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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ship emitted NO_2 in the Indian Ocean: comparison of model results with satellite data" *by* K. Franke et al.

Anonymous Referee #1

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Review of "Ship emitted NO2 in the Indian Ocean: comparison of model results with satellite data," by Franke et al.

This paper describes an evaluation of the accuracy of emission inventory estimates of NOx emissions from commercial shipping by comparison of satellite data to results from a general circulation model that uses the inventory information to estimate emissions from this source in a particular region. The methods used to extract the tropospheric signal from the satellite data (TEC) and from the model data (TEM) are similar so the comparisons should be robust. The conclusion is that the reasonable agreement between the satellite NO2 column data and the model NO2 column data for the study region demonstrates that the ship emission inventory used in the model is correct. This paper makes an interesting and relevant contribution to ACP. However, while

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the approach is clever and the paper is well-written, I was not fully convinced that the conclusion is borne out by the analysis. Before I recommend publication of this paper I would like the authors' response to the following points.

1) The principal conclusion of the paper is that the inventory estimates of Eyring et al. (2005; 2008) and of Corbett and Koehler (2003) are consistent with the analyzed SCIAMACHY data while the inventory estimates of Endresen et al. (2003) are not. The discrepancy in these inventories has been examined at length in the literature and appears to be on the order of a factor of two, as pointed out in the current paper. Thus the analysis in this paper must be able to discern differences on this order for the conclusion to be sound. The authors point out that had the model used the Endresen et al. inventory data then the model output would be a factor of two lower than the observations, which verifies the Eyring et al. inventory. But this is not shown anywhere in the paper. Nor is it clear that a decrease in model emissions will translate into a linear decrease in model atmospheric mixing ratios, as the authors state. A more convincing plot than that shown in Figure 8 would be the comparison between the satellite data and the model data with a) the Eyring et al. inventory and b) the Endresen et al. inventory.

2) It's not clear to me the reasons for showing the difference plots in Figure 7. These two plots are time series of the difference between the data from the shipping sector (S) and either of two background sectors (B1; B2). These absolute differences are the true measure of emissions of NO2 from ships, whether determined from satellite data or from the model; yet the two estimates differ by more than a factor of two, on average. Which of these estimates of ship emissions are we to believe? For some months the difference is negligible, but for others it is greater than a factor of three. In the text the authors point out that the annual cycles are different, but this must be due to changes in background NO2 since they have already pointed out in Figure 3 the lack of an annual cycle in the ship activity in this region. I think these plots detract from the paper and should be deleted.

ACPD

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Interactive Comment

3) What is the effect of the lightning source of NOx on the two data sets? Since both satellite and model data use the difference between NO2 column data in the study region and a reference region in the Pacific, it would seem that lightning generated NOx would influence the results.

4) A complete description of the uncertainties is required. It was surprising in Figure 8 that the error bars on the model data are much smaller than those on the observed data, especially since plume processing has been ignored. Also in Figure 8 the 2:1 line is shown, while in the text the 1:2 line is discussed. The latter is the one that should be shown on the plot.

5) While not strictly relevant to this paper, it would be interesting to see a comparison of SCIAMACHY and GOME-2 data over the same time period. I think the authors are correct that the discrepancy is due to transport of NOx from the adjacent landmasses and that averaging of the SCIAMACHY data is responsible for the differences seen in Figure 4. This can be easily shown without additional analysis by restricting both data sets to the same time period.

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