

***Interactive comment on* “The influence of European pollution on ozone in the Near East and northern Africa” by B. N. Duncan et al.**

B. N. Duncan et al.

Received and published: 23 February 2008

We wish to thank the reviewer for the many helpful comments. We have carefully addressed each of the reviewers concern below. The reviewers original comments are in italics.

General comments This is a well written, very interesting and important paper. I think it really clearly shows the importance of European ozone pollution on the surrounding regions, a topic that has been largely overlooked when long-range transport issues have been considered. Policymakers, such as those involved with the UNECE Convention on Long Range Transboundary Air Pollution (CLRTAP) should be alerted to the results.

We appreciate your positive comments. We think that this work is important too. We will make sure that the CLRTAP is aware of our work, as both Bryan Duncan and Arlene

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Fiore are involved in CLRTAP activities.

I have only a few, relatively minor comments, suggestions and requests for clarification. Specific comments p1914, 17-10. "We estimate that 19000 additional mortalities occur annually in these regions from exposure to European ozone pollution and 50 000 additional deaths globally; the majority of the additional deaths occurs outside of Europe." This sentence is not entirely clear. It says that there are 31000 deaths not in the Near East/N. Africa, and >25000 outside Europe, and <25000 inside Europe, but it takes a bit of thinking about. I suggest it could be made more obvious exactly what your findings on additional mortalities are; I guess by listing the key numbers from Table 2.

We clarified the sentence in the abstract with "We estimate that European ozone pollution is responsible for 50,000 premature mortalities globally each year, of which the majority occurs outside of Europe itself, including 37 percent (19,000) in northern Africa and the Near East."

p1917, 114. By "augmented", do you mean that the regional emissions inventories overwrote the global EDGAR data, or that they were somehow merged? If they overwrote, were there any problems with discontinuities at the boundaries? Clarify exactly what was done.

The base inventory (EDGAR) was replaced by regional inventories, work done at Harvard University (see acknowledgements). We changed "augmented" to "replaced" in the text, which is a better word to describe what was done. We believe that the regional inventories better describe the emissions for those regions than the base inventory. We do not consider discontinuities at boundaries an important issue as the regional inventories cover the bulk of emissions from each region (i.e., East Asia, Europe, and North America).

p1918, 114. We need to know the units of ENOx - is it in N or NO2, as this will obviously affect the EO3 value.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



We changed the name of Eozone and EHNO_3 to Pozone and PHNO_3 , as the terms describe the production, not emission, of these two secondary products.

The units of ENOX are molecules NOx/s and the units of Pozone are molecules ozone/s. While the observational ozone production efficiency (i.e., factor of “10” in Eq. 1) is unitless, it is defined as the number of molecules ozone produced per molecule NOx consumed.

Eq. 1 and 2 are only approximations. Certainly, and hopefully, better methods of treating ship emissions, including identifying key in-plume chemical reactions, will be developed in the future.

p1918, I19. What is done with ship emissions at >60 degrees latitude? Presumably they are not pre-processed at all?

We have clarified this with “We approximate Pozone between +/- 60 degrees latitude as ice reflectance enhances JNO_2 at latitudes greater than +/- 60 degrees; the shipping NOx is emitted as HNO_3 at latitudes greater than +/- 60 degrees.”

p1918, I23/24. “Boxes without other emissions sources.” Does this include natural oceanic emissions, such as DMS or hydrocarbons? (Possibly the model does not include these; if it does, perhaps this should be “Boxes without other anthropogenic emissions”).

We do not include natural oceanic emissions in our model simulation. To clarify, we modified this sentence to: “we pre-process shipping NOx in model boxes containing open sea only”

p1918, I25. Add “open sea”: Our approximation gives an annual production of ozone from open sea shipping of 4.4 Tg ozone...

Done.

This begs the question: what fraction of ship NOx is emitted in coastal regions? I.e.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

what fraction is not subject to this preprocessing calculation, and is therefore presumably not accounted for in the value of 4.4 Tg O₃?

This number refers to the annual ozone production from open sea ship emissions, not to the global burden of ozone. To avoid confusion, we now say:

“Our approximation gives an annual production of ozone from open sea shipping NO_x that equals about 0.2 percent of the total tropospheric production.”

The total shipping emissions of NO_x in the EDGAR 3.0 inventory is 3.1 Tg N, the same value used in the S1 scenario of the Eyring et al. (2007) model intercomparison. About one-third of shipping emissions are in open sea model boxes. We have clarified this in the text.

This value could be compared to the results from a recent multi-model assessment of ship emissions (Eyring et al., 2007: ACP 7, 1995-2035, 2007)- their Figure 9 shows a similar value for tropospheric ozone burden due to ships, from a similar total ship NO_x emission - even though none of the models in that work applied pre-processing.

As mentioned above, we presented the annual production of ozone, not the burden.

p1919, l1-8. Maybe comment that Eyring et al. (2007) (see previous comment), which applied no plume processing, but used similar emissions, found roughly double this contribution of ships to surface ozone (e.g., their Fig. 7f).

We added the following sentence:

“Eyring et al. (2007) reported a multi-modeled ensemble mean of 2-13 ppbv surface ozone due to shipping emissions over the North Atlantic in July, which is almost twice that as in our study; the models that participated in the study, including the GMI CTM, directly emitted shipping NO_x as NO_x.”

This discrepancy between the two studies (i.e. that the change in ozone burden due to ships is similar, but that the change in surface ozone concentrations is only half that in

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Eyring et al. (2007)) suggests that transport from the marine boundary layer to the free troposphere is rather different between the GMI model and the Eyring et al. models (stronger in GMI). I think this is a point worth making concerning the uncertainties associated with the impacts of ship emissions, in addition to the potential importance of plume processing.

See our comment above.

p1923, l14. “(2K/km)” - perhaps slightly expand on this tropopause definition. Is it if the local vertical temperature gradient across a model layer (or between layers?) is $\geq 2\text{K/km}$, then you assume you are in the stratosphere?

We simply used the same tropopause pressures (NCEP) to calculate the column in our model as Ziemke et al.; we did not diagnose the tropopause ourselves from the meteorological fields (GEOS-4-DAS) that we used to drive transport in our CTM. For the model box containing the tropopause, we accounted for the portion below the tropopause in our total TCO.

We clarified this by saying “we use the same tropopause pressures as Ziemke et al. (2006).”

p1924, l8. Delete first “and”.

Done.

p1924, l25 and Figure 4: Is it worth extending the domain plotted in Figure 4 up to 90N, so that the extension of the yellow/orange (5/10 ppbv) ozone difference contours further into the Arctic can be seen? I appreciate the main emphasis of the paper is on the Near East and northern Africa, but it would also be interesting to see the European impact on the Arctic.

Done. In Figure 4, the perturbation in July is 2-5 ppbv in the Arctic, except 5-10 just north of Europe to 80 degrees N. In Figure 6, the perturbation in October is 2-5 and in January, it is 0 to -2, except -2 to -5 just north of Europe to 80 degrees N.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p1925, I19 and Figure 6: Is it worth adding a spring (April) panel to Figure 6? This would mean all 4 seasons are shown, and spring is particularly interesting, given that most remote northern mid-latitude regions show a spring peak. I also think the switch from ozone decreases to increases is probably not symmetric either side of the summer, and it would be interesting to see and discuss this.

We added April to Figure 6 and extended the latitude range to 90 degrees N in both Figures 4 and 6.

p1928, I1-3. Presumably the definition of “Western Europe” (used for the mortality rates) is a little different to “Mediterranean Europe” (used for the population density). It is probably worth clarifying these - in particular what are the relevant fourteen world regions used for mortality? (p1927, I9)

Definitions of the 14 world regions are given in the World Health Organization (2004) document and are mapped onto the modeling grid as in West et al. (2006). We clarified this by modifying the sentence:

“Values of y_0 are derived from the World Health Organization (2004) for the total non-accident baseline mortality in each of fourteen world regions, where these regions are defined by the World Health Organization (2004) and mapped onto the modeling grid as in West et al. (2006).”

p1928, I26. “...uncertainty in ozone-related mortality calculations...is fairly substantial” - Can this be better quantified? I appreciate this is difficult (and that you follow this with some discussion on the topic), but I wonder if terminology along the lines of IPCCs “likely” (>66 percent chance) and “very likely” (>90 percent chance) etc., could be applied? Phrases like “fairly substantial” are not that useful.

We clarified this sentence with: “The uncertainty in ozone-related mortality calculations is substantial, as the uncertainty in beta alone causes an uncertainty of a factor of 1.7 in similar estimates of total mortalities, with additional uncertainties due to the ozone

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

modeling, baseline mortality rates, and the low-concentration threshold (West et al., 2006; West et al., 2007).”

Technical corrections p1917, l16. “Verstrengetal.,2005”. Add spaces and check: this is Versteng et al. in the reference list.

It should be Vestreng et al., 2005. We have corrected this.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1913, 2008.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

