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Interactive Comment

Interactive comment on "Update on emissions and environmental impacts from the international fleet of ships. The contribution from major ship types and ports" *by* S. B. Dalsøren et al.

S. B. Dalsøren et al.

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We thank Volker Matthias for rising important questions as well as noticing issues in the manuscript that need to be clarified.

Regarding the comment on our claim of providing an improved emission inventory compared to former studies: The first paragraph of section 2 outlines specifically where improvements have been made compared to previous studies; ”Compared to Endresen et al. (2003, 2007) the model presented here has been improved as fleet power is directly calculated using data for individual ships, and that the calculations are broken down on many more ship segments, allowing for greater differentiation. Compared to Eyring et al. (2005) the model presented here uses a greater number of fleet

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segments, introduces new data on activity profiles and includes detailed modelling of port emission.” Furthermore, as stated in section 3.3.1: “…the fleet breakdown structure used in this study is far more detailed than previous models, reported by Eyring et al. (2005) Corbett and Koehler (2003) and Endresen et al. (2003, 2007). This allows for improved results, provided sufficiently accurate input data”, and in the first paragraph of section 3.3.2 “Corbett and Koehler (2003) identifies engine load factors and days at sea as the most sensitive input parameters to activity based fleet modelling. Admittedly, the current study presents no fundamental improvement on the load factor data. However, with respect to the number of days at sea new data (we have used 617 000 individual ship movement records) are presented compared to Endresen et al. (2003, 2007), Eyring et al. (2005) and Corbett and Koehler (2003), which are believed to improve the accuracy of the estimates.”

This inventory presents year 2004 emissions while the ones so far have used 2000 as basis year. In that respect this is also an update and progress. This inventory is also recognised and extensively used by several scientists in the EU project Quantify studying the impacts from the transport sector. One of the tasks in Quantify has been to develop new improved emission inventories for the transport sectors. The Quantify project is now acknowledged in the manuscript.

As the reviewer points out it is a question how well the model resolution can represent chemical processes on port scales. We are aware of these limitations and therefore mention them in the text. However, we think that the results give a broad picture of the impacts from ports, though detailed impacts should be studied by regional or urban scale models. We disagree that the model is unable to resolve the ports as most ports can be discerned in figure 10 showing impacts on SO2 and NOx. Further the model is able to resolve nonlinear chemistry showing smaller relative NOx changes due to port emissions in/near ports heavily polluted from other emission sources.

We are not sure if we understand the statement “Port emissions contribute only 5 % to the total emissions. The uncertainties in the operation profiles are certainly much larger.” Yes, the uncertainty for port emissions is as large as or in fact larger (due to other factors) than the uncertainty in operational profiles. However, even if the port emissions are small the uncertainties in the emissions (20-30 % as discussed in section 3.3.2) are smaller than the port emission themselves. The results are relevant in a discussion on possible emission reduction measures, which may include options related to ports (e.g. the use of shore based power supply to ships in port). It is also important to address port emissions in particular, as they have been shown to be relevant e.g. in Corbetts study on premature deaths from PM.

A comment is made on the difference in PM emissions compared to the EMEP inventory. If one for instance compare with the EMEP emissions listed in table 3.4 of Cofala et al. 2007 it is correct that the our emissions are factors 3-6 larger for all chemical components (CO2, Nox, SO2, PM….) This we believe is due to the following reasons: Most importantly EMEP only cover the European region, secondly the ENTEC emissions for EMEP are for 2000 while our estimates are for 2004 and there has been a substantial increase in emissions over that period. The differences should to little extent result from use of different emission factors as we use much of the same sources there. There could be some differences also because of different datasets and assumptions used for operational profiles, engine loads and specific fuel consumption of engines. It should also be noted that the ship size cut-off in the ENTEC study is higher, so the data used here are based on a larger number of small ships. This will also affect the numbers.

We see the point on making the discussion on PM emissions clearer. The fractions of the PM emissions going as BC and OC are quantified in the end of section 3. The BC and OC emissions are also listed separately in Table 5 and 6. In accordance with Petzold et al. 2007, we also assume that 40 % of the PM emissions are sulphate. A line on this has been added to the end of section 3 and Petzold et al. 2007 has been included in the reference list. We have not calculated separately how large the

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contribution of directly emitted sulphate is compared to the part coming from oxidation of SO_2. However, we expect it to be small as the emissions of SO_2 are much larger than the PM emissions (Table 5 and 6)

In the comment there are several questions on why we investigate the effects of different ship types separately. The interest in the specific ship types is in part due to the ongoing activity to regulate ship emissions. The IMO is currently working to establish GHG regulations for international shipping, and is under pressure, e.g. from the EU, to implement regulations with substantial impact on emissions. The European Commission has announced that it will forward proposals of its own if the IMO fails to act, and the United Nations Framework Convention on Climate Change (UNFCCC) process is considering shipping (along with aviation) for inclusion in the global CO_2 reduction targets for the period from 2012. Regulation on other pollutants such as NO_x and SO_x has already been passed, and may be subject to further enhancement. Furthermore, regional and local initiatives to regulate shipping emissions are also emerging (e.g. California, Hong Kong, Scandinavia).

In practice, such legislation will be differentiated on ship types and sizes, often based on economic considerations (larger ships can more easily cut emissions in a cost effective manner). It is of public interest that the effects of such regulations are clearly understood, so that regulations and emission reducing options are applied in a targeted and cost-efficient manner, taking into account not only the emission volumes, but also the geographical location of the emissions and the resulting impacts. It is in this context that scientific results which clearly identify the effects of emissions from specific ship segments are important. A relevant question which may be put to the public is whether the convenience of short delivery times of high speed container traffic is worth the environmental consequences?

Weather routing is one example of emission reducing options, which also has been shown by some cost benefit analysis to be a promising option to limit emissions. In that context different ship types have differences in important characteristics as speed

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and travel distance. In order to optimize the shiproutes it is important to know both how the ships types operate (emission distribution) and the impacts.

In recent years there have been substantial differences in the development of seaborne trade and thereby emissions from different ship types (shortly noted end of section 4). Large differences are also expected in the rate of changes in the future. We adress these issues in a coming article and the differentitation on ship types in this article serves as a basis for these discussions. We have chosen to split this in two papers to limit the length and topics discussed.

Further in the topic of traffic distribution it is also questioned why it is useful to combine COADS and AMVER what about possible double counting. In section 4.3 we state that “…different reference years are applied (COADS year 2000, AMVER year 2001/2002).” This implies that even if some of the ships report to both COADS and AMVER (which is likely), there is no direct double counting of ship movements. Combining the two datasets will thus provide a previously unmatched volume of movement data which covers larger portions of the fleet than either of the sources can do alone. For further details on the strengths and weaknesses of the sources, we refer to Endresen et al (2003).

We also agree that a comparison between the COADS/AMVER data and the Lloyds data would be of interest. However, the dramatically different formats of the datasets prohibits an effective comparison: In short, the Lloyds data states the locations and times of arrivals and departures in port, but provides no indication of the routes chosen to cover the distance between the ports (although the shortest path is often chosen), whereas the COADS/AMVER data are aggregated on a level which details no information on the ports nor any information on the routes (only daily point observations).

Some questions in the comment are related to figure 2 and what we actually can learn from it. AIS data have previously not been used in the determination of factors in activity based modelling. The reason for this is the novelty of the system (the systematic

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gathering of data started relatively recently and access has been limited) and limitations in the use (section 3.3.2: “limited to vessels in coastal traffic”). Figure 2 confirms the significant variation between the ship types (as shown in the days at sea numbers calculated from the Lloyds data discussed in the manuscript), but shows that the minimum number of days at sea is lower in the AIS data than in the profiles estimated from the Lloyds movement data. We are aware that region-specific factors might influence these numbers, and that the difference in reference year could also be of importance.

We have added some discussion on uncertainties connected to using only one year of meteorological data in section 6. Our transport scheme using second order moments (Prather, 1986) is considered quite accurate and depends on a number of input fields not available in general climatological datasets. We are currently working to get several years of meteorological data from the IFS forecast model at ECMWF available in T63 in order to make an average over a number of years. We already have the ERA40 for the period 1960-2000 and those could have been used though comparisons we have done suggest that IFS is superior due to higher time resolution and known problems with the residual circulation and STE in ERA40

Summaries for 1996 (WMO, 1997) indicate that 1996 was a year with some anomalies in particular for precipitation. It is often the anomalies that are focused on in such summaries. Decisive patterns like NAO and ENSO were not in very strong negative or positive phases and we think the year was not deviating strongly from a normal/average year. While, in some parts of the world, 1996 was one of the coldest years in recent decades, the average temperature of the Earth overall was the eighth highest and 1996 was the 18th consecutive year with positive global temperature anomalies since records began in 1860. Observations over both land and ocean showed that the 1996 estimated global mean surface temperature was 0.22°C above the 1961-1990 average.

We have also added a couple of lines discussing earlier comparisons to measurement data. But mainly we refer to earlier papers as we want to limit repetition of these

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discussions here.

We agree on the comment on showing absolute versus relative concentrations. Ideally we should have shown both but need to limit the number of figures in the article. The reason why we show relative numbers is that we feel that this is more understandable or intuitive for the broader audience that we hope to reach with this article. In section 7 we also notes that the large relative contribution near Antartica in summer is mainly due to small impacts from non-ship sources and that the absolute perturbations are small. We believe that the relatively short lifetime of SO2 and NOx is the main reason why the contribution from shipping to these components level of a few hundred kilometres inland from the coast. But as the reviewer points out, the relative contribution (which is shown in figures) is also dependent on heterogenity of land emissions and a sentence on this has been added to the manuscript.

All the references in this reply are listed in the reference list of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18323, 2008.