

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

J. M. Sigler et al.

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We thank the reviewer for the thorough critique of our paper. We addressed each comment in our revised manuscript. The reviewer made two general comments. First, the point was made that we used imprecise terminology in regard to correlations (i.e., weak or strong), and we should instead base our discussion primarily on significance level, and that we must include the number of observations. We agree on these points. Throughout our revised manuscript, we provided p-values and number of observations for all relevant correlations in text and figures, and where appropriate, couched our discussion in those terms. Table 3 and 4 indicate correlations that are significant ($p < 0.01$) but we updated these tables to show numbers of observations.

Second, the reviewer felt that the end of the introduction has to be more precise of what is the aim of the article; and that specific conclusions rather than a summary at the end

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would strengthen the paper. We disagree that the aim of the article is not clearly stated (see last paragraph of section 1, p17765, lines 13-22). The paper is meant to be a companion paper to Mao et al. (2008) in which we go as step further by including RGM measurements and continued the analysis of Hg0 using updated measurements. We state specifically that the major aims of the paper are to examine observed trends in Hg0 and RGM on diurnal and seasonal time scales, examine relationships of RGM with other trace gases and meteorological variables, and consider to geographical variation of Hg species in New Hampshire given our unique set-up of three very different field sites. However, we do agree that our paper would be better served by stressing more specific new information in our final section (we will call this Summary and Conclusions). In particular, we highlighted the following: Hg0 seems consistent geographically (montane vs. coastal vs. marine) while RGM is very different (almost non-existent at PM owing to high elevation, humidity, distance from sources), repeatability of downward seasonal trend (cold vs warm months) in Hg0 building on Mao et al. (2008), the observation of lower RGM in warmer months at TF which contrasts to many previous studies, the distinctly different diurnal pattern of both Hg species at Appledore Island, the similarity of emission estimates using both CO and CO₂ as tracers, strong evidence of solar angle as a control on Hg0 and RGM at Thompson Farm, observed differences in RGM during various types of precipitation.

The reviewer made several more detailed comments about some specific points in our paper. For convenience, we include each specific comment made by the reviewer (below) and our response/intended corrections (reviewer comments in italics):

page 17773 Organo bromides do not have a short lifetime in towards photolysis in MBL. They are photolysed by short waved UV light and thus they are an important Br source at high altitudes close to the tropopause. Sea salt on the other hand is a well known Br source

The reviewer raises a good point here; while BrO has a very short photochemical lifetime, CHBr₃ and CH₂Br₂ have somewhat longer lifetimes (4 days, and a few months,

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respectively) even though previous research at AI and TF has shown them to be important marine halogens. In our text here, we will remove the reference to these two compounds being short-lived, as it is misleading.

page 17767 line 20. The conversion from Add the T and P value after the concentration as they are needed in order to convert to a mixing ratio.

Noted. we added this here (standard temp. and pressure).

Page 17768 The uncertainty of RGM is large (Aspmo et al. 2005) therefore it is very important also to discuss the uncertainty of it in the paper and not only detection limits.

This is another important issue that we should have raised in our initial manuscript. In section 2.2 (first paragraph of page 17768) we added a brief discussion of uncertainty noting that no standard references for calibration are available. The best we can do under current operations is to strive for low blank concentrations.

p 17769 line 17-19. The difference between TF (161) and PM (157) is significant but not with the AI (139) with a comparable scatter in data. Is it really correct?

Yes. We will change the reference on this page to table 2 only to avoid confusion (the summertime values are not significantly different at the three sites. The difference the reviewer notes above for TF and PM is for the full year).

Page 17778 line 23- 26. trend analysis Fig. 7. There is not written anything about type of reaction function but in Fig. 7b lineary regression is used and in b another function is used. Additional explanation is needed. Furthermore 11 to 12 observations are shown in each figure and each measurement is a 2 hour average value so that the figurs covers about 24 hour. How is data extracted?

The reviewer provided two similar comments regarding the regressions in Figure 7. The Hg observations in Fig. 7 are the same as those shown in Figure 4. We extracted two-hour temperature averages to correspond to these values (we added the number of observations to table 4). As for the type of reaction function, we were led to use a

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simple exponential regression (using SigmaPlot software) rather than a linear regression in Fig. 7b simply because a much better fit was achieved (much higher r-value). We realize that we failed to point out in the manuscript that these functions are different. We added text (on page 17778) to this effect and also mention that the exponential relationship is similar to temperature relationships to TGM desorption from soil (Sigler and Lee, 2006) and may further substantiate a thermal mechanism.

p 17779 line 12 here is an example where two r values are compared. I am not sure they really are different? They are close and the number of observations unknown? You must further explain.

We revised the text to remove this direct comparison. The reviewer is correct that r-values are quite similar and the main point here is simply to show the negative correlation that is consistent among these sites.

from line 20- 26 Hg₂₊ to have high solubility. This is not a proper wording as all ions have high solubility. RGM has a high solubility and can lead to the formation of Hg₂₊ in water!!! so replace Hg₂₊ with RGM

Noted, and done.

p 17780 line 10. In the reference to Figure 9. From the text it should be data from both TF and PM but only data from TF is shown? either add PM data in the figure or remove PM in the text

We removed PM in the text.

line 13-14. The absence of transport from south west is taken as an indication for localized sinks but it could also be smaller sources. Further discussion is needed.

This applies primarily to the summer, and we add some brief discussion here. Specifically, we speculate that the localized sink from deposition to vegetation may be stronger in the warmer months (the Hg events are weaker and the directional dependence diminishes). Also, we agree with the reviewer that source strength (i.e., emission from

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combustion sources near Manchester etc) could be smaller as well although we do not have direct evidence at this point.

References

Mao, H., Talbot, R., Sigler, J.M., Sive, B.C., and Hegarty, J., Seasonal and diurnal variation of Hg⁰ over New England, *Atmos. Chem. Phys.*, 8, 1403-1421, 2008.

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