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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling the chemical effects of ship exhaust in the cloud-free marine boundary layer" *by* R. von Glasow et al.

R. von Glasow et al.

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General comments:

We shortened the paper a bit and tried to improve the organisation of the paper.

Major comments:

ad 1)

These comments inspired us to rethink our upscaling approach. We use a different upscaling approach now in which we account for repeated plume emissions in a better way than in the approch used in the ACPD paper. This lead to different results which are a lot more realistic.

In the approach that accounts for multiple ships, it is implicitly assumed that for plume dilution only the last preceding plume is of importance and that for that plume dilution

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and/or entrainment is not important at all. If ship emissions are assumed to occur very frequently (i.e. order of an hour or less apart) this assumption is not correct any more. As stated on page 543 the "rapid mixing time" ends after about 6h and if emissions are reduced such that ship frequency is still more than six hours, the proposed consistency test (distribution of the emissions to 3 times more ships) shows a convergation of the "100 ships, poll" run towards "cont, aerosol". It fails, however, for an increase of the number of ships by a factor of 10 or 30, because then the time between plume emissions is on the order of or shorter than 1 hour and the implicit dilution assumption is not valid anymore. Furthermore, a reduction in plume emission strength also leads to a reduction of plume lifetime. This has to be accounted for in the calculation of time lag between ships (see discussion in the paper) which on the other hand implies that it would not be as strict of a consistency test as could be hoped.

This only supports what we mention at various places in the paper: the use of box models for upscaling can only give first ideas, it cannot replace further studies with high-resultion 3D models (i.e. LES models). Box model studies are, however, important to point to remainig problems that need further attention.

Section 4.1 is almost completely rewritten.

ad 2)

We agree that many additional hydrocarbon related reactions that were not included in the model will occur in a ship plume. To be able to use a comprehensive chemical mechanism like the Master Chemical Mechanism, detailed information on emissions (both compounds and strengths) had to be available which is not yet the case for ship emissions.

ad 3)

In Table 2 the exhaust mixing ratios are given. This are the values in the "puff" of plume air. The aerosol concentrations for the different case are listed in an extra table now.

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We certainly are concerned about the representativeness of the assumed ship emissions. We used data from different sources to find best values and used different emissions strengths for the upscaling model runs. In the discussion of emissions from single ships in section 4.1 we added the medians of the data from Hobbs to give the reader a better feeling for the spread of the data. We also added a few sentences on this in section 4.1.

We prefer to keep the emissions discussions of sections 2.2 and 4.1 separately because the latter would be out of context in section 2.1. In section 3.6 we describe tests of the consequences of different NO:NO2 emission rates, they would be at a wrong place in section 2.2

ad 4)

The effects of a single ship plume is relatively small. Only the overlap of several plumes and emission into prepolluted air lead to the large effects. The total emissions of aircraft even in the North Atlantic flight corridor are less than in the more frequented ship lanes which makes plume overlap less likely. Also, all ship emissions occur at the surface whereas aircraft emissions occur at different altitudes.

In emission inventories for land based point sources (e.g. power plant plumes) problems of plume expansion do occur and are sometimes accounted for by reducing the emissions.

One point that is different for ship plumes compared to land based point sources is that often over the ocean the air is a lot less polluted than over the continents.

Minor comments:

ad 1)

According to personal communication with the authors of the Kasibhatla et al study they did not exclude potentially ship-contaminated data, but as we state in our conclusion one has to be very careful with data from field campaigns that tried to sample "remote

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air" when looking at the effects of ship pollution.

We do not have information about a mismatch in nitrate between models and measurements.

We agree that other processes (like the mentioned NO2 deposition velocity) can be of importance for the discussed problem, we added a sentence in the summary to stress that again.

ad 2)

We stress the restrictions of the model stronger now. At this stage we did not develop a parameterization of in-plume effects for the use in global 3D models.

In the framework of a box model we do not make assumptions regarding stack height, plume rise etc.

ad 3)

We did include a short section on potential difference in a case with clouds and aqueous phase chemistry which was based on a preliminary study with a one-dimensional chemistry-microphysics model. To be able to assess the differences in aqueous phase chemistry of plume affected and clean clouds the composition of the aerosol particles has to be known better. We added a sentence to our discussion of cloud chemistry effects on page 547 to mention difference in aqueous phase chemistry due to the emission of soluble particles.

ad 4)

We did not shorten this section because we feel that the quantification of the NOx related O3 production is of interest.

ad 5)

We shortened section 3.5 a bit.

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ad 6)

See above: major points, 1. We changed the upscaling approach and the related discussion.

ad 7)

We did mean LES, and make it clearer in the text now.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 525, 2002.