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Interactive comment on “A seasonal study of dissolved cobalt in the Ross Sea, Antarctica: micronutrient behavior, absence of scavenging, and relationships with Zn, Cd, and P” by M. A. Saito et al.

Anonymous Referee #3

Received and published: 15 October 2010

This study by Saito et al presents a first class set of dissolved Co data from the Ross Sea and explores underlying explanations for the relationships between dissolved Co and soluble reactive phosphate as well as the implications for e.g. ecological stoichiometries, and even paleoceanographic applications. The four main conclusions are that Co has a nutrient like vertical profile, it's dissolved concentration correlates with soluble reactive phosphate, there is no evidence of scavenging removal, and that the concentration of complexing ligands in the Ross Sea was insufficient for the concentration of Co leaving a large fraction labile and bioavailable. The discussion is detailed

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and comprehensive, perhaps a little too lengthy, and I would recommend that this paper can be published almost without change but I have a few suggestions for minor modifications or aspects which could be dealt with more thoroughly.

First, I am no expert in Co biogeochemistry but through the paper I found myself feeling slightly confused by the fact that there are actually two biogeochemical roles for Co in marine phytoplankton, firstly as a “cambialistic” metal which can substitute for Cd or Zn into key enzymes. But secondly these marine phytoplankton also require Co in the form of vitamin B12, i.e. perhaps require a threshold amount of Co in this form. The paper of course gives a comprehensive introductory view of these two roles, but I found that the discussion of Co uptake rates and ecological stoichiometries, tended to focus largely on cambralism rather than detailing much of Co requirements in the form of vitamin B12. I understand that the B12 biosynthesising bacteria appear to bloom at a later time than the phytoplankton (why is an interesting question for the Ross sea in itself presumably). Perhaps this is largely because the knowledge of the partitioning of Co uptake as these two forms is unknown but I thought it would have been helpful to guide the reader a little more easily through these two processes and their implications for the results.

Second, I found there was very little discussion of the physical oceanography of the different sites that were surveyed. Clearly many of these sites are from above the deepened continental shelf and I was interested to know the sources of waters which could have been upwelling onto the shelf, and perhaps also cascading off the shelf and whether there was thought to be any exchange of Co with sediments at depth or what the sort of contributions of meltwater may have been in each case. This has been a useful exercise in other sectors of the Antarctic shelf (see Hendry et al., Mar. Chem., 2008) to try and reconcile metal relationships with nutrients. Such physical oceanographic discussion would have lead to more knowledge about what dictated the composition of the deepwaters which were priming the surface waters before the advent of the spring bloom. In particular it would have been helpful to really confirm

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the nutrient like behaviour of Co, by knowing at what time each site became stratified relative to the spring bloom to make a judgment of whether the degree of depletion at each site made sense in terms of degree of biological activity.

Interactive comment on Biogeosciences Discuss., 7, 6387, 2010.

BGD

7, C3335–C3337, 2010

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