

## ***Interactive comment on “A coupled climate model simulation of Marine Isotope Stage 3 stadial climate” by J. Brandefelt et al.***

**Anonymous Referee #2**

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### General comments

In the presented manuscript, Brandefelt and co-authors highlight the importance of equilibrating climate states in comprehensive earth system models (i.e. general circulation models - GCMs) under constant climate forcings and boundary conditions. To do this, they present a >1500-year long climate simulation ran with 44ka BP climate forcings in the NCAR Community Climate System Model version 3 (CCSM3). This interval is taken to correspond to Greenland Stadial 12 (GS 12) during Marine Isotope Stage 3 (MIS3), the period with most frequent abrupt glacial climate shifts (i.e. Dansgaard-Oeschger events – DO events). Following two previous modelling studies (Van Meerbeeck et al., 2009 and Merkel et al., 2010), Brandefelt et al. elaborate on the notion that modelling experiments studying climate dynamics behind DO events start from an

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equilibrated MIS3 climate state – as opposed to a short model integration leading to an apparent quasi-equilibrium as is common practice with GCMs. To this end, Brandefelt et al. describe differences in simulated quasi-equilibrium MIS3 climate obtained after 1500 years and the apparent quasi-equilibrium climate after 500 years. They find that, though global temperatures remained nearly unchanged, substantial warming is found in the North Atlantic region in towards the end. With a better agreement between reconstructed GS12 North Atlantic sea surface temperatures (SSTs) and the last century of their simulation, Brandefelt et al. argue that, at least in the CCSM3 model, MIS3 background climate at 44ka BP was in close equilibrium with long-term climate forcings. Since this argument is consistent with Merkel et al. (2010), who used the same model, but opposed that of Van Meerbeeck et al. (2009) who used a simpler earth system model, Brandefelt and co-authors suggest that the dynamics of simulated MIS3 climate states are strongly model-dependent. Finally, the authors argue, by showing differences in ENSO-teleconnections between the early part and the end of their simulation, that equilibrating GCMs also affects simulated climate variability.

The topic of the Brandefelt et al. manuscript is definitely relevant to the Palaeoclimate Modelling community (and to a certain extent also to the Palaeoclimatology and –oceanography communities), and the result analysis is thoroughly presented throughout the paper. However, certain doubts may be casted whether the presented methods actually lead to robust conclusions made in the manuscript. Furthermore, many of the results are not discussed in terms of the climate dynamics behind them or their context in the state-of-the-art. Balancing these factors, I recommend publication in Climate of the Past of a deeply reworked manuscript, responding to all raised comments.

### Specific comments

#### Major comments

1. Although I applaud the meticulous approach in trying to constrain the agreement between reconstructed GS12 and simulated SSTs, the proposed constraint appears

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weak to me. Even if 2 times the uncertainty of reconstructed SST values may take into account most of the accumulated estimate error of a reconstruction (e.g. biases in seasonal, micro-environmental conditions, measurement, sample size, use of different proxies, etc.), the consequent low signal to noise ratio will often lead to insignificant temperature change through time. Then, if model and data are in agreement, the reconstructed SST estimates do not necessarily perform well at constraining which part of the equilibrium simulation fits data better. For example, given a reconstructed GS12 winter SST estimate at a certain point of  $2 \pm 2^\circ\text{C}$ , if the model simulates winter SST values of  $0.5^\circ\text{C}$  in years 300-599 and  $1.9^\circ\text{C}$  in years 1200-1500, no inference can be made on improved model performance in the latter part of the simulation. With these constraints, a better agreement would only be found if the former value were, say  $-1^\circ\text{C}$  and the latter  $+0.9^\circ\text{C}$ . But even then, the reason why a better fit in winter SST is found does not necessarily imply that the climate state was closer to observations. Adding sea-ice cover as a variable to further constrain the model results does not aid much, since proxy-inferred values are bound by even larger errors and anyway are tied to the SST estimates. Another major point of concern is the inference that 30-50% of agreement between simulated and reconstructed SST would be sufficient to conclude that the simulated MIS3 climate state resembles GS12 climate and subsequently infer that climate was close to equilibrium under 44ka BP climate forcings. I therefore strongly suggest the authors to add different sources of data (e.g. temperature reconstructions from ice cores and terrestrial records) to constrain the model in order to reach robust conclusions.

2. It is quite frustrating to see a detailed description of simulated ENSO teleconnection changes between the LGM equilibrium and several intervals in the MIS3 equilibrium simulations without a discussion on the mechanisms underlying these changes. Though definitely a good point is made that model equilibration affects climate variability, the authors need to convince the reader with an appropriate discussion that the simulated teleconnections are physically consistent and which implications they have on climate. Such discussion may, in turn, help distinguish whether the latter part of the

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MIS3 equilibrium simulation is closer to reconstructed climate during GS12.

3. In the MIS3 equilibrium, simulated SSTs in the North Atlantic region are colder than reconstructed with more sea-ice expansion than the estimates based on reconstructed SSTs indicate. A too vast sea-ice expanse seems to be a recurring bias in the CCSM3 model (see e.g. Collins et al., 2006). This stands in stark contrast of a warm bias found in the LOVECLIM simulations of Van Meerbeeck et al. (2009). The authors indeed rightfully mention in their conclusions that the dynamics of simulated MIS3 background climate are model-dependent. Also, the authors discuss that the differences in simulated Atlantic Meridional Overturning Circulation (AMOC) strength and configuration determines most of the difference in SST between the models. However, if an upward bias in sea-ice extent causes most of the cooling of air and sea surface temperatures in the North Atlantic region and explains the AMOC weakening in CCSM not found in LOVECLIM, then the inference that GS12 climate was in close equilibrium with 44ka BP boundary conditions is a direct result of the bias and thus not a robust conclusion. I suggest the authors adequately discuss this issue and moderate their conclusion of stadial climate being close to equilibrium with MIS3 boundary conditions.

Minor comments:

1. p. 83 line 20: It is surprising that the authors do not discuss their results in light of their previous simulations using the same (but shorter) equilibration (Kjellström et al. 2010, BOREAS).
2. p. 84 line 19: That the simulation was ended after year 1538 looks rather suspicious. Did the simulation crash after this year? The authors should explain their choice of ending the simulation at a seemingly random time.
3. p. 85 line 19-21: The authors should mention the cause of the latitudinal insolation gradient changes... Was it precession?
4. p. 85 lines 26-27: Which of the values is meant by lower/higher? a 45m lowering

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or a 75 meter lowering of sea level? Also, it is far from a given fact that millennial-scale sea level changes followed the pace of DO events (e.g. Clark et al., 2007, AGU Monographs)

5. p. 86 lines 1-2: the authors should at least add a reference here to support their statement. (e.g. Wohlfarth and Näslund 2010 mentioned in the next sentence)

6. p. 86 line 7: It is not clear to me why the authors force the Antarctic Ice Sheet with 14ka BP reconstruction of Peltier (2004) as a proxy for 44ka BP if the 44ka BP ice sheet topography is also included in Peltier's reconstruction. (may be seen e.g. on web page [http://www.sbl.statkart.no/projects/pgs/ice\\_models/Peltier\\_ICE-5G\\_v1.2/](http://www.sbl.statkart.no/projects/pgs/ice_models/Peltier_ICE-5G_v1.2/))

7. p. 87 lines 23-24: the reference to Skinner et al. (2007) should be Skinner and Elderfield (2007). However, they did not provide the first reconstruction of SSTs for this site (e.g. Pailler and Bard 2002).

8. p. 88 lines 3 & 15: Huber et al. (2006) did not reconstruct Central-Greenland temperatures from  $\delta^{18}\text{O}$  measurements.

9. p. 91 line 29: The authors should check and, if necessary, mention whether the simulated and reconstructed temperature at depth of  $-1.9^{\circ}\text{C}$  v  $0.2^{\circ}\text{C}$  are potential or actual temperatures.

10. p. 95 lines 7-9: the authors should briefly explain why SST output from the model is missing in seemingly over 150 years between model year 195 and 569.

11. p. 97 line 23: Pollard and Barron (2003) did not simulate SSTs. Rather, they forced their atmospheric GCM with prescribed SSTs.

12. p. 100 lines 7-10: Although Rial and Yang (2007) did find internal oscillations in an older version of the LOVECLIM model leading to rapid, multi-centennial-scale SST shifts in the Nordic Seas apparently resembling DO events in this aspect, these oscillations are produced only under specific climate forcings and parameter space (e.g. found in Holocene simulations by Schulz et al., 2007, CLIM PAST). Dynamically,

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these are unlikely to represent DO events, since these oscillations disappear when improving diffusion parametrisation. I thus suggest not to refer to Rial and Yang (2007) in the context of DO events.

13. Fig. 8: why show results for interval 1139-1438 if in other places the results of years of the last 100 or 300 years of the simulation are discussed (i.e. until year 1538)?

Technical comments:

1. p. 80-81: The flow of the abstract text is not optimal with many connecting words between sentences missing.

2. p. 80 lines 5-6: instead of "5.5°C higher ... 1.3°C lower" it should be "5.5°C lower ... 1.3°C higher"

3. p. 83 lines 5-14: "Merkel ...equilibrium" a new idea is introduced here and requires a separate paragraph

4. p. 84 line 22: the first sentence of the paragraph does not appropriately introduce the idea discussed in the paragraph.

5. p. 85 line 13: "The insolation was ..." should read "The orbital insolation forcing was ..."

6. p. 85 line 17: which month is meant by "summer insolation"

7. p. 89 line 9: "1440-1539" should be changed to "1439-1538"

8. p. 93 lines 13, 15: what is meant by T1000?

9. p. 95 line 29: "Fig. 8" should be "Fig. 7"

10. p. 98 line 7: remove both references – they are not relevant here.

11. p. 99 last par: this paragraph is somewhat repetitive.

12. p. 102 lines 2-3: "seasonal mean T2m" ... in which season?

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13. Fig. 1 caption: “50°C” should be replaced by “50°N”

#### References

Clark, P. U., Hostetler, S. V., Pisias, N. G., Schmittner, A., and Meissner, K. J.: Mechanisms for an 7-kyr Climate and Sea-Level Oscillation During Marine Isotope Stage 3, in: *Ocean Circulation: Mechanisms and impacts*—Past and future changes of meridional overturning, edited by: Schmittner, A., Chiang, J. C. H., and Hemming, S. R., American Geophysical Union, Washington, DC, USA, 209–246, 2007.

Pailler, D., Bard, E., 2002. High frequency palaeoceanographic changes during the past 140,000 yr recorded by the organic matter in sediments of the Iberian margin. *Palaeogeography, Palaeoclimatology, Palaeoecology* 181, 431–452.

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Schulz M, Prange M, Klocker A (2007) Low-frequency oscillations of the Atlantic meridional overturning circulation in a coupled climate model. *Clim. Past* 3: 97-107. <http://www.clim-past.net/3/97/2007/>

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