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

Anonymous Referee #3

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Review of 'Elevated sources of cobalt in the Arctic Ocean' by Bundy et al.

The authors present novel data from an understudied region. The data appears of high quality, is definitely very interesting and as such should be published. Generally the paper is well written, but the methods and results section seem quite long. I like that the methods are detailed, but given that most methods have been described before I wonder if all details are required here and if the methods section could be condensed. The results section already contains quite some discussion. This should either be moved to the discussion, or perhaps a combined results/discussion section is more suitable for this paper (i.e. where the actual results section only briefly describes the basin wide distribution). The number of figures is also quite large and I encourage the authors to reconsider if they need them all as the sheer volume of data presented is a

bit overwhelming.

Generally, the discussion is interesting, but I thought it was strange that work already published from this cruise (e.g. Jensen et al., 2019) or the German/Dutch GEOTRACES cruises (both 2015 and 2007 GEOTRACES IPY) is barely (or not at all) discussed. There is already quite some work that provides very nice context for the current study, e.g. the work on Fe and Fe binding ligands in the TPD from the GN04 transect, the Jensen Zn study or the Mn and Fe work from the GIPY 11 cruise (data in IDP). The same can be said for the comparison with the Atlantic where the comparison is made with data quite far south along the zonal GA03 rather than the also available meridional GA02 data that seems more relevant for the discussion of advection into the Atlantic.

In the section on correlations, mixing as a driving factor in such correlations is ignored (see also specific comments) and this is something that in my opinion should be addressed. The comparison with the data from 2009 is definitely interesting, but way overstated. Most importantly, 2 data points in time are not proof of a trend. Moreover, there could be seasonal variation (as it appears the stations were not occupied at the same time/season) and the stations locations are actually quite far apart. Notably the width (area) of the adjacent/closest shelf is very different and stations were positioned ifferently with respect to fluvial input and the Bering Strait. In this study it is argued that the shelf is the most important source of Co. Thus higher concentrations in the vicinity of the large shelf area of the Chukchi Sea and Bering Strait compared to the narrow Canada basin shelf are maybe not that surprising and this needs to be explored in context of the local hydrography and currents. I also noted (based on Fig 9) that the role that bacteria play differs between the regions for the 2009 and 2015 data.

The conclusions were not completely appropriate for the current ms. The data/ms did not show at all that the Co distribution has implications for Arctic ecosystems and it is unclear how the observation of a unique Co distribution affects future changes in micro nutrients. As stated above, I do not believe that Co was shown in this work to be increasing over time. The idea that (changing) conditions in the Arctic affect the North Atlantic Ocean downstream is not new and this should be acknowledged.

Specific comments 40-43 limitation by cobalamin does not necessarily imply Co limitation as cobalamin production can be low regardless of the Co levels. So most of the cited studies do not demonstrate Co limitation.

76-80 it is stated there are regionally specific features, but the examples are not really specific regional features. Perhaps rephrase?

92 awkward sentence, please rephrase

94 what is meant with 'interpreting the role of external sources and internal cycling to the distribution'?

109 was sampled

131 can you compare filtered and unfiltered samples? Perhaps state this will be addressed later in the ms

295 this detection limit is at least an order of magnitude too high for open ocean Mn, notably in the deep. Is it a typo?

324 Fe was already defined

389 given that the data from the Canadian geotraces cruise was unfiltered, I do not think it is appropriate to call is dissolved (dCo)

408 how is the % sea ice melt determined?

427what is the % Pacific water based on?

427 here and elsewhere, the number of significant figures for Co concentrations does not seem to match the reported precision.

470 awkward sentence, please rephrase

471 what does 'that' refer too?

508 confused, 'capture the major processes contributing to modeled sources and sinks' not sure what is meant here.

516-517 Jensen et al 2019 argued that low oxygen in the sediments plays an important role for Zn and evidence for denitrification in the sediments was presented. This should also affect Co despite the fact that oxygen is not low in the water column. If denitrification occurs in the sediments, isn't it likely that also reductive dissolution of sedimentary Mn-oxides occurs? (however this discussion seems out of place in the results section)

516-534 this section is not as clearly written as the rest of the ms (specifically the last sentence was impossible to follow for me). Perhaps this can be remedied?

540-554 There is a problem with this section as for the Arctic (but also elsewhere, e.g. Aguilar-Islas A. and Bruland K. W. (2006)) it has been demonstrated that Mn in the surface of the open ocean basin is mainly derived from fluvial input, not sediments (Middag et al., 2011 (doi:10.1016/j.gca.2011.02.011). The latter study was not the exact same region, but fluvial input will be a strong source of Mn in this region too, and this needs to be discussed. However, Zn (Jensen et al., 2019) has been shown to have an important sedimentary source and might be a better proxy?

571 discussion of the recent TPD paper here seems appropriate (https://doi.org/10.1029/2019JC015920) as well as some other recent Fe work (https://dx.doi.org/10.3389/fmars.2018.00088; https://doi.org/10.1016/j.marchem.2017.10.005; https://doi.org/10.1029/2018JC014576)

572/573 'track shelf inputs due to interactions between the sediment-water exchange processes' quite vague, not sure what this means/implies

582-584 What about deposition of riverine Co in the shelf sediments and subsequent remobilization?

590 also argued for Fe (https://doi.org/10.1016/j.marchem.2017.10.005; https://doi.org/10.1029/2018JC014576)

597 see mentioned refs for humic-like substances and Fe in the TPD, seems very relevant here.

662 what is meant with depth here? I assume the slopes are determined per station and the depth is the station depth or am I wrong? Please clarify

654-670 there is a growing body of work demonstrating mixing and water mass circulation is a primary factor in driving the slopes of metal-nutrient relationships that is ignored here while the mixing of Pacific and Atlantic origin water could have a strong effect (e.g. Vance et al., 2017, doi: 10.1038/ngeo2890; de Souza et al., 2018, doi:10.1016/j.epsl.2018.03.050; Middag et al., 2018, doi: 10.1016/j.epsl.2018.03.046;Weber et al., 2018, doi: 10.1126/science.aap8532; Middag et al., 2019, doi:10.1029/2018GB006034; Middag et al., 2020 doi: 10.3389/fmars.2020.00105).

676 continues?

702 after a 40% correction, the 2015 data is 400% higher than the 2009 data. This seems to be in contrast to line 688 where it is stated that without correction the 2015 data is 3.5 times higher.

703-704 could there be a factor of seasonality (did the sampling occur in same time of year relative to start of ice melt and river discharge)? And 2 data points in time hardly makes a trend! The difference is interesting for sure, but currently the significance is really overstated, as there is no way of telling what the Co concentrations were in other years. What about the enormous difference in the size of the nearby shelf regions between the 2 expeditions?

719-720 an increase in fluvial discharge as well as timing of ice melt could also affect primary productivity on the shelf and thus sedimentary oxygen conditions and Co sup-

ply from the sediments (similar to Zn; Jensen et al 2019). And what about increased SGD, could that play a role?

725 I have some issues with this section. First, it is very odd to compare only to the zonal Noble et al. study when in this discussion the comparison to the meridional Dulaquais study would make much more sense as that also has observations much closer to the Arctic (and also states: 'the LSW was characterized by relatively high DCo concentrations'). This data is available from the IDP. Moreover, LSW is not the only water mass of Arctic origin, also the deeper components of NADW are of Arctic origin (Denmark Strait Overflow Water and Iceland-Scotland Overflow Water). So if LSW is elevated in Co due to its Arctic origin, why is LSW elevated relative to ISOW and DSOW that are also of Arctic origin? This needs to be addressed.

749 not sure what the T-S plot shows/adds or how it supports the hypothesis; basically it shows that dCo is lower in LSW than in the source waters, but you do not need a T-S plot to show this.

773 where does the Zn data come from? According to the caption it is from this study, but this is the first mention of it. Again the comparison to the GA03 section rather than the more relevant GA02 section is very odd in my opinion as all data is accessible in the IDP and provides data (and insight from the associated publications) much closer to the Arctic. I really urge the authors to make use of the data (and insights) available from the international GEOTRACES efforts.

779 quite similar. What is this statement based on given that the medians are more than a factor of 2 apart? I see there is considerable overlap, but not sure if 'quite similar' is the observation all readers would make based on the presented graph. Some explanation seems required.

781 Bit of a jump from Co to total metal concentrations. For metals with different biogeochemistry this might be different and an increase in fluvial supply in the Arctic (of e.g. scavenged Al) might have no consequence for transport to the Atlantic.

783-785 do not follow this sentence; the total inventory of Zn is small compared to Zn? And why is the Jensen et al., 2019 only briefly mentioned here? As indicated above, the comparison to the cycling of Zn would have been relevant elsewhere in this ms too.

791-792 This ms has not demonstrated there is any influence of the Co distribution (or the changing Co concentrations) on the Arctic ecosystem, just that Co concentrations could be changing. Moreover, given that Co concentrations are high, I fail to see how a further increase in Co is affecting the ecosystem. And how does the unique Co distribution affect future changes in micro nutrients?

799 as stated before, this cannot be stated like this based on 2 data points in time!

805 similar interpretations were also invoked based on e.g. the micronutrient distributions along the GA02 section (e.g. Cd, Zn, Ni, Fe and Fe binding ligands, Co). I do not mind this is not a completely novel finding, but it is appropriate to acknowledge this idea was postulated before and in fact could strengthen the case for this study on Co.

Not all figures have units on the axis (color bar fig 8, y axis fig 11)

The cited references in the text are not all in reference list

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