

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.

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Author response: We thank Reviewer #2 for their response and input. We have addressed general and specific comments below.

Reviewer #2 comment: 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).

Author response: Referee #2 suggests that the information regarding intercalibration and preservation be presented in a separate manuscript. We respectfully believe that this section is important to include here due to the slight departure from our previous

C1

approaches, the results showing successful preservation, and to support the extent to which these data are compared to previous work in the South Atlantic that employed the original methods. This is important for the credibility of the comparison discussion. Additionally, the trace metal community has been asking about this data for several years and we feel it is overdue to be published. The first author on this paper has moved to a different field of research and as a result, it may take a much longer time for us to coordinate to get the full methods/preservation work published. The figure presented here is just one of several pieces of the methods work, but we feel that it represents an important piece related to GEOTRACES intercalibration that needs to be recognized sooner rather than later.

Reviewer #2 comment: 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 author 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.

Author response: Yes, rigorous statistics were not performed to allow for an assessment of the uncertainty; however, our conclusions are couched in soft language, suggesting only that the results of our calculations “imply a need to consider the influence of changing oceanic oxygen on the biogeochemistries of metals and their influence on marine ecology.” (pg 29 line 3). We have previously applied this type of assessment to data in a prior publication (Noble et al. 2012) and do not believe this is an overreach.

Reviewer #2 comment: 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?

C2

Author response: As noted by the reviewer, there is considerable uncertainty regarding the magnitude of the proposed potential impacts on cobalt distribution (marine deoxygenation vs. anthropogenic pollution) and we feel that opining on this magnitude would be an overreach.

Reviewer #2 comment: 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.

Author response: Thank you. We also believe that these discrepancies are related to the preservation methodologies and thus cannot be fully discussed here without a full discussion of the preservation methodology. As such, these issues will be covered in the methods/preservation paper in preparation.

Reviewer #2 comment: The abstract offers a complete summary but could benefit from editing for brevity.

Author response: Thank you, we will make an effort to edit.

Reviewer #2 comment: 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

C3

might also be shortened by removing some instances of redundant text which restate material initially presented.

Author response: Thank you. We will make an effort to condense the figure captions and text and consider the addition of supplementary material.

Reviewer #2 comment: P5L1-23: This review paragraph could be eliminated without impacting the value of the manuscript.

Author response: We thought it valuable to provide a review for those readers without a background in cobalt biogeochemistry. However; we can condense this paragraph if required.

Reviewer #2 comment: P10L22: “Dulaquais Refs”

Author response: Thank you. We will fix this.

Reviewer #2 comment: 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.

Author response: Because cobalt had not previously been considered one of the GEOTRACES key constituents, we believe intercalibration efforts to be important to acknowledge in the effort to have cobalt included among the key trace elements and isotopes. Additionally, GEOTRACES papers typically include this intercalibration data, it is not much to add space-wise, and we therefore believe it warranted to include the intercalibration data here.

Reviewer #2 comment: P11L1-9: The cruise track was described in the Methods section; there is no need to repeat.

Author response: Thank you. We will shorten or eliminate this.

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

C4

Author response: Thank you, we will fix this reference

Reviewer #2 comment: P12L17: Does the Noble and Saito manuscript focus on the analytical methods? Could this be an appropriate companion manuscript to offload some of the methods discussion?

Author response: Again, please see above response regarding companion manuscript. Yes this manuscript will focus on analytical methods, but also addresses other aspects of the results therein.

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

Author response: We agree that the concept of an oxygen overlay would be helpful perhaps in a separate figure, but this figure (3) is already quite busy and an overlay of dissolved oxygen would be too distracting and would make the figure too cluttered.

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