Atmos. Chem. Phys. Discuss., 8, S12220–S12223, 2009
www.atmos-chem-phys-discuss.net/8/S12220/2009/

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.

K. Franke et al.

Received and published: 19 May 2009

We would like to thank Reviewer 1 for his review. Most of the suggestions have been incorporated in the revised paper. In the following we give point-by-point answers to the comments:

Comment 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*

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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.

Author's reply: We agree with the reviewer that a comparison of the satellite data with two similar model runs differing mainly in the amount of ship emitted NOx provides a better test of the inventories. In the revised version of the paper, we therefore included this comparison as proposed. The basic conclusion that the inventory with the higher ship emissions of around 6 Tg(N)yr-1 is more consistent with the 2002-2007 satellite data than the one that uses only around 3-4 Tg(N)yr-1 still holds.

Comment 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.*

Author's reply: In the revised version we follow the suggestion of the reviewer to base the argumentation on the mean values in region S and not on the differences between them. We have therefore removed Fig. 7 of the original version as suggested and replaced by a figure showing the differences between the results from the two model runs with differing ship emissions.

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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.*

Author's reply: The reviewer is right that although being a small effect overall, lightning NO2 could be relevant for the absolute numbers derived for shipping NOx emissions. The additional model data included in the revised version can be used to study the sensitivity of our results to different assumptions for lightning emissions. The difference in NO2 levels from the two model runs are small in those regions not affected by ships in spite of the much larger lightning source (5 Tg instead of 2 Tg) in the QFY run. In contrast, the differences over the shipping region are large as one would expect for the difference in emissions used. We therefore conclude that the effect of lightning is not a major concern for this analysis.

Comment 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.*

Author's reply: In response to the reviewer's comment, a more detailed error analysis for the satellite data has been included in the revised version of the paper. For the model the error bars show only the variance of the averaged values, which is rather small as there is no interannual variation in the model setup. It doesn't include any other uncertainties e.g. due to emissions or the neglect of plume chemistry. We made this clear in the figure caption and we discuss the possible effect of neglecting plume chemistry in more detail in the revised version. In fact, one way of quantifying the uncertainty in model calculations is the comparison of model and measurement data which is the basic topic of this paper.

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With respect to the 1:2 line, we have replaced Fig. 8 by a direct comparison of satellite and model results for the two different shipping inventories. This is a more appropriate comparison and no 1:2 line is needed for the discussion anymore.

Comment 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.*

The suggested restriction of the data sets to the same periods is exactly what has already been done in Figure 6 of the discussion paper. It can be seen that there are differences between data from the two instruments, but as the simulated difference due to diurnal variation in the model is within the standard deviation of the measured difference, we see no possibilities for further conclusions from this kind of analysis.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15997, 2008.