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Biogeosciences Discussions

Interactive comment on "Imprint of a dissolved cobalt basaltic source on the Kerguelen Plateau" *by* J. Bown et al.

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The manuscript presents dissolved total dissolved cobalt data from the vicinity of the Kerguelen Plateau, and makes the case that there is a lithogenic/sedimentary cobalt flux and that cobalt can be used as a tracer of iron. The dataset includes seven vertical profiles of cobalt showing elevated cobalt in the vicinity of the islands, a handful of supporting particulate cobalt datapoints, and no hydrographic or ancillary data included at this time. The manuscript also includes two budget figures describing fluxes of cobalt in this region. In general the manuscript appears to have a high quality cobalt data from a region of importance as a natural source of metals to the iron limited Southern Ocean. In general I think the manuscript could do a better job of providing context, particularly in the context of ancillary datasets, additional data that cite (Fe, Nd, etc), and I am

not entirely convinced of the interpretation of cobalt as a tracer for iron. I think these concerns could be addressed in moderately revised version to produce a nice and very useful study.

One of my substantive concerns is the notion that Co serves as a tracer for Fe. This would be useful if true, but I am concerned that the authors point out that the shallowest station C01 is where the highest iron and other metals were observed, but here while elevated in Co related to the open ocean station, this is not the highest Co station, with A03, A07, and A08 all showing much higher concentrations. The authors explain this with a particulate Co measurement C01, which appears to only be found in the text rather than the figure, and invoke dissolution of lithogenic material. These observations suggest significantly different source functions for the fluxes of Co and Fe (with iron either fluxing from sediments or being dissolved/released from suspended sediments earlier), which makes the connection between elements more difficult. Moreover, because station C01 doesn't show the same dissolved maxima as found elsewhere it seems that there could be other sources closer to the "A" stations rather than the C01 station, which based on examination of the hydrography in Figure 1 seems plausible. Particularly, Co distributions are known to be related to manganese oxides, and perhaps there are exposed regions of the seafloor that could be leaching/dissolving some Mn and Co, is there Mn data for this sample set?

Specific comments: Figure 2, the profile figure is too small. Reorient in two rows. Include the data in a table and/or deposit to a data management office. Add temperature, salinity, transmissometry, and nutrient datasets to the study if available. Figure 3 Co budget makes a number of assumptions, some caveats in the text would be appropriate. Using solubility, atm flux, and biological uptake from other studies and regions (and sample types for solubility) will likely introduce uncertainties. Discussion of pycnocline, but no salinity profiles are presented. Additional recent references and discussion of them in this context might be quite useful to the interpretation here. Noble et al., L&O 2012 discusses Co and Fe fluxes from OMZ/coastal environments, and

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Shelley ... Lohan in GBC in press (if available) discuss solubility relative to iron, and Saito and Moffett 2002 GCA discusses continental shelf source of Co, Ellwood et al 2005 - Co speciation across the polar front, and Saito and Goepfert L&O 2008 for use of Co in polar phytoplankton. A sediment trap Co flux is discussed, in a caption, but I didn't see mention of it elsewhere. p7298: interpretation of lateral flux from C01 as the most likely source, despite having a lower dissolved concentration. Seems odd to invoke this when the A# stations are also close to the sedimentary/coastal environment (in fact closer to the islands), couldn't they have a distinct source instead of the advected particulate material? Does the particulate material stay constant in abundance (e.g. pAl/L content) while becoming depleted in Co (a sign of dissolution, with the Al presumably being slower to dissolve)? p7299: One of the arguments is that Co dissolution from lithogenic material occurs in the water column. Why wouldn't this occur in the sediments as well? Are Co porewater concentrations really so high as to prevent this? p7301/7304: the conclusion that lateral advection is much higher than biological uptake seems a bit tenuous if uptake was not measured directly in this study. In general, some brief discussion of Co speciation might be appropriate, particularly if dissolution and biological uptake are discussed.

In general, I think one of the challenges we have with water column datasets of increasing resolution and precision is that we can see features like in this study that are suggestive of sources. But I think we need to be somewhat careful of the interpretation, because water column data doesn't prove the flux since they are not process studies (they can be consistent with them), as much as they are observations of distributions. While the quantitative efforts are a nice attempt in this study, they are more speculative than the analytical dataset due to the estimations and borrowed values from other regions/sample types as mentioned above.

One of the exciting ideas about natural fertilization sources like the Kerguelen Islands is the potential for co-limitation to be avoided because the islands provide other micronutrients beyond Fe. This dataset is one of the first I know of to be able to discuss

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this with Co data (Martin's Galapagos paper being another perhaps). Including some discussion of this distinction for natural fertilization sources would be very useful.

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