

Interactive comment on “Accounting for non-linear chemistry of ship plumes in the GEOS-Chem global chemistry transport model” by G. C. M. Vinken et al.

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We thank the reviewer for reviewing our paper and the provided comments and detailed specific comments. Please find a detailed discussion on your comments below. We adapted our manuscript in line with your recommendations. We marked updates in our manuscript with a red text color.

General comments

1. Pg 17800 L 22-27. *Could you suggest/describe a meteorological situation where the combination of ‘low speed winds’ and ‘low marine boundary layer (MBL) height’ might exist? How low? Would this be a likely situation to exist in areas where there are high*
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ship emissions? I can see that later in the same section the authors analyse the GEOS-Chem model results and describe the frequency of occurrence of the combination of low wind speed and low MBL height as 4%. But it would be good to paint a physical picture with some quantitative numbers of what such events really look like and how common they are in nature. This would give us some ideas of the limitations of the PARANOX model in its current state.

We have defined low wind speeds as wind speeds lower than 4 m/s and a low boundary layer is < 0.2 km. Our analysis of an ensemble of GEOS-Chem output for 1 January, 1 April, 1 July and 1 October shows that the combination of low wind speed and low boundary layer occurs in less than 4% of situations. We suggest that this combination might occur in polar regions or cold areas during winter. These are areas with only little ship traffic. Especially over tropical areas, where a lot of the ship emissions occur, shallow boundary layers below 0.2 km never exist due to the convection.

2. Pg 17804 L 14-15 *Could you please be more specific when describing figure 4. Is it the median value of the NO_x concentrations that is ‘in between the values simulated with the instant dilution and no ship emissions...’? Or are all the quantiles of the LUT approach in between the values of the no ship emissions and instant dilution cases? Stating the actual numbers and comparing them would be better than just referring to the figure, especially since the values appear to be so close.*

Yes, the median value of the NO_x concentrations is between the values simulated with the instant dilution and the no ship emissions models. We clarified this in the manuscript, and included the actual numbers for the medians for the PEM-West B comparison.

3. *For the discussion of NO_x concentrations given in Figure 4, can you be more quantitative about the distribution of values presented. Can you quantify in a statistical sense how different the LUT approach is to the standard approach? And how alike are the results using the standard approach and the no ship-emissions approach? Perhaps*

you could use a Mann-Whitney-Wilcoxon (also known as Wilcoxon rank-sum) test or some other type of non-parametric statistical test to compare the experimental results. In other words, it would be good at this point to make a stronger case for why the LUT approach is superior to the standard approach. Because if the 'temporal mismatches are likely to contribute to the differences between simulated and observed values', then we can't make conclusions by comparing the observations to the simulated values.

We performed the Mann-Whitney-Wilcoxon test you proposed and found that for PEMWA, PEMWB and NARE, the difference in NO_x concentrations between the standard approach (all NO_x emissions released as O₃ and HNO₃) and the model without ship NO_x emissions is statistically insignificant (α level of 0.05). For PEMWA, PEMWB and NARE the difference between all other simulations was statistically significant ($\alpha=0.05$).

4. For O₃ in Figure 5, it is particularly hard to see from this figure alone a better match from LUT approach to the observations than any other approach. Have the authors quantified the differences between the median O₃ in each experimental approach with the observations? A look at the errors (absolute or relative) in the median would be helpful at establishing the goodness of the match.

We now quantify the medians in our manuscript, and clarified that from the O₃ comparison alone we cannot conclude that our method produces the best results. The comparison is included in the manuscript to show that our method simulates O₃ concentrations comparable to the other simulations.

5. Pg 17805 L6-7. Again, to conclude that the best simulation is with LUT using the observations, some more error measurements should be reported in the text or in another figure. Comparing the median or mean values, any quantities, would help make the argument stronger.

Please see our response to the previous comment.

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Specific comments

1. Abstract:

Line 10. Could you be more specific and tell what the 'standard model' is exactly? Is it the GEOS-Chem model with its standard approach to parameterising ship emissions? If so, how does it normally parameterise ship emissions. Would be helpful to say something like 'Model X will be referred to as the standard model.' I can see that the original model is defined in section 4.1.2. I think that it would be helpful to define it earlier in the paper.

Yes, indeed we mean the standard approach used in GEOS-Chem (emitting no ship NO_x emissions, but 10 molecules of O₃ and 1 molecule of HNO₃ per emitted NO_x molecule). We adapted this in the abstract.

Lines 11-15. Please clarify. The figure 0.1 ppbv is given as a 90% increase in NO_x and then 0.1 ppbv is given as a 50% overestimate in NO_x. I am unsure how to interpret this. How can the 0.1 ppbv increase be an improvement and then the 0.1 ppbv increase when using instant dilution be an incorrect over-estimate.

We clarified these sentences. The first increase is with respect to the simulation without ship emissions. The second 50% overestimation is with respect to our new LUT approach. We compared both absolute differences with the same base (our new LUT approach) case to clarify these sentences.

2. Introduction:

pg 17792 L 8 Should read '...a factor of 1.6.'

We adapted this in the manuscript.

pg 17792 L 28-29 Sentence could be clearer as: '..., but does not account for either the effects of temperature or ambient concentrations of ...' Which species make up these ambient concentrations the authors refer to? Are they the same as those species in

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Table 2?

We clarified this sentence. Indeed we refer to the species of Table 2, especially O₃ and NO_x, we added this to clarify the sentence.

3. Model description:

pg 17794 L 6-7 Should read ‘...entrainment of ambient air into the plume.’

We adapted this in the manuscript.

pg 17794 L 8 Should explain that you are using 2K2 as an abbreviation for the year 2002. Sometimes you also use 2k2 (see page 17795, line 12). Should make the use consistent or simply use the year 2002.

We adapted 2k2 to 2002 everywhere in the manuscript.

4. Use of Model to create look-up tables:

pg 17797 L21 Typo with the chemical species HO₂NO₂.

We adapted this in the manuscript.

Section 3.1 when the spatial integrals are described as ‘cross-plume’ does this mean that the integral over each of the 10 rings is taken and then the 10 integrated values are averaged? Line 17 refers to an average over the 10 rings; this is why I ask.

The spatial integrals are integrals over the 10 rings, the word ‘averaged’ should be replaced by integrated. We adapted this in the manuscript.

Pg 17799 L 11. Define J(.) explicitly. For example, define the term in words, for example, ‘J(NO₂) is the photolysis rate constant for NO₂.’

We added this to our manuscript.

Pg 17800 L20. Time t = -5h should be t = 5h, no negative sign.

We adapted this in our manuscript.

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Pg 17800 L22. Should be space between words ‘low’ and ‘marine’.

We adapted this in the manuscript.

5. Results:

Pg 17801 L 23. Need subscript for HNO₃.

We adapted this in the manuscript.

Pg 17802 L 16-17. Authors should explicitly name the seven environmental parameters that they investigate.

We now mention the seven parameters here.

Pg 17802 L 21-23. Might be clearer if re-written. One suggestion is ‘Both the calculated fraction of NO_x remaining and the integrated NOPE are used to compute the reduction in NO_x emissions and the amount of O₃ and HNO₃ produced; this is done to appropriately simulate the effects of original emissions that took place 5h earlier and have been subject to nonlinear chemistry and dilution during the 5h period.’

We have rewritten this sentence in our manuscript.

Pg 17804 L 9 Should read ‘(0 up to 1.2 km)’.

We adapted this in our manuscript.

Pg 17804 L 14 Typo ‘... with the our LUT...’

We removed ‘the’ in our manuscript.

Pg 17805 L 19. It might be helpful to point out that the areas frequently travelled by ships are visible in the figure as almost straight lines across ocean basins. It is remarkable how closely the percent difference follows those ship tracks!

We added this in our manuscript.

6. Conclusions:

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Pg 17807 L24. In my opinion, the intercomparison referred to here needs to be more quantitative (as suggested in the general comments) before it can be called 'comprehensive'.

See our response to the general comments.

7. Tables

Table 1 Footnote: Fourth sentence should read 'In reaction (R1),...'

We adapted this in our manuscript.

Table 2 ITCT 2k2. I think should be ITCT 2002.

We adapted this in our manuscript.

8. Figures:

Figure 2, Caption. Several issues here: Should read 'The shaded areas in ... correspond to.' Units on NO_x emission strength have a typo; the number one should be in superscript. The next to last sentence is not a complete sentence. Should read '... wind velocity is taken to be...' and so forth. Should define θ_0 and θ_5 as the solar zenith angle at the initial time and the solar zenith angle 5 hours later.

We adapted this in our manuscript.

Figure 4, Caption. Second sentence. 'Each box shows...'

We adapted this in our manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17789, 2011.