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Interactive comment on "Physical and remineralization processes govern the cobalt distribution in the deep western Atlantic ocean" *by* G. Dulaquais et al.

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I have read "Physical and remineralization processes govern the cobalt distribution in the deep western Atlantic Ocean" by Dulaquais et al.. This manuscript presents a large dissolved cobalt and apparent particulate cobalt dataset from a compilation of multiple cruises that comprise the meridional GEOTRACES-A02 section. The main conclusion of the manuscript is that remineralization and mixing are primarily responsible for the gradients and distributions of cobalt observed along the transect. I think this high resolution, high quality dataset should be published in BG and it is exciting to see this lengthy cobalt section and the features therein.

In general, the manuscript follows a logical flow and makes good use of ancillary data to draw conclusions about features in the dataset. The authors estimate remineralization and mixing, though I would recommend softer language in some of the conclusions they make in the discussion of Co and P decoupling in the deep ocean. In general, the manuscript would benefit from more thorough editing for grammar, tense agreement, and sentence structure to improve readability.

The crossover stations comparison is intriguing. These data suggest a combination of temporal variability in cobalt distributions at BATS and the deep South Atlantic, and I also think potentially the detection of different physiochemical fractions by the different methods used. It might be worth suggesting this in the text. Might it also be worthwhile to mention that the South Atlantic crossover station might experiences less temporal variability due to lesser dust input or coastal influences, potentially allowing for better agreement when comparing methods at this station relative to the other two? It is great to see these datasets being compared and discussed and it highlights the complexity of cobalt and cobalt speciation, encouraging more critical thinking about what physiochemical fractions are observed with a given method.

The discussion of the Iceland volcanic eruption as an explanation for the 20pmol/kg increase in NADW is intriguing, though perhaps more care could be made in considering the fate of the dissolved flux of cobalt after seeding by the ash. In the Frogner study, the dissolution rates decreased precipitously within an hour and it seems a bit remiss to me that there was no discussion here of the potential removal of cobalt post-eruption, or how a decreased release might change the effective flux of cobalt to the water column. Could the authors perhaps address their assumptions a bit more clearly here? How did the authors obtained the 8.76 nmol Co release value? I ask because the graph in this paper does not have sufficient tick marks to be able to determine the value from the graph.

I am skeptical of the explanation that preferential remineralization of P to Co calculated in the upper 400m accounts for the decoupling of Co and P in deep waters and that

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deep scavenging is not occurring. Much of remineralization generally occurs in the upper hundreds of meters, so I am confused by the calculations in the upper water column here being applied to explain deep-water decoupling. Could the authors clarify this, perhaps by more clearly stating depth ranges over which they choose to make this claim and why? I think it is also important to keep the timescales of scavenging in mind. If scavenging were not occurring, wouldn't we expect the cobalt profiles to look like N and P? I don't disagree that scavenging may be slow relative to the timescale of circulation within the deep western boundary current, but I think it is valuable to contextualize the conclusions here within an appropriate timeframe.

The authors argue that the presence of organic ligands justifies negating scavenging in the deep waters. I would agree that complexation likely slows the scavenging process, but we have found that cobalt binding ligands are often not in excess of the total cobalt beneath the euphotic zone, presumably leaving the labile fraction susceptible to scavenging. Labile cobalt often displays intermediate water maxima and seafloor minima in the open ocean. This would suggest that deep waters inherently carry a composition of cobalt that is highly complexed relative to overlying waters, or that, along the circulation path and from raining particulate matter from above, labile cobalt in deep water is lost to scavenging over time. In the Ross Sea, we observed rather high concentrations of labile cobalt that we've attributed to the virtual absence of cyanobacteria in that cold environment, which have been demonstrated to produce cobalt-binding ligands (Saito et al. 2010). If Antarctic waters are generally characterized by higher labile and total cobalt, and that water feeds the deep South Atlantic, (where lower total cobalt is observed as observed in this manuscript, and where we have also previously observed low labile cobalt/high % complexation (Noble et al. 2012)), it suggests to me that scavenging has occurred along this trajectory or from above, presumably removing the labile fraction. I would agree that scavenging of the complexed fraction should be significantly slowed, but it may affect the labile fraction. As such, I don't think that invoking scavenging in the deep ocean while also invoking complexation as a means to protect cobalt from scavenging is a paradoxical statement (pg 15967 line 13).

I am also wary of leaning too hard on conclusions drawn from the apparent particulate cobalt (specifically with respect to scavenging) when the values are often close to the detection limit, especially when the values being compared are below the detection limit (i.e. Table 3).

Detailed suggestions: Figures 2,3,4 – These would benefit from at least a couple key station labels, particularly ones explicitly mentioned in the text and in references to these figures. Pg 15953, line 20 – the wording "unfavorable microbial oxidation" is confusing. Perhaps a clearer phrasing would be "slowed microbial oxidation" pg 15961 line 6 – should read "relatively" rather than "relative" line 19 – please insert reference to Fig. 3 line 24 – typo - "diazotrophic" line 26-27 – please correct for tense agreement pg 15962 line 2 - should read "relatively" rather than "relative" in general – please use consistent wording for nutrient distribution / nutrients distribution / nutrient distributions line 13 – should read "other" rather than "others" pg 15963 line 21 – should read "located" rather than "locate" pg 15964 line 14 – should read "PCo>5pM" rather than "PCo<5pM" pg 15965 line 11 – I would be careful of the wording "no scavenging process" here. It does not appear to be detected although it is a process that occurs, so perhaps this could be worded more carefully by saying it is slow or is not apparent relative to the timescale of water mass circulation. pg 15966 line 21 – should read "Fig. 6e" rather than "Fig. 10e" pg 15967 line 9 – should read "This leads to the hypothesis [that] there is no..." line 12-13 – please adjust the definition of hybrid-type metal to include both nutrient-like and scavenged-like behavior (hence the term "hybrid"). As it currently reads, a hybrid-type metal has the same definition as a scavenged-type element. line 19 – The 2012 paper agues that mixing could describe the features observed within the OMZ, not the deep waters. pg 15968 – lines 10-15 - It is interesting to see the increase in PCo in deep waters and evidence of benthic remobilization of cobalt. Might it be worthwhile to include transmissometer data if that is available as it would further support this argument? lines 23-25 - In the discussion of dissolved concentrations between the Western North Atlantic and southeastern atlantic, it would be useful to add the Bown et al. 2011 data to the table in order to make this point clear regarding ex-

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cellent agreement, as the reader should not have to look up another paper in order to see this. pg 15969 line 5 and 20 – I don't think isopycnes is an English word. Perhaps isoclines, isopleths, or isopycnals? pg 15971 line 7 – should read "increased by 20.5" rather than "increased with 20.5" line 8 – it would help the reader if important station labels were added to Figs 3, 4, and 5. Perhaps also add the year for leg 1 - I think this would help to compare it in time to the 2012 leg 4. line 19 – should read "graphite" pg 15972 line 3 – "Anyway" is colloquial and I would suggest removing it. pg 15974 line 8/9 – incomplete sentence pg 15975 line 1 – Saito et al. 2004 nor Noble et al. 2008 are not applicable references here and should be removed from this statement. pg 15976 line 4 – I believe Bown et al. 2011 also looked at Co:P ratios – might be nice to add this reference here. We also compared Co:P in the Noble et al. 2008 paper. pg 15978 line 21 –should read "indirect" rather than "undirect" and Noble et al. 2008 could be cited here as this work studied Co cycling in eddies off Hawaii. pg 15979 line 3 – should read "estimated to be negligible" rather than "estimated negligible" pg 15980 line 23-26 please rephrase this sentence to make it clearer.

references: Saito et al. 2010, Biogeosciences, 7, 4059–4082, doi:10.5194/bg-7-4059-2010. Noble et al. 2012, Limnol. Oceanogr., 57, 989–1010.

Interactive comment on Biogeosciences Discuss., 10, 15951, 2013.