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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.

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This paper describes model results from a detailed atmospheric chemistry box-model describing the high concentration chemistry occurring in ship exhaust. The ship plume is mixed with background air. The model results are then scaled-up to represent a ship-track, where several ship-plumes may interact. Comparison with a case that does not account for 'plumes' (constant source) suggests that NOx and O3 concentrations are reduced by a factor of 2 when plumes are taken into account.

The topic is relevant, the paper is thoughtfully written, and I compliment the authors for writing this original and suggestive paper. However, the organisation of the paper could be improved and for the readability it would be good if the length of the paper could be substantially reduced. I also see a number of weak spots, which should be



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addressed. I will discuss them below.

1. The most important one is the very high impact of the plume parameterisation that is counter-intuitive to me. Referring to Table 3, I would expect that the effect of 300 ships (poll), as compared to 100 ships (poll), should be larger than the difference of 100 ships taken as taken as a constant area source (cont,gas) or corrected for plume chemistry (100,pol). Table 3 suggests differently. I must admit that section 4.1 and 4.2 where the effect of multiple plumes and scaling-up is described is not very transparent to me. To feel more comfortable with these results I would like to see the following consistency test. Take the case for 100 ships, keep the summed emissions constant, but distribute it over 300, 1000, etc. ships. If everything is correct I would expect that the results should go into an asymptotic value towards the case 'cont gas'. This test should be shown in an additional figure.

2. A second worry is the validity of the MOCCA model for high concentration chemistry. Is the NO2 self-reaction really the only missing reaction at ppmv level NOx concentrations? Would it be feasible to compare with a more comprehensive gas phase model (e.g. MCM)?

3. The way the emissions are described and included in the model is not very clear. In section 2.2 I understand that emissions are included as a concentration in a plume 1 second after emission and a plume with 10 and 5.5 m radius. The authors should include in Table 2 the actual amount emitted by this specific ship also the particle emissions could be included. I did not see a discussion on how representative this specific ship is for a fleet with large and small ships, dirty and clean technologies (section 3.6 suggests that the is a large difference in results). In section 3.6 there is another subsection discussing emissions and then again in section 4.1 and 4.2. It would be good to give all emission details in an organized way in section 2.2.

4. The authors mention that to some extend the problem is similar to those occurring in aircraft wakes. Yet, as I remember, the effects of including aircraft plume chemistry



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were much smaller. What is the difference? Could the authors comment on this? The authors mention that formally also plume chemistry for point sources on land should be included. To my knowledge, no global or meso-scale model is doing this, and I don't think that the ozone and NOx predictions are terribly wrong. Again could the authors comment on this? Is there a reason why the plume effect for ships should be so much more important for ships than for other point sources?

Minor comments:

1. I wonder if much of the discrepancy between measurements and models is due to the tendency of research ship cruises to stay away from the large pollution sources such as ship-tracks. Perhaps the ITCT campaign mentioned in the summary could address this. Related to this: is there an equally large deviation of nitrate (over the oceans often associated with aerosol) between measurements and models? Also all models include a fairly slow deposition velocity for NO2 over ocean water. How certain can we be of this? These are of course considerations which are not the topic of this paper, but I not think it is a good idea that the impression is made that plume chemistry is the only explanation for the discrepancies.

2. I appreciate that the authors take the simplified approach as displayed in equation 5, and constrain the parameters with observations. However, as the authors remark, the meteorological conditions are very variable, making the results displayed in table 3 that are based on only one set of plume dispersion parameters hard to extrapolate. I think this should be stressed even stronger in this paper. Further, to make the paper more useful for future work, can the authors give suggestions how to use the parameterisation in a 3 D model concept. E.g. as a function of vertical diffusion coefficient and windspeed. In section 2.1 mention the assumptions on the stack height, plume rise, temperature of the plume, assumed time of release.

3. One of the conclusions of this work is that particles emitted by ships do not have a strong effect on gas phase chemistry. I would suggest that it should be clearly men-



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tioned that for aqueous phase chemistry the situation can be quite different. Soot particles consist of partly soluble substances mixed with e.g. sulfate. Clouds formed on ship emissions (ship tracks) show that these particles are good CCN. The aqueous phase chemistry of clouds formed on these particles should be substantially different from that occurring in clean regions.

4. I find section 3.2 rather lengthy and discusses well known knowledge: NOx emissions produce ozone. Maybe it can be shortened.

5. Likewise section 3.5 could be shortened.

6. I didn't figure out how exactly the overlap scheme in 4.1 is working; could be improved. As indicated above I would like to see it tested with equal emissions divided over a variable number of ships.

7. Even meso-scale models, with typically highest resolutions of 1 km, do not really resolve plume chemistry. Maybe you meant LES models?

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