

## ***Interactive comment on “Model analyses of atmospheric mercury: present air quality and effects of transpacific transport on the United States” by H. Lei et al.***

**H. Lei et al.**

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Dear Dr. Feng,

Thanks for your careful review, and recommendation to the CAM-net observations. In the revised manuscript, we have incorporated your comments. In the response below, we address each of these comments. The Reviewer's comments are italicized and our responses immediately follow.

General comments Lei et al. present a successful model development work based on NCAR CAM-Chem chemistry-climate model. Generally, it is a very good article in mer-

C5304

cury study. The work expands the function of CAM-Chem model. Most of works in this article is on model evaluation. Although the evaluation shows the good performance of the model, I'd like to point out following points that can be better handled.:

Response: Thanks for recognizing the value of our work!

1, The article well discussed the current debate on atmospheric mercury oxidation mechanism. This is a key issue for current mercury study. The authors try to stand in the middle by presenting both ozone and bromine oxidation mechanisms in the model. However, I would suggest that authors present their opinion on this issue, at least in discussion part. For your reference, my point is that bromine concentration is too low in the troposphere and thus bromine oxidation mechanism is impossible to explain the current Hg pattern. Two reasons: (1) The major source for Br would come from the air-sea exchange. As a typical distribution, only coastal regions and near ocean surface atmosphere would have enough bromine. Bromine over land would be too low to explain present mercury patterns. (2) Bromine from ocean may mainly present in ionic form. The reduction power to produce enough Br is doubted.

Revised: We noticed that there are many uncertainty points in mercury oxidation mechanisms. Beside ozone vs. bromine oxidation issue, ozone oxidation mechanism itself contains uncertainty. As one of the reviewer pointing out, the aqueous reaction of HO<sub>2</sub> reduction would be another problem. Addressing these uncertainty and debates need more measurements and laboratory works. We are not going to address any uncertainty or debates in this modeling study. The information you provided will be a value part of this paper.

2, The article cited and used a lot of measurements, which is very necessary for model validation. However, although some data have been used in other works, it doesn't mean that they are good. They may not have enough representativeness. For example, this article used the ACE-Asia vertical mercury profile in Figure 4. The measurements are made in East Asia, where the wind field and the mercury plume from China contains

C5305

large variability and rarely be consistent. It would mislead the model validation for climate scale. I would suggest to find new vertical data over land to better evaluate the model. So was in Figure 5, both sites are in coastal regions. The seasonal cycle may be mainly affected by seasonal wind fields (similar to monsoon). Therefore, they may interpret a wrong seasonal pattern.

Revised: We search and use a series of measurements from NOAA air resource laboratory, INTEx-B, and the CAM-Net measurements you recommended to better evaluate model performance. These comparison has been included in the updated manuscript. Measurements are important to modelers. I appreciate your careful review on these comparisons.

Thanks! Hang Lei

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