

The manuscript presents a novel and interesting technique to quantify the mineral dust concentration starting from calibrated multi-wavelengths absorption measurements. This manuscript deserves to be published in AMT.

Below my comments.

General comments

- 1) The proposed technique assumes that BC particles are not contained in the coarse fraction. Can the authors explain how much is this assumption true? And how the presence of coarse BC affects the mineral dust concentration calculated from the proposed method? There is a comment about this point only at lines 510-514 in the manuscript.
- 2) The authors should explain better the concept of using a virtual impactor to concentrate coarse particles in the AE33 tape. What it does actually mean? Why the simple formula $Abs_TSP - Abs_PM1$ cannot be used to estimate the absorption by dust in the UV? Is the use of the virtual impactor (VI) the way to increase the sensitivity of the technique for measuring the absorption by dust? And from this the need to estimate the enhancement factor (EF)? See another comment on this below.
- 3) A mean value of 11 for the EF is used here. However, the estimated EF varies quite a lot (Figure 5b). Would it be feasible to measure on-line the EF? How much the uncertainty of EF affects the final result compared to the other sources of error? EF uncertainty affects the MAC uncertainty and consequently the mineral dust concentration estimated from the optical measurements (?).
- 4) The on-line compensation for filter loading works well (as expected) for PM1 but not for the AE33 connected to the VI. The correction is performed off-line and a mean k is determined and used to correct the data. This introduces an additional uncertainty. Please, comment on this.
- 5) Figure 6: Is the slight overestimation of the theoretical curve (red line) at 660 nm compared to the experimental one (black line) due to dust absorption in the near-IR? The fit is performed from 880 nm assuming an AAE of 1. Dust should also absorb in the IR, isn't it?
- 6) Please, expand the Conclusion section if possible.

Abstract

Line 30: ".....highly time resolved on-line *detection* technique of *dust absorption*...". And also *dust concentration*? Maybe it would be better using the word "*quantification*" rather than "*detection*"?

Introduction

Line 56: "Desert dust impacts industrial production to a degree that has been fictionalized (Herbert, 1965)..." Interesting citation, but not very pertinent with the scientific data presented here. What do the authors mean for "industrial production"?

Line 68: "Daily time resolution of the described method has been validated.....". Please, describe briefly the method.

Line 92: ".....can be used to identify dust events....". Another advantage of using high-time resolution optical measurements, as the Angstrom Exponent of SSA, is the possibility to detect dust resuspending in the local atmosphere after the end of the episode (when there are no more air masses coming from African deserts). This has big implication for air quality. However, the Angstrom Exponent of SSA cannot be used to quantify the dust concentration whereas the proposed technique does. Maybe a comment on this could be added to the Introduction and Conclusion sections.

Lines 95-120: Here the authors present some previous techniques that can be used to quantify dust concentration in PM using dust aerosol absorption properties. It would be useful to explain what are the main advantages of using the technique proposed in this manuscript compared to the previous techniques. In which terms is the proposed technique innovative? For example, the proposed technique is also affected by problems such as the enhancement due to scattering from dust particles in the filter matrix. A sentence in the Abstract and/or Introduction could be useful.

Section 2.2.2

Line 208: "... the obtained data was compensated using fixed k values as described in ...". Please, define " k " here too.

Lines 211-223 (equations 1 and 2): Please, provide more details about the constants presented in this part of the text. For example, how was the new BC MAC at 880 nm calculated? From $3.5/2.14 = 7.77/4.74$? What is the constant 1.57? Which type of filter tapes were used during the experiment?

Line 226: The uncertainty of BC was estimated from the plots in Figure S7 as the standard deviation of the ratio BC_{tsp}/BC_{pm1} . From equation 2, this ratio is equal to $bATN_{tsp}/bATN_{pm1}$ for each wavelength. Consequently, this ratio should change depending on the relative proportion of dust and BC in the sampled air (especially in the UV). The higher the mineral dust concentration, the higher should be the ratio (at least in the UV). Isn't it? However, no correlation is observed in Figure S7a. Could the authors explain better this point? If I well understand, the lack of correlation in Figure S7a is because the absorption is dominated by BC in TSP too (not only in PM1) due to the much higher MAC of BC compared to dust particles. From this, the need of using a VI to concentrate the dust particles (whereas the BC particles are collected with a CE of unity in the VI) thus allowing measuring the absorption properties of dust. This point is commented at lines 480-485. Maybe it would be better commenting this before, e.g. in the section 2.2.1 or section 3.1.

However, based on the numbers provided in the manuscript, the dust absorption in TSP during the campaign is not negligible compared to the BC absorption: Thus, at 370 nm:

$\langle \text{mineral dust} \rangle * MAC_{\text{dust}} = Abs_{\text{dust}} (8.1 * 0.24 = \mathbf{1.94})$

<BC> *MAC_BC = Abs_BC (0.39*11.2=4.4). However, in Figure S7a a mineral dust concentration value of 15 mg/m³ will lead to a dust absorption at 370 nm of 3.6 Mm⁻¹. Please, comment about this point.

Line 336: "As shown in Figure 3 the absorption in TSP is closely related to the one of PM1". Please, see my previous comment.

Section 3.4

Figure 8b: The scatterplot was done using the mineral dust content in PM_{2.5-10}. How is the scatterplot if PM₁₀ is used instead of PM_{2.5-10}? I'm asking in case only PM₁₀ is analysed.

Line 471: The MAC from the fit is provided with a very low uncertainty (0.01). isn't too low? It should be higher given that there are important sources of error (BC, EF, k, ...). Maybe the fit should be performed with X-Y error bars.

Last (but not least) comment: Would it be possible to test this technique by simulating the mineral dust concentration (and comparing with the filters) using AE33 data collected during other dust events than those presented here? The data presented here are from April-May 2016. Do the authors have other (e.g. more recent) data (AE33 PM1 and VI) available to test the technique using the MAC and EF estimated in this manuscript?