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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 #1

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In this manuscript, Tachikawa et al. are presenting an interesting record of Western Pacific Equatorial SST (MD05-2920) based of Mg/Ca measurements. But the associated discussion is quite esoteric : the authors are using a model that couples climate and the Northern Hemisphere ice sheets (without any carbon cycle modeling) and they finally conclude on the Southern hemisphere and the causes of CO2 changes. To say the least, this is not very convincing. At each step, the connection with the (very good) original data set gets less and less clear, and the amount of approximate reasoning grows (this curve looks similar to this one, therefore...). The choice of the model setup is extremely strange: why simulating ice sheets, if finally it does not work very well (see Fig.S4) and if ice sheets do not matter anyway for the Southern Hemisphere? Why not using the carbon cycle part of the very same model (Menviel et al. 2011) in order to discuss carbon? What about the results that appear quantitatively very different in

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the data (amplitude 4°C) and in the model (amplitude 1.7°C)? To summarize, I do not recommend publication of this manuscript, at least in its present form. In particular, I have the feeling that the modeling part and the data part are not really connected, and the paper looks like the attaching of two not so well related contributions.

Main concerns

I have difficulties to see the logic in this paper. The new data is on equatorial SST, the main discussion and conclusion is about Southern Ocean sea ice and CO2. This seems awkward to say the least. Furthermore, the argumentation based on the model results is not convincing at all. For instance, there is an esoteric discussion on wind stress and Ekman pumping, with the introduction of many mathematical symbols, but at the end we only have the statement: "As shown in Fig. 6c, over the last 350 kyr the eïňČcacy of Ekman pumping calculated from TR400 varied by a factor of 2.5, in unison with austral spring insolation changes". So what ? There is something in the model that does change in accordance to the forcing (insolation) ? Since this particular number is computed from sea-ice fraction, itself a result of energy blance in this area, the result is certainly not a surprise. But what about Ekman pumping, instead of Ekman pumping efficacy ? Dit it vary by a factor of 2.5? But what about Southern Ocean stratification ? What about carbon? The connection discussed in the paper is pure speculation, and the fact that the carbon cycle part of the model was not used here, is more than significant.

p.1881- l.15: "From the close match between..., our results support that...". Since all the curves on 4 climatic cycles are (strongly) correlated, the notion of a "close match" may not be sufficient. Quite likely, almost anything with glacial-interglacial cyclicity will look "similar" to anything else with the same cycles. It is impossible to draw any mechanistic information from such "correlations".

p.1877, l.24: "The eïňĂect of Northern Hemisphere ice-sheets on Southern Hemisphere climate is quite limited in LOVECLIM and hence we expect biases of North-

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ern Hemisphere ice-sheet evolution to play only a small role for our assessment of Southern Hemisphere climate change and its response to CO2 variations and Southern Hemisphere orbital forcing". So what is the point of using a Northern hemisphere ice sheet model, if it is useless for the conclusions ? This is particularly perturbing, since the asynchronous coupling procedure is quite cumbersome and difficult to setup. But, at the end, it appears to behave poorly (See Fig.S4) and according to the authors, it was not important... This certainly does not help the reader to see the main point of the paper.

One of the most interesting result is the difference in the amplitude of the data SST ($\sim$ 4°C) and in the model SST ($\sim$ 1.7°C). Apparently, in the PMIP exercise, models tend to significantly underestimate climate sentivity in this area. I would have liked much more discussions on this point. Are the models systematically wrong ? Is the data reliable ?

"Salinity units": Salinity is unitless. It is a ratio and, as such, should be expressed in ‰ or ppt. Quoting Frank Millero ("What is PSU?" Oceanography, vol6, 1993, p67): "The use of the term PSU should not be permitted in the field and certainly not used in published papers".

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