

Interactive comment on “Global climate simulations at 3000 year intervals for the last 21 000 years with the GENMOM coupled atmosphere–ocean model” by J. R. Alder and S. W. Hostetler

Anonymous Referee #1

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The manuscript by Alder and Hostetler presents results of simulations of eight time slices during the last glacial termination performed with a coupled climate model. The authors found that for LGM and mid-Holocene time slices their modeling results are in a broad agreement with PMIP2/3 models and paleoclimate data.

General comments

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

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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 latter 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.

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.

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.

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, where? 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.

Specific comments

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

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.

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

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

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

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

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.

p. 2938, l. 28. “may have altered” is rather strange formulation for modeling paper.

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Altered or not?

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

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.

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

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

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