Clim. Past Discuss., 8, C2952–C2957, 2012
www.clim-past-discuss.net/8/C2952/2012/

Climate of the Past Discussions

Interactive comment on "A model-data comparison of the Holocene global sea surface temperature evolution" *by* G. Lohmann et al.

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Received and published: 24 December 2012

Paper: "A model-data comparison of the Holocene global sea surface temperature evolution" by G. Lohmann, M. Pfeiffer, T. Laepple, G. Leduc, and J.-H. Kim, Clim. Past Discuss., 8, 1005-1056, 2012. www.clim-past-discuss.net/8/1005/2012/ doi:10.5194/cpd-8-1005-2012

Response to the reviewer 1

First, we would like to thank the reviewer for the useful and constructive comments. We respond to the three reviewers' major comments.

1) Linear regression The reviewer suggests to test whether a linear model is appropriate to describe the shape of the Holocene trends. This is a good and sensible

suggestion, as the insolation changes are not linear in time, and nonlinear reactions of the climate system might additionally cause deviations from a linear evolution across time. As the reviewer suggested, we analyze the residual plots (standardized residuals of the fit relative to the fitted values) of all sediment records for the alkenones (Figs. 1-3) and Mg/Ca (Fig. 4), as well as the corresponding AOGCM time-series (Figs. 5-8).

In the sediment records, some cores, for example alkenone record BS79-38, show deviations from linearity. This is especially true for alkenone records and less pronounced for the Mg/Ca records. One cannot exclude that this occurs by chance as the alkenone residuals are autocorrelated in time, a point we will later discuss. Across the cores, no clear common pattern in the deviations from linearity is visible which would ask for a non-parametric analysis (Figs. 1-4). We further tested if other parametric models as polynomial models are more appropriate than our linear model: Whereas a polynomial model, as expected, results a higher explained variance, no relation between the deviations from linearity was found in the GCM and the proxies. In the case of fitting a second order polynomial, the nonlinear terms between model and data are uncorrelated.

We therefore continue to favor the linear model. While we acknowledge that this is not a perfect description of the climate response, the linear models provide a good metric to summarize the main behavior, and as we use the same metric for the GCM and the proxy results, potential deviations from linearity would not bias the results. We included a subsection in the results section.

As we mentioned before, these analyses also show, that the residuals are not independent in time for the alkenone records. This is expected, as elements of the climate system, especially the oceans provide some memory. Further, the recording process, as mixing of the sediment by bioturbation, might further increase the autocorrelation. In the revised version, we therefore account for serial correlation by estimating the effective degrees of freedoms $N_{eff} = N(1-r)/(1+r)$, where r is the lag-1 autocorrelation coefficient of the residuals.

2) Uncertainties in the SST trends The serial correlation increases the uncertainty of the slope estimates, especially for the Uk37 records. These recalculated confidence intervals for the slope estimates will be included in the revised manuscript (former Figs. 1 and 2). Because the panels were rather small in the old Fig. 1, we plan to split the former Fig. 1 into 3 sub-figures which are here shown as Figs. 9-11. The figures for the time series include the slope estimates for Alkenones (Figs. 9-11) and Mg/Ca (Fig. 12) and will be included in the revised paper.

Analogous to Figs. 9-12, we display the model data at the core locations where we include the trends plus standard error (Figs. 13-16). Figs. 13-16 show that the uncertainty in the simulated SST trends is very small. As proposed by the reviewer, we include the uncertainty in the trend analysis, by adding error bars in the former CPD Figure 4a,b. These errorbars are shown in Fig. 17 for alkenones and Fig. 18 for Mg/Ca, to be included in the revised paper.

To consider the uncertainties in the SST trends in the model-data comparison, we calculate the weighted correlation between model and proxy data trends, as suggested by the reviewer. The Pearson correlation coefficients were calculated for annual, local winter, and local summer trends. The weights were calculated with $1/sd^2$, where sd is the standard error of the slope in the proxy SSTs. The weighting of the trends with their uncertainty increases the positive correlations between simulated and observed trend patterns in all cases except for local summer where correlation is nearly unaffected by the weighting (table). The significant negative correlations for Mg/Ca and model trend needs to be evaluated in detail in a subsequent study.

3) Model deficiencies

We agree with the reviewer that current climate models have a coarse vertical resolution and crude representation of subgrid-scale processes of the ocean and would like to mention the difficulty of ocean models to properly simulate the seasonal dynamics and mixed layer. In order to get an estimate of the mixed layer depth, we compare

the modeled mixed layer depth in the control climate simulation (Fig. 19) with ocean reanalysis data (Fig. 20). The mixed layer depth of the model (HOPE-G) was calculated following the temperature criterion as described in Levitus (1982), which defines the mixed layer as the depth at which the temperature change from the surface temperature is 0.5°C. The same method was used for calculating the mixed layer depth of the ocean reanalysis data (SODA, Carton and Giese, 2008; Carton et al., 2005). Before the calculation of the mixed layer depth, the observational data were vertically interpolated to the HOPE-G model depths. We see similar large-scale pattern in the model and SODA, but also deviations especially in the Southern Ocean. We will insert some cautionary remarks into the new version of the manuscript and emphasize that the representation of the mixed layer will be essential to improve climate simulations and data-model comparisons.

Furthermore, we evaluated the ocean model component of ECHO-G in simulating the seasonal cycle in SST for the core locations (Figs. 21-23, Fig. 24). Again, we split them in different panels: one panel for data located where alkenones have positive trends (Fig. 21), and two panels for those with negative trends (Figs. 22,23), and Fig. 24 for Mg/Ca locations. The black lines represent the modeled seasonal cycle, while the red represent the observational SST seasonal cycle. For the analysis of the model data, we used the last 50 model years of the preindustrial climate simulation and for the observational data we used the full time period (1958-2001). For consistency, we analysed the 10m depth level in both datasets as SST.

We find a general agreement in the shape of the seasonal cycle, but detect a cold bias in the model, especially at high latitudes. We assume that the trends will not be seriously affected by this offset, but will mention in the revised version that deviations in the mean climate state are detected in the model. Parts of this offset could be related to the preindustrial control simulation as compared to present SSTs which are affected by the anthropogenic induced CO_2 increase. At some locations, the ability to simulate the seasonal cycle seems to be limited, especially in the tropics (e.g. in Fig.

21: 52°E,11°N; 11°E,6°S; 82°E, O°S). The result will be used to discuss the ability of climate models to simulate SSTs in the past.

Minor comments:

1) Is rephrased in the revised version 2) Rephrased: "In order to fulfill minimal statistical requirements," is not necessary, just deleted 3) Corrected 4) Thanks, done 5) Done 6) Adopted 7) Rephrased as suggested 8) We would like to avoid showing model-data SST difference divided by the model SST trend. We prefer to directly compare the SST trend in the data and model. We adopted your formulation. 9) Rephrased, we want to explain that the median is a better measure than the ensemble mean. We shortened the paragraph. 10) Clarified. Seasonal bias. 11) Done. Related to lines 25-27: we changed the formulation now. 12) We think that we are already criticizing the work directly, especially the pre-selection of proxy-records prior to the correlation estimation. 13) Done 14) We reformulated it. The proxy would record a weaker temperature signal. 15) Done 16) We clarified the sentence and changed the order to make the logical clear. 17) Done 18) Reformulated 19) Done 20) We reformulated orbital forcing. Since obliquity provides the high-low latitude pattern and since precession forcing is zero on the annual mean, we reformulate the sentences. 21) Done 22) Done 23) OK 24) Done 25) Corrected

We update the figures and analyses, and have inserted the comments in the revised manuscript.

The figures for the reply are in the supplement.

References:

Carton, J. A. and Giese, B. S. : A Reanalysis of Ocean Climate Using Simple Ocean Data Assimilation (SODA), Mon. Weather Rev., 136, 2999-3017, 2008.

Carton, J. A., Giese, B. S., and Grodsky, S. A. (2005): Sea level rise and the warming of the oceans in the SODA ocean reanalysis, J. Geophys. Res., 110, art#

10.1029/2004JC002817, 2005.

Levitus, S. : Climatological Atlas of the World Ocean, NOAA Professional Paper 13, U.S. Department of Commerce, 1982.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/8/C2952/2012/cpd-8-C2952-2012-supplement.pdf

Interactive comment on Clim. Past Discuss., 8, 1005, 2012.