Response to reviewers

We thank both the reviewers for their comments. Reviewer comments are written in bold and authors' response is given in italics.

Reviewer #1

Lines 155-156: It is explained here (and in several figure captions) what methodology has been applied to make the time series from the non-accelerated and accelerated simulations (most) comparable. I feel this should be discussed in more depth. For instance, it could be mentioned that for comparison between the two simulations, the accelerated time series are stretched onto the non-accelerated timescale (Figure 2).

The explanation provided is far from being enough to clarify this issue.
I expect that you comply with the reviewer's request of adding this discussion in the main text (around lines 155-156) and second that you explain what is "remapping" between the timescales. Since the figures in question are representing continuous lines it is impossible to see the actual datapoints that represent what is discussed.
Please elaborate further in the main text and refer to it in all relevant figure caption.

We added a clarifying sentence to the figure caption.

Then for say a 100yr interval on this time scale, the accelerated data is represented by a single point (if I understand the manuscript correct), a 10yr average in model years that is then stretched to represent a 100 years.

The reviewer version is actually clearer: you only allude to this on line 130 or so in the method section. Please clarify where and when the different timescales and periods are used.

The reviewer understood correctly. The accelerated timeseries consists of 70 points for the PIG and 100 points for the LIG, as can also be seen from the PC timeseries plots. We think the approach is clearly explained in the Methods section and the figure captions.

For the non-accelerated runs 10 10yr means are taken over which a 10-point running mean is then applied. It seems to me that the result is not completely the same. In the light of this manuscript this is not such an issue, but I think it should be mentioned, especially considering that previous and future studies might analyse results from accelerated runs in terms of decadal-millenial variability. It would be great if some advice could be given here of how such an analysis should be done, if at all, for future reference.

We added a new paragraph to the Discussion section following the reviewer's suggestion.

I am not sure to understand your new paragraph: what is interannual variability in an accelerated run? You propose to investigate it nonetheless, but since that variability is a composite of accelerated long term forcing and actual interannual variability, it is not purely interannual variability. I do not understand your point, so please reformulate. Also, the paragraph mention "it is needless to mention ..." following the reviewer's advice I find at the opposite that this is very important and need to be mentioned clearly.

Lines 139-148: The manuscript nicely describes the importance of initializing the deep ocean when applying an acceleration technique. It appears to me that the difference between the initialization of the PIG and LIG simulations could impact the results. The PIG has been initialized with a 400yr 9ka simulation. However, the LIG used this simulation to start another 400yr 130ka equilibrium simulation. Assuming that the 9ka climate is closer to the 130ka climate than the pre-industrial climate is (based on the forcing one would suspect this), then in the LIG simulations the deep ocean has had more time during spin up to adjust. Could this partly explain the differences between PIG and LIG shown in figure 12?

No. As can be seen from Fig. 2, the deep ocean temperature is similar (ca. 1.7 deg C at 1884 m at the beginning of both the PIG and the LIG simulations), i.e. there has not been much deep-ocean temperature change during the additional 130 ka spin-up phase.

But what is going on with the mixing and the overturning in the deep ocean? A cold deep ocean is likely to be common to many climate states and is thus not a good measure of how similar the two states are. The reviewer's remark does still stand thus.

Lines 367-368: In the conclusion section as well as at other places in the manuscript, the slow response time of the deep ocean and the impact it has on simulations applying orbital acceleration techniques are discussed in terms of the importance of initialization. I think it would be good to explain that there seem to be two effects (closely related, but nonetheless): 1) If indeed the initial climate state was not in full equilibrium this will more strongly effect an accelerated run compared to a non-accelerated run. 2) The deep ocean response to changes in the climate forcing during the transient run will have a lagged effect on the climate in an accelerated run compared to a non-accelerated run. This could be clarified in the text.

We believe that lines 316-336 clearly discuss the points raised by the reviewer. As to the reviewer's 1st point, we would like to stress that there is no reason to assume that the deep ocean was in equilibrium with the orbital forcing at the beginning of the PIG or the LIG, in particular given the strong meltwater-induced AMOC fluctuations at these times.

Again, I do not think that you do answer the reviewer's point: he/she does not reflect on the actual climate state ("the real world") but on the theoritical one in your manuscript. I think you could still add a sentence on the fact that the deep ocean changes in an accelerated run (once rescaled / remapped on the actual timescale) appear to occur later than in a non-accelerated run.

Lines 213-214: The pattern in the SH in the accelerated runs looks opposite to me, please clarify.

Between 50-60°S the color changes from dark blue to more light blue and even red from the early to the late Holocene (please ignore the "noise" on top of this long-term trend), i.e. the westerlies become stronger in these latitudes during the course of the Holocene.

Lines 224-225: Perhaps a reference can be given here.

Done.

Line 291: Bracket is missing.

Added bracket.

Lines 289-302: Have these changes in LIG convection in the North Atlantic been described before, if so please reference.

No, therefore we mention "not shown".

Lines 344-351: This part could be integrated into the conclusion section.

We would like to keep this paragraph as a final statement of the Discussion section. Moreover, we would like to avoid any references (i.e. Bakker et al. and Kwiatkowski et al.) in the conclusions.

Reviewer #2

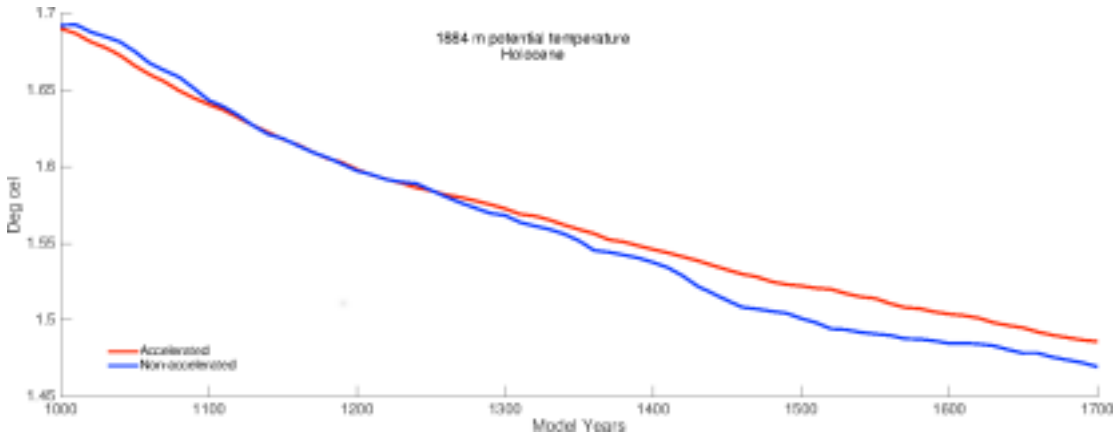
Lines: 181-184:

The initial decrease in 1884m ocean temperatures seems faster than how the difference between accelerated and normal orbital forcing develops at the beginning of the simulations. Since both models start from the same restart

(initial) state what is the cause of this adjustment in the beginning of the transient simulation seen in figure 2e? The authors mention the long adjustment time of the deep ocean in the discussion in general terms (lines 326-330), but do not mention figure 2e.

As a test if both simulations (accelerated and un-accelerated PIG) show the same rate of temperature change, it would be interesting to see the accelerated run on the true model year time scale superposed on the time series of the un-accelerated run. If they show the same rate of change it would suggest to me an adjustment to an imbalance between initial (restart) state and the applied boundary conditions (forcings) at the begin of the transient simulations. Even better, if one would add the previous simulation that was run to create the restart point. Does the temperature time series continue in a smooth way? (Note: Irrespective of the outcome of this additional check, the validity of the discussion and conclusions is robust, in my opinion.)

We plotted the timeseries as suggested by the reviewer (see below). The rates of change are indeed similar, albeit not identical. We agree with the reviewer that an adjustment to an imbalance between initial state and the applied boundary conditions at the beginning of the transient simulation plays a significant role in the deep-ocean temperature evolution - both in the PIG and LIG experiments (although the transient orbital forcing cannot be neglected). We added a corresponding remark to the Discussion section to be more specific now (lines 332-336). However, we would like to point out that, in the real world, there is no reason to assume that the deep ocean was in equilibrium with the orbital forcing at the beginning of the PIG or the LIG, given the strong meltwater-induced AMOC fluctuations at these times.



In the figure, the potential temperature at 1884m for PIG (both accelerated and non-accelerated runs) is plotted against model years. In the accelerated run, model year 1000 = 9000 years BP and 1700 = 2000 years BP. For the non-accelerated run, model year 1000 = 9000 years BP and 1700 = 8300 years BP. Time series represent the decadal mean values and no smoothing has been applied.

Both accelerated and non-accelerated runs show similar curves, supporting that a major portion of the long-term trend in the accelerated run is due to deep-ocean equilibration to the initial conditions. This figure supports the notion that in accelerated runs the climate trajectory may be strongly influenced by the initial conditions.

Line 222-225:

For guidance to the reader: Could you please add the latitudes (and optionally the range of years, where the reader can see this clearly in the figures). Visually, it is less obvious than the poleward westerly wind changes.

Done.

(Lines 326-330: see comment above on lines 181-184)

See above.