Chmiel and coauthors present total dissolved Co and labile Co along the GP15 transect, in Pacific Ocean. As highlighted by the authors, this dataset is both one of the largest datasets to date for Co and covers a largely under-studied region, which spans distinct zones of interest for the cycling of Co and other trace metals (OMZs, hydrothermal input, etc.). I find the data to be of high quality and the interpretations well-reasoned, and I am sure this manuscript will be of interest and impactful for the community. I am happy to recommend this manuscript for publication with minor revisions, and I make the following suggestions that the authors may wish to take when revising their text.

The following is organized first into more broad discussion points/comments, followed by minor line-specific suggestions:

Section 4.1: The impact of advection

I think this section needs to be framed a bit more carefully to consider that, across wide depth and spatial scales where different water masses are present, the features will also be strongly influenced by advection and mixing. It is clear that this strongly impacts the basin-scale and depth profile features of other metals, rather than/in addition to 1D vertical control. This is mentioned briefly in the middle (near Line 440), but I think it would be better to start the discussion with this, and then address how other factors on top of this influence Co. To aid this, where would AAIW, NPIW and CDW fall within Figure 8? Figure 8 Panel A is useful, but in a way it assumes that all water started at the center, with uniform dCo and PO4. So that could be better contextualized by first knowing where the different water masses "start", at least at the point that they arrive in the transect

The reader could also be reminded of the short residence time of Co (at least in the surface ocean) compared to global circulation, to justify why circulation might not always be the dominant control. This topic comes up a bit in section 4.7 as well, where the "curl" in dCo v PO4 can be partly explained by water masses.

Section 4.3: Hydrothermal sites & dCo

The authors make use of previously reported hydrothermally-enriched metals and other tracers (Fe:^{3}He &Mn:^{3}He) to infer hydrothermal input of Co, and determine the regional importance of this. I have some hesitations regarding this interpretive structure and the final conclusions

- I am not sure the metal:^{3}He ratios presented in Jenkins et al (2020) are as well-constrained and conservative as the authors suggest (e.g., near Line 550). Jenkins et al. Fig 4 shows metal:^{3}He varying by a factor of ~2 within 100 m depth range, a similar offset observed by the authors between the apparent hydrothermal maximum in their dCo cast and the ^{3}He cast. In addition, Figure 4b from Jenkins et al does not appear to show conservative Mn mixing in the plume, but rather elevated dMn independent of hydrothermal endmember (3 points near 6 nM Mn) and an open ocean endmember at low ^{3}He and low Mn. In addition, high plume heterogeneity within different distinct plumes of vent sites has been reported elsewhere in the ocean (Kleint et al., 2019). As the Loihi site amalgamates different distinct venting sites into the sampled plume, shifting position in the plume might be expected to alter the exact geochemical conditions. One more point, though I haven't seen this published, so maybe it is of limited use. But I mention it to note that others in the field are trying to solve this problem, and I'm not sure there is a clear solution yet - Maeve Lohan presented a talk at Ocean sciences 2020 discussing the difficulties in matching standard CTD ^{3}He and trace metal casts (CT31A-01, but the abstract doesn't cover much of this).

  I suggest revising the assumption of consistent metal:^{3}He across different specific venting points and making the discussion a bit more conservative. The uncertainty in reconstructing Co fluxes should be revised to include the extrapolated uncertainty/variability in Me:^{3}He. I think this section of the text still contains important information, as does the manuscript as a whole, so making this less quantitative should not alter the value of the section or manuscript overall.

- The dCo:^{3}He regression (Figure 11) reaches zero ^{3}He above the general deep Pacific background dCo (regression dCo ~2x deep Pacific dCo). Why is that? Does this indicate a second potential source of Co (e.g. Co scavenged from the plume, delivered to nearby sediments, and then an elevated flux of dCo out of vent-adjacent sediments?
- It seems the GP15 transect as a whole did not track the Loihi hydrothermal plume well. Based on Jenkins et al. (2020), the only stations that seem within the plume are 18, 19, PR & LS (their Figure 5). Elevated dCo is observed at stations LS, PR & 19, and station 18 also shows elevated lCo (and maybe also a bit of elevated dCo). The absence of hydrothermal Co outside of these stations seems expected based on the results of Jenkins et al indicating the absence of hydrothermal influence elsewhere along the transect. Also, the plume maximum depth (1100) wasn't sampled at other stations. Therefore, I don't think the necessary data are available to speak to the longevity of dCo within the plume, either for or against it. Rather, it seems that the cruise simply didn't sample the more distant plume. Again, I don't think revising the text in this way alters the significance of the section or text.

Sections 4.4 & 4.5: dCo in OMZs and implications under OMZ expansion

The authors assess the role of OMZs in enriching dCo, including variability within different OMZ regions, and quantitatively estimate the impact for future Co cycling. I found this section quite interesting, and I have the following suggestions for the interpretive framework and quantifications.

- OMZ dCo-O_2 regressions (Figure 12 and elsewhere): I don't think this [O_2] window does a good job of isolating the affects of OMZs. The plots are dominated by the behavior of dCo in largely oxic waters, which are mostly widely removed from OMZ waters. This is also visible in the dCo v O_2 distribution, which is non-linear across this range and instead seems to show a steep trend at low O_2 and a shallow trend at higher O_2, while dCo largely plots below the linear regressions in the middle of the range. The Hawco et al., 2016 data also show this. Therefore, the derived slope may be strongly biased by mixing/circulation, and may not give the most accurate information for projecting Co accumulation with decreasing O_2.

  Mn redox cycling also supports the choice of a different O_2 window – if the suppression of Mn oxide formation and reduction of Mn oxides is an important factor for dCo, then the range of O_2 values should reflect what is most relevant for dMn. This probably isn't from 200-100 µM, and instead is maybe <100 µM O_2 (or maybe even lower – the kink/bend in the plots seems to fall near 50 µM O_2, with possibly a second one near 100). Therefore, I think a more selective approach focusing on OMZ and immediately adjacent waters would give more meaningful results here.
- Differences across different OMZs: I appreciated the discussion of differences between the OMZs observed along this transect. I think this is important in understanding and projecting Co cycling as a whole. I think this discussion could benefit from better incorporation of our understanding of the cycling of other trace metals in these environments, and specifically those most relevant to Co, to strengthen a mechanistic understanding.
  - The trend between Co and O_2 is less meaningful in the subarctic OMZ. Data for dissolved Mn here suggest that the dMn peak is not at the O_2 minimum (e.g. Martin et al., 1989; Sim & Orians, 2019). Therefore, I am not sure that a priori assumption should be a coherent linear relationship between dCo and O_2. Instead, it would involve one factor contributing to a dCo peak above the O_2 minimum from pMn reduction, and a second in the O_2 minimum from the respiration of biogenic material. And in this case, across a wide OMZ, perhaps the assumption of a linear trend isn't as applicable. This is addressed somewhat, but I think could be strengthened by bringing in more of this sort of supporting data. Ohnemus et al. (2019, see, for example, Table 3) might be relevant here for building that interpretive framework, and also could be helpful elsewhere in the manuscript.

- There is elevated pMn surrounding OMZs. How might this affect the potential of dCo to escape OMZ settings?

Other general comments

Detection limits & data treatment

- I realize that it is tough to define a detection limit off of n = 2 blanks, but choosing the only blank with n = 3 to define the detection limit results in one of the lower possible detection limits. That optically isn't the best I think, and I think it would be better to use variable detection limits throughout the cruise, with samples compared against the relevant set of reagents for that analysis.
- Lines 172-173: I appreciate the authors being careful here, but the uncertainty ranges of these two overlap and therefore I'm not sure it makes sense to discuss them as different.
- Line 343, 380: It should be noted here that these values are below the detection limit, and maybe it is better not to quote a number for concentrations below the detection limit since that number isn't meaningful. This shows up a couple other places in the manuscript as well (e.g. labile dCo in deep water).

Colloidal Co: Throughout the text it is assumed that the difference between labile and dCo is due to strong organic complexes. Perhaps I'm a bit ignorant on this, but do we know to what extent colloidal Co would show up as labile v. total dissolved? If that's known, it might be good to remind the readers of this somewhere. And, if it's not known, maybe colloidal Co could be important in some sites (e.g. the hydrothermal vent site, where other metals are known to be enriched in colloidal phases). This is also where labile:dCo is different than throughout the transect.

Section 4.6 depth windows: I don't think the depth integration here, giving units of Co per m^2, is the best approach. This could be strongly biased by variable depth: at the equator, the depth range integrated here is about 3000 m, while is is about 6000 m at 12 N. I think turning this depth integrated value back into an average (i.e. a depth-weighted average) would give a more meaningful comparison. Likewise, starting this window at 1000 m may be problematic, as it catches the core of the N Pacific OMZ but is below the OMZ in the subtropical Pacific. These OMZ-specific trends already have two discussion sections, and here the focus seems to be more on the stable dCo in deeper waters. Maybe starting the depth window at 2000 m would better isolate trends in deep water.

Line-specific comments:

Line 19: Approximately what depth range counts as the "upper ocean"

Line 20: I suggest using mol:mol ratios rather than mol/l:mol/l

Line 24: Later in the text "potential xs^3He" is used rather than "estimated xs^3He". It might be better to consistently use only one.

Lines 59-60: I suggest "species" instead of "speciation".

Lines 143-147: I think it would be useful here to say slightly more about this offset – specify which method gives higher and which gives lower, and why that might be. Or if it is not consistent, that could also be stated.

Lines 236-237 & Lines 536-548: I think small movements in ship position and/or heterogeneity in the hydrothermal plume should be considered. A thousandth of a decimal degree is about 100 m, the same distance quoted as possible due to wire angle. Also, I think Lines 536-548 could be shortened a bit, to simply say that the casts seemed to catch different parts of the plume, and given the depth and local heterogeneity, many different explanations may exist for why they were different.

Lines 365-374: It might be more useful to compare with the Southern Ocean instead of the Atlantic, since the Southern Ocean is more proximally the source of North Pacific deep water.

Line 477: It is a bit ambiguous what the respectively refers to, and from my reading I would think it was slope error and R^2 rather than stations 2 and 3 (but what is shown appears to be R2 for stations 2 and 3).

Lines 492-495: The Peru margin is quite a different setting (strong OMZ), and therefore might not be the most directly applicable to this site. Data from an oxic setting might be more useful for a comparison.

Lines 499-503: de Carvalho et al (2021) could be a useful reference here.

Line 525: Does "Co distributions" refer to dCo or labile dCo or pCo, or all three?

Line 528: "Metallic" sounds a bit funny to me.

Line 588: I'm not sure I understand why a negative correlation is expected. If there is a dCo source from the vent, wouldn't a positive correlation be expected?

Line 682: It's not clear to me what a volumetric width is. Maybe the calculation can also be described in a bit more detail (perhaps in a supplement, with an example figure)

Lines 726-746: These two paragraphs are a bit redundant, and can maybe be combined into one paragraph.

Lines 806-808: It is probably both of these, and not just one or another, as the rest of the discussion section and figures identify specific examples of these two. In that respect, I think it is useful to have this short discussion section addressing the models, but I think it could be streamlined a bit. It is hard to have a detailed discussion of the differences between models and this dataset without explaining the model in more detail (which isn't the purpose of this manuscript). I recommend spending less time discussing detailed differences between the model and the new data, and center the discussion on highlighting how this dataset can aid future models improve based on the new constraints on the Co cycle discovered here. This mostly applies to the final paragraph, for example where the initial presentation suggests that dCo is more scavenged in the Pacific than expected (in contradiction to the earlier discussion), but rather it is just that the model used a different parameterization than these new data would suggest (i.e. opportunity to improve the model).

Lines 860-868: I think it is reasonable to include this, but the transitions to and from this section are a bit abrupt, and this topic wasn't addressed anywhere else in the text. Maybe it could be moved or somehow incorporated better, so that it doesn't break the flow of points before and after (as built up by the rest of the manuscript).

General comment on figures: It would be nice if the figures could be a bit more internally consistent, and a bit more easy to differentiate. A generally consistent color scheme is used throughout the figures, but this is applied inconsistently to the parameters plotted. For example, Figure 5 uses teal to mean dissolved Co while the other figures use blue to signify this, and teal to indicate labile cobalt.

Figures 3 & 5: I think adding station numbers above the top panel would be helpful.

Figure 4: It would be good to include visual color legend somewhere, even if it is a bit intuitive. Also, I find this figure a bit hard to read. Maybe the figure would be easier to read if the surface box was somehow expanded (either it took more space, or it covered 0-500 m instead of 0-1000). Also, it would be nice if the scaling for the upper 1000 m was the same for the deep stations as it is for the partial stations (7, 9, 13, 15, 17, 20, 22, 34, 36, 38).

Figure 5: It would be nice to include the latitudinal zones here, as with figure 3. It also seems like the Loihi Seamount is missing here. Also, the order of panels (PO_4 and O_2 specifically) is reversed compared to Figure 3. Finally, maybe this could be combined with Figure 3 to make an 8 panel figure.

Figure 6: The y scaling is different in each of these three panels. Maybe the bottom two could use the same range.

Figure 7: I don't think the 1:1 line actually shows a 1:1 relationship. It looks like 120 pM labile Co is less than 100 pM total dCo on this line.

Figure 9: Panel J is a great addition! Maybe the other panels could be improved by writing the approximate depth covered by this isopycnal range in parenthesis after the isopycnals. This can be re-created by going back and forth between the panels, but having it written would be nice.

Figure 10: Please use different colors for Mn, Ti and Al compared to each other and what is used for Co. Also, the ranges of LPT and SPT are different, but this isn't obvious and it gives the impression that LPT is approximately the same as SPT. If it is necessary to use different ranges, please make this more clear, and maybe differentiate the ranges so the data don't overlap.

Figure 11: Here dCo is presented as purple (panel A), compared to blue in most of the other plots.

Figure 13: I don't see corrected pCo on panel C. The color choice here could create some confusion with labile dCo. Also, it might be nice if Panel D didn't use the Co color scheme, and if the different ranges could be somehow better differentiated (as with Figure 10). As it is now, it gives the initial impression that LPT POC = SPT POC most of the time.

References:

de Carvalho, L.M., Hollister, A.P., Trindade, C., Gledhill, M. and Koschinsky, A., 2021. Distribution and size fractionation of nickel and cobalt species along the Amazon estuary and mixing plume. *Marine Chemistry*, *236*, p.104019.

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Martin, J.H., Gordon, R.M., Fitzwater, S. and Broenkow, W.W., 1989. VERTEX: phytoplankton/iron studies in the Gulf of Alaska. *Deep Sea Research Part A. Oceanographic Research Papers*, *36*(5), pp.649-680.

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