

Anonymous Referee #1

Received and published: 5 May 2015

Review of "Inter-decadal changes in the intensity of the oxygen minimum zone off Concepcion, Chile (36°S) over the last century" by Srain et al.

Major comments

1. According to the authors, one of the major goals of the study is to investigate whether the prokaryotic community is affected by OMZ intensity (page 6007, line 7-10). This statement seems to imply that oxygenation is regulated independently from the microbial communities in the water column and sediments. However, microbial respiration is an active driver of oxygenation (and consumption of electron acceptors other than oxygen). Consequently, the prokaryotic community will necessarily affect and be affected by OMZ intensity. Given the triviality of this finding, I recommend to delete or at least downplay the discussion of the effects of oxygenation on the microbial community.

A: Agree. Sentence in lines 21-23, page 6008, was changed to "...asses whether intensity of OMZ has varied over the past century in response to ocean/atmosphere circulation patterns, and whether this is reflected in changes in the prokaryote community."

2. This manuscript presents a nice interdisciplinary data set containing both inorganic and organic geochemical data. However, for readers without special expertise in one of the subject areas, the unordered succession of data description, background information and data interpretation in the discussion (e.g., Section 4.1) is quite difficult to follow. It would be quite useful to present the trace metal behavior and especially the biomarkers and their utility in a separate section before the discussion (e.g., in a separate introductory section). This section could include a table listing all biomarker groups along with their respective occurrence in specific bacteria or archaea and the corresponding references. This table would be much more useful for the non-expert than the chromatograms shown in Figure 5.

A: A new table (Table 1) as well as two new sections (1.1 and 1.2) were added to the revised version of the manuscript (page 6006, line 2; page 6007, line 6).

3. Additional comments

Page 6005, line 24-26: This sentence says that OMZs resemble Archaean prokaryotic biota which does not make sense. Please rephrase

A: Sentence was deleted.

4. Page 6006, line 20-25: I suggest adding a few more original papers (rather than review papers) and mechanistic (pore water) studies on modern sediments here and also on page 6015, line 15-20 and page 6017, line 9-10. Meaningful regional examples are: Böning et al., 2009, *Marine Geology* 259, 112-121; Scholz et al. 2011, *GCA* 75, 7257-7276.

A: New references were added on page 6006, first paragraph; page 6018, first paragraph; and page 6020, line 8.

5. Page 6008, line 9: Odd syntax; Maybe: “then the Ag cup was wrapped in a tin cup”?

A: Paragraph was simplified (page 6010, lines 4-6)

6. Please rephrase. Geochronology: Were the carbon-14 dating performed on TOC or on forams? Define the abbreviation ‘DR’.

A: Corrected on page 6010, lines 15-21.

7. Trace metal analysis: Were the acids really applied sequentially (one after another)?

A. Yes. paragraph was corrected in section 2.4, page 6011, lines 3-6.

8. Were the blanks really performed using deionized water instead of acid?

A. Deionized water, according to Muñoz et al. (2012 in *Cont. Shelf Res.*, 33, 51–68).

9. Please list the (Me/Al) earth that were used for calculating MeXS.

A: Original data are now included in supplementary information (Table S2).

10. I would like to encourage the authors to publish the original data in an online repository or to include them as a Table in the paper.

A: Original data are now included in supplementary information (Table S3).

11. Results: The results of the statistical treatment need to be visualized somewhere, e.g., in a table. Correlation plots could be added as a supplementary figure.

A: A new table (Table 2) was added in the revised version, and correlation plots are shown in supplementary information section.

12. Page 6012, line 1-5: The redox potential is not used in the discussion nor is it compared to data from other areas or settings. Therefore, I do not see the point of reporting them. The same holds for the temperature and chlorophyll data on page 6011, line 10-14.

A: We agree with the reviewer. However, we left some text necessary for the whole argument, and refer to figures in Supplementary Information section.

13. Page 6012, line 23: Replace 'lower O₂ depletion' with 'more reducing conditions' or something else.

A: Corrected on page 6015, lines 12.

14. Page 6015, line 16-21: This description of the behavior of Mo and U during early diagenesis is oversimplified. I suggest adding two sentences on the particular conditions (Fe-reducing versus sulfidic conditions) that lead to Mo and U accumulation in the sediment. Relating to this: There are some subtle differences in the downcore profiles of Mo and U. Interpreting these differences could help to explain undiscussed features in the biomarker profiles, especially since ~1980.

A: Text was modified as suggested in page 6006, lines 12-24, and page 6018, line 24 to page 6019, line 1- 14.

15. Page 6015, line 20: Tribovillard et al. 2004 deal with Mo sequestration in sulfurized organic matter, not with Cd. Please find a more appropriate, original citation for Cd behavior under high productivity and sulfidic conditions.

A: References corrected and moved to page 6007 lines 1-4.

16. Page 6015, line 25: "ammonia oxidation by marine pelagic archaea" needs to be put in the biogeochemical context of OMZs (what is the role of ammonia oxidation in this environment). Otherwise the reader is lost with this information.

A: Text was rewritten in page 6008, lines 1-5.

17. Page 6016, line 20: replace 'above and below' with 'before and after'.

A: Changed on page 6019, line 20.

18. Page 6016, line 23: GDGTs seem to increase again after 2000 (i.e., other than stated here, oxygenation decreased again after 2000).

A: Period corrected as “1979 and 2000” on page 6019, line 16.

19. Page 6017, line 18: I would rather argue that C31 hopanol is more or less constant throughout this period with peaks at the beginning (1935) and end (1970).

A: We agree. Changed as suggested on page 6020, lines 23-25.

20. Page 6017, line 20-24: I would rather argue that C31 and C32 hopanols display a common peak at 1935 and are decoupled thereafter

A: We agree. Corrected as suggested by referee. Page 6021, lines 3-4.

21. Page 6017, line 27: “abundance and structural diversity (. . .) increase” is not consistent with the decrease in C32 hopanol.

A: Sentence corrected on page 6021, line 5

22. Page 6020, line 7-9: The correlation between PDO and C31 hopanol has been mentioned on the previous page already and does not need to be repeated here. The following discussion of implications for the temporal variability in water column oxygenation is also redundant to the earlier discussion and the final statement on implications for the atmospheric N₂O budget arrive out of the blue. Please streamline this section.

A: Agree. We deleted the sentence on correlation and atmospheric N₂O is now briefly mentioned on page 6023.

23. Page 6020, line 10: Replace ‘environments’ with ‘conditions’.

A: Changed as requested.

24. Page 6021, line 4: Replace ‘sensitive redox metals’ with ‘redox-sensitive metals’.

A: Changed as requested, including Fig. 3 caption.

25. Figure 3: Please extent the x-axes to zero.

A: Done.

26. Figure 3 and 4: Why are there gaps in the vertical profiles of redox-sensitive metals and biomarkers? Some samples seem to be missing for certain parameters. This needs to be explained!

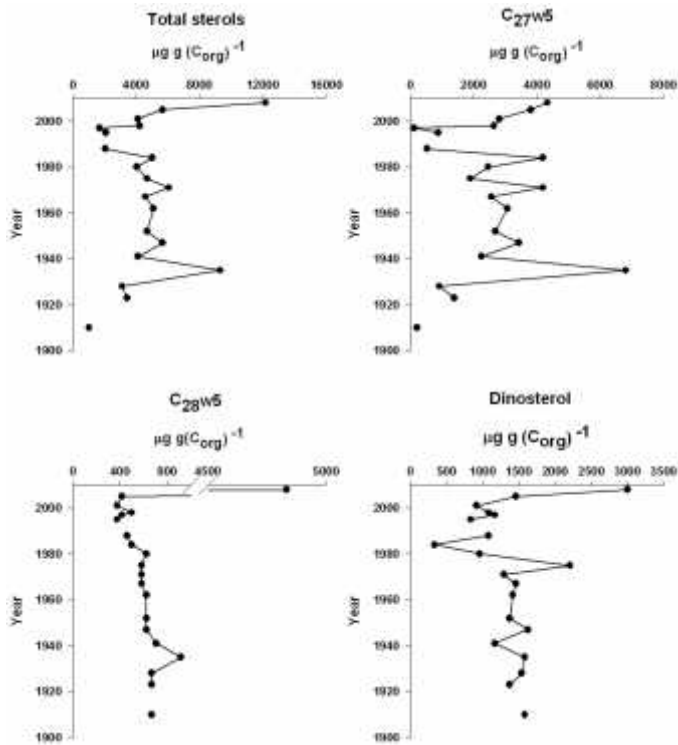
A: Some samples were lost. This is explained in the figure captions of the revised version.

Referee #2

P.A. Meyers

1. Foremost among these is the curiously different patterns of behavior of the organic biomarkers presented in Figure 4. Only the bacterial biomarkers seem to conform well to the postulated PDO influences on the coring site; the sterols and the GDGTs do not seem to be affected by the PDO cycles, and the MAGEs appear to show some combination of the hopanoid and sterol patterns. These variations are glossed over in the Discussion, yet they may be evidence of other, equally important processes that affect the microbial communities at this location.

A: We agree that other processes than “oceanography” may be affecting the microbial communities at our site. For example, and in addition to the biomarkers pointed out by the Referee, below are profiles of individual sterols, where we do not detect a clear pattern (see for example dinosterol, a marker of dinoflagellates). However, we do not have the necessary information to conclude about other important processes, and thus we prefer to “gloss over” some of the variations to avoid overinterpretation of our data.



2. Another issue, which is minor but still merits attention, is that Panel F in Figure 2 should have the same vertical 25 cm depth axis as the other panels, and it should show only the radiogenic lead dates that are the basis for the geochronology used in this study.

A: Figure 2 was modified accordingly.

3. Finally, many of the sampled intervals in the core are missing data. Were these proxies below their detection limit in these intervals, were there analytical problems, or were they simply not measured? Explain!

A: In the case of figure 3, the missing data-points refer to lost samples, whereas the gaps in figure 4 indicate that biomarker content was under the detection limit. This is now explained in the respective figure captions of the revised version.

4. A semi-technical issue is the authors' repeated use of the word "flux" when they report only concentrations. A flux is an amount per unit time, not a concentration. If fluxes were actually measured, they should be shown and discussed properly. Incorporations of mass accumulation rates would strengthen many parts of this study, but actual determination of these true fluxes is admittedly difficult and often not possible. Please correct or clarify this issue.

A: We did not measure fluxes and apologize for the incorrect use of the term in the previous version. This has now been corrected (page 6020, line 7).

5. A last issue that badly needs attention is that the paper is peppered with typographical and syntactical errors that interfere with its reading.

A: We carefully reviewed the manuscript and hope to have fixed these issues in the revised version.