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Biogeosciences Discussions

Interactive comment on “Rapid reorganization in ocean biogeochemistry off Peru towards the end of the Little Ice Age” *by* D. Gutiérrez et al.

Anonymous Referee #3

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The ms submitted by a group of authors led by D. Gutiérrez presents a variety of sedimentological and biogeochemical proxies analyzed in two high resolution sediment cores covering the last ˜700 years off Peru. Based on a shift in the mass accumulation rates of virtually all of these proxies at around 1820 AD, the authors draw two main conclusions: First, they suggest that prior to the shift (i.e. during the Little Ice Age), the ITCZ and the South Pacific High pressure cell were located further south. Second, they infer that the rapid northward migration of these climate systems to their present positions caused an increase in coastal upwelling and primary as well as secondary production, which, in turn, led to lower oxygen concentrations in the water column and at the sediment water interface off Peru. The ms is interesting and timely. It is based on a multiproxy approach, and will, eventually, make a fine contribution to the journal.

In its present form, however, I have several major concerns:

Major Concerns

1) Parallel publications: A substantial part of the data and interpretation has already been published or is currently in press elsewhere. For example, density and grey level data are given in Gutiérrez et al. (2006), and Mo as well as TOC data are presented in Sifeddine et al. 2008 (in press, Prog. in Oceanography). In the latter paper, a group of authors similar to this one concludes that the ITCZ and the Subtropical High Pressure Cell were located further south during the Little Ice Age, causing increased terrestrial runoff and reduced coastal upwelling off Peru. Gutiérrez et al. seem to arrive at these conclusions yet again in the present manuscript. The present ms shows new d15N, Cd, foraminifera, diatom and fish scale data, which, in my view, is plenty of material for a separate publication by Gutiérrez et al. However, the new and the previously published data and conclusions must be distinguished much more clearly. Rather than weakening the present ms, this change would declutter the paper and allow the authors to discuss their own findings (water column chemistry and marine production) with much more clarity.

2) Age models: The age model is derived (and presented) in terms of accumulated mass (g/cm2) versus time (Fig.2, and supplementary material). To me, this seems to be an unusual choice as core chronologies are generally presented in terms of core depth (cm) versus time. This more common way of presenting core chronology would immediately reveal the large increase in mass accumulation rates observed (but not explicitly shown) in the two cores. This is critical, because mass accumulation rates (also referred to as fluxes by the authors) of any environmental parameter (MARi) are calculated simply as the product of bulk sediment accumulation and element concentration (MARi = MARbulk * %i). The increase MARs of many proxies (Fig. 3) could theoretically be driven by an increase in MAR bulk alone. It is not entirely transparent why the authors chose mass accumulation rate rather than depth as the master variable in their core chronologies. In the supplementary information, the authors briefly

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mention compaction artifacts, and the previous paper by D. Gutiérrez et al. (2006) states that the core(s) suffered from drying and shrinkage. These obstacles, and the way they were overcome, need to be clarified, at least in the supplementary material. The main section should then briefly discuss why the authors consider the increases in fluxes to be real rather than an artifact of the MAR accumulation. Normalization of Cd and Mo to Aluminum (see Sifeddine et al. 2008) and of Bolivina seminuda to total benthic foraminifera, would also be very useful in this context.

A table clearly listing all chronological tie point versus core depth (corrected for instantaneous deposits) and accumulated mass should be included. This table should also categorize how each tie point was derived (241Am, 210Pb, 14C, or stratigraphic anomalies.) I would also suggest that Figure SF3 include 210Pb and 14C dates so that the offset between 210Pb ages and conventional 14C ages (total R) is immediately visible.

The authors derive local 14C reservoir ages at the two cores sites (188 +/- 79 yrs at Pisco and 279+/- 53 yrs at Callo). How do these derived estimates compare with actual measurements of local reservoir ages in the area? The marine reservoir correction database maintained by Stuiver, Reimer and Reimer (http://intcal.qub.ac.uk/marine/) might be helpful in this context. Given the uncertainties associated the reservoir ages correction alone (53-79 yrs) how can the regime shift at 1820 yrs AD be dated with an uncertainty of only +/- 6 yrs ? (p.4 supplementary material).

3) Interpretation of d15N: The entire 2 permill increase in nitrogen isotopes (d15N) across the sedimentological shift (1820 AD) is attributed to an increase in water column denitrification off Peru. The magnitude of this increase is then compared to the observed d15N changes associated with Pleistocene Dansgaard Oeschger cycles. I don't fully agree with the authors here for two reasons. First, part of the lower d15N signal prior to 1820 AD (i.e. during the Little Ice Age) could also be explained by the much higher input of terrestrial nitrogen, which is isotopically lighter. This potential bias needs to be discussed more explicitly, which can be done since the ms contains a lot of

information on terrestrial input (Lithics, palynofacies, d13C of organic matter). Second, the analogy with Dansgaard-Oeschger (DO) oscillations might not be entirely useful. DO cycles during the last ice age were associated with large temperature changes (on the order of 10 degree C over Greenland) and were most likely related to significant instabilities of the global meridional overturning circulation. These boundary conditions do not apply to the last 700 years studied in this ms. I think the analogy to DO cycles should thus be used much more carefully.

4) Overall organization: The main body of the paper and the supplementary material would benefit from better organization. For example nitrogen isotopes are introduced in line 2 (page 3926) but only interpreted after another set of proxies has been introduced (line 10-15, page 3926). After a discussion on the possible climatic driver of the observed shift, (chapter 3.3), new data is introduced in chapter 3.4. I suggest that the authors only present new data not previously published, and discuss this reduced amount of data more clearly.

Minor comments: Line 24, p. 3922: What is meant by sub sampling was performed following the stratigraphy ? Do the authors mean that they used a preliminary stratigraphy to choose the depths for this study ?

Line 17, p. 3926: Were benthic and planktonic foraminifera counted ?

Line 27, p. 3928: The evidence of ENSO during the Little Ice age is less unequivocal than implied by the authors (see Agnihorti et al. 2007, Cobb et al. 2003, Gregis and Fowler 2006, D' Arrigo et al. 2005)

Supplementary Material: "Therefore radiocarbon values from this group were not taken into account". This does not appear to be true for the oldest 14C date in the Pisco core, which seems to have been included in the age model despite high dispersed organic matter contents at that depth.

There are some typos throughout the ms, e.g.

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Line 25, p. 3922: Morales et al. 2006 is in the reference list, not 2007.

Line 7, p. 3926: De Pol-Holz et al. 2006 is in the reference list, not De Pol et al. 2007

Supplementary Material: the second group of organic carbon, not or

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