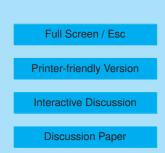**Atmospheric
Chemistry
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Discussions**

Interactive
Comment

Interactive comment on "Megacity ozone air quality under four alternative future scenarios" *by* T. M. Butler et al.

Anonymous Referee #2

Received and published: 30 January 2012

This paper describes the effect of megacities on tropospheric ozone and explores how this is likely to change under realistic future emission scenarios. It demonstrates that emissions in these regions have a relatively small impact on ozone on a global scale in proportion to their share of precursor emissions, and that under most scenarios this is likely to drop further in future. The authors suggest that this will increase the sensitivity of urban air quality to emissions from surrounding regions in future. The paper also examines the sensitivity of the results to assumptions about how projected national-level emission changes are] downscaled to the high spatial resolutions needed for air quality modeling, and highlights that further attention to this is needed during generation of emission scenarios in future. The results of the study are interesting and valuable, and the paper is appropriate for publication in ACP with some minor corrections, detailed below.

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General Comments

The model studies are performed at relatively coarse resolution that is 3-4 times larger than most megacities at the current time. While this may introduce only a small bias in assessing their global impacts, it is likely to introduce much larger errors in assessment of megacity air quality, a focus of the paper highlighted in the title. The authors need to be clearer in section 3 about the implications of their choice of resolution by noting the potential biases for assessment of air quality in particular.

Many megacities experience substantial titration effects from high NO removing ozone, and these effects are likely to vary by season and location. How much are these effects expected to influence the conclusions drawn about redistribution of emissions? Given that the countries involved vary in size greatly, the influence of redistribution will be dependent on the fractional importance of megacities in different countries as well as on latitude and emission magnitude. It is clear from the lower panel in Fig 5 that the redistribution has different effects for different cities within a single country (e.g., the US) and explanation of this finding is needed in section 4.

Specific Comments

p.130, l.13: "differing downscaling" - need to be more specific here

p.135, l.23: Were CH4 concentrations assumed fixed, or was some surface boundary condition used? A brief statement of this here would be useful.

p.135, l.28: "made more objective" is unclear; please be explicit here.

p.136, l.5: Stock et al. and Collins World Atlas are both missing from the reference list.

p.140, l.21: The section referred to here is unnumbered and appears at the end of the current section (p.141). The section should either be numbered or absorbed into the main body of section 3 here, and the forward reference "in Sect 3" (p.140,l.21) replaced with "below".

p.141, l.27: What is the justification for choosing a 25% emission reduction here? Why not remove the megacity emissions by harmonizing them over the country as a whole? Some additional explanation for these choices is needed here.

p.144, l.9-10: This finding that the redistribution of emissions has little effect on global ozone could be highlighted in a much more positive way, as it suggests that the exact distribution of emissions isn't critically important for chemistry-climate model studies. The value of this result could also be noted more strongly in the conclusions (p.148, l.16).

p.146, l.19: The 0.84% result described here in the conclusions should appear earlier in the text. It would be useful to discuss how this compares with the effect of other surface sources, either from this study (if available) or from previous studies, e.g., under the QUANTIFY project. It would also be interesting to quantify how much smaller the megacity impact would have been if the same emissions were uniformly redistributed.

p.146, l.21: "ozone precursor" Should this read "NOx"?

p.146, l.24: "relatively sophisticated" is unclear, please rephrase this and be more specific.

p.147, l.21: "Similar trends have already been observed..." please supply a reference here.

p.148, l.11: Emissions are usually generated on a country and sector basis, and the geographical distribution is a separate procedure. The call for finer resolution here is understandable, but given that different users have different needs, approaches and model grids, it would perhaps be more valuable to call for scale-independent emissions (area or vector) which could then be applied or mapped to any geographical model grid.

p.155: Tropospheric ozone column densities are usually presented in Dobson Units, so it would be helpful to note in the caption what the base case 0.75×10^{18} molecules/cm2 is equivalent to about 28 DU.

Given the discussion in section 4 it is unnecessary to show data from each city in figure 6-8. These figures are too crowded. It would be a lot clearer to average the results over each latitude band and show the mean (as a line) and standard deviation (as an error bar), and the resulting five lines would make the seasonality (and spread) much more apparent to the reader.

Typos

p.130,l.12: due -> due to

p.131,l.16: increased -> improved

p.144,l.22: remove "when"

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 129, 2012.

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