Response to Reviewer 1

RC1-1: The authors should be more clear about motivation for their study. Simulations described in the manuscript cannot contribute to understanding of the mechanisms of glacial termination since GHGs and ice sheets were prescribed. Their experiments also do not represent true transient simulations and cannot be compared with rich archive of continuous climate records which reveals strong millennial-scale variability. Even for the LGM and mid-Holocene simulations their model set-up is not realistic because the model does not account for the effects of vegetation cover change and aeolian dust. The later was not even mentioned in the manuscript. At the same time, there is a significant body of modeling studies (e.g. Mahowald et al. 2006, Takemura et al., 2009; Crucifix and Hewitt, 2005; Schneider von Deimling et al., 2006; O'ishi and A. Abe-Ouchi, 2013) which clearly indicate that climate effects of dust and vegetation are comparable (1-2C additional cooling) to the effect of ice sheets and GHGs. Even comparison with other models (PMIP 2 and 3) is limited by the fact that the authors used different ice sheet reconstruction.

We agree that the purpose and motivation for our study were not clearly articulated. We have added the following sentences to the end of the introduction to improve clarity about the motivation and scope of the study:

"The goal of our simulations is to adopt a methodological framework similar to that of PMIP and extend the time slices beyond the LGM and mid-Holocene. The simulations also serve as a base line for applying GENMOM to more detailed and focused studies of late-Pleistocene climate such as freshwater forcing and dynamic vegetation."

We disagree that our simulations do not contribute to our understanding of glacial and deglacial climate because they are not transient. While they do not include mechanisms such as freshwater forcing, they do provide multi-century time series that are useful, for example, to explore ecosystem responses to changes in mean climate and the related interannual variability in the model. As we show below, our simulations are also in good agreement with several transient simulations.

Our experimental design follows PMIP which includes non-interactive vegetation and fixed atmospheric aerosols. Similar boundary conditions were used in the transient simulations analyzed by Liu et al. (2014). We recognize that feedbacks from changes in vegetation and aerosol loading have been demonstrated to play a role in LGM and deglacial climate simulations (Mahowald et al., 2006). Not addressing the potential added effect of radiative forcing by aerosols was our oversight. We point out, however, that the global distribution of LGM aerosols is not well constrained and that estimated dust concentrations fall by over an order of magnitude between the LGM and 16 ka, and reach mean concentrations similar to PI by 14 ka (Harrison and Bartlein,

2012) which would reduce their contribution to cooling over their time slices substantially. Aerosol loading associated with volcanism is increasingly being viewed as a potential source of lower magnitude Holocene climate variability. We have revised our text to mention the lack of aerosol forcing in our simulations and potential radiative impact of aerosols. We will be modifying GENMOM in the future in order to assimilate aerosol loading and dynamic vegetation.

The ICE6G reconstruction used in PMIP3 provides more realistic ice sheet topography than the previous ICE5G. Currently, the ICE6G reconstruction is not available for our 8 time slices. However, the OSU-LIS reconstruction has similar ice sheet topography to that of ICE6G (Ullman et al., 2014) and is available for our simulation periods. The combination of OSULIS+ICE4G enabled us to use a more realistic LIS topography than that of ICE5G, particularly for the deglacial, and facilitated adjusting sea level throughout our time slides. Future work assessing the atmospheric circulation, storm tracks and the mass balance over the LIS will test GENMOM's sensitivity to different ice sheet configurations (ICE4G+OSULIS, ICE5G and ICE6G).

RC1-2: When comparing with paleoclimate data one have to be aware about limitations of paleoclimate reconstructions which by no means are "observations". For example direct comparison of global modeled SAT with Shakun et al. (2012) "global" temperature reconstruction is meaningless. Shakun's reconstruction at best represents "global" SAT anomalies outside of the NH continental ice sheets. At the same time, the latter through albedo and orographic effects, are responsible for additional cooling of at least 2C (e.g.Schneider von Deimling et al., 2006; Singarayer and Valdes, 2010; Hargreaves et al., 2012). The authors apparently try to address this inconsistency at the page 2935 but what they want to say here is unclear to me.

That our comparison is meaningless is perhaps an overstatement. Nevertheless, we appreciate the criticism and have accordingly revised the figure and section extensively. Figure 5 now displays a) our original figure to which we have added output from the three transient simulations in Liu et al. (2014) and b) the Shaken et al. (2012) and Marcott et al. (2013) data and GENMOM, the PMIP3 models and the three transient simulations averaged over 5° × 5° boxes around the core sites. The cores are predominantly coastal SST records away from the ice sheets, so sampling the models at these sites provides a more direct comparison. The addition of the transient models also puts the GENMOM changes into context for periods other than the LGM and mid-Holocene. We mention the possibility issue for seasonal biases in the reconstructions raised by Liu et al. (2014), but we do not explore the topic further.

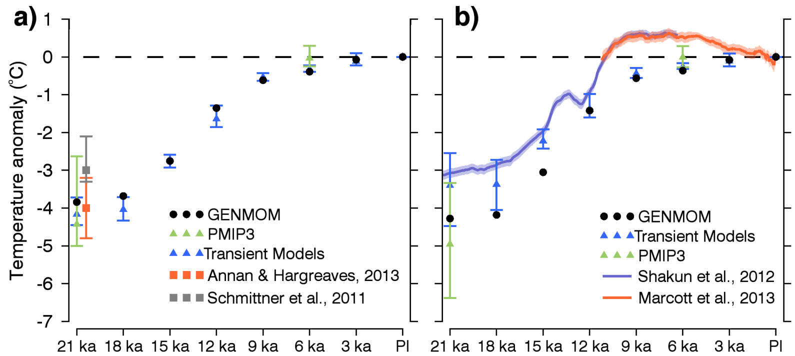

Figure 5 (revised). a) Mean-annual global air temperature anomalies, b) mean-annual temperature anomalies sampled at the core locations. Transient model mean-annual model temperatures calculated over 50-yr windows centered on the indicated times. (Full figure caption in revised manuscript text.)

RC1-3: Even more problematic is comparison with MARGO SST in the tropics. Systematic disagreement between foraminiferal-based reconstructions and other proxies (Mg/Ca, alkenones, elevation change of snow line, terrestrial data) as well as similar systematic differences between MARGO and PMIP modeling results in the Pacific and Indian oceans cast serious doubts on reliability of MARGO reconstructions in the tropics. This is why it is rather strange that, when discussing tropical SST at the LGM, the authors compare their results only with MARGO but not with PMIP modeling results. In fact, most of PMIP models simulate considerably stronger cooling in the tropics compare to GENMOM.

We are aware of potential issues with the MARGO dataset; however, the MARGO (and GHOST) data have been, and continue to be, used to evaluate model-based changes in SSTs (Harrison et al., 2013). Recent work has shown that both modern observations and models do not capture long-term SST variability displayed in proxy records (Laepple and Huybers, 2014). It is beyond the scope of this paper to go into the details and caveats of each paleo reconstruction. GENMOM is warmer than the mean of the PMIP2 models in these basins; it was not our intent to obscure that fact. The GENMOM SST anomalies fall within the maximum-minimum range of the six PMIP2 models. Moreover, on a global scale, our mean LGM SST anomaly of 2.42 °C is essentially the same as that for the ensemble of all PMIP2 and PMIP3 models (Harrison et al., 2013). We have modified the text accordingly.

RC1-4: Some important aspects of methodology are missing. In particular, what was the fate of snow accumulated over the ice sheets? Was it added to freshwater flux into the ocean and, if yes, were? What surface type was prescribed for the land grid cells which at present are covered by ocean? It is also unclear why the authors used old ICE-4G reconstruction for the ice sheets instead of more recent one.

As we discuss in Alder et al (2011), excess freshwater over land, including snow melt from the ice sheets, is totaled globally and the flux is computed as the average over the world ocean. (Similar to most models, meltwater from prescribed ice sheets is not included in the hydrologic balance.) We have modified the text to clarify.

The vegetation types for emergent land cells are prescribed from neighboring land cells. We have modified the text to clarify.

We addressed the choice ice sheet reconstructions above.

Specific comments
p. 2926, l. 24. What is the meaning of "unforced"? Obviously this AMOC change was "forced" by changes in prescribed boundary conditions.

We originally intended this to allude to the absence of N. Atlantic freshwater forcing in our simulations but we now see that it adds unnecessary confusion. The word unforced was removed from the text.

p. 2927 l. 8/9. It is unclear from the text whether "global warming" is caused only by GHGs or also to NH summer insolation. Since the latter cannot cause global warming, I would recommend to reformulate this sentence.

We revised the sentence:

"The effect of the ice sheets on climate progressively diminished from the LGM to the early Holocene as global warming driven by increasing GHGs combined with changes in NH summer insolation to accelerate ice sheet ablation."

p. 2928, l. 14. what is the difference between "time segment" and commonly used "time slice"?

Our simulations are 1100-yr long and the related time series of post-spin-up output span several centuries; however, in keeping with the standard naming conventions, we have replaced "segments" with "slices."

p. 2928, l. 17. Does "time-appropriate" means that orbital parameters were kept constant during each individual run?

Yes, the orbital parameters are held constant over each 1100 time slice simulation. We removed he phrase "time-appropriate" from the text.

P.2935,l. 2/3"SLP anomalies...are negative due to lower presser..."Sounds like tautology.

We have revised the sentence.

p. 2936 "…warm winter and summer temperature changes…" sounds odd. I would suggest to change "warm" to "positive".

We have revised the sentence.

p. 2936, l 20-24. It is unclear what is the link between global temperature and seasonality of insolations. It is known that precession and obliquity do not affect global insolation and have rather small direct impact on global temperature. Of course, in the real world insolation affects ice sheets but in the current study ice sheets are prescribed.

We did not intend to imply that changes in insolation timing drive changes in global temperature. We moved the sentence in question to the proceeding paragraph.

p. 2938, l. 28. "may have altered" is rather strange formulation for modeling paper. Altered or not?

Altered. We have revised the text.

p. 2939, l. 5,6. "The NH summer monsoons are suppressed globally". The meaning is unclear

This section has been revised and moved two paragraphs down to the discussion of the North African and Indian monsoons, which it was intended to refer to.

p. 2941, l. 24. "simulated sea-ice fraction". Firstly, the authors discuss sea ice area, not fraction. Secondly, in fact sea ice area in the NH is increasing (not decreasing) from LGM to Holocene because of increasing Arctic ocean area.

This sentence was removed and the section rearranged to address comments from both Reviewer 1 and Reviewer 2.

p. 2942, l. 8,9. "The model captures the spatial distribution of more sea ice…". Please reformulate.

We rearranged the sentence:

"The model displays increased sea ice in the western North Atlantic and decreased ice in the eastern North Atlantic and Nordic Seas where the prescribed FIS margin advances into the water (Fig. 2)"

p. 2943 , l. 25. IPCC AR5 report is now available. Please cite it instead of AR4.

The text now includes values for both CMIP3 (max AMOC) and CMIP5 (AMOC@30N).

Response to Reviewer 2

p.2928 line 20-23: this part of the text could be moved into the previous paragraph, together with the rest of the PMIP model simulation descriptions (add in line 4).

We have moved the text as suggested.

Methods:
p.2930: line 15-16: It is unclear what is meant with 'permanent sea ice': 'perennial sea ice'?

The 'permanent' was indeed meant to be perennial, we have revised the sentence.

p.2930: Question: was the doubling CO2 sensitivity estimated from a present-day climate state?

It is interesting to see that despite the lower climate sensitivity the LGM to Holocene temperature trend is in the same order of magnitude as the reconstructions suggest (see my later comment in under the Summary Section).

Yes, the 2xCO2 experiment used to establish sensitivity was relative a present-day simulation.

p. 2930: line 27: Unclear what 'which' stands for the PD or PI temperature: '[. . .], which is 1.97 C cooler than observations [. . .]'. Only afterwards it becomes clear that it must be the PD simulation.

We have rearranged the sentence to clarify we are referring to the PD simulation.

p.2930: last paragraph and p.2931 first paragraph: What does it mean that the NH temperature trend is of the right magnitude compared with observations, if the model has a low climate sensitivity in the CO2 doubling experiment?

The PD -> 2xCO_2 sensitivity of 2.2 °C implies that the 75 ppm PI -> PD change in CO_2 which is ~1/4 of the doubling, would result in ~0.55 °C change from CO_2 alone. In addition to CO_2, we changed CH_4 concentration by a factor of ~2.25 between PI and PD simulations, which would also contribute to the net warming of 0.79 °C. Quantifying the radiative contributions of CO_2 and CH_4 individually in GENMOM would require an additional set of targeted model runs, which is beyond our focus here.

p. 2932: paragraph 1: One could consider adding Renssen et al., GRL, (2005), Notaro et al, GRL, (2006) to the references.

We have added a citation to the Renssen et al. paper.

p.2932-2933, last paragraph: It is okay to choose one calendar definition over the other, however, are the insolation curves in Figure 1, the mid-month values of Berger and Loutre (1991), or are these the also now fixed-calendar seasonal averages? This issue should be resolved in the Figure 1 caption. (See also Chen et al., Clim. Dyn. (2010)).

The insolation curves in Fig. 1 are mid-month values from Berger and Loutre (1991). The caption has been updated with this information.

Results:
p.2934: line 21-23: It is unclear what is the location and direction component of the pressure gradient? North-South gradient towards the equator or towards the Mediterranean?

This sentence was an editing artifact from a previous version that belonged in an expanded monsoon section. Similar text is already in the monsoon section so we removed the sentence.

p.2934: last paragraph (line 25 +): Does he difference pattern also suggests a slight north-south shift in the pressure systems (in particular together with the later discussed rainfall it could make sense)?

The Aleutian low is expanded southward and the center of the Icelandic low is shifted to the southeast. We have revised the text with the appropriate description.

Section 3.2
p. 2935 l.10-28: The recent paper by Liu et al. in PNAS (2014) should be taken into account in discussing the differences in the global mean temperature trends of the Holocene.

The Liu et al. (2014) paper was published after our submission. We cite and draw from that paper in our revision. See discussion of Reviewer 1 Comments.

p.2936 l.12: south of the FIS: by that is meant the region which extends into the central Asian continent, right?

Yes, we revised the text to clarify this point

p.2936 l. 24: write ‘precessional shift of perihelion, and by changes in obliquity’

We have revised the text as suggested.

p.2937 l. 17: Please start the new sentence with the season '[. . .] warming over America. During summer, GENMOM simulates [. . .] consistent with [. . .]'

We have rearranged the sentence.

Section 3.3:
page 2938: l. 10-14: This is an example where the compression of complex information is dangerous. What is seen in precipitation anomalies in the model is associated through a 'short-cut' chain of causal relations. How certain is it that the described 'quasi-global' precipitation pattern is caused only by the ice-sheet /sea-ice changes and not through tropical SST changes in response to orbital and GHG forcing (locally)?

We have rewritten this section to be more spatially focused and to indicate positive anomalies in precipitation along the Gulf of Mexico and Eastern US are driven by changes in circulation, as reflected in z500 anomalies.

p.2941 last paragraph: It should be made clear in the beginning that NH sea ice area extent is controlled by bathymetry (land-sea-area changes). Area changes are in response to external forcing are thus biased.

We have clarified the bathymetric controls at the beginning of the paragraph.

p.2942 first paragraph l. 4-5: It would be better to write 'not affected by land-sea area changes with global sea level rise' (in this model at least; ice-shelf changes could indeed change the ocean area for sea ice)

We have revised the text accordingly.

p.2944 first paragraph: Please take into consideration the recent study by Marson et al, Clim. Past, (2014) (doi:201410.5194/cp-10-1723-2014)

In earlier versions of the AMOC section of our manuscript we included a detailed description of changes in AABW in our simulations based on the evaluation of the PMIP models by Weber et al. (2007), but we removed the discussion for brevity. It may not be appropriate to compare the GENMOM changes in water masses to those discussed in Marson et al, due to the lack of a freshwater forcing in our experimental design.

Section 4.2
p.2946: line 25-26: I am confused by the use of the word 'regionally coherent pattern' and 'contrasting areas of warming'. Is a coherent pattern a pattern with only positive (or negative) anomalies, whereas 'contrasting areas' show both positive and negative anomalies? Could it be labeled as 'regionally incoherent pattern'? Or does the use of words suggest an inconsistency with a reference pattern (e.g. the pattern reconstructed by proxies)?

This was sentence was ambiguous. We have revised it.

Section 5: Summary:
p.2948 l.21-27: Climate sensitivity was found to be on the low end for doubling CO2. If the LGM cooling is now consistent and in the middle range of the estimated LGM cooling, I wonder would that indicate a higher climate sensitivity during the LGM (a result suggesting a 'state-dependent' climate sensitivity?) or is it suggesting that the cooling contribution from ice-sheets (here an external forcing) is overestimated / or proxies may underestimate the global cooling contribution (e.g. they may not sample appropriately the NH ice-sheet regions). Or is the climate sensitivity and LGM cooling altogether consistent within the margin of uncertainties?

The climate sensitivity of GENMOM to a doubling of CO_2 is similar to previous studies using GENESIS and a mixed layer ocean. A lower sensitivity paired with a middle of the range LGM estimate could indicate a strong ice-albedo or other fast feedbacks in the model. Ullman et al., (2014) showed that uncertainties in the LIS topography could produce different global temperatures, and hence, drive uncertainties in the inferred paleo sensitivity. It should also be noted our LGM simulation lowers the concentration of CH_4 by nearly half that of PI, which could play a significant radiative roll. We have not performed the additional modeling simulations to quantify GENMOM's sensitivity to CH_4. The proxy stack from Shaken et al. very likely does not capture 'global' temperature with 80 sites that are predominantly coastal marine records. Our analyses indicate it is unclear if the proxy sites under or over estimate temperature change at the LGM. We feel GENMOM has a reasonable sensitivity and LGM cooling given the uncertainties in the proxies, imperfect sampling and good agreement with results of similar models.

References:

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