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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 #2

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The authors present a comprehensive analysis of satellite NO2 data from different sensors over shiptracks and comparisons to models. The topic and content of this study fits well in ACP. However, before acceptance in ACP, substantial extensions/revisions are necessary as stated below.

General comments:

- The authors claim to evaluate ship emissions quantitatively. However, the discussion of errors/uncertainties is quite short. The final message is more or less "it fits quite well", which should be definitely more precise.

One important aspect is the fact that the coarse spatial model resolution does not resolve plume chemistry. This is mentioned several times in the manuscript, with ac-

cording references. However, the possible impact on the results is not discussed quantitatively. In particular, the fact, that good agreement was observed between satellite and E5/M1 NO2, despite the fact that plume processing has been neglected, is suspicious, and might indicate two (several) effects cancelling each other out. This has to be discussed in more depth.

- The obvious shift of the shiptrack between AMVER and E5/M1 is mentioned and explained by the fact that the ship track is close to a model grid latitudinal boundary. However, since the AMVER pattern was used for ship emissions in the model (16003 3-4), the model maximum should be found accordingly to the AMVER maximum. The latter is found at ˜6°N throughout the year (Fig. 3c), which is definitely north of the maximum E5/M1 grid box (2.8°N-5.6°N). From the AMVER emissions, I thus would expect the model peak in the next latitudinal band northwards (5.6°-8.4°N).

- The authors use GOME, SCIAMACHY, and GOME-2, but ignore OMI data. Since OMI observations take place at a quite different local time (˜2 p.m.), they might in particular provide valuable insights on the aspect of diurnal variation of NO2.

I therefore suggest to

- clarify the reason for the spatial mismatch between AMVER and E5/M1 ship tracks.

- strengthen the quantitative statements, including more detailed error discussions, and in particular discuss the aspect of plume chemistry in more depth.

- include OMI data.

- deal with the detailed and minor comments below

before accepting the paper for publication in ACP.

Detailed comments:

Abstract: In its current form, I don't see that the paper meets the claim of being "the first that evaluates atmospheric response to NOx ship emission estimates from space",

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given several papers that have already dealt with NOx emission estimates from satellite observations as well as model studies on the effects of ship emissions.

16000 9-11: How do you judge about the overestimation of ship NOx in models due to model resolution? What is the impact of this effect on your study (see also below)?

16002 11-12: Giving a relative number for the accuracy is probably appropriate for polluted regions with high NO2 columns (where the uncertainty is mainly due to the air mass factor), but are misleading for low NO2 levels: In the extreme case of a column of 0, this value would be free of error! So over "clean" regions, additive errors are quite important, as may arise from the stratospheric estimation or from unidentified spectral structures that may be interpreted as NO2 in the fitting process. These systematic biases probably partly cancel out by considering the differences of neighboring regions, but still it cannot be excluded that the derived difference in mean columns has a systematic bias.

16003 3-4: If AMVER is used for the spatial distribution of emissions, then why is the E5/M1 shiptrack shifted one grid box further south??? (see also 16005 7-8)

3.2 The authors discuss differences of GOME, SCIAMACHY, and GOME2 results, giving 3 possible explanations. However, I would expect that there are also systematic differences between the 3 NO2 products from the different sensors. The authors might discuss this aspect first, making use of the temporal overlap between GOME and SCIAMACHY as well as between SCIAMACHY and GOME2. In addition, OMI data should be compared to SCIAMACHY and GOME2 for consistent time periods.

Afterwards, the authors can go through (i)-(iii), in which the addition of OMI data helps in particular to judge about diurnal variations.

3.3 This section has to be more quantitative.

- What is the expected effect of in-plume chemistry? For this particular ship track, I expect quite special conditions: due to the narrow track with high ship frequency,

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average NOx (and probably VOC) levels are far above natural background, and, on top of this, there are point-source-like ship emissions. This is different from the single-ship study in Franke et al., 2008.

- If the agreement is good, despite neglecting model resolution effects, could this indicate two (or more) compensating effects?

- What about aerosols? What is their impact on the satellites sensitivity (air mass factors) and on the NOx lifetime?

In the end, the authors should give an estimate of ship emissions from their comparison study including errors.

From the S-B2 comparison in Fig. 8 (that should be less affected by biomass burning etc.), there are several points that are closer to the dotted line that to the straight line, and from the given error bars one cannot refuse 1:1 nor 2:1. In so far, the statement in the abstract that "the results do not support ... 3-4 Tg" is not supported by the presented data.

From the error bars shown in Fig. 8, and, in addition, uncertainties arising from in-plume chemistry, aerosols, and other systematic errors, I expect a resulting uncertainty of at least 50%.

4 The conclusions should be updated depending on the extended error discussion and new results from OMI data.

Minor comments:

15998 3-5: I suggest to mention the satellite instruments chronologically, i.e. start with GOME.

15999 20-22: I couldn't find that number (56 ppbv), neither by text search nor from any figure, in Eyring et al., 2007.

16001 13: If you mention the high-res mode of GOME, you should also mention that it is

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applied only every tenth day, and, especially, it is gained at the cost of swath width, i.e. global cover of the high-res mode is quite sparse. But since this is quite irrelevant for this study, I suggest to skip it and just mention 40x320 as "nominal" GOME resolution (SCIAMACHY also has different observation modes with different spatial resolutions).

16001 25-26: You should clarify that there are many possible definitions for a reference sector, and that 180° E-220° E is your actual choice. I suggest to change the description to 180° W - 140° W, in accordance with the ticks in Fig. 1.

16006 9: ... and model

16009 the aspect (iii) should be discussed in a separate paragraph, as (i) and (ii), before summing up (i)-(iii).

16020 The authors might rethink the choice of colour bar and -range; in particular in Fig. 3b, only 3 different levels of NO2 can be recognized.

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