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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.

Anonymous Referee #2

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Tachikawa et al present a data-model investigation of tropical Pacific warm pool climate over the past 400 ky, with a focus on the role of Southern Hemisphere spring insolation through its effects on Southern Ocean sea ice, upwelling, and atmospheric CO2. At the heart of this paper lies a new Mg/Ca sea surface temperature reconstruction from tropical core MD05-2920 off Papua New Guinea. This record is of very high quality and offers the highest resolution yet attained from this region, adding to only two other available records over this time period (ODP806B and MD97-2140). As such this dataset is valuable, but its value is lost in the framework of this paper which involves a modeling component which seems to address a different question: is atmospheric CO2 driven by Southern Ocean processes due to Southern spring rather than Northern summer insolation? Now, to answer this latter question one would not go to the tropical Pacific to reconstruct SST and check if it correlates with CO2, so the connection between the

data and modeling seems disjointed and forced, and ultimately puzzling. In casting the Mg/Ca data in this light the paper misses some good opportunities to focus on tropical Pacific processes, for example ENSO dynamics, east-west gradient, monsoon variability etc.

My feeling is the paper needs to be refocused on the tropical Pacific data and how they inform our understanding of tropical processes. The connection to CO2 is a legitimate one, but whether this is driven from the North or the South and exactly how, is a different question altogether which detracts from the data. I therefore recommend major revisions here. The modeling component, should be shortened and focused on those aspects that are directly relevant to the Mg/Ca data interpretation. Alternatively the modeling part can be removed altogether and written up as a separate paper. I offer additional detailed comments below to help improve the manuscript.

Page 1873, line 25: what exactly is meant by "we mainly used data concerning the s.s. morphotype"? Clarify.

Page 1875, line 5: "The test weight varies from 8.2 to 14.5 μg for G. ruber…". This is the mean G. ruber weight at each stratigraphic level, correct? It is not the individual test weight – did you weigh individual tests? If correct you should change to "The average test weight at each stratigraphic level varies from 8.2 to 14.5 μg…"

Page 1878, line 14 to page 1879 line 2: The model gets only half the amplitude of the reconstructed warm pool SST. This is rationalized in the context of PMIP multi-model uncertainties and the statement that "the warm pool heat budget is delicately balanced by rather large individual contributions…". This may be so but it is not a very satisfying explanation here. What is needed is some sense for why this particular model has low tropical SST sensitivity and how this might affect the interpretations. See also next comment.

Page 1879, lines 7-12: Here the residual Mg/Ca SSTs (after removing the CO2 component, which itself is a questionable thing to do since it depends on a statistical correlation) is compared with the model strength of the wind-driven current off Papua New Guinea, and a high correlation of 0.88 is reported. If this mechanism is correct it should also hold in the model. Residual SST in the model and current strength in the model should correlate. Do they? If yes, fine; if not, doesn't that undermine the argument?

Page 1879, lines 22-29: I have trouble understanding what was done here. Consider rewriting.

Page 1880, line 1: change “primary drivers” to “potentially primary drivers”

Page 1880, line 3: change “understandable” to “not surprising”

Page 1881, line 7: insert “arguably” before “reflects”

Page 1882 line 21, to page 1883 line 6: These sections articulate the main crux of the hypothesis in this paper, namely that southern hemisphere spring insolation (or summer duration per Huybers and Denton, 2008) controls sea ice, Ekman pumping, and atmospheric CO2. While the attraction of this mechanism is that it provides a more direct local forcing (compared to 65N insolation) it fares no better in its explanatory power. There are prominent examples within the climate record of the last 400 ky, where climate/CO2 has failed to respond to orbital forcing (Northern or Southern), and even more spectacular examples when climate/CO2 changed wildly in the absence of orbital forcing. For example there are no detectable signals in CO2 (or Antarctic temperature) during the large insolation peaks at 150 and 175 ky BP (see Figure 6). These peaks were greater than the one that “caused” the last termination, yet there was no response.

More importantly there is the stage 12/11/10 paradox 350-450 ky ago, when insolation forcing was very weak due to very low eccentricity (see for example Figure 7), yet CO2 rose from full glacial (stage 12) to full interglacial (stage 11) values and dropped back to full glacial values (stage 10). Although the stage 11/12 transition is just outside the scope of the last 400 ky in this paper, the stage 10/11 transition occurred between

$\sim$400 and 350 ky ago. Why did CO2 drop at this time when southern insolation was so weak? This should be acknowledged and addressed.

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