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Climate of the Past Discussions



# *Interactive comment on* "Lessons from a high $CO_2$ world: an ocean view from $\sim 3$ million years ago" *by* Erin L. McClymont et al.

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This paper presents a very significant compilation of SST for a time window in the late Pliocene based on two of the most validated SST proxies. In comparison to previous reconstructions for the "PRISM interval", it benefits from greatly improved stratigraphic control, thus minimizing spurious variance or patterns from chronological dispersion. A major concern I had as a reviewer is that, given the numerous data-model comparisons of the PRISM/PlioMIP consortia, what would be distinctive? This is answered successfully on page 10: "The overall UK 37'-model agreement for the North Atlantic Ocean suggests that, as proposed by Haywood et al. (2013), a focus on a specific interglacial within the mid- Piacenzian provides an improved comparison to the climate being simulated by the PlioMIP2 models. Thus, some of the data-model mismatch in previous mid-Piacenzian syntheses (e.g. Dowsett et al., 2012) may have been due to the averaging of warm peaks which may not have been synchronous in time between sites and/or with the interval being modelled" The prime contention is that the Pliocene data imply that: “Even under low CO2 emission scenarios, our results demonstrate that surface ocean warming may be expected to exceed model projections, and will be accentuated in the higher latitudes. “ I think the paleo-sensitivity field is likely to oscillate for some time between the view that paleo-SST/CO2 constraints confirm the Charney sensitivity of 2.4-3.5oC for a CO2 doubling versus the view (implied here) that the deep time/Earth System sensitivity is considerably higher. Jessica Tierney argues that the current models are right based on her LGM reconstructions; I'm not so sure. This paper certainly favors a higher Earth System sensitivity, without an explicit numerical figure. I find myself a bit embarrassed that I do not have more critical comments to the manuscript. It is very clearly organized and written, and anticipates most objections along the way. Ambiguities are clearly recognized, for example in paleo-CO2 estimates. Here the authors handle nuances such as likely insensitivity of alkenone CO2 proxy with good judgement. The paper also includes multiple assessments of uncertainties, including geographic bias, the effects of including/excluding Mg/Ca. Examples include: increasing # of sites in the high latitude band to check on the implied reduction of meridional SST gradients, and recognizing the data bias to the Atlantic. The major uncertainty seems to be whether to use alkenone BAYSPLINE, which enhances the global warming and increases the meridional SST gradient Specific text comments: I suggest changing “a ∼100 kyr window of relatively low benthic âĂĆ18O values” to “a ∼100 kyr window of relatively depleted benthic $\delta$18O values” to avoid the possible ambiguity of what “low” means to the reader. P 7: “Thus, there is a broad, but complex, pattern of enhanced warming at the mid- and high-latitudes, reflecting a combination of regional influences on circulation patterns, and to some extent, proxy choice. This pattern is not explained by temporal variability nor sample density within the KM5c time interval: regardless of sample number per site, the standard deviation is <1.5oC (Figure S4). “ In light of the “complex pattern” and “temporal variability”, can the authors clarify what the standard deviation" refers to? I assume it's the variation at a given site within the KM5c time bin? So I suggest adding "the standard deviation at any site within the time bin is < 1.5oC" P. 7: "Mg/Ca-SST anomalies are generally lower than for UK 37', ". Since the authors have been pointing out differences in the interpretation of Uk'37 anomalies between using the Muller linear regression and the BAYSPLINE calibration, can they clarify whether the Mg/Ca anomalies are lower than BOTH calibrations or specifically the BAYSPLINE one or both? From context, given that 8 sites gave a negative KM5c anomaly (!!) It would seem that Mg/Ca deviates from both alkenone calibrations. p. 8: "KM5c is characterised by a surface ocean which is ∼2.3°C (alkenones and Mg/ Ca) or ∼3.2°C (alkenones-only) warmer than 240 pre-industrial, with a ∼2.6°C reduction in the meridional SST gradient. " Which alkenone calibration was adopted for the "alkenones-only" estimate? What would be the difference of Muller vs BAYSPLINE? p. 11: I think there's something funny about the NOAA-ERSST temperatures for the region. We have unpublished alkenone SST estimates from Site 1085 for the KM5c interval that show an anomaly of ∼3 degrees when using the WOAA. My sense is that the large Benguela anomalies arise entirely from using the NOAA-ERSST atlas and that they would fall in line with expected values if other atlases were used. The authors should at a minimum consult other atlases and explore the possibility of a regional SST bias in the NOAA-ERSST estimates. I think this is a much more parsimonious explanation than the oceanographic ones proposed in lines 340-352. This in fact is my major suggestion: to examine whether that data base imposes a significant bias to the results here. In summary, I recommend publication with minor revisions (excepting my last point). This is a very nice piece of work.

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