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# *Interactive comment on* "Mercury distribution and transport in the North Atlantic Ocean along the GEOTRACES-GA01 transect" *by* Daniel Cossa et al.

**Anonymous Referee #1**

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Review of Cossa et al. "Mercury distribution and transport in the North Atlantic Ocean along the GEOTRACES-G01 transect"

General comments: This manuscript presents a new set of high quality measurements in an oceanographically important region. These data add to our understanding of variability in oceanic Hg concentrations across different water masses. In general, I find the work to be high quality. I have a number of suggestions for improvements/corrections that are detailed in the specific comments. Most importantly, the authors make a number of assumptions about processes and mixing in their eOMP, leading to a statistical estimate of source water contributions, which are later used to infer anthropogenic in-



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fluences and fluxes across major regions. They contrast their approach with "modeling" estimates throughout the manuscript but their approach is also a modeling estimate (albeit a statistical one) and based on a measurement snapshot and should be acknowledged as such. I would like more information on the specifics of how this analysis was performed, details of the equations and specific mixing assumptions, and implications of any assumptions for results. For example, the authors ignore the influences of Hg losses through evasion etc. throughout much of their discussion and analysis and I wonder about this implications of doing this. The conclusions about anthropogenic Hg changes seem tenuous to me and should be more clearly justified in the revised paper.

Specific comments:

Abstract, Line 18: What about the other 3 percent? Why not just present the full range and some measure of central tendency?

Line 21: The particulate fraction in this work seems higher to me than in other ocean regions and the authors may want to comment on this.

Line 43: Paper by Alex Poulain's work in NGS contradicts this statement – might want to acknowledge and caveat.

Line 46: "ocean-atmosphere exchange" is important for the global biogeochemical cycle of Hg but I don't think one can say that it "dominates"

Line 48: Neither of the papers cited is about the atmosphere.

Line 49: Seems important to mention Hg redox cycling and losses through evasion here.

Line 53: There are a number of other estimates of anthropogenic Hg impacts on the ocean that should be acknowledged.

Line 56: As per comment above – I don't think doubling is an appropriate representation of the consensus in the literature or lack thereof. This is a relatively controversial topic and not directly relevant to the work presented here so the authors may simply want to omit these numerical statements. The review by Amos et al. (2015) cited later in the manuscript also explicitly discusses the range in oceanic enrichment suggested by recent work and agreement of such ranges with measured concentrations.

Lines 60-63: This seems like an odd rationalization for this work since most policy makers probably don't even know what "ocean water mass" means and certainly this is not at the forefront of discussions. I would prefer to see scientific objectives and goals since this is not intended as a policy friendly paper.

Lines 65-66: Grammatical problems. I don't think "solubility pump" is appropriate here since Hg solubility is not inversely proportional to temperature in the same way as $CO_2$ – why not just make a direct statement about thermohaline circulation/advective transport since this is what they mean.

Lines 71-72: I don't think it is only GEOTRACES that has produced these results. Please rephrase.

Lines 79-84: Would be helpful to have an overarching objective for the work, i.e., how biogeochemical variability in the oceans affects total Hg concentrations. These reads like a list of unrelated tasks.

Line 131: I think this should be "...long time since ventilation" rather than "negligible atmospheric exchange"

Lines 132-133: I think the authors are simply trying to say some Hg is transported advectively with ocean circulation rather than it "follows CFCs" – CFCs are simply unreactive tracers for circulation so it is misleading to imply they interact with Hg at all and that could be interpreted from the current phrasing.

Line 137: I think the phrasing here is also problematic because CFC concentrations are not fixed and depend on water mass mixing, time since ventilation and concentration of CFCs in the atmosphere at the last ventilation period. I would therefore recommend





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that this refer to a "past" condition/measurement from a given region

Line 138: Again – be specific and refer directly to advective transport with seawater and particles. All of this is unnecessarily confusing

Lines 177-182 – Some of this seems unnecessary – these are standard methods

Line 212-218: I would like more information on the eOMP, the residual term from the analysis, and the implications of assumptions/details like omitting water masses above 75 m depth. Equation one seems to imply the eOMP is solved simply as a mixing model – which makes sense for DOC given its long lifetime but this is not capturing some major characteristics of Hg cycling such as evasion, diffusion in the water column, uptake by food webs and particle settling. It seems like this might work well under specific conditions where these other processes have a limited influence on total Hg distribution but do think this can be assumed to be always the case

Line 256: If it is a skewed lognormal distribution shouldn't the data be lognormally transformed?

Line 270: Also variability in biogeochemistry such as DOM affecting pool of Hg available for reduction

Line 272: Why exclude the upper 100 m?

Line 351: Need to mention Hg evasion again.

Line 353: Also diffusion and eckman pumping

Line 362: Not that AOU is also affected by lateral transport processes

Line 369: AOU not from in situ measurements of oxygen and temperature or AOU for source waters estimated? Please clarify

Line 372-374 – Almost no English in these sentences – I can't remember all the acronyms used so it would improve readability to get rid of some of them

Line 382: Why would AABW be enriched in total Hg?

Line 402: We already know this. Please rephrase to acknowledge prior work.

Line 403: Reference and inference here is problematic. The DeSimone paper simply refers to primary anthropogenic releases for a given time period not all anthropogenic Hg contributions to deposition and there is variability across models in these estimates and even how you define the human fraction. This seems irrelevant to me to this paper so I suggest just deleting the problematic statement.

Line 408: Don't you mean particle scavenging not OM regeneration? OM regeneration is associated with export.

Line 415-416: The analytical uncertainty around the early numbers is very large. Suggest acknowledging this here as well.

Line 420. I am not convinced by the proposed relationship with remineralized phosphate and anthropogenic Hg proposed in this paper. Suggest rephrasing to acknowledge this is a proposed relationship not an established tracer for anthropogenic Hg.

Line 434: Subsurface ocean concentrations and their decreases is an assumption in Soerensen et al. 2012 based on observations as a boundary condition in the model to test the influence on atmospheric trends. It is misrepresented by this statement.

Line 480-481: Soerensen et al. 2016 is a synthesis of observations and calculation from established measurements. This work considered all available measurements and represents a specific time period. This study represents a snapshot of measurements and a different time period. To imply that they should be directly compared is incorrect and these differences in these values acknowledged. I would call the value presented in this work a different "model" estimate. Both are derived by multiplying concentrations by flows.

Lines 491-493: I think the paper needs to make a more compelling case for this being reasonable.





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Technical Corrections:

Line 57: I don't think anthropization is a word

Line 59: I think it is preferable to use gender neutral language

Line 147. Delete "etc." – not helpful. L ine 281: High is very subjective term. Suggest being more precise.

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