Clim. Past Discuss., 7, C332–C342, 2011
www.clim-past-discuss.net/7/C332/2011/

Climate of the Past Discussions

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

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Received and published: 21 April 2011

Reply to Referee Comments by M. F. Sánchez Goñi

We would like to thank M. F. Sánchez Goñi for relevant comments to the manuscript. Sánchez Goñi's comments are given below along with our reply.

Comment: Concerning forcing and boundary conditions with respect to ice sheets, topography and bathymetry. In their model, the GS12 simulation is forced with LGM conditions, -120 m instead of -75 m as indicated by sea level reconstructions for MIS 3. Therefore, the Barents and Kara Seas are on land instead of below sea level. They argue that at the coarse scale of the global model the impact on the simulated oceanic circulation would be small. However, I wonder whether this effect substantially

influences brine formation and sea ice cover extent in the Nordic Sea regions, a key zone for AMOC dynamics. Does the simulated reduction by 50% of the AMOC and lower SSTs in comparison with the proxy reconstructed SST could be explained by this boundary condition?

Reply: Global sea level estimates indicate a lowering of between 25 and 75 m for the period from 100 ka BP to 35 ka BP (Lambeck et al 2004). Nonetheless, geological evidence indicate that the Barents and Kara Seas ice sheet was extensive in middle Early Weichselian (60-50ka BP) (Svendsen et al, QSR, 2004). From this we conclude that the conditions that prevailed in this region during GS12 are not well known. It would be very interesting to test the isolated influence of a lowering of the sea level by 120 meters on the recent past climate (and also of the other changes in forcing and boundary conditions between the RP and GS12 simulation). Unfortunately this would require substantial amounts of computing time and is therefore beyond the scope of this manuscript. We do however agree that the possibility of an influence on the ocean circulation should be mentioned, this has been added in Section 2.2.2.

Comment: SST response. The authors find that the simulated GS12 SSTs are in agreement with reconstructed SSTs in 30-50% of the proxy sites. In my view this is a weak agreement. It is for this reason that I would like the authors to explain in more detail the tracers from which the SSTs have been reconstructed. We know, after the MARGO conclusions for the Last Glacial Maximum (LGM) (MARGO et al., Nature Geosciences, 2009), that divergences can occur between different SST proxies. Is there a particular proxy which better converge with simulated SSTs?

Reply: Although we agree it would be very interesting to look at each type of proxy separately, we do not find this reasonable for this data set. For annual mean SST, 24 out of 27 SSTs originate from alkenones. For NH winter 22 out of 29 SSTs originate from planktonic foraminifera and for NH summer 26 out of 40 SSTs originate from planktonic foraminifera. We have added a comment in Section 6.2 regarding the agreement based on different proxies.

The suggested technical corrections have been taken into consideration in the revised manuscript.

Reply to Referee Comments by Anomymous Referee #2

We would like to thank the anonymous referee for relevant comments to the manuscript. The referee's comments are given below along with our reply.

Comment: Although I applaud the meticulous approach in trying to constrain the agreement between reconstructed GS12 and simulated SSTs, the proposed constraint appears 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±2C, if the model simulates winter SST values of 0.5C in years 300-599 and 1.9C 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 -1C and the latter +0.9C. 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.

Reply: We agree that the large uncertainty of the proxy SSTs makes it difficult to determine what is a good match between the simulated and reconstructed climate. We also agree that there is little information contained in the finding that the difference between proxy SST and simulated SST at one specific site is smaller in period 2 than in period 1. The finding that this difference is smaller in period 2 than in period 1 at a substantial number of sites does however indicate that the simulated climate in period 2 is closer to the climate described by the proxies. The description of the improvement in Section 4.1 has been extended.

Our simulated GS12 climate appears to reach an equilibrium under GS12 forcing and boundary conditions. The same is true for the stadial (and interstadial) MIS 3 climate simulated by van Meerbeeck et al (2009). The difference is that our climate is colder than the proxy SSTs at most locations with a substantially reduced AMOC, whereas van Meerbeeck et al's climate is warmer than proxy SSTs (although they do not present a direct comparison) with a largely unchanged AMOC. Meerbeeck et al concluded that their results indicated that interstadial rather than stadial climate should be regarded as the equilibrium MIS3 climate. Based on our and their results we conclude that the climate simulated under MIS3 stadial forcing and boundary conditions is model-dependent. We do however not claim that the climate was in equilibrium at 44ka (based on the rapid variations in the Greenland ice core records during this period this is highly unlikely). Both our and van Meerbeeck's results rather indicate that there is something missing in the model world. This could either be an "external" forcing such as release of fresh water in the North Atlantic as suggested by van Meerbeeck et al or internal dynamics of the coupled climate system as discussed in Section 6.4. We have modified the Abstract to hopefully better describe the main findings of the present study.

The simulated GS12 climate is already compared to ice core data in Section 4.3.

As discussed by Kjellström et al (2010), Qualitative, proxy-based information for

Dansgaard-Oeschger cycles is also registered in land records (e.g. Wohlfarth et al . 2008; Wohlfarth 2010), but their often poor chronological control and lack of quantitative data limit detailed comparisons. This is mentioned in Section 2.3 of the revised manuscript.

Comment: 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 MIS3 equilibrium simulation is closer to reconstructed climate during GS12.

Reply: The main motivation for including the analysis of ENSO teleconnections in this manuscript is to show that variability analysis should be performed with care when the climate to be studied is not in equilibrium. Since the dynamics underlying the interaction between the Rossby waves originating in the tropics and the mid-latitude dynamics is non-linear there is no simple explanation for the differences in ENSO teleconnections between the RP, GS12 and LGM. We do however agreee that some discussion of the results is adequate and this has been added to Section 5.

Comment: 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.

Reply: The sea ice expanse occuring in our GS12 and LGM simulations may be an effect of the sea-ice bias found for RP conditions in CCSM3. On the other hand, the weak sea ice and AMOC response to substantial changes in the forcing and bounday conditions simulated in LOVECLIM may also be an effect of biases in that model. All climate models have biases. In this manuscript we want to point to the fact that the results of this type of exercise is model-dependent, due to all the biases in the different models. Most modelling studies of past climate performed so far (excluding the new PMIP3 simulations being run as we speek) were performed with low resolution and/or intermediate complexity models. One point to be made here is that we need a (resolution and complexity wise) range of climate models to get an enhanced understanding of the dynamics of past climate (variations).

The reduction of the AMOC and the expanse of the sea ice is coupled in our simulations and we have included a discussion of this coupling, with reference to the CCSM3 bias in sea ice for RP conditions, in Section 6.3. The bias is also first mentioned in Section 4.2.

We do not conclude, but rather suggest, from the results presented in the manuscript that stadial rather than interstadial climate should be interpreted as a near-equilibrium MIS 3 climate. We would like to argue that this suggestion is as valid as van Meerbeeck et al's suggestion that freshwater forcing is necessary to return climate from warm interstadials to cold stadials during MIS3, making the stadial climate a perturbed climate state rather than a typical, near-equilibrium MIS 3 climate.

Comment: 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).

Reply: The simulated climate after ∼500 model years (i.e.the climate studied by Kjellström et al 2010) is compared to the simulated climate towards the end of the simulation in Section 3, this is also mentioned as one of the main conclusions in Section 7. We have added two sentences to the Introduction to make this clearer.

Comment: 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.

Reply: The reason for the odd number of model years is simply that we ran out of computing time. The simulation was run on "left-over" time from other projects and we simply ran it for as long as there was available computing time. If it had crashed after 1538 years we would of course not have presented the results without saying!

Comment: p. 85 line19-21: The authors should mention the cause of the latitudinal insolation gradient changes.. Was it precession?

Reply: The difference is due to the combination of the obliquity and precession signals. An explanation has been added to Section 2.2.1.

Comment: p. 85 lines 26-27: Which of the values is meant by lower/higher? a 45m lowering or a 75 meter lowering of sea level? Also, it is far from a given fact that millennialscale sea level changes followed the pace of DO events (e.g. Clark et al., 2007, AGU Monographs)

Reply: It has been clarified in the text that MIS 3 sea level was also lower than at present. The statement about stadial and inter-stadial sea level has been removed.

Comment: 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)

Reply: Done.

Comment: 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/)

Reply: We were not aware of the Peltier data for 44ka BP, since these are not available on Peltier's web page for download. 14 ka BP was therefore taken as a period with similar climate, as stated in Section 2.2.2.

Comment: 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).

Reply: Both Skinner et al (2007) and Skinner and Elderfield (2007) are correct references for these data. Nevertheless, since it is easier for the reader to get hold of a journal paper than of a book chapter we have changed the reference to Skinner and Elderfield (2007). The relevant data is for abyssal ocean temperature, not SST. The relevant SST records have been added in the paleo-SST comparison.

Comment: p. 88 lines 3 & 15: Huber et al. (2006) did not reconstruct Central-Greenland temperatures from del-18O measurements.

Reply: Huber et al has been removed from this sentence.

Comment: p. 91 line 29: The authors should check and, if necessary, mention whether the simulated and reconstructed temperature at depth of -1.9C v 0.2C are potential or actual temperatures.

Reply: Done. Also the proxy value has been corrected to 0.55C.

Comment: 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.

Reply: This has been explained in Section 5.

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

Reply: The reference to Barron and Pollard was removed from this sentence.

Comment: 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, 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.

Reply: We have found similar internal oscillations using the latest version of the LOVECLIM model (Boström-Pöntynän,Master thesis at Department of Meteorology, Stockholm University 2010). These results are not published in refereed litterature, however this motivates us to keep the reference to Yang and Rial (2007).

Comment: 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)?

Reply: The idea is to show that the Nino3 teleconnections are not sensitive to which 300 year period is chosen once we have reached a quasi-equilibrium. Conversely, if the results of the Nino3-analysis does not differ significantly between the interval 1139-1438 and the last 300 years (1239-1538) we can say that the climate is close to equilibrium. This has been clarified in Section 5.

Reply to Short Comment by F. Marret

We would like to thank F Marret for relevant comments to the manuscript. Marret's comments are given below along with our reply.

Comment: Page 86, line 10. The paper from Näslund et al (2008) does not imply that there is no ice sheet in Alaska, but that CLIMBER-2 simulates a too thick ice sheet. This is a very weak argument.

Reply: It is correct that Näslund et al (2008) states that "However ice thickness in Yukon/Alaska is too thick.". This statement includes the possibility of no ice sheet at all in Alaska. This is also the conclusion made by Näslund et al (2008) a few lines further down on the same page of the report, where they write "The suggested model setup is good. The selected ice configuration is seen in Figures 4-2 and 4-3.". Figure 4-2 shows the CLIMBER-2 ice sheet over North America after removal of the ice sheet in Alaska.

Comment: Proxy data: The database is dated from 2002 (and two other records from 2008 and 2009); there has been many reconstructions of past sea-surface temperatures since then, in particular in the Pacific Ocean. As this paper does integrate an important comparison between simulations and proxy data, it would worth to revise the proxy records, as well to extend to other proxies (dinoflagellate cysts are not included for instance, why? See the MARGO compilation as an example). It is a fact that different proxies in a same sample may not reconstruct the same environmental variables. With regards to the extent of sea ice, as there are very few proxies, in particular for the North Pacific, this is a major issue.

Reply: We have extended the data base to comprise a total of 61 sites with 27 SSTs for the annual mean, 29 for Northern Hemispere winter and 40 for Northern Hemisphere summer. We are however not aware of any dinoflagellate cysts data that cover GS12. Although we agree it would be very interesting to look into detail at each type of proxy separately, we do not find this reasonable for this data set, at least not for annual and winter SSTs, and the scope of this manuscript. For annual mean SST, 24 out of 27 SSTs originate from alkenones. For NH winter 22 out of 29 SSTs originate from planktonic foraminifera and for NH summer 26 out pf 40 SSTs originate from planktonic foraminifera. A comparison of foraminifera and Mg/Ca based SSTs is made for NH summer in Section 6.2. We also agree that the reconstruction of sea ice extent is a major, unsolved issue in paleoclimate reconstructions.

Comment: The reduction of AMOC is an interesting result, but little is discussed about the mechanisms behind it.

Reply: A discussion of the mechanisms of the AMOC reduction has been included in Section 6.3 of the revised manuscript.

The suggested technical corrections have been taken into consideration in the revised manuscript.

Interactive comment on Clim. Past Discuss., 7, 79, 2011.