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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of resolution on ship plume simulations with NO_x chemistry" *by* C. L. Charlton-Perez et al.

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Charlton-Perez et al. examine the importance of model resolution for simulating the effects of NOx emissions in ship plumes. For this, they use a 3D LES model with simplified CH4-chemistry for the tropical MBL. They find a notable effect on NOx lifetime and ozone production efficiency, especially when extrapolating out to typical global CTM resolutions, though interestingly a much smaller impact on OH than what has been indicated by several previous studies. The idea behind this study is very good, and in particular the finding of a log-linear dependence of the various parameters on the grid box volume is quite interesting and likely very useful for future parameterization development. However, it will need several improvements to make the study, and in particular the presentation of the results, appropriately scientifically rigorous for ACP.

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Main comments:

1) In several instances, the introduction and abstract present a relatively inaccurate overview of the previous literature and current state of modeling, to the extent that I am familiar with it. In particular:

- Most importantly among the historical perspective issues, the authors have overlooked a recent study which makes similar steps towards developing a parameterization of ship plume emissions, though using the approach of employing a Gaussian dispersion model: Franke, K., V. Eyring, R. Sander, J. Hendricks, A. Lauer, and R. Sausen, Toward Effective Emissions of Ships in Global Models, Meteorologische Zeitschrift, 17, 2, 117-129(13), 2008. This is available freely at http://www.ingentaconnect.com/content/schweiz/mz/2008/00000017/00000002/art00003. The results are in some ways similar, such as the strong overestimate of ozone by dilution to global model scales. The results of the present study should be placed in the perspective of the Franke et al. study.

- The abstract mentions 'the reduction in model skill' without indicating which reduction it is referring to (see below).

- The terms 'first time' and 'explicit' in the abstract are not really accurate, since other studies have already examined this issue in principle (e.g., using nested box models), and since the finding will only really be explicit when it is done with a large-scale 3D model including something like a plume-in-grid or similar parameterization which can be compared directly with observations.

- In the introduction, which studies are the authors referring to which indicate that there is generally a substantial reduction in model skill for the MBL when ship emissions are included? A few early studies such as Kasibhatla et al. (2000) and Davis et al. (2001) did indicate this to be the case. However, in contrast to this assertion, the recent study of Eyring et al. (2007), an intercomparison of ten models, including a comparison with the limited observations that are presently available, concluded that 'in the lower

ACPD
9, C1168–C1175, 2009

troposphere . . the models are capable to reproduce ozone (O3) and nitrogen oxides (NOx=NO+NO2) reasonably well'.

- 'ship emissions are not typically included in global 3D CTMs'; although several authors have noted the uncertainties and problems with current emissions inventories, most models that I am familiar with nevertheless do include some representation of ship emissions (EDGAR, Corbett, Eyring, etc.). This statement needs to be either modified or justified with citation of several models which currently neglect ship emissions.

- 'this overestimate is usually attributed to . . . resolution'; this should be 'partially attributed', since many studies have indicated this cannot explain all discrepancies.

- The studies of Song et al. and von Glasow et al. should be mentioned in the introduction to better put this study into a proper context (at least P 8591 L 5, if not earlier).

- It would be easier to read if the discussion of Lawrence and Crutzen (1999), Kasibhatla et al. (2000) and Davis et al. (2001) were put in chronological order, also mentioning that in Lawrence and Crutzen, some observations such as those from the OCTA campaign compared to the model output did support the computed effect of ship NOx emissions, while the more extensive data available to Kasibhatla et al. and Davis et al. indicated the model simulations tended to overestimate the observations. The comparison with observations in Eyring et al. (2007) could also be added to this perspective.

- Rather than 'have been attributed by some authors to model resolution effects', more accurate would be 'proposed . . .' or 'hypothesized to be due to . . .'

2) The simulations do not include NMVOCs. Although this will be appropriate for some regions, much of the worldwide ship traffic is near the coasts along the route between the North Sea, the Mediterranean, Suez Canal, and across the Indian Ocean through southeastern Asia, where NMVOCs are likely to have a substantial impact on the re-

ACPD

9, C1168–C1175, 2009

sults. Also, ships emit NMVOCs themselves, there is still a considerable uncertainty in this. The importance of this should be discussed.

3) The simulations are done for topical conditions; likely differences for extratropical conditions should be discussed, especially in the perspective of comparing to some of the other previous studies.

4) It should be made clear in the setup description that it is assumed that the ship smokestack stays fixed at the right edge of the domain and the relative wind streams past this (this first become clear in the discussion of results).

5) A wind speed of 1 m/s is used in the simulation, since this is assumed to be the relative wind speed in the reference frame of a fixed smokestack. However, won't the surface drag and the turbulent energy spectrum be different for a simulation with a 1 m/s wind relative to the ground compared to one with an $\sim$10 m/s wind? At least this would be expected in reality, even if the model does not capture this. Please discuss the implications for this study.

6) P 8596 L 24: what is meant by the winds being 'interpolated' back to the starting conditions every six hours; are they not simply reset to the initial values? If not, please explain. Also, please comment on whether or not the recurrent temporal discontinuity in the wind fields will have any implications for the simulation.

7) The domains of the LES simulations which provide the winds are tiled together along the plume axis; please indicate whether this is done for the LES simulations that produce the winds, or only for the CTM simulations that use the winds (in that case from the smaller parent model domain), and if the latter, whether this introduces discontinuities in the winds along the boundaries where the tiling is done and any implications this would have.

8) P 8592 L 19: indicate the background of the values of ship speeds from the previous studies (e.g., are these based on Lloyds or IMO statistics or similar?)

ACPD

9, C1168–C1175, 2009

9) Are there any mean transverse winds, or is the tendency of the plume to move off to one side of the domain the result of asymmetries in the turbulent eddies (and if so, where do these originate)?

10) P 8598 L 27: the plume width does increase with distance, but the plume height actually does not; it increases for the first 4 km, but the decreases up to 8 km and stays about the same up at 20 km; if possible, please analyze the 3D fields more closely to explain this apparently unusual behavior (perhaps it is a function of the chosen contour intervals).

11) P 8600 L 19: the speculation that the asymptotic behavior between C1 and C2 depends on the similarity in the meteorology seems inconsistent with the basic idea of the study that the main effect on O3 and the related species is due to the dilution (i.e., the dependency of O3 and OH on NOx levels and the feedback this has on NOx lifetime), which one might expect to apply regardless of the similarity in meteorology. Is it possible that there is a limit to the dilution effect at very small scales such as C1 and C2? It would be very enlightening if some deeper analysis or further sensitivity studies could be done to elucidate this interesting aspect.

12) P 8601 L 2: The study of von Glasow et al. (2003) used a box model, not a global chemistry model, to examine the effect of dilution of ship plumes (this is mentioned in the first sentence of the abstract of that study, which needs to be read through more carefully by the authors so that the present study can be put in a more proper perspective with respect to the previous literature).

13) It would be elucidating to compare the rate of plume expansion (horizontal and vertical) in this study with the assumed or computed rates in the earlier studies like von Glasow et al. and Song et al. (a brief note of this is already made in the text, but would be helpful to be more detailed on this point of comparison).

14) P 8601 L 13-15: it would be good to mention explicitly that the approximation of equating NOx loss to HNO3 production is exact in these simulations, since the reaction

ACPD

9, C1168–C1175, 2009

of HNO3 with OH and its photolysis are neglected.

15) P 8601 L 23 (also p 8603 L 22): 'as suggested by Chen et al.'; was this suggestion based on observations? If so, please describe, that will help make the conclusion stronger (and perhaps indicate what other competing candidates for NOx loss would come into questions).

16) P 8602 L 13-15: the 'OH halo' result is very interesting; would this be influenced much by the inclusion of complex NMVOC chemistry (e.g., near coasts)?

17) P 8603 L 1+: for the simulations with halved emissions, are the OH and NOx slopes also approximately half as large as with the full emissions? Can the authors offer any explanation for the contrasting behavior of OPE?

18) P 8603 L 21 (and elsewhere where appropriate in the conclusions): make clear that this result is specific to these conditions and may vary under other conditions.

19) Somewhere in the manuscript it would be worth briefly discussing what the resolution does to the mean NOx mixing ratio, not just the NOx lifetime, since it is the mixing ratio which is measured and compared to in previous studies like Lawrence and Crutzen (1999) and Kasibhatla et al. (2000).

20) P 8604 L 3: running the LEM at even a higher resolution to explore the convergence (also see the note about its interpretation above) is an excellent idea for this study: if at all possible (within computational limitations), it would be very much worth doing so in a revised version.

Technical suggestions:

For the Forster et al. (2007) reference, the IPCC should be mentioned (at least in the reference list, usually in the citation in the text, since most readers will identify it by this.

P 8589 L 28: 'around 10% of the total radiative forcing' – is this 10% of the increase since preindustrial times, or of the absolute total?

ACPD

9, C1168–C1175, 2009

P 8591 L 21: Since there are many other errors such as mixing processes, chemistry and emissions of other tracers, as well as an incomplete observations set for model evaluation in the ship-affected regions, it will unfortunately not really be possible to 'fully account for the erroneous representation . . .'; this should be rephrased.

P 8592 L 17+: The description of the winds needs to be written more clearly, it took me a couple readings to understand what was meant; a sketch would help.

P 8600 L 16: 'linearly dependent on resolution' should be 'linearly dependent on the logarithm of the gridbox volumes'

P 8601 L 16: 'NOx loss' should be 'NOx concentration'

P 8602 L 1: 'emission' should be 'plume'

P 8602 L 5+: indicate whether the simulations discussed here were done at high resolution

P 8603 L 6+: are the no-ship simulations done at all the resolutions, so that e.g. C4(ships) is compared with C4(no-ships)? If so, are there notable differences between the various no-ships runs themselves due to the resolution, or are they all more or less homogeneous? Please indicate briefly in the text.

P 8604 L 13: 'starting point' is not really valid, since in principle a couple other studies have already looked at the resolution/dilution issue for ship plumes (albeit with simpler approaches than used here); 'important step forward' would be more fitting.

P 8604 L 16: the link to power plant plumes is in principle appropriate, but that research has a long history of its own, and is affected by very different chemistry, so that it is more likely that the ship research community will learn from them than the other way around.

Make figure 8 into Figure A1 to avoid confusion.

P 8606 L 19: 'dimishes' should be 'diminishes'

ACPD

9, C1168–C1175, 2009

From figure 5, move the text on the no-ships run into the main text

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8587, 2009.

ACPD

9, C1168–C1175, 2009