

Interactive comment on “Alkenone-based temperature patterns along the eastern South Pacific Coastal Ocean: the effect of upwelling and advection on the sedimentary alkenone unsaturation-index ($\frac{K'}{37}$)” by J. A. Placencia et al.

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The manuscript, “Alkenone-based temperature patterns along the eastern South Pacific Coastal Ocean: the effect of upwelling and advection on the sedimentary alkenone unsaturation-index (Uk'37)” addresses an important issue regarding the relevance of the Uk'37 alkenone index and its temperature dependence in coastal marine regions, particularly at the Chilean/Peruvian margin. This is an important region for reconstructing sea surface temperature, upwelling and nutrient conditions, as changes that initiate here propagate to many other parts of the climate system. However, the treatment and

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analysis of the data in this manuscript does not adequately support the conclusions that are drawn. For instance, relevant previous literature is not thoroughly cited/discussed and only one possible explanation for an offset of Uk'37 values from the universal calibration is thoroughly analyzed, i.e., nutrients, when other controls are largely ignored: different producing organisms, alkenone signals advected in from different regions, and seasonal offsets.

Specific comments include:

-The uppermost centimeter of sediments from multiples sites were used for this analysis, which do not necessarily represent modern ocean inputs or conditions. The authors suggest that as long as the sediments represent Holocene values, then they are suitable for analysis; however, they provide no justification for the variability in parameters such SST, nutrient values, or upwelling that would suggest that these remained relatively constant over the Holocene.

-It is unclear why the authors favor the Prah et al. 1988 calibration rather than the latest universal calibration (i.e., Conte et al. *Geochem., Geophys., Geosys.*, 2006).

-Changes seen in the relationship between Uk'37 and temperature in coastal regions is preemptively treated as a nutrient bias in this manuscript, whereas, there are many other possible causes of this result. In particular, marginal ocean environments are complex. In addition to nutrients, these environments, like lakes, have been shown to host a number of different haptophyte species (i.e., *I. galbana*, *C. lamellosa*; see Marlowe et al. *British Phycological Journal*, 1984) – each having a unique relationship to temperature. An assessment of the potential alkenone-producer is possible through somewhat complex techniques such as sedimentary DNA (see Theroux et al. *EPSL*, 2010); however, different producers could be addressed with the data at hand through analysis of the C37:C38 ratio (see Pearson et al. *GCA*, 2008). Overall, there is a lack of analysis with respect to other alkenone parameters, i.e., alkenoates, Uk37, Uk38, etc. that could help in understanding the controls on the Uk'37.

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-In addition to the above issues, the authors do not adequately show support for disregarding the hypothesis that a different alkenone signal is advected in from a different production region. Nor is there a sufficient discussion of what, how or when export to the sediment occurs in this region. Water column filtration and sediment traps would greatly aide in deciphering many of the outstanding questions.

-The negative residuals in Fig. 2 likely result from using an incorrect calibration – or potentially low concentrations of alkenones(?). What are the concentrations? What are the detection limits?

-Fig 3. is very busy and it is difficult to glean the relationships that the authors are trying to put forth.

-The results section should present clear-cut facts observed from the data, yet there are a number of speculations.

-The supplementary information should contain more information on the alkenone distributions, including: concentrations, the C38 alkenone abundance, etc.

Interactive comment on Biogeosciences Discuss., 7, 545, 2010.