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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional pollution potentials of megacities and other major population centers" *by* M. G. Lawrence et al.

Anonymous Referee #3

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General Comments

The paper aims to intercompare the potential of impact of the most populated cities in the world depending on their locations. As the number of megacities is growing and the density of population living in such megapoles is also increasing, the understanding of the large scale influence of emissions from these spots are highly relevant within the scope of atmospheric chemistry. For such purpose, the authors defined original metrics allowing quantification of both horizontal and vertical exports of artificial tracers. The choice of the metrics in term of distance and altitude from the megacities as well as the lifetime of artificial tracers is largely discussed (it could sometimes be shortened or shift in the annex). Furthermore, as this study is in some way quite academic, the authors made an effort to enlarge the discussion of their results to real

atmospheric species providing interesting conclusions.
To my opinion, this paper provides quantitative information to intercompare the transport of pollutants from megacities and will be helpful to discuss future studies of intercontinental transport of pollution from growing population centers. As a consequence, I support the publication of this paper after clarifying some technical aspect addressed hereafter and shortening some sections.

Specific Comments

Abstract:

The fourth point should be more concise and clear.

1. Introduction:

No citation is made regarding impact of megacities on global atmospheric chemistry. For example, the work of Wild and Akimoto JGR 2001 and Akimoto Science 2003 deal with similar issues (difference of intercontinental transport depending of locations). It is a pity that the papers of Butler (2006a and b) were not submitted, it does not allow to examine the interest of such references.

2. Methods:

The model description should be at the beginning of this section (spin-up and resolution of the model are useful for the understanding of the method and should appear first). Then should be the paragraph describing the choice of tracer and the way they are injected in the model (last paragraph of section 2.1). Tracer source location and metrics should be described at the end.

Table 1: Would it be possible to document the approximative surface of the selected MPC? Are the artificial tracers always injected in a single gridbox (except for the

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Szechuan basin?)? What is the approximative surface corresponding emission surface in the model?

Table 1 and 2 could be merged.

Section 2.2 try to limit the description of the metrics you finally used and put the discussion about other distance threshold (for ELR) in annex. The paragraph “To compute ELR_col ...geometric surface area of the circles) could be in annex.

Section 2.3 : Regarding the previous validation of the model, was there any evaluation of the transport using passive tracer such as Radon ? Could you please indicate the height of the first layers (layers in the troposphere or boundary layer at least)?

3. Qualitative and quantitative dispersion characteristics

Section 3.1: Sentence “However, export at this scale ... using ELR_col”, what is the tracer you deal with T1d or T10d?
Last paragraph, you explain what the table does not contain before explaining what is does contain. Last Sentence, you talk about a new metric describing the vertical export, if it is ELR_UT, it is already described in the section 2.2.

Table 4: intervals for rank are not really useful, to my opinion, it could be removed.

Section 3.2, third paragraph, you begin by explaining that export is almost uniformly largest during winter. And few sentence later you tell that it is similar throughout the year, please precise “intensity” in the first sentence and “direction” in the second one.
The paragraphs dealing with the sensitivity of results toward the choice of metrics and

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tracer lifetime (Finally, this section has focused .....peak mixing ratios at the sources are much less pronounced) could be in annex and thus removes from the paper.

Section 3.3, - second paragraph, for computing the correlation between ELR_UT and convection, how long is the period considered for computing mean convective mass flux for each tracer? - third paragraph, please precise “The spatial variability”
The text in parenthesis : (whereas using the current setup provides a lower limit to the variability....) is not useful.

Section 3.4, third paragraph, the reasons for which you exclude this supplementary metric “ We did not generally include ... in this particular case” could be removed.
In the sentence “Interestingly, some of the other source locations ... all 3 lifetime tracers”, which metric is concerned : A10 or the new one?
To my opinion, the following discussion if far less interesting than the comparison of impact of the different MPC and should be shortened.
Last paragraph, “A100 is only weakly correlated with ELR1km (r2=0.1)”, I think with a so low correlation coefficient, it is NOT correlated.

4. Conclusions
First paragraph, the discussion about mesoscale modelling should only appear in the perspectives given at the end of the conclusion.
Point 6., please cut this long sentence to clarify.
Point 7., please reformulate the first sentence to clarify.

Technical Corrections
Section 3.1: Sentence “However, export at this scale ... using ELRcol”, a parenthesis is missing.

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Section 3.2: Sentence “On the other end ....rank of 31.5” : term “On the other end” should be “On the other hand” and “of ELR_1km for the T10d tracers” can be removed as all this paragraph deals with these tracer and metric.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 13323, 2006.