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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal changes in gaseous elemental mercury in relation to monsoon cycling over the Northern South China Sea" *by* C. M. Tseng et al.

Anonymous Referee #2

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General comments

This paper presents data from 12 cruises on the South China Sea (SCS) showing enhanced GEM concentrations in winter relative to summer and an overall enrichment of several fold in this region relative to the global background. The authors use back trajectories and modeled differences in air-sea exchange from cruise means of dissolved elemental mercury (DEM) to conclude that the dominant controls of the observed variability are continental pollution sources and wind directions dictated by the East Asian monsoon cycles. This is a plausible hypothesis based on the data however the extent to which the ocean source has been explored is somewhat preliminary.

The authors report cruise averages for DEM with a sample size of three measurements if I am reading correctly (averaged over 4-6 hours) and model fluxes of Hg0 based on those values. They use the relatively lower concentrations of averaged DEM in winter and undersaturaturation measured at this time to conclude oceanic evasion is not a probable source of the seasonal enrichment. I think this begins to build a good case for their argument but without simultaneous air-water measurements it is difficult to completely dismiss oceanic evasion as source because DEM can be so spatially and temporally variable. For example, the authors attribute diurnal variability in GEM to oceanic evasion acknowledging substantial variability that is not captured by three numbers? Since methods/instrumentation (Andersson et al. – various papers) exists for these measurements I think the authors should slightly reframe their main conclusion to be that evidence points to continental sources and wind monsoon direction as the factors dictating seasonal differences in GEM and this confirming this with high resolution air/water Hg0 – measurements should be the priority for future research.

Minor comments:

Introduction: Many of the background references in the introduction seem out of date. For example: Line 21: Lifetime of Hg0 is well-established as several months to a year not 1-2 years.

Introduction: I suggest the authors double check the number for fraction of global anthropogenic emissions from Asia. A few updated inventories have come out recently and should be acknowledged.

Section 2.4 – Would be useful to present range across stand air-sea exchange models for fluxes instead of the one number from Wanninkhof since this is on the high end of modeled values. This could be easily bounded by the Liss and Merlivat and Wanninkhof models estimates to capture the potential range.

Section 3.1. Environmental conditions – Using the bounded estimates for air-sea exchange – how much of a modeled difference do you get in oceanic evasion with a mean

wind speed difference by season of 3 m/s? This could help establish the rationale for a continental pollution source of variability instead of an oceanic source.

At the end of section 3.1 on diurnal variability there is a statement: “Overall, evasional Hg losses estimated as a significant source of Hg to the SCS air are confirmed convincingly” that seems to contradict the major premise of the paper. Also, I am not sure the source is “confirmed convincingly” and what does “significant” mean in this context (statistically)? Perhaps the authors meant to say that the diurnal variability in GEM may be explained by evasional losses. However, the magnitude of diurnal variability attributable to evasion is not as large as the seasonal difference, postulated to originate from continental sources.

Section on Seasonal variability in GEM, page 12212 – lines 4-7 – Soerensen et al. in two paper in 2010 proposed an explanation for seasonal variability in GEM in coastal North American and European sites – and also ship cruises that should be acknowledged here.

Same paragraph – the authors jump right into SST as a control on DEM and presumably evasion but this is not well explained. I don’t think the SST difference is strong enough to explain a change in evasion. There is no reason to believe that SST is a good indicator of net reduction of Hg(II) except indirectly through UVA and UVB intensity as a control so I think the authors are confusing DEM concentrations and the physical exchange process

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