The paper describes comparisons of lidar-derived, in situ and modelled particle mass concentrations ($PM_{10}$) during the MEGAPOLI campaign in the Paris area in summer 2009. A 355-nm backscatter lidar in a mobile van was used to investigate the aerosol distribution in the mesoscale around the major pollution source of the city of Paris. Cross sections along and across the pollution plume could be taken in this way and compared to the results of two chemistry transport models. The approach is interesting. However, the paper is not very carefully written. Important issues have been ignored in the error discussion. The wording is often not very clear, and there are many language errors in the paper.

MAJOR COMMENTS

1) My major concern is related to the approach of converting backscatter measurements to extinction profiles and the extinction profiles to mass concentrations. In their extinction retrieval the authors use lidar ratios which vary strongly from day to day, but which are obviously kept constant during one van ride. On the other hand, they switch between urban, peri-urban and rural models for the extinction-to-mass conversion during a ride. Optical-to-mass conversion factors depend on the particle size distribution and the refractive index. The same is true for the lidar ratio. Thus it is an antagonism to assume that the lidar ratio is constant when it appears to be necessary to change the extinction-to-mass ratio. Furthermore, the approach to determine the lidar ratio by using the sun-photometer optical depth as a constraint is completely unclear. How is this retrieval done? The sun photometer is at a fixed location whereas the lidar is moving through an area with highly varying particle mass concentration and thus optical depth. Although the authors provide an error discussion, they completely ignore the error that is introduced by the lidar-ratio estimate. The lidar ratios vary by at least 30%-50% from day to day. How can it be excluded that they don't vary by the same amount from place to place (when going into and out of the pollution plume)? The resulting uncertainty directly converts into the extinction uncertainty and thus into the mass estimate. This error is probably the largest one, but not discussed at all. It puts the whole comparison into question.

2) In order to compare measurements and model results the authors define two measures, the root mean square error and the mean absolute percentage error and present all their observations in terms of these numbers. Later on, in the discussion part (see Sec. 5.4.) they start discussing biases and possible reasons for an under- or over-estimation of $PM_{10}$ by either the models or the measurements (also in the abstract it is said that the models underestimate wet $PM_{10}$ concentrations). Why is the bias not defined and considered in the basic investigations?

3) The discussion on wet and dry aerosol mass is a bit confusing throughout the paper. It is not completely clear at which points the authors consider wet conditions and where humidity growth is neglected (the question arises, e.g., for the optical-to-mass conversion factors).

4) The figures are of bad quality. Numbers and text are hard to read and symbols cannot be distinguished (e.g. Fig. 3, 5, 6, 7).

SPECIFIC COMMENTS

1) Abstract: It is not clear what is meant with urban and peri-urban relationships.

2) p 11865, l 15: …in a later paper… Why later? Is there no reference? Isn't there an overview paper of the campaign for this special issue?

3) p 11866, l 8/Tab. 1: Table 1 more or less duplicates information given in the text and could be omitted.

4) p 11869, l 5-8/Tab. 2: It is unclear how the lidar ratios are observed.

5) p 11873, discussion of the lidar ratios: Lidar ratios of 90 sr appear rather high. Such values have been observed in fresh smoke plumes (small, absorbing particles). How can you prove that this value can be taken as typical for a larger area/whole day? Can you exclude that observations are dominated by a local source?

6) p 11874: It is unclear, how the authors deal with wet and dry particle mass. Does $C_0$ hold for wet or dry aerosol?

7) p 11876, l 19-21: "Figures 3 and 4 show…" This is not correct, respective results are presented in Figures 3 and 5.

8) p 11877, Eq. 3: It should be multiplied with 100% to make it a percentage error. Why not defining a bias here as well?

9) p 11878, l 23: "Figures 3b and 6b show the results…" It is unclear which figures are really meant here.

10) p 11882, l 21-22: Units are missing.

11) p 11884, l 14-17: "The PM10 concentrations over Paris were not systematically under-estimated in studies made before 2005, because before 2007 the AIRPARIF measurement network did not measure a large fraction of semi-volatile PM, stressing the importance of an accurate representation of secondary aerosols." This statement is misleading. Obviously, both models and measurements underestimated the mass concentrations.

12) p 11886, l 7: Explain AMS.

13) p 11886, l 9: "…secondary inorganic aerosol (inorganic and organic)…" Is inorganic or inorganic and organic?

Table 4: There are several acronyms/abbreviations not explained.

Tables 6 and 7: Units are missing for mean wet $PM_{10}$.

Figure 4: "…variability observed over 20 profiles…" What does it mean?

GRAMMAR, TYPOS (just examples, not complete)

p 11864, l 20: …about12…
p 11867, l 8: …to be exceeded has on more than 35 days…
p 11868, l 28: …a horizontal with a mean velocity…
p 11869, l 15: …Hybreid…

p 11873, l 3: …Raut et Chazette…
p 11876, l 14: …are then been computed…
p 11878, l 13: …angstrom…
p 11879, l 6: …urban relationships is considered…
p 11879, l 18: ...circular lidar-van circuit...
p 11883, l 16: ...,and…
Tab. 4: …Mechnistic…