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Interactive Comment

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 #3

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This is a very interesting paper that addresses the relevant research topic of estimating NOx emissions from international shipping by using the only type of measurements readily available for such estimates: satellite observations. The authors deserve credit for trying to make sense of a small signal that barely overcomes the retrieval detection limit. Nevertheless, the findings of the authors are plausible and the paper is well-written, but there are a number of issues that should be addressed to make it suitable for publication in ACP.

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 don't learn anything about this in the current manuscript; this should be repaired.

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 10ˆ15 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 10ˆ14 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 don't specify the approach they'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.

Specific comments

P16003, lines 14-16: I don'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 authors 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).

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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).

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.

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.

P16009, l10: typo repspectively.

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.

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

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in the conclusions again?

P16012, l20-22: I don'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.

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