

Interactive comment on “Elevated sources of cobalt in the Arctic Ocean” by Randelle M. Bundy et al.

Anonymous Referee #2

Received and published: 14 May 2020

This is an important, well written manuscript. I think it should be published in Biogeosciences, however I have a number of comments that should be addressed first. My comments are listed in the order I came across them in the manuscript, not by importance.

Line 2: Has Co actually every been shown to limit phytoplankton growth in the ocean? Certainly not in many regions? Suggest to modify phrasing accordingly.

Line 40: Again, might be misleading to state that Co has been found to be the growth limiting nutrient?

Line 130: “pressurized filtered air’ – N₂ gas?

Line 131: States collection methods for Co, then nutrients, and then back to Co again.

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Move nutrient sampling to end of paragraph?

Section 2.2: Not clear if samples for Co were acidified for storage or not?

Section 2.8:

- Briefly comment here on how PISCES-v2 performs in terms of physics/ice/rivers/macronutrients/chlorophyll in the Arctic Ocean, as these are key for interpreting the results. - Please state whether Co concentrations regulate phytoplankton growth in the model. - Please indicate whether there are Co binding ligands in the model, what their sources are etc. (as DOM complexation of dCo is inferred as important mechanism protecting against scavenging in the observational data, and could be an important factor contributing to differences with the model)

Lines 355–359: Repetition of how PHW can be identified?

Line 513–516: Perhaps rephrase to make clear that it is dissolution of Mn-oxides with Co bound to it is a Co source (not the Mn-oxide itself)

Section 3.5: There is a lot of discussion in this results section. The discussion including comparison to other regions and hypothesized mechanisms controlling Co distributions should be moved to the discussion section.

Line 553: Insert 'as indicated by dMn concentrations' when referring to shelf inputs?

Line 557: Replace 'diminished' by 'low' (i.e. diminished relative to what?)

Line 559: Which transect is being referred to here?

Lines 620–623: Sentence unclear – please rephrase

Line 679: Rephrase to state that it is a combination of restricted upper ocean scavenging in combination with continued deep water scavenging alongside restricted water mass mixing? i.e. Need to keep clear that there is expected lower scavenging in surface waters as these are argued to be important for leading to the high dCo there? If

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this is not what the author's intended to say then this bit needs a little rephrasing.

Line 706: Rephrase to state that it is the observed increase in dCo over two time points. i.e., seasonal or inter-annual variability could explain this.

Paragraph starting line 736: Can this not be investigated with the PISCES model output? If the model is doing a good job in replicating Co in the Arctic for the correct mechanisms, then would export into the Atlantic ocean not be replicated by the model? If so then the authors can be a bit more quantitative about this statement (e.g. providing an approximate fraction of Arctic-sourced Co in the Atlantic). If the model is not replicating this, then this would still be interesting to comment upon (i.e. either the model or the proposed mechanism is incorrect).

Line 799: Again the authors should stress here that this is just two time points of observations and therefore not enough data to say whether Co concentrations in the Arctic are increasing with time.

Figures 8 and 9: Possible to make the colour bar numbers larger?

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