

Interactive comment on “Inter-decadal changes in the intensity of the Oxygen Minimum Zone off Concepción, Chile (~ 36 S) over the last century” by B. Srain et al.

Anonymous Referee #1

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Review of “Inter-decadal changes in the intensity of the oxygen minimum zone off Concepcion, Chile ($\sim 36^{\circ}$ S over the last century” by Srain et al.

Srain and colleagues present inorganic (trace metals) and organic geochemical (lipid biomarkers) data for a shallow sediment core from the Chilean upwelling area covering the last 100 years. They interpret the data with respect to coupled changes in bottom water oxygenation, sedimentary redox conditions and microbial community structure related to the Pacific Decadal Oscillation (PDO). The joint interpretation of redox-sensitive metals and lipid biomarkers is interesting and innovative and the conclusion that water column oxygenation and sedimentary redox conditions seem to be coupled to the PDO is well supported by the data. I therefore recommend publication

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of this manuscript in Biogeosciences.

The readability of the discussion could be greatly improved by some streamlining and re-structuring. Furthermore, I disagree with a number of observations in the biomarker records and identified a few literature gaps and inappropriate citations. These issues should be addressed before the manuscript is ready for publication.

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.

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.

Additional comments

Page 6005, line 24-26: This sentence says that OMZs resemble Archaean prokaryotic

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biota which does not make sense. Please rephrase

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.

Page 6008, line 9: Odd syntax; Maybe: “then the Ag cup was wrapped in a tin cup”? Please rephrase. Geochronology: Were the carbon-14 dating performed on TOC or on forams? Define the abbreviation ‘DR’.

Trace metal analysis: Were the acids really applied sequentially (one after another)? Were the blanks really performed using deionized water instead of acid? Please list the (Me/Al)earth that were used for calculating MeXS. 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.

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.

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.

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

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

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the biomarker profiles, especially since ~1980.

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.

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.

Page 6016, line 20: replace ‘above and below’ with ‘before and after’.

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

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).

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

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

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.

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

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

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

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!

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