Response to reviews on "Lidar-derived PM10 and comparison with regional modeling in the frame of the MEGAPOLI Paris summer campaign", P. Royer et al.

The authors appreciated the comments of reviewers that help to improve our manuscript. Please find a point-by-point discussion and answer of the issues raised by the reviewers. For sake of clarity the remarks of reviewers are represented in bold and the citation of the manuscript in italic.

Anonymous Referee #1

I strongly recommend a revision with the help of an expert of the English language. It would have helped a lot if this would have been done before the paper was submitted.
The manuscript has been completely reviewed. Grammatical and language corrections have been made.

My main concern is about the use of the so called "optical-to-mass relationships". I think they are not applicable in every situation, in particular a better distinction between wet and dry aerosols has to be made.
page 11863, line 19: "mean wet PM10": is it really wet PM10? Your extinction-to-mass relationships are measured for dry aerosols (page 11865, line 1). Later you claim that you may use them because the relative humidity was low. Now you talk about wet PM10? What is correct?
page 11865, line 1: Do the optical-to-mass relationships also hold for wet aerosols? I would doubt this.
page 11865, line 11: So TEOM measures also dry PM10 but the lidar measures under ambient conditions (which you would probably call "wet"), but uses an extinction-to-mass ratio that was taken for dry aerosols. How can this be put together?
page 11874, line 4/5: But these optical-to-mass relationships (which is unspecific and not a nice word) were derived under dry conditions, haven't they? You use them for aerosols under wet conditions.
The presentation of the method used to derive wet PM_{10} concentrations in Section 4.2 has been clarified. Optical-to-mass relationships are only available in conditions when aerosol can be expected to be dry (RH<55%). For RH>55% hygroscopic effects have to be considered by taking into account an aerosol mass, size and scattering growth factor. For the comparisons with AIRPARIF at 200 m RH is always below 55% so that the extinction-to-mass-ratio can be calculated for dry aerosol. For integrated PM_{10} the comparison with models are realized with wet PM_{10}. The uncertainty due to RH effect has been shown to account for ~30% of the total uncertainty of wet integrated PM_{10}. All these points have been highlighted in the new version of the paper.

Added in the text :

"The method to retrieve PM_{10} concentrations from lidar measurements has been first applied to aerosol observed in an underground railway station of Paris (Raut et al., 2009a, 2009b). The theoretical relationship between PM_{10} and aerosol extinction coefficient ($\alpha_{ext,355}$) is given as a function of the density of particles ρ, the mean cubic radius $\overline{r^3}$ and the mean extinction cross-section $\overline{\sigma_{ext,355}}$ by (Raut and Chazette, 2009):

$$PM_{10} = \rho \cdot \frac{4}{3}\pi \cdot \frac{\overline{r^3}}{\overline{\sigma_{ext,355}}} \cdot \alpha_{ext,355} \qquad (1)$$

If we only consider a monomodal lognormal accumulation mode which is sensitive to humidity effect, the cubic modal radius can be written as a function of the modal radius radius r_m and geometrical dispersion of the monomodal distribution σ:

$$\overline{r^3} = {r_m}^3 \cdot \exp\left(\frac{9}{2}\ln^2(\sigma)\right) \qquad (2)$$

Assuming that the geometrical dispersion is not affected by humidity we can write Eq. 1 under the following form :

$$PM_{10,wet} = PM_{10,dry} \cdot \underbrace{\frac{\rho_{wet}}{\rho_{dry}}}_{f_u(RH)} \cdot \underbrace{\left(\frac{r_{m,wet}}{r_{m,dry}}\right)^3}_{f_\varepsilon(RH)} \cdot \underbrace{\frac{\sigma_{ext,355,dry}}{\sigma_{ext,355,wet}}}_{1/f_\gamma(RH)} \cdot \frac{\alpha_{ext,355,wet}}{\alpha_{ext,355,dry}} \qquad (3)$$

f_u is the aerosol mass growth factor given by (Hänel, 1976):

$$f_u(RH) = \frac{1+u\cdot\frac{RH}{1-RH}}{1+\frac{\rho}{\rho_{h_2o}}\cdot u\cdot\frac{RH}{1-RH}} \qquad (4)$$

with u the aerosol mass increase coefficient, ρ and ρ_{h2o} the density of dry particle (1.7 g.cm^{-3}) and water vapor (1 g.cm^{-3}). The mean day-to-day values of u computed with ISOROPIA in the PBL (and the variability along the track) are reported in Table 2. Note that for cases of 1 (u=0.23) and 2 July 2009 (u=0.21) with air masses coming from Northeast and East, Randriamiarisoa et al. (2006) found a similar values of 0.23.

f_ε is the aerosol size growth factor (Hänel, 1976):

$$f_\varepsilon(RH) = (1-RH)^{-\varepsilon} \qquad (5)$$

with ε the Hänel size growth coefficient. ε and u are linked by the following relationship:

$$(1-RH)^{-\varepsilon} = \left(1+\frac{\rho}{\rho_{h_2o}}\cdot u\cdot\frac{RH}{1-RH}\right)^{1/3} \qquad (6)$$

f_γ is the aerosol scattering growth factor (Hänel, 1976):

$$f_\gamma(RH) = (1-RH)^{-\gamma} \qquad (7)$$

with γ the Hänel scattering growth coefficient. Randriamiarisoa et al. (2006) reported values of γ between 1.04 and 1.35 in a suburban area south of Paris. In this study we used a mean value of 1.2±0.15.

A empirical optical-to-mass relationship between $PM_{10,dry}$ concentrations in PBL and dry extinction coefficient $\alpha_{ext,355,dry}$ has been established from nephelometer and TEOM in-situ measurements (Raut and Chazette, 2009):

$$PM_{10,dry} = \underbrace{C_0 \cdot \omega_{0,355} \cdot \left(\frac{700}{355}\right)^{-a}}_{1/s_{ext,355}} \alpha_{ext,355,dry} \qquad (8)$$

where $s_{ext,355}$ is the specific extinction cross-section at 355 nm, $\omega_{0,355}$ is the single-scattering albedo at 355 nm and a the Angström exponent between 450 and 700 nm which is assumed to be the same as the Angström exponent between 355 and 700 nm. C_0 is the slope of regression analysis between the nephelometer scattering coefficients at 700 nm and the TEOM PM_{10} measurements performed simultaneously during several campaigns in Paris and its suburbs.
By combining Eq (3) and (8) we can derived wet PM_{10} concentration with $\alpha_{ext,355,wet}$ measured from lidar :

$$PM_{10,wet} = C_0 \cdot \omega_{0,355} \cdot \left(\frac{700}{355}\right)^{-a} \cdot \frac{1+u\cdot\frac{RH}{1-RH}}{1+\frac{\rho}{\rho_{h_2o}}\cdot u\cdot\frac{RH}{1-RH}} \cdot (1-RH)^{-3\varepsilon} \cdot (1-RH)^{\gamma} \cdot \alpha_{ext,355,wet} \qquad (9)$$

Specific major comments
Title: I do not think it is necessary to mention the MEGAPOLI project in the title, it is not essential for this paper. Instead the authors should include the ground based observations which represent a large part of the paper. One possible title could be Comparison of lidar derived PM10 concentrations in Paris with ground based observations and chemistry transport model results
The new title will be: "*Comparison of lidar derived PM10 concentrations with regional modeling and ground-based observations in the frame of the MEGAPOLI experiment*". Ground-based observations have been added in the title. The MEGAPOLI project is still mentioned in the title as all measurements have been performed in the frame of this campaign. The mobile lidar was one of the major innovative instruments of the campaign MEGAPOLI-Paris.

Abstract
page 11863, line 2: My impression is that the lidar observations are the central part of the paper, not the comparison and "validation" (a term that is not appropriate here) of the model. This should be expressed here and in the title. The models cannot be validated with these observations, you can test their performance under these specific conditions and might identify some of their shortcomings.
The authors agree that the models cannot be strictly "validated" with these observations. However, the authors feel that the part concerning comparisons with chemistry-transport models (CTMs) is an important part of the paper. Note that the question is also raising for the others observations. In this important exercise of comparison, the uncertainties should be considered. It was not done satisfactorily in the previous version. The first sentence of the abstract is now : "*An innovative approach using mobile lidar measurements was implemented to test the performances of chemistry-transport models in simulating mass concentrations (PM10)* ".

page 11863, line 17: The term "urban relationships" (and later also "peri-urban relationships") is very sloppy. You should find a better, more accurate description of what you mean by this. Additionally the relationships are introduced much later.
The terms "urban and peri-urban relationships" have been clarified in the abstract ("relationships assuming peri-urban and urban-type particles"). These terms are clearly explained in Sect. 4.2: *"Urban relationship has been determined from in-situ measurements in the center of Paris during ESQUIF (Chazette et al., 2005) and LISAIR (Raut and Chazette, 2007) campaigns, respectively in 1999 and 2005. Peri-urban situations have been identified during ParisFog in 2007 (Elias et al. 2009) and ESQUIF campaign data. They correspond to measurements directly influenced by urban sources, but taken outside urban centers. Rural conditions influenced by pollution in the Paris area have been encountered during the MEAUVE campaign in 2001 (Lavigne et al., 2005)… For the comparisons with AIRPARIF and CTMs simulations, the urban parametrization will be used for lidar observations inside the pollution plume in the inner suburbs of Paris, peri-urban relationship for measurements outside the pollution plume in the inner suburbs and measurements inside the plume far from Paris. Rural relationship will be applied for observations far from Paris center outside the pollution plume."*

page 11863, line 18-23: It is not clear which altitudes or range of altitudes is covered by these statistical values.
The altitude of the comparison (~200 m) between lidar and CTMs has been precised in the abstract.

page 11864, line 3: What is the problem with the vertical diffusion? Before you say that the comparison of the vertically integrated values is not worse than the one mentioned before. This would tell you that the vertical distribution of the aerosols is ok.
Different parameterizations of vertical diffusion (or PBL schemes) exist and the uncertainties related to these parameterizations is high (see Roustan et al. 2010 and the discussion in paragraph 5.5). The word « turbulent » between vertical and diffusion is now added P11864 line 3 for clarity. Errors in turbulent diffusion could both affect PM values at 200 m height and vertically integrated ones.

Abstract in general: You should explain what is the advantage of taking the lidar observations for the comparisons. Why can't we stay with the standard methods and use the ground based values? In particular, in those situations you evaluate here, the advantage of the lidar is hard to see. The PBL was always well mixed and we can assume that the PM10 values at ground represent upper altitudes quite well. So why do we need these kind of observations?
To better justify the interest of the study, the following points have been added to the abstract : "*The major advantage of using vertically resolved lidar observations in addition to surface concentrations is to overcome the problem of limited spatial representativity of surface measurements. Even for the case of a well-mixed boundary layer, vertical mixing is not complete, especially in the surface layer and near source regions. Also a bad estimation of the mixing layer height would introduce errors in simulated surface concentrations, which can be detected using lidar measurements. In addition, horizontal spatial representativity is larger for altitude integrated measurements than for surface measurements, because horizontal inhomogeneities occurring near surface sources are dampened.*"

Introduction:

page 11865, section 2.1.1: This section cannot be understood if you are not a specialist in lidar. You give the impression that the "acquisition" (whatever that is) is based on a laser (line 8-10). What are "residual heights" (line 20)? Can extinction profiles with 20 s averaging and 15 m vertical averaging already be used? What is the typical resolution of a final profile that is used in this study?

This part has been clarified. The term "residual heights" has been removed. The final vertical resolution of the data is 15 m after filtering for a temporal sampling of 20 seconds.

page 11868, line 14: Is the depolarization ratio used somewhere in your analysis?

Depolarization ratio is not used in our analysis.

page 11869, line 25: What are "good weather conditions"?

"Good weather conditions" has been replaced by "partially cloudy or cloud free conditions".

page 11870, line 20: You go down to very high horizontal resolutions of 2-3 km but you stay with only 9 vertical levels up to 12 km. This seems to be not appropriate and not state of the art. Why don't you take more levels? Could you comment on the difficulties of the low number of levels. The same holds for Chimere which has only 8 levels up to 5.5 km.

Although the vertical layers extend to 12 km, most of the vertical levels are inside the boundary layer, as the concentrations above the boundary layer have a limited impact on the concentrations within the boundary layers. The heights of the levels are now added in the manuscript. The impact of the number of vertical levels is limited in the CTMs used here as the meteorology is computed offline with a much larger number of levels (25). The number of vertical levels used in the meteorological model is now added. The impact of the low number of levels in the CTM is non-negligeable, but it is lower than the uncertainty linked to the modeling of the vertical diffusion (see the paper of Roustan et al. 2010 where the uncertainties linked to the number of vertical levels and the modeling of vertical diffusion are compared). Added in the text: « *In all simulations with POLYPHEMUS, 9 vertical levels are considered from the ground to 12 kilometers with the following heights:40m, 120m, 300m, 800m, 1500m, 2400m, 3500m, 6000m and 12000m*. », Vertical levels heights in CHIMERE simulations are similar (40 m, 120 m, 240 m, 460 m 850 m, 1500 m 2800 m 5500 m. In both models, density of vertical levels is much enhanced in the first km of the atmosphere." and « *The MM5 model (Dudhia, 1993), version 3.6, with a horizontal resolution of 36 km and 25 levels from the ground to 100hPa height.* »

page 11872, line 45: If you run MM5 on 15 km. how do you get the fields for the 3 km run of Chimere(same page, line 5)? Isn't that a problem for the final resolution of the results?

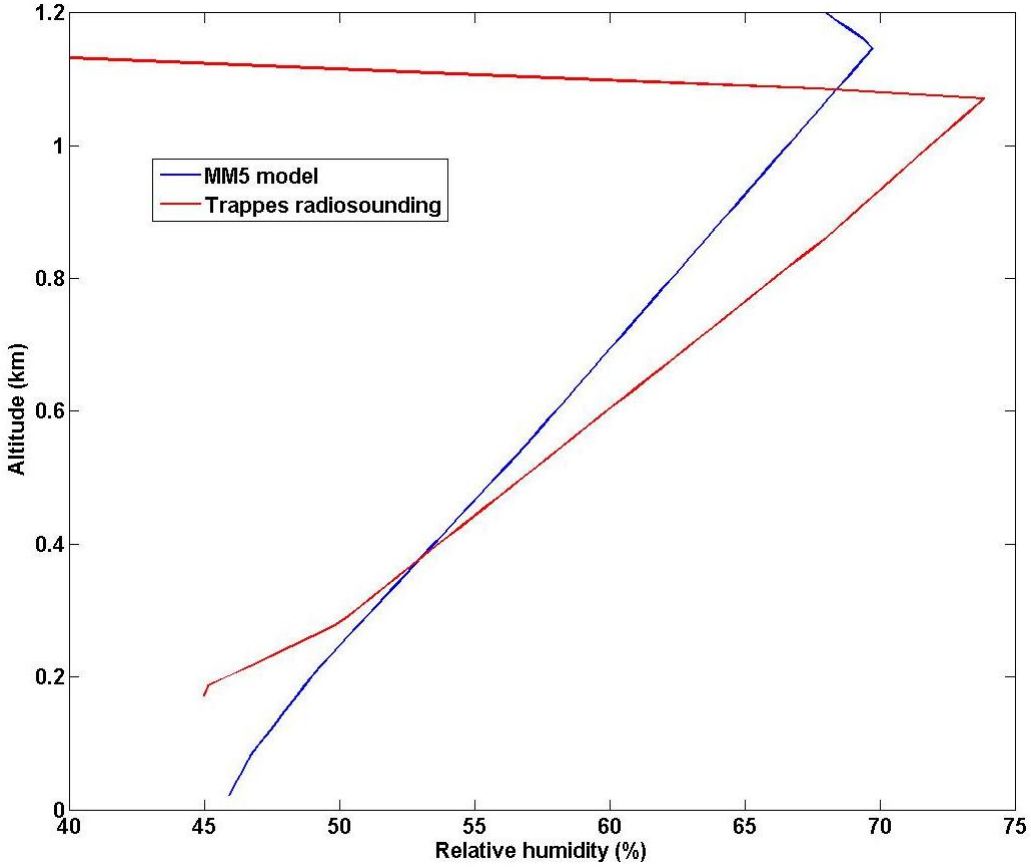
The meteorological fields are interpolated from the results of MM5 to the CTM grid. Although the coarse meteorological resolution increases the uncertainties of the simulation, it simulates well the dispersion of the plume. This procedure is possible for the Ile de France

region around Paris, because the region is flat, and wind fields are mostly determined by the synoptic weather conditions.

page 11872/73, section 4.1: This is one of the weaknesses of the single wavelength lidar approach. You need to make a number of assumptions about your lidar ratio (e.g. independence on height, no AOD values from sun-photometers at night and under cloudy conditions, climatological values for the LR in a number of cases). You should say more clearly that your extinction values were not directly measured but derived from backscatter values. You should also say something about the errors you introduce, in particular in cases when the lidar ratio has to be guessed.
page 11875, line 11: What about the uncertainties on 15, 21 and 29 July when no AOD values were available and the lidar ratio is a pure guess?
The authors agree that Raman lidar measurements would have helped to constraint the inversion in many cases when no AOD values from sun-photometers measurements were available. This concern 3 among the 10 cases studied in the manuscript. For these cases the LR has been taken to 34.4 sr which correspond to the values determined for 28 July 2009 with the same wind direction. The mobile measurements have generally been realized between 13:00 and 16:00, when the PBL is well-mixed, so that the assumption of a constant LR throughout the column is certainly right.
It is clearly said in part 4.1 that extinction values are retrieved with a Klett algorithm using the mean integrated *LR* from lidar/sun-photometer coupling.
"*The range-corrected backscatter signals from the 10 MD involving the mobile lidar have been inverted into extinction coefficient profiles using a Klett algorithm (Klett, 1985) with the mean integrated LR values determined as described above (see values in Table 2). On 15, 21 and 29 July 2009, when cloudy conditions prevented from retrieving LR values using the sunphotometers, a LR of 34.4 sr has been used corresponding to the value of 28 July 2009 obtained with Southwest wind direction. The sources of uncertainty linked to the conversion of lidar measurements in extinction coefficient profiles are discussed in Sect. 4.3.*"

page 11876, line 20: Figure 4 does not show spatial distributions.
There was a mistake. Figure 5 was meant instead of Figure 4. This has been corrected.

page 11878, line 5: Can you really use the Trappes sounding to explain the increase in aerosol extinction at the top of the PBL? And if this is a hygroscopic effect, your optical-to-mass relationships may not be valid and produce artefacts.
The increase of aerosol extinction coefficient at the top of the PBL is clearly related to the increase of RH up to 70% at the top of the PBL as shown by Trappes sounding and MM5 model. This can explain the increase of wet PM10 at the top of the PBL. The hygroscopic effect on optical-to-mass relationships is now taken into account (see answer above). Note that the RH profile derived from MM5 is in good agreement with the Trappes radio sounding (see Figure hereafter).



page 11881, section 5.1: You should find a better way to describe the use of different "relationships" than expressions like "GBML/peri-urban" or "peri-urban parameterization". All these are unprecise expressions.
These expressions have been removed in the text.

page 11881, line 17: Does Chimere include dust aerosols?
Dust particles including suspended dust, soil dust and advected mineral dust from arid regions (through boundary condition) have been taken into account in the model. This is already stated in page 11871, line 18 "*The aerosol module includes primary organic (POA) and black carbon (BC), other unspecified primary anthropogenic particulate matter (PM) emissions, wind-blown dust, sea salt, secondary inorganics (sulfate, nitrate and ammonium) as well as secondary organic aerosols (SOA) from anthropogenic and biogenic origin, and particulate water.* ».

page 11882, section 5.3: What is the purpose of this integrated PM10 values if you consider only well mixed cases and only altitudes up to 1 km? You cannot expect new insights. It would make more sense to look at the completet PBL and see e.g. if the model does not represent the vertical mixing and therefore the dilution of pollutants. This could be a reason why PM10 concentrations are wrong.
We have not only considered well mixed cases. All cloud free or partially cloudy cases of the MEGAPOLI campaign have been considered here. We have deliberately restricted the altitude range for comparison to 1 km height in order to avoid an increase of RH and cloud formation at the top of the PBL. This is clearly mentioned in the text: "*The top of the PBL has been deliberately excluded from the analysis to avoid an increase of RH and the presence of clouds in this part of the atmosphere*". Integrated PM10 values up to 1km gives us a good

information on average values of PM in a large part of the planetary boundary layer (which extends from 1.2 to 1.8 km height).

page 11882, line 20: Do the values of the integrated PM make sense? I would expect something a factor of 1000 higher.
There was a mistake with the unit ($\mu g.m^{-2}$ has been replaced by $mg.m^{-2}$).

page 11886, line 9: Is it secondary organic or inorganic aerosol? This sentence is confusing.
This sentence has been corrected. In addition, it has been corrected that AMS measurements refer to PM1 aerosol and NOT to PM10 aerosol. :
"*AMS and soot measurements during the MEGAPOLI summer campaign at the Golf site/Livry Gargan at the north-eastern edge of the agglomeration made evident that secondary aerosol (inorganic and organic) made up on the average about two thirds of PM1 aerosol*."

page 11887, line 15-17: This sentence needs to be reformulated.
This sentence has been replaced by "*Lidar measurements give information on the vertical repartition of aerosols concentration in the atmospheric contrary to in-situ ground-based measurements*".

page 11887, line 18: Could you really use a Raman lidar for such an approach on a van? Integration times would be much longer and 16 mJ at 20 Hz not enough to detect the Raman signal with sufficiently low statistical error.
This sentence was confusing. Raman lidar measurements are a better alternative than sun-photometer measurements as they enable to retrieve a LR profile during both nighttime and daytime and in presence of high clouds. This sentence has been changed into: "*The use of a N_2-Raman lidar, measuring extinction-to-backscatter profiles during both daytime and nighttime and in presence of high clouds, could significantly improve the retrieval of PM_{10} from a ground-based lidar.*"

Table 2: Are the "Levels of pollution" from the lidar or from ground based observations?
Levels of pollution indicated in Table 2 are from ground-based observations. This information has been added in this Table.

Table 4: What is "Climatology"? What is the resolution of MM5 for Polyphemus? What is "LA"?
« *Climatology* » is now replaced by « *Boundary conditions* », the resolution of MM5 or Polyphemus is added, as well as the acronym of LA (Laboratoire d'Aérologie). A reference for the OC/BS emissions used in CHIMERE and provided by Laboratoire d'Aérologie will be added :

"Junker C. and Liousse C., "A global emission inventory of carbonaceous aerosol from historic records of fossil fuel and biofuel consumption for the period 1860–1997", Atmos. Chem. and Phys., 8, 1207, 2008."

Table 5: Why is there no C0 for dust?
The specific extinction cross-sections ($s_{ext,355}$) for dust aerosols has not been statistically established over Paris due to very few observations of dust events reaching the surface. It has

been determined from density of particles ρ, mean cubic radius $\overline{r^3}$ and mean extinction cross-section at 355 nm $\overline{\sigma_{ext,355}}$ (Raut and Chazette, 2009):

$$PM_{10} = \underbrace{\rho \cdot \frac{4}{3}\pi \cdot \frac{\overline{r^3}}{\overline{\sigma_{ext,355}}}}_{1/s_{ext,355}} \cdot \alpha_{ext,355}$$

The following sentence has been added in the manuscript: "*Concerning dust aerosols it has not been possible to determine statistical relationship due to the lake of dust events reaching the surface at Paris. A dust specific cross-section has been determined using a theoretical relationship given in Eq 1 (Raut and Chazette, 2009) assuming a mean density (2 g.cm^{-3}), a mean cubic radius (7.03 10^{-3} µm^3) and a mean extinction cross-section (6.72 10^{-10} cm²).*"

Figure 4: Why are the lidar measurements extended down to ground if you do not include observations at ground?
Lidar measurements below the altitude of full overlap have been removed in this figure.

Figure 1: The background colors of the map make it difficult to see the symbols.
The size of the marker has been increased in this figure.

Figure 6: Legends and captions are too small to be readable.
The fontsize of legends and captions has been increased.

Minor comments / expressions that are difficult to understand
page 11863, Line 2: What is an original approach?
"original approach" has been replaced by "innovative approach".

page 11863, Line 11: "contrasted levels": better "different levels"
page 11885, line 21: boundary layer height
These corrections have been made in the manuscript

Anonymous Referee #2

However, the paper is not very carefully written. Important issues have been ignored in the error discussion.
The error discussion has been rewritten to include the uncertainty due to evolution of the lidar ratio along the track and relative humidity effects.

The wording is often not very clear, and there are many language errors in the paper.
Grammatical and language corrections have been made.

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.
The author agree that in the lidar ratio (LR) is not constant along the track. Mobile measurements with a Raman lidar could remove this uncertainty but it would be difficult for daytime measurements due to the low Raman backscattering cross-section. With our measurements performed here with a Rayleigh-Mie lidar we have assessed this source of uncertainty which is now taken into account by considering the day-to-day variability of the lidar ratio values retrieved with the coupling between lidar and sun-photometer, throughout the month of July 2009.

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.
This part has been clarified: *"Fixed ALS450 Rayleigh-Mie lidar profiles from SIRTA ("Site Instrumental de Recherche par Télédétection Atmosphérique") have been averaged over 5 minutes around each sun-photometer measurements. The height-independent LR values (Table 2) are determined using a Klett algorithm (Klett, 1985) to invert the mean lidar profile and a dichotomous approach on LR values converging until the difference between lidar and AERONET sun-photometer AOD at 355 nm is below 0.02 (Chazette, 2003)."*

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.

We have checked the LR values and we have noticed on lidar profiles that some clouds were remaining on AERONET level 2 data. These LR values have been removed in this new version of the paper, which induces slight changes in the values. LR values are 52.1±6.4 sr, 63.3±14.1 sr, 80.1±12.6 sr, 29.7±1.2 sr, 40.7±2.4 sr, 42.8±8 sr and 34.4±2.1sr for 1, 2, 4, 16, 20, 26 and 28 July 2009, respectively. This gives a interdiurnal variation of 12.3%, 22.3%, 15.7%, 4%, 5.9%, 18.7% ad 6.1% which lead to an uncertainty of 7%, 12.5%, 9%, 3%, 3%, 10.5%, and 3%, respectively. This uncertainty is now considered in the error budget and accounts for 16% of the total error in retrieved PM_{10} at 200 m.

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?

Biases have not been defined because it requires to consider a reference dataset (modeling or observation) for the comparison. We preferred to give the mean values for each measurement day in Table 6 and 7, average differences between observations and simulations can easily deduced from theses mean values.

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).

This part has been clarified in the text. For lidar measurements, humidity growth has been neglected for RH<55%. For the comparison at 200 m, RH is below 55% so that lidar PM_{10} can be directly compared with AIRPARIF dry PM_{10} without correction of the humidity effect. For the comparison of wet integrated PM_{10} the mass and size growth factors have been computed using a mass increase coefficient u, a size growth coefficient ε, a scattering growth coefficient γ and RH values from MM5 model.

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).

Fontsize and markersize have been increased in these figures.

SPECIFIC COMMENTS

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

The terms "urban and peri-urban relationships" have been clarified in the abstract ("*relationships assuming peri-urban and urban-type particles*").

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?

The overview paper of the campaign is not yet available. Nevertheless, this paper will be included in a special edition dedicated to Megapoli.

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

4) p 11869, l 5-8/Tab. 2: It is unclear how the lidar ratios are observed.
LR are determined with lidar/sun-photometer coupling. This has been clarified in the manuscript : "*LR (± its day-to-day variability) are retrieved from coupling between fixed lidar and sun-photometer measurements (see Sect.4.1)*."

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?
For cases of 15, 21 and 29 July when no AOD values are available for the determination of LR we have taken the LR value of 28 July 2009 (34.4 sr) instead of 90 sr. For the case of 28 July the wind direction is Southwest as in cases of 15, 21 and 29 July 2009 and we can thus supposed that we observed the same aerosol type.

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?
This part has been clarified. Optical-to-mass relationships have been determined for dry aerosols. A correction of mass and size increase is necessary for RH >60%. The correction has been now presented within the paper.

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

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?
This has been added in this equation.

9) p 11878, l 23: "Figures 3b and 6b show the results…" It is unclear which figures are really meant here.
Some explanation has been added: '*Figure 3b and 6b shows the spatial and temporal evolution of PM_{10} at 210 m along the track.*'

10) p 11882, l 21-22: Units are missing.
Units ($mg.m^{-2}$) have been added.

11) p 11884, l 14-17: "The PM10 concentrations over Paris were not systematically underestimated 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.
Replace by
« *Before 2007, the AIRPARIF measurement network did not measure a large fraction of semi-volatile PM, underestimating PM10 concentrations. This underestimation may explain why modeled PM10 concentrations over Paris were not systematically under-estimated compared to measurements in studies made for years before 2005 (e.g. Roustan and Seigneur 2010), stressing the importance of an accurate representation of secondary aerosols in both models and measurements.* »

12) p 11886, l 7: Explain AMS.
The definition of the acronym ("*Aerosol Mass Spectrometer*") has been added.

13) p 11886, l 9: "…secondary inorganic aerosol (inorganic and organic)…" Is inorganic or inorganic and organic?
It is both inorganic and organic secondary aerosol. This has been clarified in the text (see answer above).

Table 4: There are several acronyms/abbreviations not explained.
Acronyms and abbreviations have been defined.

Tables 6 and 7: Units are missing for mean wet PM_{10}.
Units have been added in both tables.

Figure 4: "…variability observed over 20 profiles…" What does it mean?
explained
The following precision has been added in the manuscript: *"Data have been averaged over 20 lidar profiles: the mean profile is represented by the solid line and the variability by the shaded area."*

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…
All these grammar and typo errors have been corrected.