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Interactive Comment

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

Anonymous Referee #3

Received and published: 29 May 2013

The manuscript 'Southern Hemisphere orbital forcing and its effects on CO2 and tropical Pacific climate' by the authors Tachikawa, Timmermann, Vidal, Sonzogni, and Timm is a combined model-paleo data study which investigates the link between Southern Hemisphere (SH) orbital forcing and tropical climate variability via CO2 forcing. Based on results from a 400 kyr long tropical Pacific sediment record and an accompanying climate model simulation the authors propose that insolation-driven changes in Southern Ocean (SO) sea-ice cover were the pacemaker for variations in the sea-air flux of CO2and thereby global (and tropical Pacific) climate. The idea of SH orbital forcing as a pacemaker of glacial-interglacial climate variability is certainly thought provoking and in contrast to common perception. Accordingly the study is of interest to the journal's readership.

With regard to the design of the modeling study it is not clear why a complicated Northern Hemisphere ice sheet model was used instead of a prognostic carbon cycle. The manuscript is concisely written, although it lacks clarity at some points. The results seem robust, however, the main conclusion drawn mostly relies on the temporal agreement between several climate records. Since temporal correlation is an indicator of the strength rather than the cause of a relationship, the actual mechanism proposed still remains unquantifiable and thus speculative, especially because CO_2 in the model simulation is prescribed and not treated as a prognostic variable. I suggest publication after major revisions.

Major comments:

As already mentioned above my main concern is that a causal link from orbital forcing to CO_2 and climate is made only from a temporal agreement between several proxies, while the crucial point (strength of the effect on SO CO_2 outgassing) cannot be quantified. I see that it is tempting to rely on this mechanism, however, there are a number of other key regions for air-sea gas exchange that probably have not acted neutrally and thereby may have contributed to the global CO_2 signal as well (e.g. North Atlantic, upwelling regions). Furthermore, an attempt to quantify the impact of a change in Ekman pumping efficiency on the CO_2 flux in the SO is necessary to estimate the order of magnitude of the expected response.

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 as shown in Fig. S3c. Due to semi-annual insolation forcing the min/max of the peaks may have shifted due precessional changes. I assume that it is actually the neglect of this effect, which induces the alternation of positive and negative seasonal biases (Fig. S6), which

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otherwise would indicate that seasonality would have switched between warm-biased and cold-biased on precessional time scales.

The motivation for the experimental setup of the model simulation needs to be clarified. After reading the comments by reviewers #1 and #2, there seems to be some resentment about its usefulness. As far as I understood the model provides the crucial link to show that the sediment record next to the surface (SST) signal is registering the Antarctic climate signal in its benthic proxies. Since SO processes are proposed as important pacemakers for G/I variability, this connection can only be made here via a climate model. However, I agree with both reviewers that it is not very convincing to explain a complicated setup of NH ice sheet modeling, which (1) does not work very well, and (2) is then explained to be of minor importance. The inclusion of a prognostic carbon cycle would have been much more important for this type of study to get the main conclusion away from pure speculation to some quantifiable measure.

Minor comments:

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

p. 1875, l. 9: what is the definition for 'moderate dissolution'. How do you know it does not affect the Mg/Ca signal?

p. 1876, l. 8: singular: 'desert'

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

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

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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.

p. 1880, l. 2: 'Kohler' should read 'Köhler'. Use 'K\"ohler' in latex. See also in references.

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).

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.

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.

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).'

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.

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