Biogeosciences Discuss., 5, S2278–S2281, 2008
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Biogeosciences Discussions

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Interactive Comment

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 #2

Received and published: 20 November 2008

General comments

This paper argues that a biogeochemical 'regime shift' occurred during the early 19th century in the Peruvian upwelling system, based on an analysis of two marine sediment cores. The authors use a number of proxy measurements to develop this argument, some of which were previously published elsewhere, and some of which are new. The authors go on to speculate that the observed regime shift was linked to a change in atmospheric circulation, with numerous teleconnections throughout the tropics.

I think this is an interesting paper, which is generally well-written, and deserves to be published. However, I feel that the authors need to better acknowledge the weaknesses in their arguments, and present their data in a more transparent fashion. Specifically, my two primary concerns relate to the age models, and the presentation of downcore records in terms of accumulation rates (fluxes). Resolving these will require that Figures 2 and 6 be redrafted, as well as substantial modifications to the text.

Specific comments

The construction of age models at these sites is obviously a challenge, given the lack of foraminifera for 14C dating (and the apparent unreliability of laminae as annual layers?). However, it is clear that the authors have put a lot of effort into the attempt, and have succeeded in developing useful tiepoints. Unfortunately I find the discussion difficult to follow in places, and worry that the authors have overstated their confidence in the age model in certain places.

The age models rely on three lines of evidence: 1. The presence of radiogenic Am and Pb in the upper 25cm; 2. Correlation of slumps to historical earthquakes; 3. Radiocarbon ages of bulk organic matter.

As I understand it, the first provides multiple tie points over the 25-cm range where 210Pb is present (extending to 130y before present?). This seems like a strong chronological constraint. However, these tie points are not listed; the depth and age of each tie point needs to be specified.

The second line of evidence seems reasonable, though I am not familiar with the earthquake history of the region and would like some additional information (I can't access the Dorbath paper): were the two cited earthquakes much larger than any other historical earthquakes? Or were there a number of similar magnitude earthquakes, eg. ˜7-7.9? How can we be confident that the largest earthquakes generated the largest slumps, when slumps can occur in association with relatively small earthquakes, dependent on the sediment loading? I would like to believe these ages, but am not familiar with the use of slumps as age constraints and would simply like more information. Again, the depth ranges used for tie points should be explicitly specified here.

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The radiocarbon ages, however, are problematic. It seems clear that the organic matter 14C ages present maximum ages only - since all organic matter is likely to be retransported to some degree, and there will always be some amount of old carbon present. This has been amply shown by prior workers in other environments, where bulk organic matter is always older than the sediment in which it is found, often by thousands of years (see, for example, Mollenhauer & Eglinton, L&O 2007). Therefore, I think the 14C can really only be used to provide a maximum age at each point, within error bars that reflect the analytical uncertainty.

It follows from the above that the inferred MAR for the core prior to the lowermost 210Pb measurement is a minimum MAR (since the sedimentary ages are actually the maximum possible sedimentary ages). It is therefore possible that the MARs prior to the regime shift are much higher than inferred by the authors. As a result, MARs are not reliable at this site.

I therefore would insist that the authors present their sedimentological measurements in Figures 3 and 6 as mass fractions (% or ppm) or abundances (number per g) rather than as fluxes. The fluxes are simply not reliable without much better age control in the lower part of the core.

Additional comments:

Section 3.2: Why is more comparison not made with the Agnihotri et al. record? It is quite similar and should provide reinforcement for the observations made here.

- Cd is not likely to be a reliable nutrient tracer when measured in bulk sediments, but should be just as sensitive to sedimentary redox environment; this part should have more thorough references.

- The discussion here often switches chronological direction; it would be better to be consistent in referring to sequences of events moving forward in time toward the present.

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Section 3.3: I was under the impression that the ENSO activity during the LIA was a matter of debate? Has this debate been resolved?

- I don't understand how the d15N at the core sites presented here can be confidently interpreted as reflecting water column oxygenation, while the d15N in Mejillones (which looks identical within age model error, to my eyes) is interpreted as reflecting a 'delayed and gradual' change in upwelling-favourable winds? This statement requires two things: first, at least a brief discussion of why the d15N records at the two sites would reflect different processes (I don't believe that they do), and second, an estimate of errors in the age models.

Supplementary information: what is a 'real stratigraphic boundary'? Please use clearer wording.

Interactive comment on Biogeosciences Discuss., 5, 3919, 2008.