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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The paper of Butler and co-authors "Megacity ozone air quality under four alternative future scenarios" addresses an important question regarding future air quality projections. An original approach is implemented to document the relative influence of megacities and background air quality over the course of the 21st century. The latest source of public anthropogenic emission projections is implemented in a chemistry transport model. By redistributing the emissions of megacities, their impact on background ozone levels is quantified. The emissions scenarios are presented in details, and an innovative technique to redistribute the emissions of the megacities is introduced to avoid the shortcomings of previous annihilation (that consist in zeroing-out emissions over a given area) techniques.

It is found that the impact of megacities at the global scale is small (compared to their

emission potential) and it remains so in the projections for the 21st century, hence supporting the findings of a previous paper of some of the co-authors. Thanks to the technique of redistribution of the emission, the sensitivity of ozone in megacities to local and global concentrations is assessed. Summertime ozone air quality in megacities becomes more sensitive to the background in the future, this sensitivity being exacerbated in the more pessimistic scenarios.

The methodology and presentation are of high quality, the results are analysed thoroughly and the findings contribute to an important environmental question that raises significant scientific challenges. Consequently I support the publication of this manuscript in ACP. Nevertheless, the uncertainties associated with the implementation of a global model, and emission inventories designed for climate studies (the RCPs), to document urban air quality should be highlighted further. More specific suggestions on how to handle this comment are given below.

General comments

P141L1-17: The main advantage of using a global model to address the issue of megacity air quality regards the worldwide consistency of the evaluation. It also comes with a number of drawbacks, including the resolution. This paragraph is useful to document the related uncertainty however this paragraph (and the unique reference quoted) is not sufficient to support the robustness of the conclusions drawn in the paper. There are strong uncertainties associated with implementing a 2.8degree resolution model to address air quality at the scale of megacities and this should be acknowledged more clearly. Specifically it would be interesting to know the sensitivity of ozone regimes (and not only the bias) to emission perturbation change with the resolution. In addition it is not clear what is used for reference for the bias (the T106 simulation or observations?).

P145: The main result of the paper on the projected impact of megacities lies in Figures 7&8 which is difficult to read. A box plot with time in the x-axis and in the y-axis the mean and standard deviation of responses across all megacities and for the summer

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months for each RCP would be more explicit.

P148L20-25: The ability of the RCP to propose a suitable representation of urban emissions is very questionable. It should be recalled that they were not designed for air quality studies. Earlier in the paper a factor two uncertainty between local and global emission inventories is mentioned. Some concluding remarks on the associated uncertainty, and the overall small signal found here and in Butler and Lawrence (2009), would prevent from any misinterpretation of the results.

P131L21-22: The references quoted do not support well the statement on the relative increase of the role of the background in the context of decreased emissions. None of the referenced paper addresses trend analysis, and the 1999 paper as well as paper published 10 years emphasize the importance of background emission. They would thus support a lack of change of the relative role of local and long-distance pollution. Alternatively, there are studies, e.g. in the HTAP initiative, that would be better suited to support this statement. The authors should consider a revision of the organisation of this argumentation since the changes in the sensitivity of megacity AQ is part of the question addressed in the paper. It is thus surprising to find such a definitive statement in the introduction and a dedicated discussion in the conclusion would be more insightful.

Specific comments

P131L6: "[Megacities] have the potential to influence air quality on regional and even global scales" a reference to support this statement would be useful given that the only published paper referenced in the paper on that topic (Butler and Lawrence, 2009) tends to minimize this impact.

P131L17: define what is meant by “toxic pollutant”.

P132: The apparent contradictory findings of Lawrence et al. (2007) and Butler and Lawrence (2009) are addressed in the second paper. But it would be useful to refer to

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it briefly here to avoid the feeling that paragraph 2 and 3 of page 132 are contradictory.

P133L15: Isn't CMIP an ensemble of couple ocean-atmosphere models?

P133L20-23: Isn't it due to the underlying hypothesis of RCP8.5 (no climate policy) that there can't be any co-benefit for air pollution? Also some clarification would be useful about the explicit resolution of technological measures in RCP2.6 and 8.5. To my knowledge it is the RCP2.6 that uses an implicitly resolves technological improvements (Kuznets curves) while realistic emission factors related to the current air quality legislation are used in the RCP8.5, at least until 2030.

P135L23: Are 100% of the primary nitrogen emitted as NO in the model?

P136: How sensitive is the megacity detection algorithm to projected changes of population density?

P138L5: It would be interesting to add the RCP-8.5-P to Figure 3.

P144L2: "their global impact, are overestimated in RCP-2.6" shouldn't it be the case for both RCP2.6 and 4.5?

P144: A factor 2 local difference is mentioned between local and global emission inventory. Is this uncertainty reduced when aggregated over several megacities? The average and sigma over several cities for both types of inventory should be provided to support the statement on the increased robustness of the results when using a large number of cities.

Minor comments:

P136: A couple of significant digits should be added to Table 1 in order to avoid "zero" emissions.

P141L2: "coarse model resolution".

P141: The paragraph on the resolution should be the second paragraph of section 3

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and not the 3rd.

P142: The readability of Section 4 (Results) would be improved if there were subsections.

P142: "and the percentage change in surface ozone mixing ratio from the redistributed run" please be more explicit with regards to how is computed this percentage, is it (mask minus redistributed) divided by redistributed ?

P144: remove "when" in "in ozone mixing ratio when from the 2005"

P145: "as a percentage change for 2030, 2050, and 2100" please be more explicit with regards to how is computed this percentage.

Figures: The titles of the Figures should be made more "human readable" and resemble less variables of a computer code.

Figures S1-S3 are difficult to read because the order of the colour scale differs from the order in the plot.

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