

Interactive comment on “SST phases in the open-ocean and margins of the tropical Pacific; implication on tropical climate dynamics” by L.-J. Shiau et al.

Anonymous Referee #1

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Shiau et al. present new high resolution alkenone unsaturation-based estimates of past sea surface temperature (SST) variability off SE Papua New Guinea spanning the last 320,000 years. Together with five previously published SST records, this new data set is used to assess the phase of tropical Pacific SST variability with respect to orbital forcing, ice volume and greenhouse gases (GHGs). The authors find that tropical Pacific SSTs are not simply linearly related to either of these parameters. Based on the varied phasing of the different regional SST records, the authors deduce a sequence of events during global deglaciations: Higher tropical SSTs resulting from decreased latitudinal insolation gradients and increasing low latitude insolation are posited to cause increased CH₄ production in tropical wetlands due to increased moisture supply. This,

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in turn, the authors postulate to promote increasing CO₂ and deglaciation, which would then raise continental and continental margin temperatures.

Without direct access to the data sets, it is impossible to rigorously evaluate the phasing relationships presented here, so I will trust the demonstrated expertise of the senior authors on this aspect. Overall, however, I find the sequence of events summarized above somewhat farfetched and hard to follow. More gravely, my main concerns revolve around the compatibility of the data sets compared by the authors in the first place. For one, the authors inter-compare SST estimates based on foraminiferal Mg/Ca and alkenone paleothermometry. Contrary to their cavalier dismissing of any possible bias by this approach, the literature provides many examples from the tropical Pacific where these two proxies lead to fundamentally different SST reconstructions (in terms of both absolute SSTs and timing of change), including some of the very records/sites analyzed here (for example de Garidel-Thoron et al. 2007; see also Mix 2006, Steinke et al. 2008, etc.). In fact, the authors do not even apply the same UK37 calibration to the alkenone records they discuss (non-linear Conte equation for their own new record versus linear calibration of the previously published records). While the latter will not necessarily affect the phasing per se, it certainly aliases the direct inter-comparison of the SST records.

Second, the authors rely on benthic d₁₈O stratigraphy for some of the records, and planktonic d₁₈O for others, with the tacit assumption that this will not affect their interpretation. A substantial phase offset between benthic and planktonic d₁₈O, however, is indeed illustrated by the authors' own data in Fig. 2, which shows a significant lead of planktonic over benthic d₁₈O across termination II. This has also been observed at many other sites throughout the tropical Pacific.

Finally, the authors need to spend more time discussing, and contrasting their interpretation with previous studies investigating orbital controls on tropical Pacific sea surface temperatures, and surface ocean conditions in general, as well as the distinction between “continental” and open ocean sites, including Lea et al. 2000, Beaufort et

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al. 2001, Rafter&Charles 2013, Pena et al., 2008, and Kiefer&Kienast 2005, respectively, and many others. In addition, I wonder why the authors chose not to include all available tropical Pacific SST records covering the time period investigated here. For instance, the Mg/Ca records of sites TR163-19 and ODP1240 are missing, for which both Lea (2004) and Pena et al. (2008), respectively, even provide spectral analysis in their papers.

In summary, I can see a thoroughly revised version of this contribution published in *Climate of the Past*. For this, the authors need to address the concerns raised above in great detail, and tighten up the somewhat lengthy presentation of the diverse phasing of tropical Pacific SST records.

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