Atmos. Chem. Phys. Discuss., 6, S7203–S7205, 2007
www.atmos-chem-phys-discuss.net/6/S7203/2007/


Atmospheric Chemistry and Physics Discussions



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

**M. G. Lawrence et al.**

Received and published: 12 March 2007

We especially appreciate the extensive and thorough comments of this referee and plan to follow them to the extent that we can. Here we provide short responses to the major comments (numbered as in the review), and will provide more details plus responses to the minor comments along with the revised version.

1) The suggestion to include some discussion of the prevailing meteorological conditions is very sensible. In doing this, we will have to balance between the additional material and the general request of all three referees for shortening the overall manuscript. Probably this can be coupled with the other suggestion of this referee for separating the general and MPC-specific discussions, and the relevant meteorological conditions can then be discussed for a few selected MPCs.

2) We will try to refocus the manuscript to bring out the new findings better and separate them more clearly from the more evident results.

3) Since we've been working on this on and off for over two years, the relationship between the details for the individual cities and the overall results has probably become too familiar to us. The suggestion to separate these discussions for new readers makes very good sense and we plan to follow this in the revised version.

4) We agree that adding information on the application of the results to real tracers in the conclusions would increase the relevance and plan to do this in the revised version.

5) We agree that it would be good to test the robustness of the results in various ways (as we suggest in the conclusions), but believe that doing so here will not be possible combined with the objective of shortening the overall manuscript. However, we are already planning a follow-up study in which this will be done with the ECHAM5/MESSy chemistry-climate model (Joeckel et al., ACP, 2006), which is designed to easily allow a variety of model configurations (including various convection parameterizations), and is thus a very good testbed for the overall robustness of the results. We are also considering setting up a model intercomparison on this basis, if we can manage to fit this into the framework of one of the projects we are involved in.

6) We doubt that the scale limitations of the model will invalidate the qualitative results and the main conclusions, since we are focusing on a scale that we know the model performs well on based on earlier studies (as discussed in the manuscript). However, we cannot rule this possibility out until well-designed comparisons with regional models like those suggested in the conclusions are eventually carried out, and agree that this is a very significant priority for future studies (and are also considering this in one proposal we are currently writing).

Specific comments (only responses to a few main ones):

The correlation of the metrics with latitude was in one of the earlier versions of the



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manuscript, and we will consider reincorporating it, but it will probably be preferable for the sake of not making the manuscript too much longer to just indicate the correlation coefficients in the text (which is done already for ELR_UT). The relationship to convective mass fluxes is also already given for ELR_UT; we will look at this for the other metrics and include this if it provides significant new information.

We are thankful for the comment about the electronic supplement being appropriate; it is good to have this confirmed from the referee perspective and we will keep it in the final version.

The suggested modification to the caveat in the conclusions is well-stated and will be followed.

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