Atmos. Chem. Phys. Discuss., 8, S12206–S12211, 2009
www.atmos-chem-phys-discuss.net/8/S12206/2009/


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 3 for his detailed and helpful comments. Most of the suggestions have been incorporated in the revised paper. In the following we give point-by-point answers to the comments:

**Main comments**

*The authors find that their satellite observations are consistent with model simulations driven by current emission inventories (6 Tg N/yr), which is quite a bit higher than previously inferred from space (4 Tg N/yr). But they do not explain what is so different between the current study and the estimates by Richter et al. in 2004 [GRL]. Was the SCIA-data used there not representative compared to the multi-year mean used here? Does the model used here simulate strongly different NO2 lifetimes than previously*

*assumed from Song et al.? This is important information and we don8217;t learn anything about this in the current manuscript; this should be repaired.*

It is not clear to us what the referee is concerned about. In the paper of Richter et al., 2004, the SCIAMACHY data were compared to the updated Corbett and Koehler (2003) shipping inventory which has a total of 6.87 Tg(N) per year and was distributed according to the AMVER data. This inventory already was very similar to the one by Eyring et al (2005) which is used in the model run in this study. We therefore do not see any contradictions between the two studies.

Clearly, the use of only 21 months of data and the assumption of simple values for NOx lifetime in Richter et al. lead to much larger uncertainties in the emission estimates than in the current study, where multi-year averages are used and compared to full 3d CCM model runs.

*Section 2.1 provides a limited discussion of the satellite retrievals only. I think more detail should be provided on how the retrieval technique accounts for processes that influence multiple scattering (and thereby the AMF) within the atmosphere. Also the error discussion is very limited: the small column signals shown in this study will have larger errors that the 34% cited here, and this deserves more attention. The 34% number may be realistic for strongly polluted regions with columns of several times 10710;15 molec.cm-2, but for the shipping lane signals shown here, the spectral fitting error is on the order of the estimated column itself (several times 10710;14 molec.cm-2), and thus not negligible in monthly means with a limited number of samples (SCIAMACHY!). In addition, getting the a priori profile shape right in a 100 km-wide shipping lane is very difficult, and the authors don8217;t specify the approach they8217;ve taken for a priori profile shapes within the shipping lanes (and just outside of them), nor do they say anything about the errors associated with their approach.*

In response to the reviewer's comments, we have added a more detailed discussion of the approach taken for the retrieval and the uncertainties to the revised manuscript,



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including sampling.

The reviewer suggests using model profiles of NO2 as a priori information in the retrieval of satellite data. This approach has been used to ensure consistency between model and measurements in previous intercomparisons asking the question: Are the satellite columns compatible with the assumption that the model has the correct vertical profile and column. However, in this study the situation is different in that we know that the model profiles of NO2 could worsen the retrieval since the model has a coarse spatial resolution compared to the satellite data and thus would reduce accuracy. Using the model profiles over the shipping regions would lead to assumed satellite sensitivities which are not representative of the real situation even if one allows for the shift of one grid box. Therefore, using the model profiles would not increase the consistency here but rather force the satellite columns to a wrong value.

Consequently, we have assumed a simple box model of the NO2 distribution assuming a well mixed boundary layer of 700 m height and no NO2 in the rest of the troposphere. Analysis of the vertical sensitivity of the measurements shows that this is not a critical assumption - changing the mixing height to 300 m or 1000 m respectively changes the airmass factor by only about +/- 10%.

The same airmass factor has been used for both the shipping region and for the control regions. This is not strictly correct but the impact on the results is expected to be small for the following reason. If we assume that the NO2 which is not resulting from ship emissions has a similar vertical distribution over the shipping region as in the control areas, the same error will be made in both regions and therefore cancel in the difference.

**Specific comments**

*P16003, lines 14-16: I don8217;t see why the authors have taken a 6-year average of the model data. It would be very interesting to investigate whether model simulations and satellite observations are consistent in their interannual variability. I think the au-*



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*thors need to justify this approach, and clarify the reasons why yearly-resolved analysis is not attempted; lack of statistics? This issue is even more relevant as GOME-2 data from one year is integral part of the analysis, introducing interannual variability into the analysis (see for instance January 2008 in Fig. 4).*

We agree with the reviewer that an analysis of the interannual variability in the shipping produced NO2 columns is interesting and in particular the large values in January 2008 as seen by all three instruments (SCIAMACHY, GOME-2, and OMI) are intriguing. However, we have here not investigated this further for two reasons: a) the sampling of SCIAMACHY is relatively poor and averaging over several years greatly improves the significance of the data and b) the model set-up is nudged to winds from ECMWF but uses constant emissions, in particular for biomass burning. Therefore, it can not be expected that the model run gives a good representation of the interannual variability in the NO2 fields. This point has been made in the revised manuscript.

*P16003, lines 17-21: the authors explain that they follow the same stratospheric correction procedure for the model data as for the satellite retrievals. This makes sense, and addresses the presence of background NO2 in the remote troposphere; the same assumption of zero background NO2 is made in model and retrieval alike. Unfortunately, this consistency does not hold for the vertical distribution of NO2 in the model and as used in the retrieval. The assumed NO2 profile in the retrievals does not originate from the ECHAM5/MESSy1 model, and this leads to additional errors in the comparison of model and satellite NO2 fields, because the AMF could be quite different if ECHAM5/MESSy1 profiles were used. I think this aspect needs to be discussed as it likely represents an important source of systematic error in the comparison, and ultimately in the inferred emissions. The authors have shown to be aware of such issues, as they emphasize the importance of a "consistent data analysis method for the comparison of model and satellite data" (P16005, line14-15).*

As discussed above, we do not agree with the reviewer on this point. We would like to compare the NO2 columns from model and measurement, and for this, we need to



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use the best possible vertical distribution for the satellite retrievals. We have added this discussion to the revised manuscript.

*P16006, l17-18: please quantify what "good agreement" means here. Judging from Figure 4, absolute levels are comparable, so RMS errors could do. Correlation analysis (in space and/or time) would also give us a sense of what good agreement means here.*

We have added RMS errors to this paragraph to make it more quantitative.

*P16008, l18-20: the diurnal variation in NO2 is not only the result of increasing photochemical loss in the morning hours, but it is (potentially) also influenced by the diurnal variation in emissions as shown in recently published work on the diurnal cycle in tropospheric NO2 observed from space. The text in 3.2 should be updated to reflect this*

We have updated the text accordingly. However, for shipping emissions, we believe that assuming 24/7 operations of the ship engines is a good assumption and the main driver of diurnal variation is photochemistry and possibly also variations in wet and dry removal.

*P16009, l10: typo repspectively.*

Corrected.

*P16010, l24-25: I think the authors should provide some support for the assumption of a linear relationship between emitted NOx and its response as NO2 column. NOx emissions influence OH concentrations that feed back to NO2 lifetime and hence NO2 concentrations.*

The reviewer is right that the link between emissions and NO2 columns is non-linear by nature and assuming a linear relation is only a first approximation. This point has been addressed by including into the revised paper results from two model runs which differ only in the ship emissions used. Any non-linear effects are taken into account, at least at model resolution. As expected, the main conclusion of the paper remains



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unchanged.

*P16011, l16-17: perhaps it is not really relevant to mention the regions B1, S, and B2 in the conclusions again?*

Changed as suggested.

*P16012, l20-22: I don8217;t think the authors have explained where the difference between 72 and 90 Gg(N)yr-1 for AMVER and ICOADS comes from. Their work gives one space-based constraint on emissions in the shipping lane, so it is unclear why the estimates should be so different.*

In both the AMVER and ICOADS inventory the same emission totals are assumed. However, because the spatial proxy is different between AMVER and ICOADS, the resulting emission total for the region S is different. This has been explained in the revised version.

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