

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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“Physical and remineralization processes govern the cobalt distribution in the deep western Atlantic ocean.” by G. Dulaquais et al.

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The manuscript present dissolved and apparent particulate cobalt (DCo and PCo respectively) vertical distributions along GEOTRACES-A02 transect in the western Atlantic Ocean. The authors showed that remineralization should be considered as a ma-

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For pathway that controls cycling of DCo in intermediate waters and that the decrease of dissolved cobalt concentrations in deep water is mainly due to mixing processes rather than scavenging. The present study also evidenced that dynamics structures such as eddies can affect the vertical distribution of DCo in surface waters. The authors report a huge cobalt dataset (47 DCo and 15 PCo vertical profiles, more than 600 samples analysed) which I consider as an high resolution dataset regarding the scarcity of ancillary DCo dataset along the western Atlantic ocean. The DCo dataset is of high quality but I would like to advise the authors to be more cautious regarding interpretation of PCo data as most of the measurement seems to be below the detection limit of the FIA system (below 10 pM). This said, PCo vertical distributions exhibits consistent trend along the section and PCo data are moderately used throughout the manuscript. The hydrography of the vast and complex area of study is well described and illustrated with temperature, salinity, and dissolved oxygen latitudinal distributions. The authors have made a really nice use of their understandings of biogeochemical and physical features along the studied transect to interpret deep cobalt biogeochemical cycling. Such efforts brings new interesting insights regarding, meso/large scale DCo transportation, characterization of potential new external DCo source (volcanic activity) and internal processes that affects DCo distributions throughout the whole water column. I recommend only minor changes and clarifications detailed below before the manuscript is published. General comments: The introduction is a bit confusing; maybe the state of the art of dissolved cobalt biogeochemical cycling should be limited to the deep reservoir and then be more focused on the processes occurring in the deep reservoir and how they might impact cobalt global cycling. There are several calculations presented and explained along the article and it is at some point difficult to read and understand at the same time. My suggestion here would be to make a budget table or/and a scheme which would made the synthesis of all the fluxes calculated to help the reader. This would also allow to remove a part of the calculations explanations from the text and make the reading easier. It would be really nice to add more general thoughts about how each described processes interplay with the global biogeochem-

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ical cycle of cobalt in the ocean. Please be consistent with the number of significant figures when PCo or DCo concentrations are reported. This remark can also be applied to fluxes (i.e. P15971, L20, 0.054 ± 0.014 to 2 ± 0.22 10^{-11} mol m⁻²d⁻¹). Please also be consistent with journal names in the reference list (sometimes mentioned with abbreviations sometimes full names).

Specific comments: Abstract L25: "dilution and mixing": dilution can be a consequence of mixing but it is not a process by itself. 1/Introduction P15953, L23-27: unclear, how oxidation can occurs in anoxic conditions? P15954, L9-20 and P15955, L5-8: these two paragraphs should become one and the authors should discuss why these findings can be important in the context of the present study. 2/Methods P15955, L18-20: what was the time of the year for leg3 and leg4? P15957, L23-24: "Bown et al., 2011" not 2010; "DCo and PCo concentrations shown in this study are corrected with their respective reagent blanks", already mentioned line 20. 3/ Results P15962, L16: The highest value reported here, 93.27 ± 3.31 pM, is somehow out of range considering the 0-75 pM range for standard Co addition. L20-22: PCo has already been defined in the method section. P15963, L21: "locate" should be written "located" P15964, L6-8: adding the numbers of events and run a P test would strengthen the main statement of this sentence ("strongly correlated"). 4/Discussion: P15965, L4-15: would this paragraph have been more useful in the general introduction? P15966, L5 to P15967, L2: UV treatment step might be crucial as well regarding significant differences observed between DCo measurements made by different labs. By this I mean that the UV lamps characteristics and the UV system device can be more or less efficient. Do the authors have also an idea about the Co extraction recovery prior to ICPMS compared to the pre concentration step during FIA-chem. analysis? P15966, L19: remove "for" P15971, L27: remove one of the "the". P15974, L17-L19: previous section begins with the same observation. P15976, L4-5: "In intermediate and deep waters. . .mesopelagic waters", please rephrase P15977, section 4.4.1, any reasons why surface waters are define as the 0-150 m layer? 5/ Conclusions P15981, L16-23, it is also important to mention that eddies and vertical diffusion could both acts as Co

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sinks as well. . . P15982, L13-15, this statement has already be written at the beginning of the conclusion (P15981, L5-8).

Tables: Please be consistent with the number of significant figures reported for each parameter. Table 3: PCo concentrations, considering the standard deviations reported here, the variability of PCo concentrations seems to be high in each water layers considered, is this mainly due to the uncertainties of the measurements? Probably, because concentrations are much lower than the detection limit. This should be mentioned somewhere in the text. Figures: Figure 2: dissolved oxygen (O₂) instead of O₂. Figure 3: legend on each nutrient vertical section (reported as PO₄, NO₃, SiO₂) should be consistent with the description made in the methods section (Phosphate (HPO₄²⁻); Silicate (Si(OH)₄⁻); etc). Figure 5: error bars should be added on DCo and PCo vertical profiles. Axis numbers and dots should be a bit bigger. Figure 6: Axis numbers and dots should be a bit bigger. Any ideas on what could explain that the weakest correlation is observed for samples that have been analysed with the same technique ((f) FIA-Chem; data comparison between Lohan et al., and Dulaquais & Boye)? Figure 7: I have the feeling that this figure is not really usefull, is it? Figure 8: Axis numbers should be a bit bigger.

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