Review #2

This is an important, well written manuscript. I think it should be published in Bio- geosciences, 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.

We clarified this statement to refer to cobalt's role as a co-factor in cyanocobalamin, which has been shown to limit phytoplankton growth in several regions of the oceans. Cobalt has also been shown to be serially limiting with nitrogen and iron (Browning et al., 2017). We updated this sentence to read, "Cobalt (Co) is an important bioactive trace metal that is the metal co-factor in cobalamin (vitamin B_{12}) which can limit or co-limit phytoplankton growth in many regions of the ocean."

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

We amended this section to read, "Due to its low concentrations, strong organic complexation, and its presence in cobalamin, dCo or cobalamin have been found to be limiting or co-limiting nutrients for phytoplankton growth in several regions (Bertrand et al., 2007, 2015; Browning et al., 2017; Martin et al., 1989; Moore et al., 2013; Saito et al., 2005). Growth limitation can be due to either a lack of dCo, or cobalamin (Bertrand et al., 2012; Bertrand et al., 2007; Browning et al., 2017), as cobalamin is only synthesized by cyanobacteria and some archaea (Doxey et al., 2015). However, many phytoplankton utilize cobalamin for the synthesis of methionine (Yee and Morel, 1996; Zhang et al., 2009), and therefore must obtain it from the natural environment (Heal et al., 2017)."

Line 130: "pressurized filtered air" – N_2 gas?

Correct, the samples were collected using filtered pressurized air and not N_2 gas.

Line 131: States collection methods for Co, then nutrients, and then back to Co again. Move nutrient sampling to end of paragraph?

We have now moved the nutrient methods information to the end of the paragraph.

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

Samples were not acidified. We clarified this in Section 2.2: "...samples were kept refrigerated (4℃) and un-acidified until analysis (Hawco et al., 2016, 2018; Noble et al., 2016). LCo samples were double-bagged and stored at 4℃ and un-acidified until analysis."

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.

The PISCES model has been used extensively to examine global scale biogeochemical cycling. We did not choose here to make an extensive model evaluation study in the Arctic Ocean since this was not the focus of the paper. Instead, the goal was to explore how the model's cobalt sources, sinks and internal cycling performed and how they may help provide additional insight into the driving mechanisms behind the observed distributions. Thus we chose to focus the model evaluation on cobalt rather than a suite of extra tracers that would lengthen the manuscript and distract from its focus. More details on PISCES can be found in the cited reference publications (Aumont et al., 2015; 2017; Tagliabue et al., 2018).

- Please state whether Co concentrations regulate phytoplankton growth in the model.

Co concentrations do not regulate phytoplankton growth in the model. Uptake of Co by phytoplankton in the model is explicitly modeled based on a maximum cellular quota and allows for variable Co/C ratios (Tagliabue et al., 2018).

- Please indicate whether there are Co binding ligands in the model, what their sources are etc. (as DOM complexion 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)

Organic cobalt-binding ligands are modeled and are linked to the relative abundance of nanoplankton in the model as a means of representing the production of cobalamin or pseudocobalamin by picocyanobacteria communities. Co ligands also have a minimum deep ocean concentration in the model to stabilize dCo in the deep ocean and prevent scavenging. A few additional details about the model have been added to the manuscript in section 2.8 based on the suggestions above. Extensive details can be found in Tagliabue et al. (2018).

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

We have shortened this to read, "The PHW can be clearly identified from the elevated macronutrient concentrations (Fig. 2D), and temperature maximum within the salinity range of 31-33 (Steele et al., 2004; Steele and Boyd, 1998) (Fig. 2A, C)."

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).

We have clarified this and rephrased.

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.

We removed several lines of text from this section and combined it with the text in the second paragraph of section 4.1 of the discussion.

Line 553: Insert ‘as indicated by dMn concentrations’ when refereeing to shelf inputs?

This has been inserted.

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

This has been changed to indicate that there is diminished scavenging of dCo in the model relative to other ocean basins (Tagliabue et al., 2018).

Line 559: Which transect is being referred to here?

This has been changed to specify the GN01 transect.

Lines 620–623: Sentence unclear – please rephrase

This has been changed to read, "The interaction between rivers and shelves requires further study, as the shelf sediments might behave as "capacitor" for dCo, accumulating Co from rivers and sinking organic matter and then releasing Co to the overlying water during reductive dissolution in the sediments. Although the mechanism is uncertain, it is clear that the riverine source dominates the distribution observed near the North Pole where dCo and LCo concentrations remain high despite the distance from land, and that organic complexation likely plays a role in the distal transport of this dCo (Charette et al., 2020)."

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

This section has been amended as follows:
"This evidence, combined with the coinciding maxima observed in pCo and pMn, suggest that scavenging occurs in the upper water column, but that additional scavenging continues to occur in deeper waters. The elevated pCo concentrations in the deep Arctic compared to other regions (Lee et al., 2018) suggest that scavenging over long timescales continues to add to the pCo pool. The strong stratification in the Arctic likely prevents high concentrations of dCo from mixing between the modified surface waters, the PHW, and the deep Atlantic water (Steele et al., 2004). Thus, it is likely a combination of limited upper ocean scavenging, and strong stratification between water masses, that keeps the elevated dCo and LCo confined to the surface waters in Arctic, yielding the intense scavenged-like profile of Co in this region compared to other basins (Fig. 3)."

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.

We have updated this sentence to read, "While there is not enough data to state whether the river dCo flux has in fact changed over time in the Arctic and the observed changes could be due to seasonal or interannual variability, several other studies have documented an increase in river discharge due to increases in permafrost melt over time (Doxaran et al., 2015; Drake et al., 2018; Kipp et al., 2018; van der Loeff et al., 2018; Tank et al., 2016; Toohey et al., 2016)."

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).

The downstream impact of the Arctic on the Atlantic Co distribution can be seen in the results of Tagliabue et al. 2018, but were not focused on in that paper. The clearest way to quantify the influence of the Arctic on the North Atlantic dCo distribution would be to "turn off" the Arctic sources of dCo in the model and see how that impacts distributions in the North Atlantic. However, this is not straightforward and fully masking all of the sources of Co in the Arctic is currently not possible in the current model framework. Alternative tests, such as picking experiments that led to strong reductions in Arctic Co levels (e.g. the experiments where bacterial activity did not affect the Co scavenging rate from Tagliabue et al, 2018) are also not ideal as the Arctic signal itself is not isolated.

We also felt we were not able to be quantitative about the Co source to the North Atlantic from observations alone because the transformations and source regions of Labrador Sea water are not entirely understood at this time (Le Bras et al., 2017), so it is difficult to say with the current data what proportional of the high dCo signal seen in Noble et al. (2016) is due to additional dCo sources on the western margin of the US.

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.

We amended this section to make sure it is clear that the dataset is limited. It now reads, "Co was also shown in this work to be increasing over time on the shelf in the Canadian Arctic, possibly due to increases in river inputs from thawing permafrost, though this is difficult to constrain in the present limited dataset. Given the potential increase in Co over time in the Arctic and the modification of low-salinity Arctic waters as they exit the Arctic into the North Atlantic and the Labrador Sea, it is difficult to determine if there is a net flux of Co out of the Arctic and into the North Atlantic, however evidence in this work suggests that the distinct Co waters of the Arctic likely impact downstream micronutrient concentrations. These impacts are likely to become increasingly important in the future, with increased warming and changes to Co sources in the Arctic basin."

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

Yes, we made the labels on Figures 8-10 larger.

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