

## ***Interactive comment on “Radium-228-derived ocean mixing and trace element inputs in the South Atlantic” by Yu-Te Hsieh et al.***

**Anonymous Referee #1**

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Hsieh et al (2020) use radium-228 to derive vertical and horizontal mixing rates of trace elements in the South Atlantic. These calculations improve our understanding of trace metal cycling in this part of the ocean, and this manuscript is therefore an important contribution to the field. However, the manuscript could be improved by clarifying when and where certain model assumptions are applied, and by considering some suggested changes/clarifications to the box model calculation. Additionally, I have concerns about the curve fits used to calculate the vertical mixing rates that should be addressed before publication. My comments and suggestions are detailed below, divided by section.

Introduction:

The introduction shifts back and forth from talking about the Southern Atlantic to talking  
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about nutrient limitation more generally. The authors may wish to re-organize the text so that it starts more broadly and then focuses on the South Atlantic to introduce this specific study. In particular, I recommend moving the second paragraph (lines 41-48) farther down (perhaps making it the second to last paragraph instead), so that there are not two separate “In this study. . .” statements.

Line 68: the first reference for continental shelf inputs should be “Rutgers van der Loeff et al.”, not “van der Loeff et al.”

Methods:

It is not clear where each of the cruises started and stopped. Both are described as following a 40 deg S transect, but it is not clear how much overlap there was. It would be helpful to color code the lines on Figure 1a to show each of the cruise tracks (perhaps one color to show JC068 and another color or dashed line to show overlapping sections?).

Line 80: What about the trace metal data? Were all elements measured on both legs?

Line 103: The authors mention that a separate sample is collected for Ra-226 measurements, but do not explain why the larger volume samples cannot be used for this measurement. Please add an explanation of cartridge collection efficiencies and the reason for a separate Ra-226 aliquot.

Three different collection Ra methods are described. Was any intercalibration between methods conducted? (e.g. collecting samples at the same depth using different methods?)

Line 121: The authors explain that the Ra-226 aliquots are spiked with Ra-228. How large is the spike, how can you be sure that no seawater Ra-228 contributes to the “spike” signal? The authors mention that chemical blanks are monitored throughout the procedures, but it is not clear whether this is a seawater sample or a Milli-Q blank. I understand chemical blanks to be reagents only, not including seawater background

activities.

Line 128: The comma after CRM-145 is unnecessary and can be deleted

Line 138: If using a global dataset, why not include more recent GEOTRACES data as well? Alternatively, did the authors consider using an Atlantic-specific trend rather than a global trend? The Ra-226 – Si relationship can vary by basin.

Line 159: Change “on the decade timescales” to “on decadal timescales”

Line 165: The authors state here that vertical mixing could affect horizontal distributions of Ra-228, but that the sample resolution is not good enough to account for this input, and they therefore ignore vertical mixing. This is at odds with the section of the paper where they explicitly use the vertical distribution of Ra-228 to calculate vertical mixing rates. It is not clear to me how they can argue that vertical mixing is insignificant in one case, and the main control on Ra in the second case.

Line 173: This sentence is confusing as written (too many commas) and should be re-phrased.

Line 176: Why is the Ra-228 background determined from the mid-water column? If this is being used to calculate horizontal mixing at the surface, a more appropriate Ra-228 background activity would be the surface water activity in the central South Atlantic (perhaps the ANT XV/4 surface activities? Or GEOTRACES data from the central North Atlantic could also provide a comparison)

Line 185: This sentence states that the two scenarios (mixing only, diffusive only) are used to bracket the range of estimates. However, it is stated in line 193 that the diffusive only case is used nearshore, and the mixing only case is used past the shelf-break. Throughout the methods section, it is confusing which assumptions are applied in which environments (e.g. when vertical mixing is ignored, or when advection is ignored)- perhaps it would help to structure the methods section by geographic region rather than by the individual assumptions?

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I am not entirely convinced that an advection-only scenario is valid. In other words, what is the basis for ignoring diffusive mixing? Are there other studies that have used Ra-228 in this way before? To the best of my knowledge, most other Ra-based studies ignore advection rather than diffusion.

Results:

Line 214-215: delete “with expectations based on” so the sentence reads “are consistent with GEOSECS and TTO observations. . .”

Line 213: Are the authors specifically comparing the vertical profiles with Southern Atlantic GEOSECS and TTO stations here, or the entire Atlantic? If the entire Atlantic, the Charette et al (2015) North Atlantic GEOTRACES data should also be referenced.

Discussion:

Vertical mixing:

Does the exponential curve fit for Ra include any of the data in the surface layer, or does the curve fit start below the mixed layer (Figure 6)?

Station 2 does not have sufficient Ra data (only one point) below the mixed layer to fit a curve. This station should be removed from the vertical mixing analysis.

Some of the linear regressions on the trace element vertical profiles start below the mixed layer, while others do not (Figure 4). Why does this vary from station to station? In particular, the Zn data has particularly poor linear fits, and it is not clear whether all the data were included in the fit (the deepest sample at all three stations are below the end of the dashed line).

Box model/trace element inputs:

Why are the calculations of Ra/TE fluxes put in an appendix? I recommend moving these into the main text, as they form the basis for the conclusions of the paper.

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Line 305: change “may not be unreasonable” to “are reasonable”

Lines 309 – 311 are repetitive with line 299-301

Line 319: delete “in fact” so the sentence reads “which is very close..”

Line 321: I recommend saying that the Fe fluxes are “slightly lower than” previously reported fluxes, instead of saying that they “compare well with” these other estimates, as they are an order of magnitude lower.

Line 328: I don’t understand why these estimates must represent a lower bound for what is expected in the South Atlantic. If the concentrations of Zn are lower in the Southern Atlantic, wouldn’t a lower flux be expected, making these reasonable estimates rather than lower bounds?

Line 335-340: The Southern Ocean Fe fluxes reported by Dulaiova et al (2009) are almost exactly equal to the winter mixing Fe fluxes in the North Atlantic reported by Achterberg et al (2018). However, the authors state that the Fe fluxes they calculated are similar to Dulaiova but lower than Achterberg.

Line 363: stating that the integrated timescale is “relatively short” makes it seem like the half life of Th-234 is too short to capture changes in POC flux. It could be the opposite, that the biology is changing faster than the Th. I suggest removing “the integrated timescale is relatively short” so the sentence reads “given that the mean life of Th is 35 days”. This way both possible conditions are accounted for.

The loss via sinking out of the surface box is estimated using <sup>234</sup>Th-based POC fluxes, and TE/C uptake ratios in phytoplankton. As I understand it, using the TE/C ratios in phytoplankton aims to isolate the flux from biology. Why not estimate the total flux? The box model sources include inorganic inputs, so the total source from sinking particles should be considered, not just the organic flux. This can be done by multiplying TE/<sup>234</sup>Th ratios by Th fluxes, rather than adding the extra step of converting to C in the middle. TE/<sup>234</sup>Th ratios are available for the North Atlantic (Hayes et al., 2018)

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and South Pacific (Black et al., 2019). If it is not possible to find TE/<sup>234</sup>Th ratios for all metals, perhaps this can at least serve as a comparison to the biology-based fluxes for the metals where data is available.

In Hayes et al. (2018), the total Fe flux is significantly smaller than the biological flux estimated here; perhaps the lab-based Fe/C ratios over-estimate the actual ratios in this region?

The depth horizon for the surface box is not mentioned in the text, but is noted as 50 m in Figure 8. Why was this depth chosen? Please add an explanation to the text in addition to noting the depth in the figure. Particle fluxes can vary greatly depending on the depth chosen- see Buesseler et al. 2020 for example.

In the conclusion (line 383) and abstract the authors mention that particle inputs may need to be accounted for to close gaps in the mass balance. What are some examples of these possible particle sources? Is dust deposition not considered a particle source? It seems like some particle inputs and outputs are considered in the box model but not others.

Figures:

Figure 1a: It would be helpful to add “Cape Basin” and “Argentine Basin” to the map, as these locations are referenced throughout the text.

Figure 1a: “GEOSEC” should be changed to “GEOSECS”

Figure 1b: The colorbar extends over a much larger range than necessary; as far as I can tell, there are no samples with a salinity below 34. Shorten the range of the colorbar (e.g. 34 – 36) so that changes in the study area can be more clearly seen. I also recommend using a different color scale- this one has non-linear changes in both hue and brightness that make some gradients appear sharper than others (and will not print well as black & white). The highest peach values can also be misconstrued as orange (falling lower on the color scale between yellow & red, as opposed to being

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read as the highest values).

Figure 4: The caption says that dashed lines show linear regression trends with the uncertainty. It's not clear how the uncertainty is shown- should there be shading around the line? Or are the authors referring to the equations next to the lines?

Figure 4: Why do some of the lines not extend through all of the data? (e.g. panels g and j)

Tables:

Table 1: On my PDF, it appears as if half the caption is above the table and half is below. I am not sure if the authors intended to put some information at the end (information about extrapolated Ra-226 activities & errors), but this should be included in the top caption.

Appendix D:

Line 400: The end of this sentence is missing.

Line 444: What two surfaces are the authors referring to?

References:

Buesseler et al. 2020. "Metrics that matter for assessing the ocean biological carbon pump." *Proceedings of the National Academy of Sciences*. DOI: 10.1073/pnas.1918114117

Hayes et al. 2018. "Flux of Particulate Elements in the North Atlantic Ocean Constrained by Multiple Radionuclides." *Global Biogeochemical Cycles*. <https://doi.org/10.1029/2018GB005994>

Black et al. 2019. "Insights From the <sup>238</sup>U-<sup>234</sup>Th Method Into the Coupling of Biological Export and the Cycling of Cadmium, Cobalt, and Manganese in the Southeast Pacific Ocean" *Global Biogeochemical Cycles*. <https://doi.org/10.1029/2018GB005985>

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-377>, 2020.

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