distinguished. Labels have been added on the figures.

Referee 2.

.My main concern with this paper is that it lacks focus. The authors state that their goal is to provide an overview of PMIP2 results for LGM and mid-Holocene, highlighting the change in global temperature and precipitation. As the authors note, quite a few papers have been recently published that discuss different specific aspects of the new PMIP2 simulations. This manuscript presents a very general overview of the global response in temperature and precipitation, extended by a more detailed discussion of some aspects of the simulations that are not really covered by the previous studies. These new aspects are mainly the location of the ITCZ and the strength of the snow and sea-ice feedbacks. Several interesting new results are presented, but these results are not always analysed in detail, such as the counterintuitive relation between precipitation change at 6 ka and the precipitation in the control experiment (shown in Figure 9), or the finding that interactive vegetation induces land surface processes in favour of a drier climate (p. 1313). This is not very satisfactory and I would urge the authors to provide a more in-depth analysis in these cases. The paper is already quite long, so extending it to provide a detailed explanation for these interesting results seems not an ideal solution. One alternative could be to focus the paper on Section 4, and to publish the results presented in Section 5 elsewhere.

As indicated in the general introduction to our response to the reviewers, we have decided to follow the suggestion from the editor and we split the paper into 2 parts. Part 1 covers section 1 to 3 of the original manuscript, as well as part of section 4 on the evaluation of precipitation in the Sahel and North of India. We have also included a comparison with data using the benchmarking diagrams developed for PMIP1. The second part focuses on the feedbacks on the ITCZ and on the role of snow and sea-ice in mid and high latitudes. The discussion on the link with the control simulation has been enlarged. Unfortunately it is not possible to go very far in the analyses at the moment. The reason is that it is very difficult to understand differences between models, and the present state of the database does not allow to perform a complete

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analysis of the heat budget, and of the water budget for all the models. All the variables required for the database were not provided for all the models. This is why we have decided to better show the differences between OA and OAV simulations, both for the climate change at 6ka and for the control simulations. We have also added a diagnostic on the surface albedo.

Specific comments .The title is not very informative. I suggest replacing ‘new results from PMIP2’ by something like ‘the hydrological cycle in the tropics’ (if the paper is focused on Section 4). We change the titles they are now :”Results of PMIP2 coupled simulations of the mid-Holocene and Last Glacial Maximum. Part 1: large-scale features”, and

“Results of PMIP2 coupled simulations of the mid-Holocene and Last Glacial Maximum. Part 2: feedbacks with emphasis on the location of the ITCZ and mid- and high latitudes heat budget.”, respectively for the first and the second part.

.Page 1300, line 17-18: ‘the models are run long enough for the trends over the final 100 years to be small’. Please specify how you have defined ‘small’.

In the PMIP2 protocol it is up to the modelling group to decide if the simulation is correct or not, and if the trend is reasonable. However, some of the models show substantial drift. The trends in surface air temperature are now provided in a table for all the simulations, and a paragraph has been added to comment the numbers.

.Page 1300, line 27: ‘For most of the modelling groups, the version of the GCMs used for PMIP2 is identical to the version used for future climate change predictions’. I propose to indicate in which cases model versions were used that are not identical, e.g. in Table 2. All the models listed in Table 1 were used to run the IPCC AR4 scenarios. The point is that the IPCC version is either slightly different from the PMIP one, or the resolution higher. We indicate in the text that only IPSL and MIROC3.2 med res, were run with exactly the same model and resolution for past and future climates.

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.Page 1301. I suggest mentioning that different atmospheric model components have been used in PMIP1 and PMIP2. So the differences between results of PMIP1 and 2 are not just due to the different way the oceans are treated (and to a lesser extent the ice sheets), although this is probably the dominant factor. And what about the land surface components?

We added this remarks in the text. It is indeed important to realise that the coupled simulation are better than atmosphere alone simulation performed several years ago, mainly because of the improvement of the atmospheric model. Also the surface schemes improved a lot during between PMIP1 and PMIP2.

.Page 1301, line 2-3: 'They also all include a sea ice model' Do all models include a thermodynamic-dynamic sea-ice model? Yes they do, at least to the knowledge of the lead author.

.Page 1306, line 17: 'results for 6 ka suggested that the northern limit of precipitation shifted to the north over the Sahel region'. Please explain what results. The section has been reorganised and the explanation on the basic mechanism of monsoon change at 6ka is introduced in the first part (section 3.2)

.Page 1309, line 13: ' suggest that the change in the ocean circulation has a large impact'. Please explain what change. The paper is reorganised and the ocean feedback is discussed in section 3 of part 2.

.Page 1312, line 24. 'More investigation is needed to fully explain it'. As mentioned, this is not very satisfactory. What do you mean by being 'saturated'? Please elaborate. We do agree with this and we would have liked to be able to provide an explanation. However it is not straightforward and too many data are missing in the database to be able to make a complete comparison at this stage. We decided to enlarge the discussion and formulated hypotheses for future investigation.

.Page 1313, line 10: 'Part of it' Please explain. Part of what? Paragraph reworded

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.Page 1313, line 25: 'Additional analyses are needed to fully understand this result'. As mentioned earlier, I would propose to provide these analyses in this paper. It would make the paper much more interesting. See previous answer on the same line .Page 1315, line 17. 'Interestingly in the southern hemisphere sea-ice was larger during summer and smaller during winter in the PMIP1 simulations'. It is not clear to me why the results for PMIP1 are different. Is this just due to CLIMAP's seasonal reconstruction for sea-ice cover? We checked the CLIMAP data and indeed absolute values have the right seasonal cycle. The point is that there is more sea-ice in CLIMAP, so that the difference is maximum during the austral summer. This questions the reconstruction of sea ice in a region where the data cover is poor. Of course results from the OA and OAV simulation may also be erroneous.

.Page 1316, line 3-4: 'This can be attributed to the colder conditions induced by the ocean response to the insolation forcing'. Please explain in more detail what the ocean response to insolation forcing is. For instance, how much colder is the surface ocean, and where? How long is the lag in the ocean's response? The precision has been added.

.Page 1299, line 11: '0 k' should be '0 ka'. Please use 'ka' throughout the manuscript. Done

.Page 1300, line 18: please rephrase the sentence starting with 'The last 100 or 200 years' Sorry, we do not see the point.

.Page 1301, line 7: 'Earth system model' should be 'Earth system models'. Done

.Page 1301, line 12: 'simulations of mid-Holocene' should be 'simulations of the mid-Holocene' corrected

.Page 1308, line 8: ' of precipitation for precipitation located to the north of the maximum precipitation.' Please rephrase. Done

.Page 1309, line 1: 'event' should be 'even' Corrected

.Page 1310, Section 4.2. I propose to first discuss the results for the LGM and subsequently those for the mid-Holocene, as done in the rest of the paper. done

Page 1313, line 13. ' for which the vegetation is prescribed the vegetation simulated'. Please rephrase. Done

.Page 1314, line 9. 'For each the model' please remove 'the'. done

.Page 1318, line 16: '3/3 ' should presumably be '3/4'. Yes. This has been corrected.

.Page 1320, line 3: 'strengths the warming' should presumably be 'strengthens the warming'. yes. This has been corrected.

.Figure 1 and 3 are too small, I cannot read the scale. All figures have been redrawn

.Figure 2: It appears that only results of 5 AO models are shown instead of 6 models as indicated in Table 2. Why? Because the group did not provide this variable for the mean seasonal cycle.

.Figure 6: Please provide (a), (b) etc. and units. An explanation of the different markers is also missing done

.Figure 7: I suggest to explain in the caption that the control experiments are different from Figure 5, and that the model subset is different. Please also explain the meaning of the +-markers. Done.

.Figures 10, 11, 12, 13, 14, 15: please provide units Done

.Figures 11, 13: please modify header, should be NH and SH instead of HN and HS. The lead author is FrenchË correction done

.Figures 11-15: Please make the x-axis labeling consistent, as the notation for 'months' is different. It is now fixed, it is part of a problem when using the ferret software with so many files opened simultaneously, each of them having its own definition of the time axis (the definition and the specific calendar of each model, a nightmare for the

analyses).

.Figure 13: what does '1000' mean? Why are the values for January missing? Same answer than above.

.Figure 14, caption: 'substet' should be 'subset', 'waver' should be 'wave'. Corrected.

.Figure 15: I suggest to remove Figure 15, as it is not really discussed in the text. This figure has been removed

Comments from Julia Hargreaves

Comment 1: .When I questioned the length of the paper, the desire of the authors was that this paper not be split up into LGM and mid-Holocene sections because of the requirement for a single reference for the PMIP2 project. My first comment arises from this suggestion. If the paper is to be a reference for the project then it should present the set up of the experiments as far as possible to be reproducible by other scientists who were not involved in the project. The authors should bear this in mind while revising the manuscript and make sure all important details are included.

We decided to follow your recommendation to split the paper into two parts. We hope you will find that each part is better focussed and that the overall study is more interesting and more informative this way.

Examples: .Section 2.1 Table 1 which contains most of the important details should be referenced sooner. We included the reference to Table 1 at the beginning of section 2.

.There should be a reference for the present day distribution of vegetation. This is not possible because the vegetation used is model dependent. This information has been added in the presentation of the control experiment.

.Perhaps the question of spin-up times should be further elaborated. Weber et al (this issue) found evidence that not all the models had reached equilibrium, so perhaps this problem needs highlighting. Yes you are right and referee 2 raised the same point. A

table with trends in surface air temperature and a paragraph explaining it have been added in part 1.

.Just my personal gripe, but I would like to see discussion of the extent to which the protocols are intended to produce realistic simulations of the climates in question and to what extent they are intended to be model sensitivity tests, only loosely based on the paleo-climate epochs in question. We added comments to answer this question in the text, when describing the experimental protocol.

.Section 2.2 It would be interesting to see some overview of which variables/timescales were included in the database. Should there be a reference for CMIP? It is not possible, because the list is quite long. This is why we refer to the web site where the lists for all the components and the frequency of the outputs can be found.

.I think a column showing if the models listed participated in CMIP could be added to Table 2. In Section 2.2 it says, “we also present model results from PMIP1”, and yet I do not think those models are included in Table 2? If that is the case, please label Table 2 clearly as PMIP2 models only. All the models listed here, or a very closed version, were used for IPCC AR4. Only IPSL and MIROC were run with exactly the same version and resolution. This is in the text.

.In several places it is written “one model” or “most of the models”. Why do you not name which models show which behaviours? It seems like you are hiding something by excluding this information. For example, some people might assume that EMICs are the outliers! Also, don’t individual modelling groups want to be able to see easily where their model lies in the ensemble? As someone not involved in the PMIP2 runs, I would still like to know where “our model” (MIROC) lies, but I can’t work it out from the text or figures. The original idea of this study was to use the different model results as an ensemble. But I understand your point, and all the figures have been redrawn to as to highlight the different models, but only for PMIP2.

Comment 2: Section 3 .While the model results of PMIP1 and PMIP2 are compared

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and shown to have differences, there is no indication as to which is a better representation of the climates of 6ka and the LGM. While I don't expect a detailed comparison with data, some discussion of whether the data are more consistent with PMIP1 or PMIP2 would add some useful context. Without this context I find this section rather meaningless. A comparison with data was added in the last section of part 1 for the tropical regions. For the other regions, we only refer to already published studies.

Comment 3: .In general I find the manuscript rather rambling. I don't really grasp the importance of all the details and wonder if they are all required. I am sorry to be so vague about this part but my impression is that a thorough revision of the text with the aim of increasing the clarity of the arguments presented could greatly improve the manuscript. This is true and we hope the new versions will not give you the same feeling

Minor comments: Section 1 "Section compares the large..." A number "3" is missing? Corrected.

Section 3 You might say what model variable you are using for the temperature. Is it surface temperature, or 2m temperature or some other variable? Done

I think "dispersion" is a confusing word and would just use "range" or "spread". In the text, "cooling between DJF and JJAS (Figure 2c)". JJAS is not defined in the paper. In the caption to Figure 2 it says not "JJAS", but "JJA". Which is it? We have tried to avoid using the word "dispersion". Section 4.1 "pannel"->"panel" Corrected

"..some of the additional simulations included in this set". I am not sure what this means. Do you mean some of the simulations included in Figure 7 that are not included in Figure 6? The section has been reorganised Section 4.3 "no show"-> "not shown" "Part of it"-> "Part of this" corrected

"Indeed, in order to quantify the vegetation feedback without ambiguity, one should compare the OAV 6ka experiment with an OA 6k experiment for which the vegetation

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is prescribed the vegetation simulated in the OAV 0k experiment, as suggested by the PMIP2 protocol.” As well as containing a number of typos, this sentence does not make sense to me in that I cannot work out what it is that is suggested by the PMIP2 protocol. We now present the best solution when describing the PMIP2 protocol. It should be easier to understand this part now that has also been revisited.

Is it OAV or AOV? There is an AOV in the first paragraph of this section. It should be OAV everywhere

Section 5.1 “account” -> “accounts” Corrected

Section 5.2 “In addition, it is difficult with standard model output...” What does this mean? I suspect you refer to the limited model variables available on the PMIP database, but I am not sure. Please clarify. It is because all the variables were not provided for all the models. This limits a lot the possibility to produce analyses including all the model results. Interactive Discussion “...estimations of cloud radiative forcing is ambiguous over highly reflecting surfaces..”. Do you mean, “...the estimate of cloud radiative forcing over highly reflecting surfaces is uncertain.” ? Yes, I suppose ambiguous is a gallsicism. In fact, I do not see clearly the difference here.

Acknowledgements: I think you had better clarify the date stamp. Is it 10th January 2006 or 1st October 2006? OK; done.

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Interactive comment on Clim. Past Discuss., 2, 1293, 2006.

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