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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The climate impact of ship NO_x emissions: an improved estimate accounting for plume chemistry" *by* C. D. Holmes et al.

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Both referees have provided helpful comments to improve and clarify our manuscript. We appreciate their thoughtful comments and address each of them below in bold.

REFEREE 2

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 should address. See detailed comments below.

We agree that more extensive observations would enable better evaluation of plume chemical parameterizations. This is now stated in the conclusions: "The limited observations of ship plume composition during aging hamper efforts to widely evaluate the parameterization, but we have shown that it is consistent with available data."

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.

Done.

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 NOx chemistry besides plume parameterization.

We deleted this phrase.

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 CH4 oxidation and O3 production and how important is it compared to the other meteorological and chemical factors in the look up table (?)

We have added Fig. S2 in the Supplement to show how O3 production and CH4 oxidation respond to wind speed. In Sect. 2.1, we add "CH4 oxidation and O3 production can vary by a factor of 2 between wind speeds of 2 and 18 m s-1."

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?

As is now explained in Sect. 2.2, "The a term derives from a literature survey of multiple CTMs and radiative transfer models (see SI and Holmes et al. 2011)" and is not specific to GEOS-Chem. We also added a section on "Radiative forcing from long-lived O3 and CH4" to the Supplement to provide more details.

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.

Stratospheric water vapor is now included in all of our estimates of CH4 RF. Text, tables and figures are all updated accordingly.

P3435, L15-16: Why is not surface deposition of NOx included in the Gaussian plume model? Wind speed was added as an extra factor in the look up table and it should not 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.

We tested the effects of adding dry deposition of NOx to the Gaussian plume model. It turns out that N2O5 is already rapidly consumed on sulfate and other

aerosol in the ship exhaust, so neglecting dry deposition had negligible impact on our results. Other NOx species deposit too slowly to affect the plume chemistry over 5 hours.

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.

At the end of this paragraph, we added, "Aerosol indirect effects, black carbon, and organic carbon also contribute to radiative forcing from ships (Eyring et al., 2010) but are beyond the scope of this study of ship NOx."

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

The revised sentence says, "Our global RF calculation using parametric plume chemistry is the first to account for sub-grid scale ship NOx chemistry."

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