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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Atmospheric Mercury Network: measurement and initial examination of an ongoing atmospheric mercury record across North America" *by* D. A. Gay et al.

Anonymous Referee #3

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This paper aims to provide an overview of NADP's Atmospheric Mercury network (AMNet) and provides a detailed description of the network's sites, instrumentation used, network quality protocols, and more. In addition, the authors present atmospheric mercury data comparisons from 22 locations, and highlight contrasting values for site locations such as urban versus rural and coastal versus high-elevation sites.

I consider an extensive and detailed description of this unique network, along with presentation of initial observational data, an appropriate subject for publication in ACP. The first part, the description of the network, methods and operation protocols, and quality assurance and control are well described and ready for publications with minor

revisions (see below). However, I consider the section on initial inter-comparison of data between stations as weak, poorly referenced, and not ready for publication as is. For example, the authors highlight differences between various groups of stations (such as coastal versus continental, rural versus urban/pollution impacted), but they do not extensively discuss the reasons for such differences and don't provide references that show the various emission, deposition, and redox pathways that may lead to these observed patterns. I fully understand the providing a detailed discussion of the patterns at all stations is not within the scope of this publication and that such discussions have been and will continue to be done by individual site investigators; still, the currently presented highlights on differences between groups of stations lacks detail and discussions of the possible reasons for differences, and is very poorly referenced. I also miss an analysis of spatial patterns across the country in association with some of the model predictions published in the literature, such as vertical mixing of oxidized Hg in the western U.S. and other processes that are well known to control atmospheric Hg patterns.

I suggest to completely revise the sections on GEM, GOM, and PBM measurements and inter-comparisons between sites, including discussion of potential reasons for differences and much better referencing of previous work associated with this discussion. The main highlights of such a station inter-comparison and spatial patterns also should be provided in the abstract and in the summary.

Detailed comments

Abstract: Page 10522, lines 20-30: We present atmospheric mercury data comparison by time at 22 unique site locations. What is unique about these 22 stations? Need to highlight and discuss in detail the inherent differences between various groups of stations in regards to GEM, GOM, and PBM patterns. Also should provide range, median/means, minimum and maximum values of observations.

Network Objectives: Page 10525, lines 19-24: This section is quite weak, I seem to

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recall much more examples of publications associated with individual stations, comparisons with model data, etc. Maybe a separate section should be added that provides current published and data products (publications, novel investigations, model constraints) that summarize the use of the AMNet program network and data for current scientific discussion of atmospheric Hg patterns.

Monitoring locations: Page 10526 line 6: “but major gaps remain” clarify where major gaps exist, such as in the Western U.S. and high-elevation sites.

Quality assurance and validation: lines 5 to 10: the authors discuss the utilization of 35 potential flags, and warning limits, but all of this is not very specific. I understand that the details of the QAP process has been published elsewhere (NADP 2011 and Steffen et al, 2012, but the authors should be more specific what the quality control involves. What really would be helpful is to provide a statistical and quantitative overview of the main reasons for data rejection - this only can be done using a large network with many stations, and we could learn about the causes for major quality issues that would benefit the many operators of this technique; Line 11: “identifying anomalies”: how is this done? Line 18: “performs annual site audits” is this correct, are there annual site visits to all sites? Or what do annual site audits include?. Line 23 to end: I hope the authors or Tekran system developers will follow up with a detailed response paper to Gustin et al., 2013 showing that many successful testing, inter-comparison, and field studies have been performed with the current Tekran speciation system.

Network observations and analysis: this is the weakest part of the paper, this section should be completely revised and should discuss in detail observations at the stations and within groups of stations, supported by the many publications that discuss sources, sinks, and transformation of atmospheric mercury. Line 26: “ranges from approximately 2500 to 11000 observations per site” they should provide a quantitative measure of data coverage per site, days of the year, or % coverage; Page 10530, line 8: Faïn et al., 2009 (ACP) should also be mentioned here as well; Lines 22-24: discuss reasons for urban GEM enhancements and reference studies showing urban emission

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sources; Page 10531, lines 1 to 4: “we postulate that site elevation and local effects may explain the differences” how, explain what local effects and how elevation would play into this? Line 6: Minimum observations rarely went below 0.5 ng m-3” explain which stations showed lowest values, quantify the percent of observations below a certain threshold (e.g., 1 ng m-3), and discuss in detail reasons for low observations below global background with supporting references. Line 10 to 12: same for values above 3 ng m-3; which sites showed highest values, what are likely causes for these, and discuss in detail the pollution sources that have been reported for GEM in the literature. Lines 12 and following: the same needs to be for GOM and PBM, not a single references is given in this entire discussion part, discuss reasons, sources, sinks for GOM and PBM and how they relate to the groups of stations and/or spatial patterns of observations. Page 10532, line 1 and lines 10-15: “mining sources” there are several studies that reported observations from the mining sites in eastern Nevada and observations in southern Idaho, these should be discussed in respect to the observations around Salt Lake city. Similar, there are studies from the Dead Sea which should be mentioned in lieu of GOM and PBM patterns observed at the Great Salt Lake; Page 10532, line 8: “. . .expected to have higher particulate levels”: there are good references out that discuss gas-particulate partitioning of Hg (e.g., Amos et al., 2012, and others). Generally, the ratios of GOM and PBM should be discussed, not only for this one station but for a multitude of stations/groups available in this network. Page 10532 Line 20 to 24: “Definite conclusion based on the analysis of the group results are not recommended. . ..due to limited number of sites. . .. “ I don’t understand this, this study provides the best spatial, multi-station measurement record I have ever seen, the authors should highlight the unique opportunities of grouping data into different locations and spatial areas and discuss the major patterns. Clearly, there will always be uncertainties and some outliers that determine some different patterns within a certain group. Page 10533, lines 5 to 15: “. . .in contrast to previous studies and model suggesting mid-day GOM production in the marine boundary layer. . .” I don’t think the observations reported by the network are in contract to mid-day GOM production. It should be mentioned that GOM

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in the marine boundary layer is quickly taken up (and deposited) by sea salt particles (e.g., work by Holmes et al.). In addition, GEM shows some of the lowest values at coastal stations, in support of GEM oxidation in the marine boundary layer. These observations should be discussed in detail by other studies also reporting coastal Hg patterns (including Engle et al., 2010; Engle et al., 2008, Sigler et al., 2009, Mao et al., 2008, and others).

Summary, page 10535, lines 6 to 20. Currently, the summary is very non-specific and weak. It does not give a summary of the manuscript, and it should be improved to summarize the network goals, network set-up, and the data provided in this initial overview of measurements.

Figure 3. it is very difficult to see both patterns of GOM and PBM in one figure panel. I suggest to show these data in separate panels as has been done in Figure 2.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 10521, 2013.