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Interactive comment on "Constraints on ship NO$_x$ emissions in Europe using GEOS-Chem and OMI satellite NO$_2$ observations" *by* G. C. M. Vinken et al.

Anonymous Referee #2

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Review of "Constraints on ship NOx emissions in Europe using GEOS-Chem and OMI satellite NO2 observations"

In this manuscript, the authors report on the application on an improved method to constrain ship emissions in European waters by combining OMI NO2 observations with GEOS-chem model simulations. They extend on previous work dealing with emission inversions from other sources in several important aspects: a high resolution nested model version is used, results from plume chemistry calculations are inserted into this model, the non-linearity between NOx emissions and NO2 columns is taken into account, and the effect of changes in NO2 air mass factor with changing emissions are

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also considered. The results show some misplacement of shipping lanes in current bottom-up inventories and indicate the need for variable and significant changes in ship emission strengths in different European waters.

The paper is overall well written and reports on an interesting and innovative study. The results are relevant for air pollution research and atmospheric modelling and the study therefore fits well into the scope of ACP.

Unfortunately, the description of the nice work performed is sloppy and unclear in many places and leaves the reader (or at least: this reader) with many questions. I therefore cannot recommend the paper for publication unless the points listed below have been clarified in the manuscript.

Major Comments

1) Consideration of emissions during the 2.5 h time step in the plume-in-grid approach (P 19357): The authors correctly point out that the satellite view is not that of a plume of age 2.5 hours but rather a composite of plumes which one could assume to be formed continuously in the shipping lanes. Why this can be modelled by simply adding fractions of NO_2 left after 2, 1, and 0.25 h is not clear to me at all. Please explain. It would also be good to comment on the effect the continuous emissions have on the background NO_2 levels mixed into the plume in the plume model.

2) Comparison of GEOS-chem and OMI data (Sec. 2.3): here, the average over the full European domain is compared, showing quite reasonable agreement. However, for the present study it would be much more relevant to compare the NO_2 values in the regions with ship emissions and in polluted regions close to these areas. Judging from Fig. 3, this will give quite different results (higher model than measurement NO_2 in the Mediterranean, much higher OMI than GEOS-chem NO_2 in the North Sea). Please extend Fig. 4 with additional regions.

3) The same argument holds for the comparison of DOMINO2 and DOMINO2GC –

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the interesting changes are over the shipping lanes and there I would be surprised to see differences of only 10% as stated in Sec. 3.3 – I expect better spatial resolution + plume chemistry to result in larger differences over shipping lanes also to increasing instead of decreasing values. Please comment.

4) Selection of unpolluted scenes (Sec. 2.4): This is not explained at all! Which criteria have been applied to select the data used in the inversion? Please elaborate.

5) It is not entirely clear to me how Eq. 4 is being used – is that an iterative process? If so, does it converge? Please comment.

6) Displacement of emissions in Bay of Biscay: How can that be the case (I thought that the emission inventory used is based on actual ship positions) and how has the inversion corrected the misplacement (or was this done manually in an ad hoc way)? Please explain.

7) Top down emissions: In the paper it is claimed repeatedly that 39% of all ship emissions are constrained by the inversion. However, looking at the coverage of OMI data used, I can't believe that this is the case. I rather assume that the 4 scaling factors derived from analysis of Figs. 7 have been applied to the much longer tracks shown in Fig. 8 assuming that they are representative for the full shipping lane. While this is probably a reasonable assumption, I feel that it must be clearly stated in the paper and would recommend not to put so much emphasis on the 39% which are not really the fraction of emissions directly constrained by observations.

8) Along ship track averages: How have these been computed? Considering the model resolution of 0.5 x 0.67 degrees, I do not see how you can have so smooth curves in Figs 7 and 9. How was the integral taken – have the boxes in Fig. 5 been rotated, interpolated, and integrated along the line visible in the emission inventories? Where did the integration start and stop? In particular in the Baltic Sea where quite some good will is needed to discern a shipping lane in the data it is important to explain exactly what was done to create the data which are the basis of the inversion.

9) Please also explain how exactly the linear background was found and why the model data were treated this way instead of just using two runs, one with and one without shipping emissions.

Minor Comments:

Introduction: I'm not an expert but to my understanding, proposed legislation does not set limits on nitrogen oxide emissions but rather on technology used in new ships.

P19358, L27: an moderate => a moderate

P19359, L24: OMI retrieves => The first step of retrievals on OMI data yields

P19365, l10: . . . OMI NO_2 change *from a priori changes* is never larger . . .

P19366, L3: base => based

P19366, L12: emissions => emissions in GEOS-Chem

P19366, L25: Is that differences between columns or between enhancements relative to a background?

Fig. 8: Is that for 2005?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19351, 2013.