Authors' response to reviewers

We would like to sincerely thank all the reviewers and Editor for their comments. We have tried to attend to all the points raised by them. Reviewer comments (RC) and Editor comments (EC) are written in bold and authors' comments are written in italics.

The changes made in the revised version of the manuscript have been highlighted in color.

RC #1

Main comment: My main concern is that the approach is rather descriptive and lacks a more quantitative approach to assess if the orbital acceleration technique impacts the simulated climate evolution. A more quantitative method or matrix should be designed to clearly indicate which regions and variables (and therewith processes) can and cannot be investigated based on simulations applying orbital acceleration techniques. An example could be a test if the simulated values are significantly different from each other taking into account natural variability in the system, however other and perhaps better methods can be designed.

To quantify the acceleration-induced biases and to better specify the regions were these biases are greatest we calculated global maps of root-mean-square differences between the accelerated and the non-accelerated runs over the low-pass filtered surface temperature timeseries for the PIG and the LIG (Fig. 12). We added a paragraph to the Discussion section. The new figure shows that the largest acceleration-induced biases are found in the Southern Ocean and the North Atlantic/Nordic Seas.

Minor comments: Page 5620, lines 10-17: It is mentioned that the impact of applying acceleration techniques is mainly limited to the high latitudes, a point that is supported by figure 3 for surface temperatures. However, figure 2 shows large differences in the behavior of global mean temperatures at larger depths. Are those differences also confined to the high latitudes or do they perhaps finger print the main pathways of deep ocean circulation and thus a very different spatial extend compared to the surface temperatures?

The deep-ocean biases are global scale and related to the huge heat reservoir. For clarification we added "global-scale" to line 316 (Discussion section).

Page 5620 lines 10-17: A point related to the above, is that it is not fully clear to me why deep ocean temperatures are presented. To investigate if future model studies that apply orbital acceleration should or should not investigate deep ocean temperature changes? Or are they only presented to make the point that high latitude surface differences are related to deep ocean temperature biases? Does the study solely focus on surface variables? Please clarify in the text.

The deep-ocean temperatures are presented to make the point that high latitude surface biases are related to the deep ocean. For clarification, we added a new paragraph near the end of the Methods section.
Lines 156-161

Page 5621, lines 9-10: Is that the only, or main reason to use GCMs?
We rephrased the sentence for clarification.

Page 5623 line 3: Consider including a sentence explaining the changes in annual mean insolation.
Included now.
Lines 118-121

Page 5623 lines 10-12: For the PIG an ensemble mean is presented. Please clarify why this is done, why only for a single period and how this might potentially impact the result. Specifically I'm thinking of the amount of internal (decadal to centennial) climate variability that is present in the single runs while it is averaged out in the ensemble mean.
We agree with the reviewer. Taking an ensemble mean for only one experiment is a flaw in the design of the study. We revised the study by taking only one simulation for the accelerated PIG, using identical initialization as in the non-acclerated PIG run. We revised the Methods section and all the figures that include accelerated PIG results. The conclusions were not affected.

Page 5623, lines 17-22: Fixed GHG values are used. Would your results differ if also GHG changes would be imposed with an acceleration factor of 10? In other words, do the results only apply to accelerating the orbital forcing or is it likely that the same is true for accelerating GHG changes?

We added a paragraph to the Discussion section to address this point.

Lines 337-343

Page 5623 line 27: What about the initialization of the accelerated PIG simulation? Is the same procedure applied? If not, does that perhaps partly explain the differences in the temperature evolution at larger depths?

We revised the Methods section (see above). This point should be clear now (same initialization).

Page 5624 line 9: It would help the reader if a short description is given of the kind of analyses that will be presented and how that allows one to investigation the impact of using orbital acceleration techniques.

We added a paragraph at the end of the Methods section.

Lines: 156-169

Page 5625 line 4: In figure 2 it is apparent that the temperature evolutions at 1884m depth between the PIG and the LIG differ strongly while at other depth levels they are much more consistent and in line with the similarities in the orbital forcing. Please shortly discuss what could explain these differences.

It is the huge volume (heat reservoir) of the deep ocean water masses that determine the long adjustment timescale. We think this is clearly expressed in the Discussion section.

Page 5626 line 2: Please indicate how these results (850hPa zonal wind changes) help in identifying the impact of orbital acceleration. It appears a little arbitrary which variables and levels have been investigated and which have not. Please clarify.

We hope that the new paragraph near the end of the Methods section clarifies this point.

Page 5627 line 3: In line with the point above, please shortly mention why EOFs are investigated, how do they help to investigate the research question of this manuscript?

EOFs are designed to investigate and visualize spatio-temporal variability. We hope that the new paragraph at the end of the Methods section clarifies this point.

Page 5628 lines 1-11: The conclusion that regions that are in direct contact with the deep ocean are more likely to be biased by the applied acceleration technique sounds reasonable. However, the data and analysis to support this finding is rather limited and appears only loosely based on the geographical extend of the regions where differences are found. Following my main comment, a more quantitative approach could lead to a more clear pattern showing the deep oceans and high latitudes as regions in which the impact of orbital acceleration is greatest. This would also allow one to pinpoint more specific regions in which the acceleration technique impacts the results and if they are or are not directly driven by the connection (e.g. the North Atlantic, the Southern Ocean, North Pacific?, Arctic?)

We calculated a root-mean-square-deviation map (see above) to address this very important point.

Page 5628 lines 1-11: How did the authors come to the conclusion that sea-ice plays an important role in explaining their finding? Please provide a more thorough description.

For clarification, we added the EOF analyzes for the fields of sea-ice concentration (new figures 6&8). The Results section has been changed accordingly. The EOFs clearly show the relation of sea ice to surface temperature.

Page 5628 lines 1-11: Are there perhaps still other processes that could explain the described differences between the accelerated and non-accelerated simulations?

These are the processes we could clearly identify in our analyzes.

Page 5629 lines 3-8: It would be very useful if the description of the regions that should not be targeted when applying orbital acceleration is more specific than

only mentioning the high latitudes. Only the regions of deep convection? Only ocean regions? What kind of processes should not be targeted in investigations applying orbital acceleration? Such a more detailed listing would provide a useful reference for future paleoclimate modeling studies.

Our new fig. 12 (RMSD map) now clearly shows the regions that are most strongly affected by the acceleration-technique. In addition, we revised the Abstract, Discussion and Conclusions sections to make our point clearer: Acceleration does not significantly affect the simulation in low latitudes, whereas biases can be substantial in high latitudes. Therefore, acceleration should be avoided in studies of extratropical climate change.

Page 5629 lines 21-23: How is the importance of initialization of the transient simulations in certain regions established?

The deep-ocean has an adjustment timescale of the order of 1000 years or longer. In a non-accelerated simulation of the Holocene, starting at 9 kyr BP, the deep ocean would adjust to the forcing within the first 1000 years even if the initial conditions are "wrong". After that spin-up phase, the climate trajectory would be basically independent of the initial conditions. In an accelerated simulation with acceleration factor 10, it would take the entire Holocene for the deep ocean and the surface regions which have a direct connection to the deep ocean to adjust. For clarification, we added "deep ocean" to our statement and added further explanations to the Discussion section.

Figure 1: consider depicting insolation changes since the provide a much more direct picture to the reader as to the evolution of the main climate forcings applied in the simulations.

Since we already added several new figures to the revised manuscript, we would like to avoid adding even more figures (one figure would not be sufficient to visualize the temporal evolution of seasonal-latitudinal insolation changes). Instead we refer to Loutre et al. (2004).

Figure 2: Why is an additional running average applied to the non-accelerated values? And if this is done in all figures, perhaps the description of the applied averaging in the main text should be adjusted.

Applying a 10-point running average to the decadal mean values represents a low-pass filter for variability at periods below a century. This variability does not exist in the accelerated run, because the below-century variability is already filtered out by using the decadal means. Therefore, we found that the timeseries of accelerated and non-accelerated simulations can better be compared when applying this additional filter to the non-accelerated run.

Figure 3: How are the two fields subtracted in the right-hand-side panel since the accelerated runs consist of ten times less data points. Please clarify

Accelerated and non-accelerated timeseries have been mapped onto the same time axis by linear interpolation.

Figure 4: The zonal winds appear affected for most of the globe and not limited to the high latitudes. Furthermore, in the LIG simulations the impact on the NH westerlies appears of similar size as the impact on the SH westerlies. Please explain why only the differences in the SH are described in the main text.

We rephrased the corresponding paragraphs in the Results and Discussion sections, now also mentioning the NH westerlies and the trade winds.

Figures 5-7: Please clarify why in this part of the analysis no running mean is applied to the non-accelerated model output. Is the EOF analysis impacted by the fact the there are ten times less datapoints in the accelerated simulations? Consider calculating a difference plot for the EOFs (if meaningful).

The goal of the EOF analysis is to identify and compare the modes of variability in the analyzed climate fields, hence additional filtering is not necessary. The meaning of an EOF difference is a priori unclear, but the root-mean-square deviation map (new Fig. 12) is a more reasonable way of quantifying the differences anyway.

Technical comments: Page 5624 line 16: "as we go further deep". Please reformulate. Page 5629 line 13: Comparison with what? Please rephrase. Figure 3: Consider putting the latitude on the vertical axis.

We rephrased, but did not change the axes.

RC #2

Main comments: I am slightly disappointed that there is not a word on the acceleration technique itself, although I am sure that it is described elsewhere.

We added more explanations to the Methods section describing how we performed the acceleration.

All the analysis remains mostly qualitative. There is few quantification of the difference between the results obtained with and without the acceleration technique. Along the same line, location of disagreements often remains vague (the high latitudes, the sites of deep water formation).

To quantify the acceleration-induced biases and to better specify the regions were these biases are greatest we calculated global maps of root-mean-square differences between the accelerated and the non-accelerated runs over the low-pass filtered surface temperature timeseries for the PIG and the LIG (Fig. 12). We added a paragraph to the Discussion section. The new figure shows that the largest acceleration-induced biases are found in the Southern Ocean and the North Atlantic/Nordic Seas.

The authors do not justify their choice of the variables displayed. Although surface temperature seems to be an obvious starting point, the choice of the wind field and the deep ocean temperature should be explained. Other variables, such as sea ice extent or temperature at different levels in the atmosphere, could have been chosen as well.

Sea ice is now included in the revised version (Results section and new figures 6&8). Moreover, we added a new paragraph near the end of the Methods section to explain the choice of variables.

I have an uncomfortable mixed feeling with the paper. I have the impression that throughout the paper, the authors tend to show that the acceleration technique is doing a very good job and that the differences between accelerated and non-accelerated simulations are minor. However, the conclusion is doing the reverse, insisting on the major discrepancies. For example, I read in the Discussion that 'Except for some highlatitude regions, in particular the Southern Ocean, the acceleration technique does neither ham- per model intercomparison nor model-

data comparison studies' but in the Conclusion it is stated that 'the acceleration technique may compromise transient climate simulations over large regions in the Southern Hemisphere'. Even if the reservation of 'some high latitudes of the Southern Hemisphere' appears in the first sentences, the ideas conveyed are not exactly along the same line. At last, I remain with a strong question. Is it useful or not to use an acceleration technique?

We see the reviewer's point. Therefore, we revised the Abstract, Discussion and Conclusions sections to make our point clearer: Acceleration does not significantly affect the simulation in low latitudes, whereas biases can be substantial in high latitudes. Therefore, acceleration should be avoided in studies of extratropical climate change. We hope our point has become clearer now.

The authors decided to test the method on interglacial periods. Do they think that the choice a warm periods influence their results? Which results do they anticipate for climatically different times slices (for example, glacial periods or terminations)?

We addressed this question in a more general framework at the end of the Introduction: The basic assumption for the application of this acceleration technique is that orbital forcing operates on much longer timescales than those inherent in the atmosphere and upper ocean layers. We would like to avoid any further speculations.

Minor comments: The PIG-accelerated simulation is the only one to use an average of three members. I urge the authors to consider only one member, along the line used for the three other simulations. Alternatively, they could use three members for each of the four simulations. How is the PIG accelerated simulation initialised? Does it use the same initial and boundary conditions as the non-accelerated PIG simulation?

The reviewer is right. Taking an ensemble mean for only one experiment is a flaw in the design of the study. We revised the study by taking only one simulation for the accelerated PIG, using identical initialization as in the non-acclerated PIG run. We revised the Methods section and all the figures that include accelerated PIG results. Description of initial conditions should have also become clearer now. The conclusions were not affected.

The authors indicated that "For the analyses of the model results decadal means (referring to model years) have been used from all the transient simulations ". If I understand well, this mean that an average over 10 'real' years in the case of the non-accelerated simulations is compared to an average over 100 'real' years. Could the authors consider using an average over the same number of 'real' years.

We did that for the calculation of the root-mean-square deviation maps (Fig. 12) in order to have the same number of data points. For all other analyses this has no effect.

Moreover, Working with average of model years means that there are ten times less data points in the accelerated simulations than in the non-accelerated ones. I am therefore wondering how the authors computed a difference between the two types of simulations (with a different number of data points).

Accelerated and non-accelerated timeseries have been mapped onto the same time axis by linear interpolation.

P5625 – line 5 "zonally averaged surface temperature". I assume that the authors mean 'anomalies from the beginning of the simulation'. Please clarify.

This should be clear from the corresponding figure caption.

P5626 – the EOF. As a non-specialist of the EOF, I do not understand the figures, how they are produced and what they exactly represent. I can only figure out what they should be. Would it be possible to provide more information?

We provided more information at the end of the Methods section.

P5626 – lines 22-26: Doesn't this conclusion confirm that the comparison should be performed on time series averaged on the same number of real years (and not model years)?

No, this result was related to the use of an ensemble mean for the accelerated PIG. In the revised version (using only one accelerated PIG run rather than an ensemble), the explained variances are similar, hence these lines have been removed.

P5626 : although the potential role of the sea ice is described, neither the evolution of the sea ice during the simulation, nor its difference between the accelerated and the non-accelerated simulations is discussed.
We added EOF analyzes for the fields of sea-ice concentration (new figures 6&8). The Results section has been changed accordingly. The EOFs clearly show the relation of sea ice to surface temperature.

P5627 – line 27 : "Rather surprisingly". Could the authors explain why they were surprised? What were they expecting?
We deleted "rather surprisingly".

P 5628 – lines 5-7 : It is the first time in the paper that a link between surface temperature, upwelling and deep ocean temperature is presented. A more elaborated discussion would be welcome. Moreover, I invite the authors to provide a more precise identification of the region(s) under concern.
The Discussion section has been substantially revised taking these points into account.

P 5628 – line 24-25 : "In these regions, inappropriate deep-ocean initial conditions may severely compromise accelerated runs, strongly determining the climate trajectories". This idea would be worth to be included in the conclusion. Or is it only a conclusion from Lunt et al., and Timm and Timmermann?
It is included in the Conclusions section.

Although I did not checked the references in details, I couldn't find the reference 'Govin et al'
We included the missing reference.

RC #3
The overall structure of the manuscript is very well done. The method section describes the important aspects of the model, the initialization of the simulations and the forcing. With the exception of a description how the orbital forcing is

actually accelerated in the model. For experts, it may be much too obvious, but for the sake of clarity it should be explained more clearly.

We added a concise explanation to the Methods section.

The results that were chosen here as examples to compare the differences between accelerated and un-accelerated runs are all key variables that characterize the transient climate response. However, the motivation why they have been chosen should stressed.

We added a new paragraph near the end of the Methods section to explain the choice of the variables.

In the discussion -albeit being a GMD and not a CP journal contribution - there could and should be some more attention to the physical processes, in general. In particular, in the discussion of the processes that lead to the differences in the simulations. Not only between accelerated (A) and un-accelerated simulations (UA), but also between the different outcomes of the comparisons for the two different time periods. The latter I believe makes the study very interesting, because it has some surprising outcomes to discuss: Obviously the forcing is similar but has subtle [compared with other pairs of time intervals] differences between the two time periods (PIG, LIG). It appears that the larger discrepancy between A and UA simulations are identified in the PIG, in which the orbital changes are a smaller in the precession component. What components of the orbital forcing or what feedbacks could explain the different outcomes? One candidate could be feedback from changes in sea-ice. Therefore, I would encourage the authors to add to the existing paper a few more results and a deeper discussion of their results.

We revised the Results and Discussion sections following the reviewers suggestions. In particular, we added EOF analyzes of the sea ice concentration to demonstrate the close link to surface temperature. In the Discussion section we hypothesize that the stronger orbital forcing during the LIG compared to the PIG puts a stronger constraint on the evolution of surface climate such that biases associated with the exchange with the deep ocean have a weaker impact.

Minor Comments:

The method section says that the results shown for the simulation PIG accelerated is the three-member ensemble average. It should be noted that this affects the signal-tonoise ratio (externally forced response to internal variability). As a result I would expect in the time series shown in Fig.2 - Fig. 4 that the results based on LIG accelerated 3- member ensemble average appear smoother compared with PIG results. That seems to be the case, but in the precipitation case of the EOFs (Fig. 7 and Fig. 8) it is surprising that the second mode carries an orbital response in LIG, but not in PIG. So, given that it should be 'easier' to detect an orbitally forced signal in the second EOF in PIG accelerated (compared LIG accelerated) it suggests that the larger forcing during LIG caused a different hydrological response pattern. That needs to be discussed, what could cause the hydrological response to spread over two EOF modes in LIG. But please make sure that EOF modes 2, 3 (4) are not close in their eigenvalues (explained variances) and the orbital mode does not show up in mode 3 (or 4).

First, we would like to note that we have revised the study by using only one PIG accelerated run instead of the ensemble, such that the comparison between accelerated and non-accelerated PIG run has become more strictly now. As to the stronger LIG forcing compared to the PIG, we agree that the stronger forcing puts a stronger constraint on the temporal evolution of the surface climate. We added a paragraph to the Discussion section regarding this point. In fact, about 65% of the precipitation variance during the LIG is related to orbital forcing (and spreading over the leading two EOFs), whereas it is only 31% during the PIG (and only contained in the first EOF). We note that orbital variations do not show up in higher modes and that the explained variance of the third precipitation EOF is less than 5% in all experiments.

p4l25 -p5l2 Description of the orbital forcing PIG vs LIG: Please describe the obliquity changes with more attention to the details. Over the time of the PIG and LIG the changes are different in magnitude but they show a similar change in the beginning. This is important for the discussion of the different early PIG and LIG responses, in my opinion (e.g. Figure 2, why is LIG ocean temperature

changing so different in the early parts of the simulations?). Furthermore, the conclusion comments on the importance of the initial state, too.

We note that the obliquity evolution during the first 7,000 years of the LIG (130-123 kyr BP) is very similar to the obliquity evolution of the PIG (9-2 kyr BP). The "difference" may be the result of an optical illusion due to the fact that the LIG is plotted for a 10,000 year interval in Fig. 1, wheras the PIG is plotted for a 7,000 year interval only.

p5l22-p6l2: Please make a statement if 400 years were sufficient to bring the deep ocean close to the steady-state. In other words can you assert that the first few 100 model years in the accelerated forcing are free of initial-state adjustments? And second, the orbital precession between 9ka and 130ka is about 0.02 units off in the beginning. Could this affect the first few hundred model years in LIG accelerated simulation, and thus the interpretation of forced response in the initial warming seen in the deep ocean?

We note that the LIG was started from a 130 kyr BP time slice simulation, whereas the PIG was started from a 9 kyr BP time slice simulation as pointed out in the Methods section. However, 400 years of spin-up were certainly not enough to bring the deep ocean to an equilibrium. Therefore, the first O(1000 yr) in the non-accelerated runs may be affected by the initial condition, whereas the entire run may be affected by the initialization in the accelerated runs. Hence, our statement that in high-latitude regions (where the surface is connected to the deep ocean), "the climate trajectory can be crucially determined by the deep-ocean initialization of the transient simulation" (Conclusions section). We further added remarks to the Methods section (pointing out that 400 years of spin up were not enough to bring the deep ocean to a steady state) and to the Discussion section (explaining the influence of initialization on the climate trajectory).

Results:

The description of the results is well written. However, somewhere in the text (Section Discussion) one could explain the physical response of the system that causes the reported changes in the climate variables. In particular, the role of sea-ice for the ocean surface and deep ocean temperature response is important.

A figure with sea ice area time series would be a good addition to the figures 1-4 before looking at EOFs.

We added the sea ice EOFs to the manuscript (new figures 6&8). These EOFs nicely show the long-term trends and their relation to sea surface temperature. Hence, we did not include an additional sea ice area time series.

p8l2 Consider a short paragraph on describing wind changes in subtropical regions/ tropics. What happens to trade winds, for example? Or refer to papers, if this had been discussed elsewhere in more detail already.

We added a description of the trade wind changes in the Results section.

p8l13-p8l15 The EOF analysis should be motivated (e.g. "A frequently applied technique in analysing climate modeling is the Empirical Orthogonal Function (EOF) analysis" [ref to be added, e.g. books by Wilks, Statistical Methods in the Atmospheric Sciences, Academic Press) , von Storch and Zwiers, Statistical Analysis in Climate Research, Cambridge Univ. Press].

We added a paragraph at the end of the Methods section to motivate and introduce the EOF analysis.

p9l3-p9l9 For the discussion, I find the differences in the spatial structure in the North Atlantic between LIG and PIG results interesting. Could it be an indicator for the different seaice albedo feedbacks?

It may be related to excessive sea ice concentration in the LIG simulations, as now discussed in the Discussion section.

p9l24-p927 The AMOC changes could be described in connection with the ocean temperature response already. In fact, it would be worth mentioning that overall the AMOC is strong and shows relative small changes. Therefore, shifts in rainfall in tropical/monsoonal regions are (to first order) free from internal AMOC-related changes.

We added a statement at the end of the Results section.

p10 Discussion: This section should be expanded: (1) Discussion of causes for the difference in the polar regions and deep ocean. (2) Discussion of different

orbital responses between PIG and LIG (e.g. two EOF modes representing orbital response in LIG vs one during PIG (provided that the EOF results support my point, see comment above). (3) It would be also insightful if the authors could discuss the role of obliquity and precession in connection with sea ice changes (Timmermann et al., 2014).

The Discussion section has been expanded following the reviewer's suggestions.

p.11 Conclusion: p11l22-p11l23: The initialization problem has not been discussed explicitly and it deserves clarification. If it is a main conclusion, results and discussion must address the problem.

We now address this problem in the Discussion section.

Technical comments p6l27: Write "Not only is this trend variability missing in the accelerated run, also the general [...]." p7l12: write "[...] simulation lags (and underestimates) the cooling of the nonaccelerated run." p7l29: Rewrite the sentence part "Similarly, for the LIG as well a poleward shift [...]". Unclear grammar/meaning p8l15: Start a new paragraph with "Even though the general pattern [...]" p11l2: Govin et al. 2014: Reference missing?

All technical comments have been addressed in the revised manuscript.

Suggestion: It would be very insightful to have a figure similar to the plots in Fig.2 but for top of atmosphere shortwave radiation times (1 - planetary albedo) (calculated with monthly mean data). This would show where the energetic changes take place and how large they are.

Since we already added several new figures to the revised manuscript, we did not add this suggested supplementary figure.

EC

Dear authors,

In my role as Executive editor of GMD, I would like to bring to your attention our Editorial:

http://www.geoscientific-modeldevelopment.net/gmd_journal_white_paper.pdf

http://www.geosci-model-dev.net/6/1233/2013/gmd-6-1233-2013.html
This highlights some requirements of papers published in GMD, which is also available on the GMD website in the 'Manuscript Types' section: http://www.geoscientificmodeldevelopment.net/submission/manuscript_types.html
In particular, please note that for your paper, the following requirements have not been met in the Discussions paper – please correct this in your revised submission to GMD.
"– The paper must be accompanied by the code, or means of accessing the code, for the purpose of peer-review. If the code is normally distributed in a way which could compromise the anonymity of the referees, then the code must be made available to the editor. The referee/editor is not required to review the code in any way, but they may do so if they so wish."
"– All papers must include a section at the end of the paper entitled "Code availability". In this section, instructions for obtaining the code (e.g. from a supplement, or from a website) should be included; alternatively, contact information should be given where the code can be obtained on request, or the reasons why the code is not available should be clearly stated."
"– All papers must include a model name and version number (or other unique identi- fier) in the title."

We changed the title accordingly, and added a statement regarding code availability to the acknowledgments.