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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Exploring the relation between aerosol optical depth and $PM_{2.5}$ at Cabauw, the Netherlands" *by* M. Schaap et al.

M. Schaap et al.

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Dear editor, colleagues,

Below we address the reviewer issues/questions raised during the open discussion of the paper Exploring the relation between aerosol optical depth and PM2.5 at Cabauw, the Netherlands;. We like to thank the reviewer for his comments. We think that the readability of the paper has improved considerably as a consequence of his comments. Below we have listed all reviewer comments and provide answers in Italics.

Anonymous Referee 2 The paper by Schaap et al is a useful addition to the growing use of satellite data for air quality work. In this paper the authors study the AERONET AOD-PM2.5 relationship from the ground, use a lidar to assess cloud cover, use a ceilometer to obtain mixing layer height, examine the AERONET AOD-PM2.5 relation-

ship for midday conditions, compare the MODIS AOD-PM2.5 relationship, and finally create a PM2.5 map using these relationships. The authors certainly understand the problem; otherwise they would not have used the multiple data sets to address these issues. Having said that, the paper is not well written to capture the analysis and results. The abstract and conclusions especially are rather weak and do not give the reader the most important points of this paper. Case in point 8211; If the mixing layer height information did not improve the AOD-PM2.5 relationship then it should be stated in the abstract clearly. Therefore, I think that the paper needs quite a bit of editing. The authors need to rewrite the paper to provide a well thought out flow to the paper.

Reading the paper again after not seeing it during the review period we agree with the reviewer that the reader should be guided through the paper in a better way, especially in section 4 and 5. Hence, we have put more introduction sentences at the start of these sections and paragraphs to explain the goal of that piece of text. Furthermore, in section 5.1 the order of the discussed items has been changed to allow easier reading. Finally, we have fully rewritten the abstract and conclusion section, which have improved significantly in our opinion. The new abstract and conclusions section can be read in the paper.

Other points Why is 18 ugm-3 considered high? Are there standards in Netherlands and Europe; like the United States? If so, we need to see a Table of what is considered high and what is low etc;

This level is high in comparison to other areas in Europe. Especially for the rural background. Hence, it is a relative statement and I have put 8220;rural background8221; and 8220;relatively8221; in the sentence: The measured average PM2.5 concentration was 18.2 956;g/m3. This rural background level is high in comparison to other areas in Europe (Putaud et al., 2004) and confirms that the Netherlands are characterised by a relatively high PM burden8221;.

In Europe there is a new target value for PM2.5 (25 956;g/m3). It is not a limit value

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and the target value does not reflect a strong ambition in our opinion. There is almost no place Europe where it is exceeded and thus the value is not very strict. Note that the US limit value of 15 is likely to be exceeded over the majority of the area of the Netherlands.

How good and relevant are the satellite spatial maps in the area? How can we assess the uncertainties in such a broad scale?

This is an important issue. We have identified that the gradients in the high resolution AOD distribution over the Netherlands do not appear to be very realistic, e.g. the high values of PM2.5 around Lille and near the northern coast of the Netherlands. This might be caused by spatially varying systematic errors that are present in the MODIS AOD data, particularly due to unaccounted variability in surface reflectance. Hence we have advised to devote special attention to the validity of the gradients in high resolution AOD data products.

Why was the LIDAR not used to assess aerosol heights to understand the AOD-PM2.5 relationship?

Pending the implementation of the wavelet method (as used by de Hay et al.) on the data from other lidar instruments available at Cabauw, including the RIVM backscatter lidar, we used the MLH data from the site of de Bilt, about 25 km North-East of the Cabauw station. Labour intensive manually derived MLH values have been derived from the RIVM backscatter lidar for a selection of days. For these days we found that the manually retrieved MLH was in agreement with automatically obtained findings. Hence, we used these for the whole study. We have put this information in the text in section 2.2, together with the additional discussion on the Ceilometer data (see point 5 under specific comments).

Specific comments. 1. Page 2, line 6-7, Inclusion of newer references for health impact of PM2.5 would be a good idea.

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We have added the following reference:

Kappos, A.D., P. Bruckmann, T. Eikmann, N., Englert, U. Heinrich, P. Hoppe, E. Koch, G. H. M., Krause, W.G. Kreyling, K. Rauchfuss, P., Rombout, V. Schulz-Klemp, W.R. Thiel and H.E., Wichmann (2004), Health effects of particles in ambient air, Int. J. Hyg. Environ. Health, 207, 399-407.

We did not remove the original references to acknowledge the impact of these studies. Note, this was already changed as a result of the technical changes before the paper was published in ACPD.

2. Page 3, para 2, line 7-8, Reference should be; Wang and Christopher, 2003; Also this study did provide any analysis on western USA

The reference is corrected, the reference and the others are placed there as these studies provide an overview over the whole of the US. Note, this was already changed as a result of the technical changes before the paper was published in ACPD

3. Page 4, last para of introduction, It will be more appropriate to call section 1, section 2; instead of chapter 1, chapter 2 etc

We have searched the paper for the word chapter and replaced it with section. Note ,this was already changed as a result of the technical changes before the paper was published in ACPD

4. Page 4, para 2, line 4-5, CIMEL measure in 10 channels. If this specific CIMEL is built to measure in 4 channels then please specify, otherwise it may create confusion to readers.

The instrument is a CIMEL built to measure at 4 wavelengths as is stated in the text. We have added the word specific in the text to draw the attention of the reader. Note, this was already changed as a result of the technical changes before the paper was published in ACPD.

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5. Page 7, Haij et al, 2007, a brief discussion on MLH retrieval from ceilometer would be useful to readers.

We have expanded the description of the MLH retrieval and incorporated a small discussion on the methodology used by the Haij et al including a quality indication in section 2.2:

8220;The method by deHaij is a wavelet method for the automatic determination of mixing layer height from backscatter profiles of an LD-40 ceilometer and introduces a quality flag to identify the reliability of MLH layer detections. The performance of the Wavelet MLH algorithm was analysed by comparing the results with MLH estimates from radiosondes, wind profiler and research lidar measurements. A correlation coefficient of 0.64 was found between ceilometer and radiosonde determinations when using only ceilometer MLH detections with good quality. The quality flagged MLH determinations show successful layer detection can be obtained in about 50 percent of all measurement cases. Unfortunately, the ceilometer at Cabauw was not operational for most of our sampling period. Pending the implementation of the wavelet method on the data from other lidar instruments available at Cabauw, including the RIVM backscatter lidar, we used the MLH data from the site of de Bilt, about 25 km North-East of the Cabauw station. Also, manually derived MLH values were obtained from the RIVM backscatter lidar during selected cases, which were in agreement with automatically obtained findings.8221;

6. Page 8, para 1, line 3, Check Levy et al., 2008 papers for revised accuracy numbers and refer them.

We have done this (20 percent was changed into 15percent): 8220;The accuracy of MODIS AOD over land is 0.05+/-15percent (Levy et al., 2007a).8221;Note, this was already changed as a result of the technical changes before the paper was published in ACPD.

7. Page 9, Fig 2, No data for Oct-Nov, any clarification why this data is not used?

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Unfortunately, we have had a technical malfunction and a roll-over from the internal data-buffer, which caused a loss of data. We have been cautious and have used only data that we trusted one hundred percent. We may have been overly selective, but we feel this is better than including data we are not sure about.

8. Page 14, para2 , line 5-6, Terra and AQUA are morning and afternoon satellite respectively. Rewording required to the sentence describing this fact.

This sentence was indeed not very concise. We have changed the sentence in the correct order and added the word respectively to indicate which of the two is the morning and afternoon satellite:8220;MODIS/TERRA and MODIS/AQUA have their overpasses in the late morning and early afternoon, respectively.8221;Note, this was already changed as a result of the technical changes before the paper was published in ACPD.

9. Page 18, para 2, line 9-10, most of the PM2.5 sites in US comes under EPA network called AirNow not IMPROVE. IMPROVE is a speciation networks, which does not make daily observations.

I have included both networks in the text as the references used IMPROVE data. However, the statement is general so using both names appears to be sensible. Note, this was already changed as a result of the technical changes before the paper was published in ACPD.

10. Page 19, para2, 1-5, this discussion give impression that you are the first one to use collection 5 for such studies. There are studies such as Gupta et al., 2008, Hutchison et al., 2008 (in your reference list), which also used collection 5 MODIS data.

This impression was certainly not the message of this paragraph. We intended to illustrate the importance of the change in collection in the interpretation of the data. I have reworded the sentence by removing 8220;in contrast8221; and instead of using 8220;we8221; I have generalised the statement by using 8220;recent studies8221;.

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Note ,this was already changed as a result of the technical changes before the paper was published in ACPD.

11. Page 20, section 5.2, Change in title of this section would be appropriate, as no mapping has shows under this section. Figure 11 is already discussed in section 4.

I have changed it to 8220;5.2 Estimated PM2.5 distribution8221; as the discussion presented here covers the representativity and the validity of the distribution.

12. Page 28, Table 1, Minimum value of PM2.5 is reported as 0.0, does not look like real value? Also, was wondering if you have included 0.0 values in your data while calculating statistics?

The nature of the TEOM-FDMS makes a measured zero concentration possible. It is always difficult to decide how to handle zero, close to zero and values below detection limits. In this case there are 10 data points with a zero concentration, which were included. They do not impact the statistical data due to this very low number compared to almost 4000 data points. Also, these zero concentrations are mostly during rainy conditions, in which no AOD data are available.

J. Vidot

Dear authors, I think you did a mistake with one reference in the text and in the references list. You referenced (Vidot et al., 2008) page 17941 line 26 for a empirical comparison between PM and AOD but the good reference is (Vidot et al., 2007): Vidot J., Ramon D., and Santer R. (2007), Atmospheric particulate matter (PM) estimation from SeaWiFS imagery, Remote Sensing of Environment, Vol. 111, Issue 1, pp. 1-10.

We would like to thank Dr. Vidot for this remark. I have indeed seleceted the wrong reference in my literature database. We have corrected the reference.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17939, 2008.