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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling and assimilation of lidar signals over Greater Paris during the MEGAPOLI summer campaign" *by* Y. Wang et al.

Anonymous Referee #3

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The paper comes after a previous publication on the assimilation of lidar observations in a CTM (Wang et al. 2013 in ACP) where the assimilation is based on an empirical relationship between aerosol concentrations and lidar measurements of the European lidar network (EARLINET). The present paper is different since it is proposed to assimilate the lidar signal instead of mass concentration to improve the PM10 or PM2.5 simulations. The work plan is well defined and the scope of the paper is relevant for publication in ACP. I agree with the comments of reviewer 1 and 2 saying that the paper needs major improvements before publication. The paper is also very difficult to read because the links between the different results and also the different sections are

poorly discussed.

Specific comments

1)I do not understand why the data of the MEGAPOLI campaign provide a better benchmark for the assessment of the assimilation method compare to the previous paper using a longer time period and larger area. This should be better explained in section 2.2. It is said it will be done in a forthcoming paper but better to be more convincing about the added value of the present approach. 2)The section 3.1 on the assessment of the reference altitude zref is not very well justified, it comes in the lidar OSSE development section , I agree that it is needed for calculating the scattering ratio in the model but the method to derive zref is only relevant to the lidar data analysis. This is a very well known procedure in lidar data processing and the added value of this paper is not obvious on this topic. I agree with reviewer 1 that if an improved technique is proposed is should be compared to other approaches commonly used. What is the scattering of the different calibration factor derived when applying this technique ? 3)Section 4 called model evaluation is the weakest part of the paper. The comparison results provided in Table 2 and 3 on the model simulations of PM10, PM2.5 and AOD are not discussed while they show a large variability. The bias in PM10 compare to PM2.5 is not obvious to understand. It is also difficult to assess the role of the horizontal variability, and of the the small number of observations (6 diurnal cycles and 2 stations for AOD, 4 for PM2.5). Only PM10 data can be considered fairly representative of the horizontal variability. It would be helpful to have some horizontal map of the daily maximum of PM10, PM2.5 and AOD from the model simulations and PM10 horizontal distribution from the campaign surface network. The bias between model runs and data on PM10 was already mentioned in Royer et al. 2011 but with smaller values while the same campaign data are considered. It is not clear as it stands where is the added value of section 4 on model evaluation when considering the results already published in Royer et al. 2011.

4)In this paper the assesment of the model runs in this paper should be based on the

comparison with lidar profile as discussed in section 5. A discussion on the difficulty of the model to reproduce the upper altitude aerosol layer near above 2 km should be added. Generally speaking this section on lidar does not read very well because the differences are mentioned but without an overall synthesis of the discrepancies. Discussions about the temporal change are difficult to follow because the X axis scales of FiG. 4 to 9 are always changed. Why not trying to compare the plume distribution derived from the mobile lidar to illustrate the model ability to reproduce the horizontal variability ?

5)I am not very familiar with the data assimilation numerical techniques but my question is how the assimilation impact the PM10 distribution in the lower layers when the lidar shows upper altitude layer due to long range transport not seen by the model ? A more physically meaningful constraint could be to modify the boundary conditions from the large scale domain which probably control the occurrence of these upper altitude layers.

Technical details:

Figure 1 is very difficult to read especially to identify the stations measuring PM2.5

Figure 4 to 9 (not blue points only red and black lines)

In figure 4 to 9 add a panel with a map showing the position of the different profiles.

Add the full date in the first column of the tables

Why only 4 days for the AOD observations in Table 3 ?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27115, 2013.