

## ***Interactive comment on “Physical and optical aerosol properties at the Dutch North Sea coast” by J. Kusmierczyk-Michulec et al.***

**Anonymous Referee #1**

Received and published: 12 March 2007

General:

The paper deals with the analysis of an 1.5-year AERONET Sun photometer time series. These vertical column-integrated observations were conducted in the Netherlands and have been related to surface meteorological data and surface observations of PM<sub>10</sub> and the black carbon concentration. Although a huge amount of results is presented, the key results are not presented in a conclusive and convincing way. Too many aspects are mixed, too many questions remain open. Figures and discussions are not well organized (as outlined below).

Column observations are compared with surface observations. Situations with clean days according to PM<sub>10</sub>, and at the same time, large optical depth of  $>0.6$  (at 555nm)

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are found. How is that possible? No discussion! No discussion on a possible influence of lofted layers (Saharan dust, fire smoke, urban pollution from Great Britain that may have been transported mainly above the maritime boundary so that PM10 surface measurements were not affected).

No discussion on the effect of relative humidity on aerosol optical properties is given. This important impact is completely ignored. Especially for the Netherlands, I expect a comparably large moisture influence. The same amount of aerosol particles can easily cause AODs that differ by a factor of  $>2$  as a function of relative humidity. Without some kind of normalization (based on daily radiosonde humidity profiles), optical depth values for the sea breeze cluster cannot simply be compared with AODs approaching from the east (from the continent). High humidity may, e.g., explain the paradox finding of a clean (according to PM10), polluted (according to AOD) day.

The authors relate meteorological surface data with column information in the old fashion. They just use wind direction to identify different air masses. Why does AERONET provide backward trajectories? Because it is known that the AOD is a strong function of the path way the air mass took when crossing all the different aerosol source regions. Simple west-wind and east-wind categories certainly give first hints, but are clearly insufficient for an in-depth inspection of the AERONET AODs. The Netherlands may be an exception, one may think. But usually cluster analysis based on trajectories is demanded. I was happy to see that the lidar community picked up this way (Wandinger et al., air mass modification..., 2004, JGR Vol 109). They were obviously forced to do so. So, grouping of column observations according to backward trajectories (at least up to an arrival pressure level of 850 hPa) is state of the art. If you feel unable to do that, you should at least discuss this point in more detail.

A strong part (at least an interesting point for me) of the paper is the EOF analysis. The parameters found here seem to better explain the link, e.g., between PM10 and AOD. But even here, many aspects remain undiscussed. I would like to see direct comparisons of results when going the traditional way of data analysis (mean value,

deviations from the mean, link of these deviations to deviations from respective PM10 means) with the respective EOF method parameters (by using amplitudes, eigenvalues). These comparisons should be done in ONE and the SAME plot.

I would recommend to focus on this point in this paper. This is a new aspect in the AERONET business. I would recommend to change the title: EOF analysis of AERONET observations ... This would attract many aerosol scientists.

Please avoid the presentation of so many tables. Tables give often the impression that the authors were unable to make the point. Also some of the figures are not needed (as outlined below).

To be clear: No doubt, good work is done, interesting results are presented. Thus, it would be a pity if the style of the paper would prevent many scientists from reading it (because of too many tables, not well organized figures, unsatisfactory discussion and conclusions).

Major revisions along the suggestions (Details) given below are required. I am convinced that these suggestions will significantly improve the paper.

Details:

Title: In the case that you re-organize the paper (EOF in the focus) state EOF in the title. By the way, the present title is rather unspecific. At least '... based on AERONET observations and ...' should show up.

Abstract:

must be rewritten after all the revisions...

Section 1: Introduction:

One may give a reference to this lidar paper mentioned above (Wandinger et al., JGR 2004). They tried to get an idea about the optical depth distribution over Europe and where the main sources are, and what a 'clean' optical depth (pure maritime value) is.

## Section 2: Experimental data:

A general point: Change (throughout the paper) Angstroem COEFFICIENT by Angstroem EXPONET.

A maximum measurement wavelength of 1020 nm does not allow the retrieval of an aerosol volume size distribution up to 15 microns. The maximum radius may be 2-3 microns.

Note, because of the limited range of wavelengths (440-1020 nm) artifacts in the retrieved size distributions can show up. In the desert, you may get an artificial fine mode (to get the optical data reproduced). In urban areas, you get a coarse mode (for some compensation reasons). So, the size distributions have generally to be interpreted with some care. Sure, the main information (pronounced fine mode or pronounced coarse mode) is trustworthy.

AERONET provides backward trajectories: Are these calculations based on HYSPLIT? or what is the source? That should be stated.

## Section 3: Data overview:

Why do you use such a 'strange' wavelength (555 nm,  $= (440 + 670) / 2$ ). Why not using the 500nm wavelength? ...for better intercomparison with other data sets.

Table 1: Why do you not provide mean values and standard deviations for each month? (as usual, and for better comparison with other AERONET time series published).

Fig 2: Why not presenting daytime mean values (as open circles) and in addition the monthly means (with STDs as error bars). The diurnal cycle of the AOD (aerosol optical depth) is then removed, trends and large-scale features may become visible.

I personally do not believe any  $AOD > 0.6$  over western/central Europe (except during Saharan dust outbreaks, with lofted plumes in the free troposphere). Nevertheless, as long as the Angstroem exponent (A.E.) is larger than 0.8, one has to accept these

values. If  $A.E. < 0.8$  cloud screening was not ok. Was the relative humidity in the PBL high during the high AOD events? Please provide more information in cases with  $AOD > 0.6$ . Why are these AODs that high. Is it possible to check the Cabauw lidar/ceilometer data concerning free tropospheric layers?

Figure 4: What is the message of this figure? For the smog days, BC/PM10 values are low for  $A.E. < 0.8$  ( $A.E.$  = Angstrom exponent), and high values for the rest of the  $A.E.$  values. This is obviously trivial. Maritime air shows less relative BC, continental more relative B.C. Thus, do we need a figure to demonstrate that? Do we need fits and related equations? My opinion: No!

Figure 5 is a bit confusing. The grey and white areas between the lines are misleading. In this way the areas are emphasized, but the lines are of importance. They should be pronounced by using thick (mean) and thin solid lines (minimum, maximum).

The discussion on page 1563 is weak, as long as you do not provide some idea about the natural variability. E.g., the maximum between 1.2 and 1.6 ( $A.E.$ ) may no longer be clearly visible (reliable) if standard deviation bars would be added for each class.

Here, the discussion would be significantly improved if backward trajectories would be used. If the AODs would be grouped with respect to long trajectories (strong winds), short trajectories (e.g., stagnant conditions), trajectories that never were over land (only North sea from the north), and so on, a much better classification would be possible. Just wind direction and wind speed information is simply not enough for a satisfactory analysis of column values.

Table 2: What is the message of Table 2? The message for me: there is no message (no correlation). Should be left out. And if presented, then with standard deviations. Standard deviation is often the most important parameter in atmospheric science.

Section 4: The Empirical Orthogonal Function (EOF) method:

The last paragraph of the foregoing section should be moved to become the first para-

graph of this section.

My feeling is that the paper is not well organized. All the theoretical background should be given before any measurement result is shown

So, please start with the experiment section, introducing the obs. techniques and the products, then present a theoretical background section, and finally present the result and discussion section. According to this, section 3 would be the first part of a result section.

Section 5: Application of the EOF method ....:

Regarding Eq.(13): If the product of  $h_1$  and  $\beta_{i1}$  is the deviation from the mean, it would be nice to visualize the time series of  $h_1$ ,  $\beta_{i1}$ , and the deviation from the mean itself for a certain time period. The 'added' value of Eq.13 is that the 'we can analyze all differences .... of the discrete wavelength'. Is it possible to visualize this added value? An attempt is made with Figure 6. Here one could show the deviation from the mean in addition, and  $h_1$ . Or is  $h_1$  just 1?

Table 3:  $\alpha$  is still the Angstroem exponent?  $\gamma$  is ... what? On the other hand, Table 3 is not discussed. Do we need the Table?

Table 4: is (again) very busy. Only the first three columns are relevant. Figures should be made from the most important findings. The rest should just be mentioned (... e.g., no pronounced relationship is found...). Again, clusters according to trajectories would be state-of-the-art.

It is confusing when the AERONET data are not shown as ONE block (Figures 2, 7, 9, 10, then 3 and 5, before any correlation is shown).

It is confusing when Figure 7 shows obviously seasonal means (again means without STD bars, must be added!), and this for all seasons, but for different wind clusters (NE-SSW in spring and autumn versus SSW-NE in summer and winter). What shall the reader learn from such mixtures?

It is confusing when Figure 9 gives the impression to be a continuation of Figure 7 (same titles, spring, wind sector, etc, summer etc., autumn, etc, winter etc.), but then single events are shown! What does it mean, when you show a single event, but indicate that this is the mean value. Please provide the TRUE mean value (average of all single events with proper STD bar)! Extreme cases may be presented for single events in addition.

It is confusing, when showing some trajectories, also for the 'mean' single events (18 April, 26 June). Why do you show trajectories for one, single event, even if this event shows almost mean conditions. Do you want to say that the respective trajectory represents mean air flow conditions?

Again, grouping of cases according to trajectory clusters and presentation of the mean values with STD bars for these clusters is the most solid ground on which a constructive discussion can be based. In any other case, most of the discussion is just speculation.

Reorganization of plots and discussion is strongly recommended. The mix of basic results (sometimes seasonal mean, sometimes single events), then correlation results in between, solid discussion, and speculative discussion (because nothing better is known) is rather confusing. Better organization will already significantly improve the paper.

As mentioned above, the humidity effect is completely left out (ignored)...although Shettle and Fenn is referenced! This is not acceptable! How can you discuss a clean day (characterized by a low PM10) and AODs up to 0.7 simultaneously (page 1568), but provide zero explanation for such contradictions. The large optical depth obviously is caused by a high relative humidity (in the column!) or by a lofted layer, not visible in the surface data. Please clarify that!

The discussion on page 1571 clearly shows the general need for trajectories. Surface wind information is only useful for a rough grouping.

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The discussion on the A.E. values is boring (page 1572). In central Europe urban haze is omnipresent? Thus one should keep the discussion as short as possible.

Again, the fine-mode and coarse-mode mean radii are certainly interesting products of the AERONET retrieval, but may be not just trustworthy. As mentioned, the coarse mode radius (because of the maximum wavelength of 1020nm) is questionable. So, keep the discussion short. I would omit Eq.(15). You are always quick with equations (fits), but not just with proper (exhausting, but critical) explanations (sorry for my spontaneous comment).

Figure 11 is useless.

Table 5 is not needed.

Table 6 is not needed.

Finally, I am wondering whether there are no other AERONET data sets from Europe that could be used for comparison. A broader context of European haze conditions would be desirable.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1557, 2007.

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