

## ***Interactive comment on “The climate impact of ship NO<sub>x</sub> emissions: an improved estimate accounting for plume chemistry” by C. D. Holmes et al.***

**Anonymous Referee #2**

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General remarks: This study reveals the error of not including ship plume-chemistry in global chemical transport models and further the consequences on the estimation of radiative forcing from key greenhouse gases. The overall impression is that this is a solid and clearly presented study using sound and valid scientific methods giving credibility to the main conclusions. Uncertainties and challenges are to a large degree well explained though I think the need for better coverage of measurement data could be highlighted somewhat more. This still remains a challenge for a complete evaluation of different plume parameterizations and the error of just using instant dilution. I recommend publication but have some (mainly minor) comments I suggest the authors

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should address. See detailed comments below.

Detailed comments:

Page 3429, Line 17: Figure 1 is cited here without being discussed. It is discussed much later in the manuscript. I suggest to remove the citation in the introduction and to change the figure order in the manuscript accordingly.

P3431, L4-5: “the most comprehensive and detailed global model”. Though it could very well be correct this is a rather subjective claim as no objective comparisons with other models exist and there are a lot of issues going into NO<sub>x</sub> chemistry besides plume parameterization.

P3433, L8-9: Wind speed is added as a factor in the look up table. Since this is a new factor introduced in this study I miss some more information to the reader. How is it influencing CH<sub>4</sub> oxidation and O<sub>3</sub> production and how important is it compared to the other meteorological and chemical factors in the look up table (?)

P3434, L11:  $a = 0.34$  is used. The origin and further use of the “a” factor on other models was a bit unclear. Is this the factor representative for GEOS-CHEM (i.e. model specific) based on an earlier calculation with a model version (Holmes et al. 2011) similar to the one used here? Is it this “a” factor that is used when the equation later is used to calculate the RFs from other models or is it the “a” factor representative for each model or an ensemble mean study (for the other parameters in the equation you use ensemble means from several from other studies). If you are using the same “GEOS-CHEM a factor” for all models how will this add to the uncertainty?

P3434, L17-18: Why is the contribution from stratospheric water vapor not included? Though it is uncertain it is quite common to include in other studies quantifying methane associated RFs.

P3435, L15-16: Why is not surface deposition of NO<sub>x</sub> included in the Gaussian plume model? Wind speed was added as an extra factor in the look up table and it should not

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be so much effort to include deposition. I suggest including it in future studies as not having it certainly adds some unnecessary uncertainty to the calculations.

P3440, L1-4: The discussion of a cancellation in the RF of sulfate and nitrate components is a bit misleading. Indirect effects have been shown to be very important for shipping in previous studies. It should be stated more clearly that you haven't included indirect effects and that these might be important.

P3440, L6: "our RF estimate. . . .to be the most realistic RF estimate to date...". Again a subjective statement. See earlier comment.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3427, 2014.

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