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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluating BC and NO_x emission inventories for the Paris region from MEGAPOLI aircraft measurements" *by* H. Petetin et al.

Anonymous Referee #1

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Interactive comment on "Evaluating BC and NOx emission inventories for the Paris region from MEGAPOLI aircraft measurements" by H. Petetin et al.

MS No.: acp-2014-363 Title: Evaluating BC and NOx emission inventories for the Paris region from MEGAPOLI aircraft measurements Authors: H. Petetin, et al.

Petetin et al. present a novel approach to evaluating BC and NOx emissions from a whole large city (Paris) based on airborne measurements of the large-scale downwind plume. The BC and NOx concentrations observed during the level flights (about 600 m a.g.l.) across the pollution plumes and an atmospheric chemistry-transport model

driven by BC and NOx emission inventories are used in this approach. To minimize several errors in the model, the integrated values of the excess BC and NOx above the background concentrations and the BC/NOx ratios are compared between the observation and the model simulations. From the comparison the BC and NOx emission inventories are evaluated. BC and NOx concentrations observed at ground site in Paris (LHVP) are also examined, and it is confirmed that the ground observation, predominantly influenced by the local emissions, is not appropriate to detect the emissions from the whole city. Petetin et al. carefully examine the sources of the uncertainties including meteorological data, vertical mixing, and analytical uncertainties, and finally find the significant biases in the BC and NOx emission inventories used in the model simulations. Although there are still relatively large uncertainties in the estimations, the proposed approach is considerably useful to constrain the whole emissions from the large city. I found that the paper is well written, the approach is excellent, and containing material that should be published. I strongly recommend this paper for publication in Atmospheric Physics and Chemistry with minor revisions described below.

General comments:

If the BC and NOx sources are collocated, BC/NOx ratio would better constrain the emission ratio because the errors associated with the atmospheric transport are minimized. However, there are some difficulties in simulating the atmospheric NOx concentrations due to the dry/wet deposition and the chemical processes. I think CO is more appropriate to constrain the BC emissions because CO is also burning process-related species, is more conservative for the relevant time scale, and is more accurately measured than NOx. Actually, the measurements of CO were conducted at LHVP (Lopez, et al., 2013, ACP, 13, 7343-7358) and during the MEGAPOLI airborne measurements (Freney et al., 2014, ACP, 14, 1397-1412). There is no need to add the CO data and the discussion in the revised manuscript, but if the authors agree with this comment, I think it would be better to mention briefly the possibility to use another species to constrain the BC emissions. If there are associated studies on the BC emissions using

ACPD
14, C10277–C10280, 2014

CO and so on, it would also be better to add the information.

As the authors pointed out, how well the model reproduces the vertical profiles is one of the important error source for the emission estimation. Although only lateral observations are examined in this study, it is mentioned that the vertical profiles up to 3 km a.g.l. were measured at the end of the several flights (P. 29257, L. 24-25). If so, I think it would be better to compare the observed vertical profiles with the simulated vertical profiles because such comparisons could allow us to more directly validate the model performance and to evaluate the model uncertainty.

Specific comments

1) P. 29245, L. 3: Is “horizontal variability of the boundary layer height over the aircraft trajectory” discussed in this paper?

2) P. 29245-29246, Section 3.1: Several analyzers were used to detect EC, NOx, and so on. If those analyzers are commercially available, it would be better if you clarify the model of the instrument and the manufacturer.

3) P. 29249, L. 10: “residential/tertiary” is the description of SNAP sector 2 here, but “small combustion plants” is in Table S1.

4) P. 29250, L. 25: “Dudhia, 1993”, not “Dudhia et al., 1993”.

5) P. 29254, L. 15-16 and Fig. 6 right panel: Please clarify how to compute the diurnal profiles of BC, NOx and BC/NOx ratio. Are they the averages for the flight dates or for the all July dates?

6) P. 29257, L. 24-25: If the vertical measurements were conducted in the pollution plumes, I think it would be better to show the vertical profiles. Do those vertical profiles convince us of the well-mixed condition in the boundary layer?

7) P. 29258, L. 3: The value of 30 percentile is used for the background determination in this study. Does the value of the percentile affect the plume integration?

ACPD

14, C10277–C10280, 2014

8) P. 29262, L. 8-10: These diurnal variations mentioned here with the lowest value in early morning can be also seen in Fig. 6. I think Fig. S7 in the Supplement is not needed. The lowest value of BC/NOx diurnal variation seems to lower than 0.05 micro-g m-3 ppb-1, close to 0.03 micro-g m-3 ppb-1.

9) P. 29262, L. 22-24: The BC/NOx emission error factors for TNO inventories don't seem to be underestimated (see Fig. 12 and Table 6).

10) P. 29279, L. 24-25: "Schmidt, H., Derognat, C., Vautard, R., and Beekmann, M.", not "Schmidt, H. and Derognat, C."

11) P. 29295, Fig. 6, caption: "BC, NOx and BC/NOx ratio concentration" should be "BC and NOx concentrations and BC/NOx ratio".

12) P. 29300, caption: "on the top right" should be changed to "on the right".

13) Fig. 11, Fig. 12, and Fig. 15: It would be better to add the labels of the x-axis, "July date (UTC)".

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29237, 2014.