Atmos. Chem. Phys. Discuss., 9, C733–C735, 2009
www.atmos-chem-phys-discuss.net/9/C733/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "What can we learn about ship emission inventories from measurements of air pollutants over the Mediterranean Sea?" *by* E. Marmer et al.

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Received and published: 12 May 2009

This is on the whole a nice paper assessing the impact of emissions from shipping on the trace gas and aerosol chemistry of the Mediterranean Sea, using three different state of the art emissions datasets in a chemistry transport model, and evaluating the respective strengths and weaknesses of the emissions datasets by comparing the model output to regional in situ and satellite observations. It is appropriate for publication in ACP with a few minor to moderate modifications.

First, the review of the first referee is very thorough and I basically agree with all of the suggestions that are made. I would especially like to emphasize support of the

following recommendations: 1) the differences between the emissions datasets should be stated more carefully throughout, e.g., qualifying that the two orders of magnitude difference noted in the abstract only applies on a very limited basis, while the important emissions on even a small regional basis are generally quite consistent; 2) it would be very valuable to the study if it were possible to carry out a sensitivity simulation with an assumed temporal variability in the emissions, quantifying at least the likely range of impact of this, rather than just indicating this is a need for future studies.

Beyond this, my only significant comment is that the manuscript lacks an appropriate perspective in several cases, and some effort needs to go into putting it into a better context for the reader (especially ones that are less familiar with the history of this work), in at least three main aspects:

1) this work should be placed better in the context of past literature. Dr. A. Richter pointed this out for satellite studies in his comment; in addition, the results here should be put in perspective of the results from seminal papers on this topic, particularly Capaldo et al. (Nature, 1999) and Lawrence and Crutzen (Nature, 1999), and the few major developments since then; beyond these, in the abstract it is stated that "very little has been done to evaluate their consistency with atmospheric measurements at open sea", which neglects the fact that a comparison to aircraft observations was already included in Lawrence and Crutzen (1999), and a much more detailed comparison (leading to a debate about the overall significance of ship emissions for NOx) was focused on in Kasibhatla et al. (GRL, 2000) and Davis et al. (GRL, 2001), and similar comparisons are available for other species.

2) the satellite comparison should also be placed in the context of the uncertainty in the satellite retrievals; I am not very familiar with OMI, but for GOME and SCIAMACHY often a detection limit of about 10^15 molec/cm^2 is assumed, and the observed values in Figure 11 are not much above this; a brief discussion of the OMI uncertainty should be included (perhaps in an appendix), as well as a clear indication in the text of what "weak but significant signal" means, and that this error will be in addition to the (already

relatively large) standard deviation bars included in Figure 11.

3) finally, the results should also be placed in the context of what is already known about the behavior of TM5 from other studies and evaluations compared to global observations; this is particularly relevant for BC, which is found here to be overestimated even in the no-ship-emissions simulation, but also applies to NOx and O3, helping the reader to calibrate the extent to which the TM5 simulation is known to reasonably simulate global atmospheric chemistry (depending on how it compares, e.g., in the Stephenson et al. and Dentener et al. intercomparisons, this might end up helping strengthen the overall conclusions).

Beyond that, only a couple minor comments:

A careful grammar check needs to be done before ACP; although it is quite good already, there are a few typos like "zerro" on p. 7168, l 19, and "are not constraint with..." (p. 7172, l. 24) which could easily be caught in the revised version.

The sentence in lines 6-8, p. 7171 is unclear to me; perhaps what is meant is "...are accounted for by domestic traffic"?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7155, 2009.