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Interactive comment on “Lidar-derived PM₁₀ and comparison with regional modeling in the frame of the MEGAPOLI Paris summer campaign” by P. Royer et al.

Anonymous Referee #1

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Review of the paper “Lidar-derived PM₁₀ and comparison with regional modeling in the frame of the MEGAPOLI Paris summer campaign” by P. Royer et al.

The paper presents aerosol lidar observations taken from a driving van in the Paris area and compares the results to ground based observations and chemistry transport model results.

The paper is interesting because it combines different types of observations and results from two regional atmospheric models in a highly polluted area. A number of improvements is needed before the article can be published. It is difficult to read, sometimes it is not entirely clear what the authors want to say. Therefore I strongly recommend a

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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.

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.

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

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.

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.

page 11863, line 18-23: It is not clear which altitudes or range of altitudes is covered by these statistical values.

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

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you may use them because the relative humidity was low. Now you talk about wet PM10? What is correct?

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.

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?

Introduction:

page 11865, line 1: Do the optical-to-mass relationships also hold for wet aerosols? I would doubt this.

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?

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

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

page 11870, line 20: You go down to very high horizontal resolutions of 2-3 km but you

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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.

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?

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 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.

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?

page 11876, line 20: Figure 4 does not show spatial distributions.

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.

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.

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page 11881, line 17: Does Chimere include dust aerosols?

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.

page 11882, line 20: Do the values of the integrated PM make sense? I would expect something a factor of 1000 higher.

page 11886, line 9: Is it secondary organic or inorganic aerosol? This sentence is confusing.

page 11887, line 15-17: This sentence needs to be reformulated.

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.

Table 2: Are the “Levels of pollution” from the lidar or from ground based observations?

Table 4: What is “Climatology”? What is the resolution of MM5 for Polyphemus? What is “LA”?

Table 5: Why is there no C_0 for dust?

Figure 4: Why are the lidar measurements extended down to ground if you do not include observations at ground?

Figure 1: The background colors of the map make it difficult to see the symbols.

Figure 6: Legends and captions are too small to be readable.

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Minor comments / expressions that are difficult to understand

page 11863, Line 2: What is an original approach?

page 11863, Line 11: “contrasted levels”: better “different levels”

page 11885, line 21: boundary layer height

There are several other sentences obscure. It takes too long to write them all down.

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