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Climate of the Past Discussions

Interactive comment on "Southern Hemisphere orbital forcing and its effects on CO_2 and tropical Pacific climate" *by* K. Tachikawa et al.

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The first part indicates the comment of the referee and the following text is our answer.

-My main concern is that a causal link from orbital forcing to CO2 and climate is made only from a temporal agreement between several proxies, while the crucial point cannot be quantified-.

As already shown in the answer to Referees #1 and #2, we decided to concentrate on the processes affecting the tropical SST on orbital timescales by data-model comparison. The causal link from orbital forcing to CO2 and climate will be presented elsewhere as an independent paper.

-The analysis of a potential seasonal bias correction is not clear to me. As far as

I understand this calculation is based on two assumptions, which the authors may explain a little further: (1) The seasonal productivity cycle of a zooplankton organism follows the chl a signal, which is produced by phytoplankton. (2) The phasing of the seasonal cycle of chl a is constant over the last 400 kyrs. Concerning the second point I would argue that there is a distinct secondary maximum in chl a in austral spring-.

Referee #3 is right about the assumptions, and we agree with him/her about the necessity of more explanation. We calculated a production-weighted average SST record to establish a time series of seasonal bias (Figure S6). Monthly foraminiferal production is assumed to mimic the present-day chlorophyll-a concentration pattern (Figure S3c) and to be constant for the studied period. Then, the weighted average SST is computed by combining the seasonal foraminiferal production with simulated seasonal SST records. Consequently, the second production peak in austral spring is already taken into account in the seasonal bias. The weighted-average SST record is in close agreement with the simulated mean annual SST and in phase with the Mg/Ca-SST, which suggests limited impact of the seasonality of foraminiferal production on Mg/Ca-SST at the studied site. We will add this explanation in the revised version.

-The motivation for the experimental setup of the model simulation needs to be clarified-
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As shown in answers to Referees #1 and #2, we decided to concentrate on the tropical processes on orbital timescales by data-model comparison. The models are a simplification to the complex Earth System, but for the discussed mechanisms are the state-of-the-art modelling tools when it comes to simulating full glacial cycles over 100ka and longer (Timm et al., 2008;Timmermann et al., 2009). Here, our objective is not to precisely reproduce SST variability, but to clarify the processes influencing the WPWP SST and its sensitivity to individual forcings and processes in the ocean-atmosphere system.

-p. 1870, l. 19: add 'the' before 'equator'- -p. 1871, l. 1: variability of what? I'd suggest

adding 'SST' before 'variability'- -p. 1872, l. 8: add 'a' after 'with'- -p. 1872, l.17: remove 'the' before New Guinea- These minor points will be corrected in the revised version.

-p. 1875, l. 9: what is the definition for 'moderate dissolution'. How do you know it does not affect the Mg/Ca signal?- 'Moderate dissolution' is defined in Tachikawa et al. (2008). Briefly, when the foraminiferal tests are subjected to partial dissolution, size-normalized test weight becomes lower and test loss during cleaning becomes higher. With 'moderate dissolution', the test loss does not exceed 80% with the short (Mg) cleaning, which is the case of the studied core. In addition, there is no systematic trend between test weight and foraminiferal Mg/Ca.

-p. 1876, l. 8: singular: 'desert'- It will be corrected.

-p. 1876, l. 18-19 replace 'intervals for LOVECLIM' by 'intervals between LOVECLIM components'-. It will be corrected.

-p. 1877, l. 22: Here you mention that the last glacial does not terminate completely. How about other glacial periods in the time period of the last 400 kyr? Does this affect your results, since later on you mention that the mechanisms of changed Ekman efficiency would only work in cold climates when sea-ice cover is sufficiently large. How good is the model's representation of sea-ice cover? Already for the modern situation there seems to be some mismatch to sea-salt reconstructions-.

We will revise the focus of the model and proxy analysis in the revised manuscript. Southern Ocean dynamics will be discussed in a separate manuscript. Hence, we just briefly respond here: The glacial cycles in the simulation are interesting in itself, since the last glacial maximum is significantly larger than the ice-age maxima in the previous cycles. Therefore, understanding the cause for the termination-problem is tightly coupled to the question what caused the development of a stable and large LGM state in the model. This will be part of another research study. The direct effect of Northern Hemisphere ice-sheet coverage on SH sea ice during the glacial cycles

is small in our model (we have studied this with LOVECLIM using prescribed NH ice-sheet conditions). Primarily the SH-NH polar latitudes are forced by local insolation and CO2 in our model. In addition, changes in the ocean overturning circulation can add a significant teleconnection. However complete shutdowns of the overturning circulation were not observed in the conducted experiments (without explicit freshwater forcing in the ocean).

-p. 1880, l. 2: 'Kohler' should read 'Köhler'. Use 'Kn"ohler' in latex. See also in references-. It will be corrected.

-p. 1881, l. 15: I don't really see the 'close match', other than that all variables with G/I cyclicity will somehow be similar (see also Rev. #1)-. This part will be removed in the revised version.

-p. 1883, l. 4-6: This sentence seems to overstate the results. Since cause and effect cannot be separated from a simple regression analysis, I'd suggest being more cautious at this point-. This part will be removed.

-p. 1884, l. 18: This paragraph reads more like a summary than a conclusion. I'd suggest naming it accordingly or rephrase into real conclusion-. Conclusions will be revised since we will focus on the tropical processes affecting the WPWP SST.

-Fig. 4: The agreement between model and proxy data is striking since regression slopes vary by a factor of two. Therefore, please mention in the caption something like 'Please note that the amplitude of the scale for the model results (right axis) is only half the amplitude of the data (left axis).'- The suggested sentence will be added.

-Fig. 6: Maybe this is due to the very small size of the Figure in the CP online format, but I do not see a black line in any of the sub panels, although mentioned in the caption-. This figure will be removed from the revised version.

References Tachikawa, K., Sépulcre, S., Toyofuku, T., and Bard, E.: Assessing influence of diagenetic carbonate dissolution on planktonic foraminiferal Mg/Ca in the

southeastern Arabian Sea over the past 450 ka: Comparison between Globigerinoides ruber and Globigerinoides sacculifer, Geochem. Geophys. Geosyst., 9, Q04037, 10.1029/2007gc001904, 2008. Timm, O., Timmermann, A., Abe-Ouchi, A., Saito, F., and Segawa, T.: On the definition of seasons in paleoclimate simulations with orbital forcing, Paleoceanography, 23, PA2221, 10.1029/2007pa001461, 2008. Timmermann, A., Timm, O., Stott, L., and Menviel, L.: The roles of CO2 and orbital forcing in driving southern hemispheric temperature variations during the last 21 000 yr, J. Clim., 22, 1626-1640, 10.1175/2008jcli2161.1, 2009.

Interactive comment on Clim. Past Discuss., 9, 1869, 2013.