

Interactive comment on “The response of the Peruvian Upwelling Ecosystem to centennial-scale global change during the last two millennia” by R. Salvattecí et al.

Anonymous Referee #1

Received and published: 12 December 2013

General comments

The authors have produced a robust paleoclimate study that contributes to our knowledge of Eastern Tropical Pacific (ETP) climate variability during the last 2,000 years. This is truly a multi-proxy study of the type needed to unravel the changing water column and sedimentary dynamics that can complicate interpretation of records underlying the Peruvian Upwelling Ecosystem (PUE). This manuscript is exceptionally thorough and detailed in terms of site/core description, methods, and proxy interpretation. The authors have certainly provided readers with all of the information they need to assess their interpretations. The authors have also provided a comprehensive and well-written introduction that will serve as a valuable resource to readers.

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This work represents a valuable addition to the research published on ETP climate changes and the PUE. Its findings are important and consistent with established records and mechanisms of regional or Pacific climate change (e.g., Cascayunga Cave and the Ocean Thermostat Hypothesis). The data derive from a region that is very sensitive to large-scale climatic changes (ENSO), but from which high-quality and continuous records are rare. I recommend the manuscript for publication after consideration of the comments below.

In general I found that the interpretations in section 5.2 were rather far reaching considering the difficult nature of developing the time scale and interpreting each proxy. For example, parts of the DACP and MCA are missing, limiting general statements about the uniform behavior of climate during warm versus cold phases. As the authors point out, there are also discrepancies between the export production proxies, and I mention some potential problems in interpreting TOC below. This being said, everything is presented in a logical framework, the proxies are directly comparable, and the reader has all the information needed to judge the strength of the hypotheses.

Specific comments

I do not fully agree with the generic interpretation of TOC as an export production proxy. TOC percentage is necessarily related to export production, redox conditions, and the delivery of terrestrial organic carbon. It may also be biased by selective winnowing from ocean currents. The table in SM7 demonstrates that within reason, TOC is as closely correlated with V and Re (redox indicators) as it is other export proxies such as Ni, Cu, and Cd. Furthermore, the elevated TOC% during the CWP may simply be a function of enhanced preservation in recently deposited sediments. I thus encourage the authors to be more tentative in their treatment of TOC as an export proxy. For example, interpretations of export production in Fig. 4 are based partly on TOC, and it may not be a valid proxy in this sense.

The interpretations in terms of ITCZ variability may be a bit tenuous. Is the ITCZ clearly

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defined on the Peruvian coast, or is precipitation variability tied more closely to ENSO events? The Pisco record is in anti-phase with the Haug et al. (2001) record during the MCA, as would be expected for a straightforward ITCZ relationship. But then why are these records in phase during the early part of the record? More discussion is required to explain this change in character and the implications for the ITCZ.

Are the RWP and DACP well developed in other parts of the tropics, or is this a novel record of their expression?

The authors cite erosion by ocean currents as a potential problem early in the manuscript (bottom of p. 5487) but then do not discuss it later. Could bottom currents have caused bulk or selective sediment redistribution, for example between the marine and terrestrial proxies in the authors' records?

Are there geochemical results from the slumped sections that were removed from the stratigraphic sequence? If so, are the data reasonably homogenous?

On p. 5498 (lines 7-8), the authors suggest possible control of export production by changes in OMZ intensity (paraphrased). How would this work? Wouldn't the OMZ normally respond to changes in organic carbon export? Could something else be contributing to changes in the OMZ, perhaps involving changing current regimes? In any case, more explanation is needed on this point.

The strong correlations between the Pisco and Cascayunga Cave records are very interesting. Perhaps the authors could further interpret their records specifically in relation to the findings of Reuter et al. (2009).

Technical corrections

The quality of writing is generally very good, but there remain some minor language errors. For example, on p. 5488 the end of line 2 should read, "...may erroneously lead to inference of abrupt climate changes," and the end of line 24 should read, "inducing a La Niña. . ." I haven't pointed out each case, but I recommend that the authors make

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another fine-scale review of the language.

I think p. 5488, line 18 should read, "the warm Western Pacific and cold. . ."

On at least one of the records figures (maybe Fig. 2), it would be nice to see the stratigraphic positions of the radiocarbon dates, and the slumped and laminated sections.

Interactive comment on *Clim. Past Discuss.*, 9, 5479, 2013.

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