

Interactive comment on “Intra-interglacial climate variability from Marine Isotope Stage 15 to the Holocene” by R. Rachmayani et al.

Anonymous Referee #2

Received and published: 8 September 2015

General remarks

This paper presents a suite of 14 snapshot experiments that have been performed with the CCSM3 model. The focus is on climate response to astronomical forcing during five different interglacials. The work presented here is not completely novel, as some of the experiments have been published before, and there is also some overlap with the work of other modelling groups. Still, this set of simulations is impressive as it provides an extensive view of interglacial climate variability in space and time. In my view, this paper is certainly of interest to the readership of *Climate of the Past*. It is generally well written and has clear figures. However, I have several suggestions for improvement, as detailed below. In particular, the paper should provide more discussion with respect to the experimental setup and the results of others, and should also provide better

C1587

explanations of some of the results presented.

Main comments

A- Introduction. The introduction should be extended to provide more information. In particular, it would be useful to the reader to briefly discuss some main characteristics of the interglacials considered here, such as the length, the maximum temperature anomaly reconstructed compared to preindustrial and the relative sea level, if available. It should also be explained why the early Brunhes interglacials (MIS 13 and before) are different from the later interglacials, as later in the paper this is referred to. In addition, the main findings of previous modelling studies that have focused on several of the considered interglacials, should be briefly discussed. This is especially relevant for the Herold et al. (2012) study that was conducted with the same CCSM3 model. It should also be explained more clearly what the novelty of the present study is compared to these previous studies. This should include a rationale for selecting these specific interglacials and these 14 time slices, as this is not clear from the introduction.

B- Setup of experiments. Orbital forcing: The authors should discuss the season definition that they have used for the insolation in the different experiments (see Jousaume and Braconnot, 1997). I suspect that the date of vernal equinox has been kept fixed at today's value. The choice of calendar should be made clear, as it has potentially a huge impact on the results.

C- Results. The results section could be improved, as the explanation of the results is in a few instances not very convincing.

On page 3079, line 15, the warm conditions in winter in the Arctic in the Group I experiments is discussed. "However, anomalously warm conditions in the Arctic stand in contrast to the global DJF cooling at 6, 9, 125, 405, and 416 kyr BP. The Arctic warming is due to the remnant effect of the polar summer insolation through ocean–sea ice feedbacks ". I wonder if this is the full explanation. Why is this Arctic winter warming not present in the other Group I simulations for 504 ka and 579 ka? For instance, look-

C1588

ing at the insolation anomalies in Figure 2, the forcing looks very similar for 125k and 504 ka, but the Arctic warming in winter is absent in the simulation for 504 ka. Please elaborate.

In Section 3.5 (page 3081), the effects of obliquity is discussed by comparing the anomalies of 416 minus 394 ka and 495 minus 516 ka. It is concluded on line 22 that in the 416 ka and 495 ka cases with maximum obliquity forcing, the boreal summer temperature in monsoon regions is lower than in the minimum obliquity cases because of higher rainfall. However, as can be seen in Figure 8f, the precipitation anomalies are very small (less than 0.2 mm/day) in monsoonal areas in the 495 ka case, making this conclusion highly unlikely, at least for 495 ka. I think it is more plausible that the negative insolation anomaly at low latitudes depicted in Figure 7 is the direct cause of the negative temperature anomaly. For the 495 ka case, the June-July insolation at 10°N is more than 10 Wm⁻² less than in 516ka.

At line 20, the small rainfall anomaly in the Sahel in the 495-516 ka plot (8f) is explained by the high precession at 495 ka which counteracted the obliquity-induced increase in monsoonal rainfall expected by the authors. This is an implausible explanation, as precession has similar values at 495 and 516 ka (Figure 1). However, even if precession values would have been different, the modelled climate does not "see" the high precession (or high obliquity), as it is only forced by the insolation anomalies that result from the changes in astronomical parameters. These insolation anomalies are shown in Figure 7. I think it is deceptive to consider variations in astronomical parameters as direct forcings of climate change in particular areas. Instead one should consider the net effect of these astronomical parameters on the insolation, which as a result varies per latitude and per month as is clear from Figure 7.

D- Discussion and conclusions. The discussion should be extended to include several limitations of the study. As mentioned in the conclusions, the model experiments did not include appropriate ice sheet configurations, while it is known that changes in ice sheets also affected interglacial climates. The potential effect of prescribing prein-

C1589

dustrial ice sheets should be properly discussed, and not just be mentioned in the conclusions. In fact, it is not a conclusion from this study.

In addition, also the impact of the choice of calendar on the results should be discussed in Section 4. The conclusions should also stress more clearly what the added value of this study is compared to the various other recent modelling studies that have focused on interglacial climates. Do the experiments provide improved understanding of certain features seen in proxy-based reconstructions? If so, where?

Minor comments

Page 3074, line 24: "have usually set to extreme values" should be "have usually been set to extreme values".

Page 3074, line 26: "our analyzes are based on realistic orbital configurations and hence climate states". I disagree with this statement. The fact that realistic orbital configurations are prescribed does not necessarily mean that the simulated climate states are also realistic. For instance, preindustrial ice sheets have been prescribed in all experiments, while it is well known that there have been substantial changes in ice sheet configuration during the considered interglacials, which will have impacted the climate as well.

Page 3075, line 6. Starting from the preindustrial spin-up, each experiment was run for 400 years, of which the last 100 years were used in the analysis. After 400 years, the deep ocean is still adjusting to the change in forcings (e.g. Renssen et al. 2006). For this reason, other similar studies have used a longer run time, for instance, 1000 years in Yin & Berger (2012) and Herold et al. (2012). Although in the present study the focus is on the surface climate, for which 400 years is probably sufficient, I would still suggest that to discuss this issue in Section 2.2.

Page 3080, line 6: I propose to rephrase this sentence (2x precipitation). "precipitation shown in Fig. 5 exhibits intensified precipitation. . ."

C1590

Page 3080, line 11: " The most interesting results regarding the tropical rainfall response to astronomical forcing appear in Group III, where the monsoonal precipitation anomalies show opposite signs in North Africa and India." This is the case for the 615k simulation, but it is not clear for the 394k experiment, as Figure 5 clearly shows for 394k enhanced precipitation in N Africa and India. Please revise.

Page 3080, line 25: "In high Arctic latitudes, vegetation advances (NPP increases) in the Group I simulations. . ." If NPP increases, does it necessarily reflect an advance of vegetation? It could also reflect a change of the vegetation at the site itself, couldn't it? I would say it is not so straightforward to interpret simulated NPP changes in terms of shifts in vegetation. But maybe the authors have also checked other output from their DGVM to come to their interpretation. If this is the case, I suggest explaining this in the manuscript. The same is true for the NPP decline in the Arctic in the Group II simulations.

Page 3082, Section 3.6. I would propose to explain in more detail how the correlation maps are constructed and what they mean. The values of the GHG forcing are not necessarily independent from the values of precession and obliquity. For instance, CO₂ and CH₄ levels in the atmosphere depend on exchange between carbon pools, which in turn is affected by climate due to changes in astronomical parameters. So if there is a positive correlation of temperature with GHG forcing, we are not purely looking at correlation to the radiative forcing, but potentially also at the correlation to orbital forcing in the background. What does the correlation to GHG radiative forcing mean, and how should it be compared to the correlation with precession and obliquity?

Page 3088, line 12. I do not consider CCSM3 a "state-of-the-art" model, as it was released more than 10 years ago. We have already the next generation: CCSM4 (and CESM).

Additional references

Joussaume, S. and Braconnot, P. 1997: Sensitivity of paleoclimate simulation results
C1591

to season definitions. *Journal of Geophysical Research* 102, 1943-1956.

Renssen, H., Driesschaert, E., Loutre, M.F., Fichefet, T. 2006: On the importance of initial conditions for simulations of the Mid-Holocene climate. *Climate of the Past* 2, 91-97.

Interactive comment on *Clim. Past Discuss.*, 11, 3071, 2015.