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

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Dear Dr Beaufort

We have carefully considered all comments and suggestions raised by both reviewers. In the following pages you will find an itemized description of how these comments were treated in our corrections to the manuscript. In this revised version Figure 1 was redone in order to better understand the precipitation variability over South America and the ITCZ displacements. We hope you will find that the issues have been properly addressed and the manuscript can now be accepted for publication in Climate of the Past.

Sincerely

Renato Salvatteci

Anonymous Referee #1

General comments

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.

Reply: In this version of the manuscript we took into account the general comments of the anonymous referee #1 and the interpretations in section 5.2 are now more cautious.

Specific comments

1. 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.

Reply: In the revised manuscript we took into account the reviewer's comment. Interpretations of export production are now based on the standardized average of Ni and Cu (Fig. 4). These two metals, as explained in the text, reflect the original presence of organic matter even if it is partially lost after deposition (Tribovillard et al., 2006).

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

Reply: At interannual timescales, precipitation variability on the northern Peruvian coast is associated with ENSO events, but only very extremes El Niño events (e.g. 1998) can trigger precipitation and floods further south. Precipitation variability at seasonal timescales is associated with meridional displacements of the ITCZ and the SPSH. A weakening of the SPSH and the southward displacement of the ITCZ during austral summer induce the development of a rainy season (Garreaud et al. 2009, Lavado et al. 2012). In the revised manuscript we further discuss our terrigenous input record and other continental records (Reuter et al., 2009; Bird et al., 2011) with regards to the ITCZ displacements and SPSH expansion/contraction. The Pisco record is in anti-phase with the Haug et al. (2001) record from the MCA towards the present as expected for a straightforward ITCZ relationship, but there is an apparent in phase relationship from 300 to 800 AD. In this period the magnitude of precipitation change is higher compared to the precipitation changes during the LIA, but the timing and direction of the precipitation changes are associated with ITCZ displacements. The Haug et al. (2001) record shows a decreasing trend from ∼400 to∼750 AD indicating a southward migration of the ITCZ while the Pisco record starts an increasing trend in the same time interval.

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

Reply: To our knowledge the present manuscript presents the most reliable oceanic record for the last 2,000 years in the Eastern Tropical Pacific (ETP). There are very few studies showing the precipitation changes in the ETP to the RWP and DACP such as for example, the grain size record of El Junco Lake in Galapagos (Conroy et al., 2008). This record suggests large changes in precipitation during the last 2 millennia. Increased precipitation is inferred in Galapagos during part of the DACP and the LIA, and lower precipitation during the RWP, part of the DACP and the MCA. These results are in part coherent with the results presented in our manuscript.

4. 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?

Reply: The intensification of the Poleward undercurrent has been made responsible for hiatuses in the sediment record at longer timescales (Reinhardt et al. 2002), but little is known about the effects of this current on sedimentation processes at shorter timescales. It is known that strong bottom currents can cause selective sediment redistribution, if currents are stronger, there would be a higher chance to re-suspend light-density organic matter, and therefore to leave behind high density components (i.e. detritics). A more thorough analysis should be done to understand the effect of change in this current like a ratio between a heavy and a light element or changes in grain size. However the strong correlation of the terrestrial proxies (Al and Ti) with δ15N (which should not be affected by selective sediment redistribution) suggests that the bottom currents didn't cause large changes in sediment redistribution and that the proxies are truly reflecting biogeochemical changes.

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

Reply: We developed on the slumped sections of core B-14 the same suite of proxies that are shown in the manuscript. The homogeneous sections show lower variability in comparison to the contiguous laminated sequences. Additionally we develop higher resolution records on several cores as for example gray level measurements and XRF measurements. The results of these analyses effectively indicate that the slumped/homogeneous sections show lower variability than the laminated sections of the core.

6. 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.

Reply: A vertical expansion of the OMZ produced by changes in ocean circulation (i.e. reduced ventilation) or global warming (Stramma et al., 2008) could reduce the vertical oxygenated habitat for some organisms. This condition is adverse for a large group of macro organisms that cannot survive or avoid hypoxic zones. Then, a shallower oxycline could reduce the habitat for several pelagic organisms, diminishing their abundance, and modifying the flux of organic matter to the sediments. In the geologic past, reductions of ocean oxygen were responsible for massive extinctions (e.g. Wignall and Twitchett, 1995). In the corrected version of the manuscript we removed this idea that is beyond the scope of our work.

7. 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).

Reply: In the revised manuscript we discuss in more detail our findings with those obtained from Cascayunga Cave (Reuter et al., 2009) and Pumacocha Lake (Bird et al., 2011).

Technical corrections

The minor language errors were corrected.

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

Reply: Figure 2 was modified according to the reviewer's suggestions

Anonymous Referee #2

General comments

In general, I shall suggest the authors to develop more some parts of the article, and reduce very much other parts. The paper, in general, is not easy to read because it is very lengthy. There are many sentences that could be removed/cut as well as too many repetitions that distract the reader from the main message. I urge the authors to get a critical read over the paper and remove everything obvious or repetitive and try to write the paper in a much more straightforward style. On the other hand, all the methods are in a lengthy supplement and key informations must be described - even rapidly - in the article.

Reply: In the revised manuscript we now explain very briefly the methodological steps that were only in supplementary material in the original version. We also discuss the precipitation record in more detail. Additionally we have removed all repetitive parts rendering the manuscript more concise and easier to read.

Specific comments

1. You may avoid in some cases assigning oceanic features you describe to ENSO and/or ITCZ, or at least clarify the use of these terms rapidly and keep only one generic term, as it is impossible to disentangle whether changes in oceanic features are due to changes in the seasonal ITCZ or inter annual alteration of the Walker circulation.

Reply: In the revised manuscript we compare the ITCZ displacements and SPSH expansion/contraction with changes in precipitation in our record. El Niño/ La Niña-like conditions is used to define the persistent mean state of the PUE during the contrasting NH periods only in terms of productivity and subsurface oxygenation.

2. In the same vein, try to not use the terms "cold/warm periods" while you assign productivity changes associated with the LIA/MCA periods. The temperature pattern during those periods was not uniform, and your way of using those periods is sometimes awkward (e.g. page 5500 paragraph 5.2 "Our results show that during the cold periods (DACP and LIA), the PUE exhibits El Niño-like conditions with low export production". If it's El Niño-like I anticipate the LIA and DACP to be warm periods in the Peruvian upwelling!)

Reply: In the revised manuscript the terms "cold period" and "warm period" were replaced by Northern Hemisphere (NH) cold period and NH warm period. With this correction we hope to avoid misunderstandings.

3. Please clarify the use of the precipitation proxies. Unlike everything else, you get rid of it in paragraph 3.2.1. using an obscure publication and your description of the "rationale" is not understandable.

Reply: In the revised manuscript we use the precipitation proxies to assess if precipitation over the continent is more associated with changes in the ITCZ meridional displacement and SPSH expansion/contraction than changes driven by strong zonal (El Niño-like) shift of precipitation (Gutierrez et al., 2009). The rationale is now better explained in section 3.2.1.

4. Putting upside down the precipitation proxies in Figure 2 would help the eye to visualize synchronous changes in the proxies.

Reply: In the revised manuscript the precipitation proxies in Figure 2 are now plotted according to the reviewer's suggestion.

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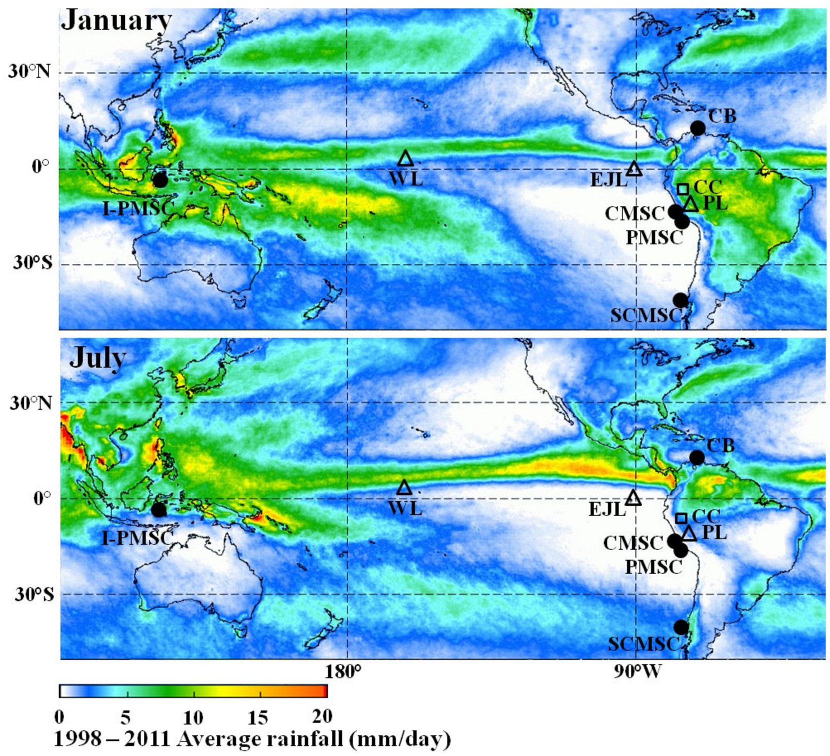

Fig. 1. Seasonal changes in mean precipitation (1998-2011) and locations of the records discussed in the study