

Author's response to the interactive comment by J. L. Toney.

We acknowledge the positive and constructive comments made by J. L. Toney, and we expect to be able to answer all aspects of her suggestions.

Specific comments include:

1) The uppermost centimetre 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.

Re: It is true, but now we explain more carefully the features of core site used in this study.

...Several published records along the Peru-Chile margin, have described that the sedimentation rates of near-surface sediments range around between 0.04 and >0.20 cm yr⁻¹, increasing successively southward off Chile (e.g., Hebbeln et al., 2001; Muñoz et al., 2004). This latitudinal pattern responses to different climate/oceanographic regimes, the resulting terrigenous/marine inputs, topography, winnowing, and also bioturbation effects (mixed layer thickness); factors that strongly determine if the near-surface sediments represent over the past hundred (e.g., sediments off Peru and northern Chile) or few thousand years because of winnowing and/or bioturbation effects (e.g., sediments off central Chile) (e.g., Hebbeln et al., 2001; Muñoz et al., 2004). Although surface sediments may cover the last several hundred years of deposition, they are all of Holocene age, and thus can be considered suitable for the objectives of this study.

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

Re: It is true. Now we included more discussion about Conte's calibration.

3) 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.

Re: Yes, totally agree. How I said before, we can to incorporate in the new version the K37/K38 ratio, to get a more complete interpretation of the sedimentary record offshore Peru-Chile margin.

4) In addition to the above issues, the authors do not adequately show support for discarding 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 aid in deciphering many of the outstanding questions.

Re: We agree, and the discussion includes other mechanisms, such as advection, etc.

5) 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?

Re: Although, nowadays I have not the specific laboratory information, we did extract enough amount of alkenones to estimate correctly UK'37-temperatures. In fact, all concentration data was corrected with an internal standard (C35 straight chain ketone), which ranged between 80

and 90%. On the other hand, alkenone concentrations, di- and tri-unsaturated C37 methyl, C38 methyl and ethyl, and C39 ethyl ketone constituents ranged from ~0.1 to 0.96 µg/g dry sediment.

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

Re: Fig 3 was eliminated.

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

Re: We agree, and in the new version there are not more speculations.

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

Re: We agree, but unfortunately I don't have the complete dataset (C37, C38, C39 concentrations) because only 39 sediment samples (n = 83) were analyzed by my old laboratory group 5 years ago! And the remained samples were obtained from literature. However, I can to include a reduced amount of alkenone abundance (K37, K38, ME) data and complement with the published information (Prahl et al., 2006) to improve our discussion.

References:

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