

Interactive comment on “A cobalt plume in the oxygen minimum zone of the Eastern Tropical South Pacific” by N. J. Hawco et al.

N. J. Hawco et al.

msaito@whoi.edu

Received and published: 26 July 2016

See combined response to reviewers in response to reviewer #1.

Specific responses to Reviewer #2

Title: The paper contains many more interesting observations (and corresponding interpretations) than just the Co plume in the oxygen minimum zone. Consider a more general title that fully captures the comprehensive character of the presented data set.

This is a valid point, and there are advantages to having papers with long complex titles versus shorter memorable ones. We will consider this point further.

Line 47, abstract: Add 'in' before 'oligotrophic regions'.

Will change

[Printer-friendly version](#)

[Discussion paper](#)



Line 72: It would be useful to define labile Co and the abbreviation LCo here.

We appreciate the suggestion. We will put the definition for labile Cobalt here.

Line 75: Does it matter whether the Mn oxides are formed by bacteria or abiotically? I assume they would be enriched in Co either way (see also Line 497).

Perhaps not in the scope of this manuscript, but biogenesis or abiogenesis of Mn oxides have important implications for the sensitivity of Co and Mn scavenging in the past and future.

Results section: The Results section would be easier to follow if it was divided into subsections, e.g., vertical Co profiles, lateral Co distribution, etc.

We plan to organize the results section into more subsections.

Line 293: Add number of gyre station so that it can be easily identified in Figure 3.

Will change.

Line 299: 'overturning' (typo).

Will change.

Line 313: Add reference to figure.

Will change.

Line 321: Upwelling of P-rich and O-poor water 'results' in high dCo? Rephrase for clarity.

Will change.

Line 329: Figure 13?

Will remove reference to this figure.

Line 340: Add figure reference.

[Printer-friendly version](#)

[Discussion paper](#)



Will add.

Line 359: “On the GP16 transect . . .”.

Will add.

Line 417: I assume you mean “low O₂/high dCo water masses mix with high O₂/low dCo water masses”. Rephrase for clarity.

This is correct, will rephrase.

Line 424: Is the LCo:O₂ trend shown in the figures? If yes, add figure reference; If no, C3 add ‘not shown’.

Will add not shown.

Line 433: What is meant with “double 0 μM intercept”? $2 \times 0 \mu\text{M}$? Rephrase for clarity.

Will rephrase. “Double” in this case referred to the actual dCo concentrations at 0 μM O₂ being more than twice the concentration of what the intercept of the linear Co:O₂ regression predicted.

Line 434: What exactly resembles “profiles of N₂ . . .”?

Will add clause describing how the profiles are similar. What is similar about them is the fact that both dCo and excess N₂ from denitrification both peak just below the oxycline rather than having a consistent value throughout the entire depth range of anoxia. Because excess N₂ results, in part, from heterotrophic remineralization within the upper depth strata of the OMZ, the similarity between N₂ profiles and dCo in this region imply that the dCo maximum reflects remineralization of sinking biomass.

Line 444: The 50 m depth range is not shown with separate symbols in Figure 5c. Later in the text other depth ranges are discussed which are not shown either (Line 457). I suggest adding more color codes to Figure 5c to indicate all the depth ranges and corresponding covariation trends discussed in the text. Otherwise the discussion

[Printer-friendly version](#)

[Discussion paper](#)



is difficult to follow.

This is a good suggestion and we will modify the figures in accordance with the three depth ranges discussed in the text.

Line 492: What is meant with 'redox barrier'? Do you invoke a biological process or just that Mn oxides do not form at very low oxygen concentrations?

The instability of Mn oxides at low oxygen is the simplest explanation. We will rephrase to indicate that the particulate dataset is consistent with thermodynamic and kinetic arguments against Mn-oxidation in OMZs.

Line 499: Figure 7C does not show pCo.

This is correct. Will change to figure 6C.

Line 502: Remove 'in'.

Will change.

Line 518: Figure 14 is a summary figure which has not been introduced at this point of the discussion. I recommend referring to actual data here and to restrict references to Figure 14 to the Conclusions section.

This is an excellent recommendation, we will reserve figure 14 for the conclusion section.

Section 4.3: See comment above on reductive Mn dissolution in the water column.

Yes, we plan to clarify that a coastal Mn and Co source does not appear to originate in the sediments underlying the stations reported here, leveraging the calculations reviewer #2 introduced from the Scholz et al. 2011 study.

Line 545-549: The Peruvian shelf occasionally experiences oxidation events which also favor Mn deposition and burial (see discussion in Scholz et al., 2011).

This is an important point that will be added to this section. This may very well be why

BGD

Interactive
comment

Printer-friendly version

Discussion paper



only the shallowest station has a benthic Co and Mn maximum!

Line 590: “oxidizing conditions . . . prevent reductive dissolution . . .” is a misleading statement. The sediments at the western Pacific margin are certainly Mn-reducing in the shallow subsurface but the oxic surface sediments prevent diffusive escape of the pore water Co and Mn into the water column.

We appreciate this point and will rewrite to differentiate between in-situ redox conditions (downcore) in the sediment and the mass transfer of Co and Mn from such sediments to the open ocean, which appear to be limited due to re-precipitation in surface sediments and in the water column.

Line 594-Line 606: Because of low water exchange kinetics, Co is incorporated into pyrite and does not tend to form its own sulfide minerals (Morse & Luther, 1999, GCA 63, 3373-3378).

We are thankful for referral to this reference. We will modify this paragraph to indicate incorporation into pyrite rather than direct CoS precipitation.

Line 613: Does ‘crust’ refer to ‘andesite’ in this equation. It does not matter what you take but it should be consistent throughout the manuscript.

The range in calculated fluxes represents calculations from both upper continental crust, as defined by McLennan 2001, and Andesitic endmembers, as defined by Taylor and McLennan 1995). We will make this more explicit in the paragraph.

Conclusions: I really like Figure 14 and therefore recommend to introduce it more explicitly at the beginning of the Conclusions (something like: “the major pattern and underlying processes identified in this contribution are summarized in Figure 14”). The major finding can then be summarized by guiding the reader through Figure 14.

We appreciate this suggestion and hold reference to figure 14 until the conclusions, where we will explicitly introduce it as a conceptual model for the Co cycle here.

[Printer-friendly version](#)[Discussion paper](#)

Caption Figure 1: What depth or potential density do the isolines correspond to?

This represents oxygen at 300m, which was accidentally omitted from the caption. We will remedy this.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-169, 2016.

BGD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)

