Clim. Past Discuss., 2, S794–S806, 2007
www.clim-past-discuss.net/2/S794/2007/

Climate of the Past Discussions

CPD

2, S794–S806, 2007

Interactive Comment

Interactive comment on "Linking glacial and future climates through an ensemble of GCM simulations" *by* J. C. Hargreaves et al.

J. C. Hargreaves et al.

Received and published: 18 January 2007

Thanks to the reviewers and commentators for their helpful suggestions. There was some overlap in the comments so we address them all here in a single response.

G. Schmidt Comment: **The principle result of this study is that there is an asymmetry between positive and negative forcings in the context of the LGM. This issue was also recently addressed in Hansen et al (2005, "Efficacies of Climate Forcings", JGR) and the results shown there are a useful context for these results. Specifically, Hansen et al show that reductions in greenhouse gases are less efficacious than increases in greenhouse gases in the GISS model (GISS-ER)- similar to the results shown here (figure 5 for instance). However, there are two elements to this. First, the radiative forcing in the model (adjusted stratospheric temperatures, using the WMO tropopause, Fa - Table 1) is slightly asymmetric:**

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

EGU

2xCO_2 is 4.12, while 0.5xCO_2 is -4.07. Secondly, the efficacy of these forcings is different as well (Ea=0.94 for a cooling, Ea=1.02 for a warming). This leads to an overall factor of 0.91 (=4.07*0.94/(4.12*1.02)) less temperature change for a nominally equivalent GHG forcing in the cooling case. This is almost exactly the factor seen in the mean of these experiments (i.e. 0.91*0.76 = 0.69 which is close to the mid point of the figure 5 historgram). Thus, there is already published support for these results in another model (at least for the pure GHG forcings). Note also that these are fully coupled model results. However, there are a couple of lessons as well. Firstly, the radiative forcings in any specific model cannot be assumed to follow the line-by-line calculations of Myrhe et al or IPCC (2001). These should be calculated. Forster and Taylor (in press) have these numbers for the AR4 models at 2xCO_2 and they range from 3.5 to 4.1 W/m2 (MIROC (medres) is 3.66 for 2xCO_2 so this might not be very important in this case, but the LGMGHG forcing should be calculated similarly).

Author response: *Both reviewers also touched on this issue, so we provide a single response here. As you surmised, we had indeed assumed the standard forcing ratio as Crucifix (2006) did based on the IPCC. For computational reasons the calculation of the radiative forcing for all members of our ensemble is outside the scope of this work. However, results from two different versions (termed the high and low sensitivity versions) of the T42 slab ocean version of MIROC3.2 (Yokohata et al), show that the response of the radiative forcing when CO_2 is increased (to 570ppm) or decreased (to 185ppm) in the model follows the expected logarithmic relationship. Since none of the parameters we changed in the EnKF exercise are related to the determination of radiative forcing we also expect the relationship to hold for our ensemble. Additional explanation has been added to Section 4, mostly in the 2nd paragraph, including reference to Hansen et al (2005).*

G. Schmidt Comment: **Secondly, the efficacies of other forcings - in particular those with significant spatial structure (surface albedo from the ice sheets, dust**

CPD

2, S794–S806, 2007

Interactive Comment

or vegetation) - may have values that are substantially different from those seen in the GHG case. Generally, those forcings which are weighted towards the poles have a bigger impact than those concentrated in the tropics due to the increased strength of the ice albedo feedback.

Author response: *Agreed, and we have mentioned in the conclusion that dust and vegetation may alter the pattern.*

——-

T. Schneider von Deimling

Reviewer comment: **The work of Hargreaves and colleagues is an important contribution towards analysing this issue by considering a perturbed parameter model ensemble which has been run for LGM and 2xCO2 conditions. They analyse the link between both climate states on a global, regional and local scale. As a key finding they infer a non-linear feedback behaviour between positive and negative GHG forcing from their model ensemble. This has impor tant implications for reducing uncertainty in climate sensitivity (by constraining model output with paleo data) if confirmed by other models. Furthermore the authors under took an important step to analyse the impact of the model parameters on the model response to give a physical interpretation of the asymmetric feedback behaviour. They conclude that a strong non-linearity in the cloud response for cooling and warming seems to be the key determinant. This work constitutes an important step towards gaining insight into the feedback behaviour of climate models, based on the temperature response to past and future climate forcing. Underlining the relevance of this type of study for reducing uncertainty in future climate change I would like to add some comments regarding the robustness of the main findings presented here.**

Author response: *We can't quite agree with your characterisation of a "strong" nonlinearity between warming and cooling - the typical cooling is 90% of the value expected*

CPD

2, S794–S806, 2007

Interactive Comment

from a log-linear response, which is certainly clear enough in the analysis but not such a major effect in terms of the other uncertainties present, and similar to that already estimated in the GISS Model E. We have emphasised more strongly the various uncertainties.

Reviewer comment: Major specific comments: **1. The conclusions drawn in this paper are based on an ensemble of versions from the MIROC model that cover a range from about 4-89702; C for climate sensitivity (CS). While the upper end of this range can be regarded as extreme, it gets obvious that the ensemble does not span the full range of plausible CS towards smaller sensitivities (there is a growing consensus of about 29702; C for a lower bound). Given the lack of ensemble members with small to moderate CS it is not possible to judge to what extent the conclusions drawn here are characteristic for high to ver y high CS model versions of the MIROC model or to what extent they are valid more generally. It would be informative to see whether the characteristics/correlations inferred are robust if the ensemble will be split into e.g. an ensemble covering CS<6K and an ensemble covering CS>6K. If systematic differences between those two ensembles are derived the ensemble with CS>6 should be given lower weight as its members are less likely to be consistent with paleo data.**

Author response: *The median climate sensitivity of the ensemble is close to 5.5C so we split the ensemble in two to investigate your question. We have found no evidence that the statistics of these two sub-ensembles are significantly different, in fact they are remarkably similar. For example, for the data illustrated in figure 5 the mean and standard deviation of the ratio of temperature changes for the 119 member ensemble are 0.68 and 0.11 respectively. The comparable values are (0.69,0.11) for the lower 60 climate sensitivities and (0.67,0.10) for the higher 59 sensitivities. Of course that does not necessarily mean that these results can be directly extrapolated to lower sensitivities. Indeed it appears that the MIROC model cannot produce a reasonable climate with sensitivity much lower than 4C (Annan et al, 2005). No change made to*

CPD

2, S794–S806, 2007

Interactive Comment

paper.

Reviewer comment: **2. The authors mention the omission of dust and vegetation forcing, which both - although of lower weight compared to the ice sheet and GHG forcing - crucially have contributed to the LGM cooling. In contrast to GHG forcing those two forcings are spatially strongly inhomogeneous and reveal a pronounced latitudinal dependence. When accounting for those two additional forcings in the experimental design one might expect that the difference between the LGM and LGM-GHG simulations will be larger than shown here (with possible implications for the conclusion that the LGM-GHG experiment is more similar to LGM than to 2xCO2).**

Author response: *The general point is noted, although it appears that even spatially inhomogeneous forcings tend to give somewhat similar global patterns (eg Hansen et al). Certainly we would expect to see cooler tropics with a more comprehensive set of forcings (as in your own work) and anticipate that the PMIP community will be moving in this direction to enable more quantitative comparisons of models and data. Comments added to Section 3 (just before section 3.1) and Conclusion.*

Reviewer comment: **3. The use of a slab ocean enables this kind of ensemble experiments with an AGCM - at the expense of neglecting effects of dynamical ocean changes. While those changes might not play so much a crucial role for 2xCO2 experiments it is quite likely that the strongly modified boundar y conditions of the LGM have a pronounced impact on the state of the glacial ocean, resulting in modified over turning circulation and modified meridional heat transports - which in turn have an effect e.g. on sea ice extent and the related ice albedo feedback. Up to now the few existing fully-coupled model simulations do not give a consistent picture of how dynamical ocean changes affect the LGM cooling. The point I want to make is that redoing this experiment with a fully coupled design (what would be far too expensive) would add a further aspect uncertainty. So especially when analysing the latitudinal aspects of correlations**

CPD

2, S794–S806, 2007

Interactive Comment

EGU

one could infer a (slightly) modified picture from a fully-coupled model design as one might expect that the impact of changes in ocean heat transport is more similar between the 2xCO2 and LGM-GHG experiment than between LGM and LGM-GHG.

Author response: *Agreed. Comment added to Conclusion.*

Reviewer comment: **4. My last main comment touches upon the issue of the choice of model parameters and the resulting spread of model feedbacks in the ensemble. The analysis of parameter correlations has revealed that asymmetry in the cloud feedback is most crucial for explaining the simulated temperature responses. In this regard it would be interesting to see to what extent the chosen parameters allow for different realizations of the other main model feedbacks (lapse rate, water vapour, albedo). This would allow judging whether the correlations inferred are primarily caused by a set of models which strongly differ in respect to their feedback behaviour of clouds - or whether they are representative for a model ensemble covering a broad range in the main model feedbacks (ideally those should cover a range comparable to structurally different GCMs, see the work of Colman, Climate Dynamics (2003) and Soden Held, Journal of Climate, 2005). It is to be expected that the different feedbacks show different forcing-response characteristics - an issue of importance especially when the latitudinal temperature profile is discussed. The authors mention in the text that they plan a further ensemble experiment in which different parameters will be varied. By quantifying the spread of the feedbacks in the current model ensemble one could get an idea in how far the current parameter choice is able to cover the feedback behaviour of structurally different models.**

Author comment: *Due to computational reasons it was only possible to output a small fraction of the possible variables from the whole model ensemble, so a complete answer to this question is outside the scope of this study. However, we varied a large range of different parameters (25 in total), and they are certainly not all related to only* *cloud feedback, but cover a wide range of feedbacks including those suggested by the reviewer. The parameters shown in the table are those related to the changes in T2. Different results may be obtained by looking at different variables, but this work has not been undertaken. We plan to look at the parameter effects in more detail later and in fact some work is in progress.*

Reviewer comment: Minor specific comments: **Page 954, line 26-28: The use of the term "simple climate model" is misleading. When talking about "simple climate models" one commonly refers to box models or EBMs. The model used in the study cited ranks among the class of intermediate complexity models which fill the gap between simple climate models and GCMs. Furthermore when referring to the MIROC model as "more sophisticated" one should be explicit at this point by clarifying that a slab ocean design was used - to make clear the difference e.g. to the PMIP-2 model design.**

Author response: *Agreed. Text changed.*

Reviewer comment: Page 958: **Line 5-6: 2-3 extra sentences to clarify the meaning of 0.24 would be helpful.**

Author response: *There isn't that much more to say - it is simply the 99% significance statistic from the student T test assuming independent samples from a normal distribution. This statistic is only used as a rough guide to significance in this paper to make comparisons between the different results clearer. The cutoff point is somewhat arbitrary, but some cutoff had to be used to make analysis of the 25 parameter data set tractable. Added "assuming a independent samples from a normal distribution" to the text.*

Reviewer comment: **Line 21: bias in air temp. over land ice The argument that this bias has no effect on the results as only temperature changes are analysed applies to the difference between 2xCO2 and CTRL which both reveal comparable areas of land ice. Yet the LGM climate is characterised by a much larger area of**

land ice, so the difference between LGM and CTRL should not cancel out? How large is the bias?

Author response: *There is a slight difference in the absolute values of the temperature changes between LGM and present day climate (for the control parameter set, North of 80N the maximum error is about a degree and south of that the error is much smaller). Although, for computational reasons it has not been tested, we do not expect the error to change much over our ensemble. When we stated that we thought is likely that our main results were unaffected we refer to the correlations between the temperature differences for the different experiments, rather than the values themselves. The paragraph has been rephrased to hopefully make this more clear.*

Reviewer comment: Page 961: **Line 18-25: The same point (that tropical SSTs and Antarctica provide a good area to constrain the ensemble) is made in Schneider von Deimling et al. and could be cited here.**

Author response: *Yes. Reference has been added.*

Reviewer comment: Page 963: **Line 8-16: The main issue is the exact location of the red line (here shown to be aligned at 0.76) and the related conclusion that 80% of the ensemble members show a weaker LGM response compared to 2xCO2. The value of 0.76 (given by the ratio R of LGM-GHG to 2CO2 forcing as 2.8/3.7) is consistent with the best-guess from IPCC for CO2 radiative forcing. I assume that this IPCC estimate is discussed here (no further information is given whether the numbers are derived from MIROC). The assumption is made that R is the same for all model versions. The chosen parameter perturbations are likely to cause different CTRL climate states, with e.g. different cloud patterns, which can result in different cloud radiative forcings. Thus slightly different values of the "real" radiative forcing for each model version might result if calculated by an offline scheme. It might prove that not only the cloud feedback reveals an asymmetric behaviour but the radiative forcing as well (to a certain extent). In**

CPD

2, S794–S806, 2007

Interactive Comment

case an offline calculation routine is already implemented in the model code it would be interesting to test (for a couple of runs) the robustness of R.

Author response: *See author response to G.Schmidt's first comment.*

Reviewer comment: Page 964: **Line 13-15: A few comments to what is meant by "behaviour of ice in clouds" would be helpful. E.g. what is meant by "distribution of ice in clouds" - is it mainly the latitudinal distribution, the vertical, or both?**

Author response: *It is difficult to be more precise, but prctau relates to the time scale for ice to be precipitated. Therefore, the higher the value, the larger the total amount of ice in the cloud. Sentence added for clarifying the definition of prctau.*

Reviewer comment: **Line 23-25: On the other hand the albedo feedback is likely to vary more strongly between different models for LGM than for 2xCO2. The variation of the temperature response depends on the variation on all main feedbacks.**

Author response: *In agreement with M.Crucifix's comment on the same sentence, this whole sentence has been removed*

Reviewer comment: Table 4: **For the analysis of feedback asymmetry behaviour it might be interesting to have a look at the parameter ranking for the ratio of LGM cooling vs. 2xCO2 warming?**

Author response: *The appropriate values have been added to the table. Section 5: Third paragraph adjusted. New paragraph added describing these results. Conclusion: Slightly rephrased ink between ice in clouds and the asymmetry.*

Reviewer comment: Technical corrections: Typing errors: **Page 953: line 26; p.954: line 16, line 26 citation "Schneider von Deimling" instead of "von Deimling"; p.959: line 1, line 19; p. 960, line 18; p.961: line 6; p.964: line 3; p.966: line 17?**

Author response: *Fixed*

Reviewer comment: Figures: **Fig1: contours and numbers too small to catch**

Author response: *You can see them if you zoom the pdf in the electronic version! A new figure had been made without contour labels, and I have increased the size of the figure to cover two columns*

—-

M. Crucifix

Reviewer comment: **Note that Crucifix shows that 3 models (including MIROC) out of a sample of four cool less at the LGM than expected from the 2xCO2 experiment. MIROC3.2 may therefore not be anomalous, as the authors suspect.**

Author response: *See G.Schmidt's first comment, which raises a similar point. Sentence added to end of Section 4*

Reviewer comment: **It is also quite impressive to see how much the resulting ensemble is compatible with palaeoclimate data evidence (page 959). Considered together with Crucifix 2006, this point raises the question of the apparent difficulty to formulate climate models that frankly contradict LGM data. Any comment ?** Reviewer comment: **If Figure 3 provided a robust estimate of the mean and uncertainty on the LGM / 2xCO2 temperature ratio, it would be possible to combine it to the LGM data uncer tainty to estimate the confidence interval of climate sensitivity. It is up to the authors to see whether this step can reasonably be crossed, given that the lack of structural differences between the ensemble members probably leads to overestimate the correlation coefficient between LGM and CO2 temperature changes. Likewise, would a 0.6 correlation between LGM and CO2 temperature changes in Antarctica be sufficient to effectively constrain global warming ? The authors response is certainly positive, but it might be worth substantiating this point by rough ("first order") mathematical argu-**

CPD

2, S794–S806, 2007

Interactive Comment

ments.

Author response (to the 2 comments above): *You raise the issue of whether models can "frankly contradict" the data, and also more generally the value of the LGM in constraining climate sensitivity. Although our modelled LGM simulations generally look reasonable in comparison with the data, the fact that we are missing some significant negative forcings suggests that the ensemble is probably too sensitive overall, even if the various uncertainties in forcings and responses makes this hard to quantify accurately (ie, we would expect a simulation with all the correct forcings to show some signs of disagreement even if not strongly). However, we don't expect to establish strict and tight bounds on climate sensitivity by this approach alone, but the issue is (in our view) more to investigate how best to use evidence from the LGM state to contribute to an overall assessment (eg Annan and Hargreaves, 2006). As you correctly note, with only a single model available, our results are necessarily rather tentative. We understand that LGM simulations are planned with some ensembles of the HADSM3 model (as used by Murphy et al., 2004; Stainforth et al., 2005) and it will be interesting to see whether their wide range of climate sensitivities generate weakly or strongly unreasonable LGM simulations at either end of the range, on a local or global basis. No change to manuscript.*

Reviewer comment: **About this, the authors claim that "the drier climate at the LGM resulted in a decreased water vapour feedback". There is certainly a decrease in water-vapour content, but it is not obvious how this reduces the strength and uncertainty on the cloud feedback, given that even the modern distribution of clouds is very imperfectly represented by state-of-the-art climate models.**

Author response: *Agreed. Offending comment removed from manuscript.*

Reviewer comment: **Finally, it is not entirely clear why the authors produced three 40-member ensembles, rather than one big 120-member ensembles.**

Author response: *The use of 3x40 member ensembles was for a mix of computational and historical reasons, discussed briefly in Annan et al, SOLA 2005. Two explanatory sentences have been added to the subsection titled "Ensemble of MIROC3.2 runs".*

Reviewer comment: **After these comments, I am only left with a few editorial suggestions. (1) Page 953 : "... especially when considered in combination with other lines of evidence..." : The authors should be more explicit**

Author comment: *Information added.*

Reviewer comment: **(2) Page 955 : give full meaning of "T2"**

Author comment: *Already defined, as 2m temperature, on Page 953.*

Reviewer comment: **(3) Page 956 : "model error": This is probably what Annan and Hargreaves, QJRMS 2002, call "uncertainty on the model error", and what Rougier calls "discrepancy". It might be worth briefly clarifying this point for the audience of Climate of the Past.**

Author comment: *Reference to Rougier added.*

Reviewer comment: **(4) Page 958 : "similarly reasonable" : replace by "equally reasonable" (?)**

Author comment: *Changed.*

References: Annan, J. D., Hargreaves, J. C., Ohgaito, R., Abe-Ouchi, A., and Emori, S.: Efficiently constraining climate sensitivity with paleoclimate simulations, SOLA, 1, 181-184, 2005.

Annan, J. D. and Hargreaves, J. C.: Using multiple obser vationally-based constraints to estimate climate sensitivity, Geophys. Res. Lett., 33, L06704, doi:10.1029/2005GL025259, 2006.

Crucifix, M.: Does the Last Glacial Maximum constrain climate sensitivity, Geophys.

Res. Lett., in press, 2006.

Hansen, J., Sato, M., Ruedy, R., Nazarenko, L., Lacis, A., Schmidt, G., Russell, G., Aleinov, I., Bauer, M., Bauer, S., Bell, N., Cairns, B., Canuto, V., Chandler, M., Cheng, Y., Genio, A. D., Faluvegi, G., Fleming, E., Friend, A., Hall, T., Jackman, C., Kelley, M., Kiang, N., Koch, D., Lean, J., Lerner, J., Lo, K., Menon, S., Miller, R., Minnis, P., Novakov, T., Oinas, V., Perlwitz, J., Perlwitz, J., Rind, D., Romanou, A., Shindell, D., Stone, P., Sun, S., Tausnev, N., Thresher, D., Wielicki, B., Wong, T., Yao, M., and Zhang, S.: Efficacy of climate forcings, Journal of Geophysical Research, 110, 2005

Murphy, J. M., D. M. H. Sexton, D. N. Barnett, G. S. Jones, M. J. Webb, M. Collins, and D. A. Stainforth, Quantification of modelling uncertainties in a large ensemble of climate change simulations, Nature, 430, 768-772, 2004.

Stainforth et al: Stainforth, D. A., Aina, T., Christensen, C., Collins, M., Faull, N., Frame, D. J., Kettleborough,J. A., Knight, S., Mar tin, A., Mur phy, J. M., Piani, C., Sexton, D., Smith, L. A., Spicer, R. A., Thorpe, A. J., and Allen, M. R.: Uncertainty in predictions of the climate response to rising levels of greenhouse gases, Nature, 433, 403-406, 2005.

Yokohata et al: Yokohata, T., S. Emori, T. Nozawa, Y. Tsushima, T. Ogura, and M. Kimoto (2005), Climate response to volcanic forcing: Validation of climate sensitivity of a coupled atmosphere-ocean general circulation model, Geophysical Research Letters, 32 (L21710), 2005.

Interactive comment on Clim. Past Discuss., 2, 951, 2006.