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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol indirect effects from shipping emissions: sensitivity studies with the global aerosol-climate model ECHAM-HAM" *by* K. Peters et al.

Anonymous Referee #2

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GENERAL COMMENTS

This paper presents a global model study of the ship-induced direct and indirect aerosol effect and an estimate of the resulting radiative forcing. Several uncertainties concerning the emissions of aerosol from shipping are investigated using sensitivity simulations, although an important source of uncertainty, namely the geographical distribution of emissions, is not considered.

Given the still very limited amount of model studies on this topic available in the literature, this work represents an important contribution to improve the quantification of aerosol effects from ship traffic. The scientific method and the underlying assump-

tions are presented in sufficient detail and the relevant literature is cited and discussed. Nevertheless, a more precise comparison with previous modeling studies is necessary.

The manuscript is well structured and clearly written. The quality of the figures can be improved. This concerns their size (panels are often too small), the labeling of the color-bar and the highlighting of the regions of statistical significance (see detailed comments below).

I recommend this paper for publication in ACP, after a major revision addressing the following issues.

MAJOR REMARKS

Model vs. observational estimates of RF: in a previous paper, the same author team performed an observational analysis of the large-scale impacts of ship emissions on clouds, as mentioned in the Introduction (Peters et al. 2011b), and found no significant effect. The current manuscript is now focusing on the same problem using a modeling technique and finds a significant effect, in reasonable agreement with previous modeling studies. Such discrepancy between model and observations is very interesting and the authors should elaborate more on the possible reasons for it. I would discuss this in a separate section.

Model evaluation: model evaluation is not mentioned at all. Please provide at least one reference for that and summarize the main strengths/weaknesses of the model. A key issue is the representation of (low) clouds in the global model, as already noted by the other reviewer. This is a major source of uncertainty for this kind of studies. I suggest to include a comparison with observational data for clouds (e.g. ISCCP).

Comparison to previous work: throughout the manuscript, the authors often refer to the assumptions and results of Lauer et al. (2007), who performed a similar work. The two studies actually use a similar model and basically the same methodology. Therefore similarities and differences should be analyzed more systematically and related to resulting estimate of the AIE. In particular: 1) What could be the impact (if any) of the different horizontal resolution (T42 vs. T63) adopted in the two studies? 2) What are the main differences in the emission inventories, in terms of total emissions, size distribution and geographical distribution? 3) What are the impacts of the different cloud schemes and aerosol models? For example: according to the model description, aerosol nitrate is not considered in HAM, while it seems to be included in Lauer et al. 4) Which chemical mechanism is used in the model? What are the most important reactions and reaction cycles included? How do this compare with Lauer et al.?

Geographical distribution of the emissions: the uncertainty in the geographical distribution of emissions is not assessed, although this could have a potentially large impact on the resulting estimate of the AIE. I suggest to consider an additional sensitivity study, where the QUANTIFY inventory (based on ICOADS/AMVER) should be replaced by, for example, the inventory of Lamarque et al. (ACP, 2010).

Statistical significance: compared to Lauer el al. 2007, a relatively low confidence level is adopted (90% vs. 99%) and nevertheless the regions of significance are much less and quite limited. How can this be explained, given that a similar nudged dynamics is used, with a comparable number of simulated years? How reliable are the conclusions, for example regarding aerosol number (Fig. 4 and Section 3.2) or RF effects (Fig. 8 and Section 3.5) where most of the features which are discussed occur outside the marked regions of significance?

Effect of reducing carbonaceous emissions: the effect of carbonaceous emissions as simulated in the experiments BnoBC and BnoC is basically negligible. This is not surprising, given the relatively low emissions of these species by shipping. The authors claim that this sensitivity study is important in view of future ship-fuel regulations. However, such regulations will deal mostly with sulfur, which is currently very high in ship fuels (Buhaug et al., 2nd IMO GHG study 2009). Therefore a sensitivity study with reduced SO2 emissions (like in Lauer et al., ES&T 2009 or Righi et al., ES&T 2011) will be much more valuable.

Figures: the use of black contours to mark significant changes is confusing (especially in Figure 3, Figure 7 and Figure 8). I would rather use a hatch pattern (e.g. diagonal lines) to mark them or simply mask out the non-significant regions (in white or gray). The color-bar labeling is ambiguous: it is not clear, for example in Figure 3, what should be the value of the tick between 4 and 7 (5.5?) or between 7 and 15 (11?). In the caption, please specify that these are ship-induced (relative) changes and which kind of time average is shown (multi-year or specific year?).

MINOR REMARKS

Table 1 and Figure 1: since the AeroCom emissions for the shipping sector are not used in this study, I find these two panels quite misleading. I would remove Figure 1 and put the values for total emission of different species as additional columns in Table 2. This will also help to highlight the differences among the experiments and to compare with previous studies.

Figures 10 and 11: these could be merged in a single figure.

P7074-L14: the smallest value should be given first: -0.32 to -0.07. The same applies to other parts of the manuscript.

P7074-L15-17: "The magnitude of the AIEs depends much more on the assumed size distribution". Please mention the dependence on the geographical distribution.

P7074-L17-20: as mentioned above, the different geographical distributions used in previous studies could explain some of these differences.

P7076-L1-3: "largest contribution to positive RF". Please add "anthropogenic". Anyway, according to IPCC the third contribution should be tropospheric ozone.

P7077-L26: replace "0.6" with "0.60" for consistency.

P7078-L3: give a reference for the total GHG RF value (e.g. Forster at al., IPCC 2007).

P7082-L25: please specify the value of the height.

P7083-L17-25: in this context, it would be useful to refer to the previous modeling study by Righi et al. (2011), who considered various size distributions corresponding to different ageing of the ship plume.

P7084-L22: I would put a comma after vice-versa.

P7085-L9: since a 90% confidence level is adopted in the manuscript, I would write this value here.

P7086-L3-8: this is not surprising, given the relatively low emissions of these species by ships. The last sentence should refer to Lauer et al. 2009, who reached a similar conclusion.

P7089-L22: replace "atmospheric radiation" with "radiative budget".

P7090-L15: name these regions. How do they compare to previous results, considering the differences in the geographical distribution of the emissions?

P7091-L10: replace "models'" with "model"

P7091-L15: I would replace "Figs. 9, 10, 11, 12 and 13" with "Figs. 9-13"

P7094-L1: replace "0.6" with "0.60" for consistency.

P7094-L3: replace "inrease" with "increase".

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7073, 2012.