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

General Comments

This manuscript is an interesting initiative in order to introduce to a broader –and probably European- audience the work being done by the North American Atmospheric Mercury Network. An impressive data set is collected since 2009 and severe quality criteria are applied to the data. This manuscript is more an attempt of promoting and establishing the merits and benefits of such a network than a solid scientific discussion based on experimental facts and data.

I am not sure that this manuscript in his current format is appropriate to publication in a journal such as ACP. To my opinion, it would be a good introduction to a special issue dedicated to AMNET, followed by other papers that are data scientifically sound and based on the analysis of the data.

The authors may decide what should be the scope of this paper. If the paper is an attempt to present the network only, then it should be reinforced by a better discussion on data quality and procedures, and by a discussion on the scientific strategy of data processing, modeling, and priorities. If the paper wishes to present some data, it has to be seriously improved. The analysis of data is very poor and would deserve a more comprehensive discussion.

While the introduction, objectives and needs for the network are clearly presented, I found it at times repetitive. For instance, the network objectives as presented in section 1.1 are similar to what is written in section 4. The introduction can lead to a misapprehension of the content of the paper as it is orientated towards important scientific issues: estimation of dry deposition and wet deposition. These two aspects are not dealt with in the following parts of the ms, but I think they would deserve a better discussion.

The section on quality assurance fails to present the artifacts on speciation measurements carried out with Tekran units. Authors start a sort of controversy that looks private. I feel it not appropriate, and I would better like a comprehensive list of artifacts and how they are handled or not within the network.

Now, as said above, the scientific discussion on data is poor and under-referenced. I acknowledge that a clear attempt was made to discuss the data, but the analysis does not reach the standard of ACP publication. I admit that this is a huge task ahead due to the size of the data base, so I suggest to the authors to stick to a pure introductive paper andt additional /companion papers should be focused on in-depth data analysis.

Specific comments

Abstract: isn't it 21 sites instead of 22

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Line 19-21: I would rather write "three measurable atmospheric Hg fractions", because other fractions that are not measured by speciation units contribute to Hg dry deposition as well. These

fractions include particulate Hg with particles exceeding 2.5 µm, organic mercury and GOM that is not collected/or not proven to be collected by speciation units.

Line 21: The impact of Hg dry deposition on ? Please clarify if it does impact the budget of global hg deposition or something else. Gustin et al also investigate source of GOM in the dry deposition. I am not sure that dry deposition is only of local origin since particles (biomass burning for instances) can travel on thousands of kilometers. I suggest reviewing more deeply the sources of Hg that could be involved in dry deposition.

Line 25: "GEM can be (…)" These sentence is odd –but I am not a native English speaker.

Table 1: Inlet height is given in meters probably. Please add this information in the table.

Is the site of Chester appropriate regarding inlet height? Does it comply with sitting criteria of the NADP SOPs?

Figure 1: The map is not clear enough. I wish to be able to locate the sites: you may include the name of the site or at least give a map with latitude and longitude. Also, you might precise by a color code, what are the sites used in the data analysis (21 sites)

Page 10527-28

About potential interferences with ozone. This paragraph looks polemic. Speciation units are the best fully automated instruments that are today available. There are artifacts that could be discussed in this section such as denuder efficiencies (what are the GOM species collected?), the quasi impossibility to compare parallel PBM measurements, the use of versatile sodalime (and what does it release and retain?),and even more importantly the calculation of detection and quantification limits for GOM and PBM. These artifacts could be discussed and if available should be fully referenced. Then, ozone interference might be a problem too, but I don't see the point to frontally criticize Gustin et al studies. It should be done in a critical letter. I agree (my own experience) that GEM should intercom pare very well between two co-located instruments (and that I do not understand why this paper was published with such a strong analytical problem), but this is not the place to refute this paper. Moreover, citing Prestbo et al 2011 as a reference is not appropriate since this is not a peer-reviewed article.

P10528

Line 20: zero air is not a universal term. Either define it, or prefer a more meaningful term such Hg-free air or equivalent.

While I acknowledge the authors to make this explanation short, I think it should more precise. You should precise that heating (pyrolysis) convert oxidized fraction of GOM and PBM into GEM that is subsequently released into the 2537.

Line 25: precise that there are reported as standard (1 atm, 0°c) cubic meters. This is very important due to the differences in altitude in the network.

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line 25 :are PBM considered here? If so, don't call it gaseous hg fraction

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line 12: “Strikingly, the GOM and PBM2.5 median and mean values were five to ten times greater than any other site (Fig. 2b, c).” Is it really striking? H100 is closed to a volcano that emits oxidized Hg and a bunch of oxidant that may convert GEM. As said by the authors, it also received (probably) enhanced GOM and PBM from the high troposphere.

Line 15: this sentence does not contribute to a real scientific statement and discussion. It should be move to a more general discussion and a perspective section.

Line 23: The authors might try to give an answer. One straightforward thing is to check the proximity of strong sources such as combustible-using plants (power plant, incinerators, crematorium, and so on).

Why do the authors expect that 15 of the 21 sites have mean GEMem between 1.3 and 1.5? I don’t get it.

Line 29: why not discussing GA 40 which is also rural with a mean below 1.3? NY20 has an odd 10.5 ng m3 in 2010 for a rural site. I don’t see how the authors can state that NY20 and VT99 have contrasting GEM mean? They look pretty the same (according to the table, and given the analytical precision).

The right discussion would be to discuss why these sites are lower than the rest. I don’t understand what local high elevation causes onto NY20 and VT99 data set. The authors should elaborate more on this.

Page 10531:

“Year to year, the median change at the typical site was small and limited to 0.1 or 0.2 ngm−3.” I don’t understand this sentence. What is a typical site? Are you trying to talk about trend by pooling together the 3 years of data?

“The average GEM value does not appear to predict the number of extreme values.” Yes, of course not! How could a mean predict something? I do not understand.

Page 10533:
“Perhaps at these sites, the combination of relative remoteness and coastal locations both led to lower values.” This does not sound like an in-depth analysis. Why does a coastal location bring lower GOM?

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line 6: please provide a ref at least. There are ship-based studies that could not be compared to your data set. Moreover, short term campaign shouldn’t be compared with a 3 year average. The 3 year mean may hide production of GOM that occurs during day hours (and maximize at noon) .
Line 13: is there a significant difference in precipitation amounts between coastal sites and urban sites?