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Biogeosciences Discussions
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Interactive comment on "Coastal Sources, Sinks and Strong Organic Complexation of Dissolved Cobalt within the US North Atlantic GEOTRACES Transect GA03" *by* Abigail E. Noble et al.

Anonymous Referee #2

Received and published: 9 January 2017

1. Does the paper address relevant scientific questions within the scope of BG?

This manuscript describes a large dataset of dissolved, labile, and particulate cobalt concentrations produced from samples collected over two US GEOTRACES section in the North Atlantic. The biogeochemical cycle of cobalt fits well within the aims and scope of the journal and this work would likely be interesting to its readership.

2. Does the paper present novel concepts, ideas, tools, or data?

The authors are at the forefront of cobalt research in the marine environment and have produced a novel dataset through their participation in the GEOTRACES program.

3. Are substantial conclusions reached?

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The authors identify two major subsurface sources of cobalt to the study region, one each at the eastern and western margins. They also identify atmospheric deposition, riverine sources, hydrothermal activity, and nepheloid bottom layers as other factors impacting the availability of cobalt.

4. Are the scientific methods and assumptions valid and clearly outlined?

The methods are state of the art and described at length. The manuscript could be shortened if the portions directed solely at describing method improvements, e.g. Section 3.3 were spun off into a separate manuscript. I believe that such a manuscript may be in prep (pg 13, line 11).

5. Are the results sufficient to support the interpretations and conclusions?

The processes identified as possible explanations for the observed biogeochemical features are generally well justified. However, I found the text attempting to anticipate changes in cobalt availability as a function of changing oxygen concentrations to be less compelling (Section 3.6). While there is a relatively robust relationship between the concentration of dissolved cobalt and that of dissolved oxygen, I feel that the authors have failed to adequately assess the uncertainty in the calculation that leads them to predict that the inventory of cobalt could increase by as much as 20%. They make passing reference to “implications for the ecological balance within this basin” but leave the readers to guess what those implications might be. The authors conclude with a paragraph suggesting future anthropogenic cobalt pollution; do they expect this potential pollution to have a greater impact on cobalt concentrations than marine deoxygenation?

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

I am curious about the intercalibration efforts described in Section 2.3. One of the most important legacies of the international GEOTRACES program will be improved inter-

calibration among laboratories making highly precise measurements with specialized techniques. GEOTRACES and SAFe consensus samples were included in the analyses and those results were reported. However, the authors report a lack of agreement among samples that were shared with other groups as part of a GEOTRACES "crossover" station (pages 10 and 13). Perhaps the reasons for these discrepancies could be more fully explored if matters of methodology were discussed in a companion paper.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

The authors give proper credit to related work including their own study in the South Atlantic as well as recent GEOTRACES-related intercomparison and intercalibration work. They also suitably reference the historical literature.

8. Does the title clearly reflect the contents of the paper?

The title is suitable.

9. Does the abstract provide a concise and complete summary?

The abstract offers a complete summary but could benefit from editing for brevity.

10. Is the overall presentation well-structured and clear?

Overall, this manuscript is well written but I encourage the authors to consider whether they would be better served by breaking the methods discussions into a companion paper or perhaps into supplementary material (see comments in #4). Without that extra discussion and Figure 4, the paper would still be impressive as it describes a large dataset covering measurements made from samples collected across the North Atlantic basin.

As it currently stands, the manuscript is quite long with a large number of figures (14) which include lengthy captions. The manuscript might also be shortened by removing

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some instances of redundant text which restate material initially presented.

11. Is the language fluent and precise?

Yes.

12. Are mathematical formulas, symbols, abbreviations, and units correctly defined and used?

Yes.

13. Should any parts of the paper be clarified, reduced, combined, or eliminated?

See above.

14. Are the number and quality of references appropriate?

Yes.

15. Is the amount and quality of supplementary material appropriate?

NA.

Specific comments:

P5L1-23: This review paragraph could be eliminated without impacting the value of the manuscript.

P10L22: “Dulaquais Refs”

P10L19-26: Why mention the IDP? If the authors feel that the higher values are real, then I suggest they include those data and not discuss the intercalibration.

P11L1-9: The cruise track was described in the Methods section; there is no need to repeat.

P11L26: It appears to be that Figures 2 and 3 are reversed.

P12L17: Does the Noble and Saito manuscript focus on the analytical methods? Could

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this be an appropriate companion manuscript to offload some of the methods discussion?

Figure 2 (or 3): It would be helpful to overlay the dissolved oxygen data onto the ODV section plots.

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