

## ***Interactive comment on “Gaseous elemental and reactive mercury in southern New Hampshire” by J. M. Sigler et al.***

### **Anonymous Referee #2**

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Anonymous Reviewer Comment: acpd-2008-0458

Sigler et al 2008 ACPD 8, 17763-17803.

### **General Comments:**

The paper provides useful information on elemental mercury and reactive mercury concentrations at 3 sites along the US east coast. The strength of the paper lies in the interesting and informative contrast in diurnal and seasonal mercury dynamics between an elevated site, a near shore - low elevation site and a site offshore well within the MBL. These 3 sites provide valuable and contrasting scientific data and the authors provide very good discussion and insight into the effects of elevation, boundary layer dynamics (PBL and MBL) and contrasting influence of meteorological conditions, nat-

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ural marine biogenic emissions (halogens) and anthropogenic pollutants on elemental and reactive mercury at each of the respective sites. Although the period of record for elemental and especially reactive mercury data are relatively short this, contrast in sites does provide interesting insights into dynamics in these contrasting environments and is worthy of further study. The paper is informative, well written, articulate and well suited for publication.

#### Specific Comments:

- the authors provide a very good review of available literature on the effects of various trace gases and incorporate data at their sites including previous analyses by coauthors. It would be very useful if a more focused study of trace gases including the dominant marine halogens could be conducted at all 3 sites for a more prolonged period.

- question: have levels of marine biogenics been accompanied with measurements of sea surface temperature and algal biomass? I don't recall a clear discussion on seasonality of the halogen concentrations and that influence of mercury speciation. If these data are available a more thorough presentation of those data would enhance the paper or at least a more detailed reference if published elsewhere.

- my main criticism with the paper involves section 5.3 - wind direction. It is possible that the use of wind roses is not the best representation of wind flow or boundary layer dynamics. Wind roses are best suited to describing 2 dimensional transport at distances less than ~ 50km and are not well suited to some environments such as coastal zones or complex terrain. If there are nearby local emission sources wind roses may be adequate in simple terrain. If the authors wish to examine potential sources beyond 50 km (regional scale transport) such as indicated by the reference to Boston, the paper would be much improved if they used trajectory models such as HYSPLIT. Knowledge of regional emission sources, especially in the case of elemental mercury, can be used in conjunction with trajectory analyses to gain insight into the importance

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of specific sources and source regions in relation to observed Hg levels. Further statistical based techniques such as Probability Source Contribution Function (PSCF) or Quantative Transport Bias Analysis (QTBA) analyses can provide additional insights in anthropogenic source apportionment. The authors should remove the references to wind roses and preferably add trajectory analyses.

- the authors have an interesting data set and are encouraged to work closely with meteorologists and modellers to further explore physical and chemical dynamics at these sites. Of particular interest is the subject of marine chemistry and mercury interactions at mid latitudes.

#### Technical Corrections:

- p.17768:methods: "denuders are replaced on a 10 day basis at TF and PM, more often on a 2-3 week basis at AL..." ... is this correct? 2-3 week more often than 10 day?

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17763, 2008.

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