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Interactive Comment

Interactive comment on "Study of the effect of different type of aerosols on UV-B radiation from measurements during EARLINET" *by* D. S. Balis et al.

Anonymous Referee #1

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General comments:

This manuscript discuss the effects of different type of aerosols on UV-B radiation. Currently, there are several areas of uncertainties regarding aerosol effect in UVB, if compared to the visible range for instance. Therefore, this paper deals with an important topic. Moreover, it is an interesting approach to combine Lidar measurements with indirect measurements of single scattering albedo (SSA).

In the next section, some specific issues for the revision are raised. First, there are some details of the approach that should be clarified or explained in more detail. Second, it seems that the case in Fig. 2 is not the most suitable for the verification of SSA approach. Third, some of the conclusions drawn from the cases 1-3 are difficult to

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endorse. Once the following points have been addressed or justified by the authors, I think this paper is suitable for publication.

Specific comments:

The input data used in RT model simulations should be explained in more detail. For instance in Figure 2), was the sza used in RT model EXACTLY the same than during each 323 nm irradiance measurement (and not taken from the header line of corresponding Brewer spectra, for instance)? What value for the surface albedo was used? How about the ozone - was it an average of the ozone values based on Brewer direct sun measurements or the measurement nearest in time to Brewer irradiance measurement?

Together with the Figure 2a) there should be a figure, showing the evolution of the AOD data used. Was this AOD in Figure 2) for 323 nm estimated with Angstrom coefficient from 399 nm or the value at 399 nm as stated in section 3.2? Generally, in all cases the wavelength of AOD used in RT model should be stated very clearly. It seems that in the Table 1) AOD values of "AOD416" are given. To what instrument this "MFR", used to measure these "AOD416" data, refers to? There is no discussion about "MFR" in section 2, however they are printed in the Figures 4-6.

It seems that the case in Figure 2 is not the most appropriate for the validation of the SSA estimation. First, AOD values during that day were rather low. As the authors state, the accuracy of SSA estimation is 0.2 for low aerosol conditions (AOD $<$ 0.2). In August 10th, AOD values for several wavelengths are available from http://www.iup.physik.uni-bremen.de/˜hoyning/altweb/LACE98/AOT/AOTData.htm The AOD values at 0.361 nm in that day, for instance, were typically around 0.12. So, the uncertainty of SSA values is more than 0.2.

The authors argue that the approach in August 10th is further justified by two references; Ansmann et al. 2002 and Bundke et al. 2002. I think neither of them offers very strong and clear support.

Ansmann et al. 2002 write: "A thin, slowly descending aerosol layer was detected between 3 and 4 km on 10 August (particle optical depth of 0.02-0.05). According to backward trajectories this layer originated from forest fires in western Canada" It seems that this descending between 3-4 km is observed also in Figure 2b of Balis et al. Figure 2b shows also the evolution of boundary layer depth and mixing height, but I am not convinced that the figure 2b would give an indication about any change in aerosol type. Ansmann et al. also write: "flow turned from north on 10 August over east to southeast on 11 August". A more precise timing would be interesting to see from the data of wind direction. From the statement of Ansmann et al., one gets an impression that the wind direction did not change before 11 August. Moreover, they say that "the particle optical depth at 532 nm increased from 0.05 (10 August, late evening) to 0.35 (11 August, early morning)".

Therefore, I would argue that the article by Ansmann et al. 2002 itself does not give clear support to the Figure 2b. However, the data of wind direction, for instance, could do this. So, the authors may want to consider of including data of wind direction during August 10th.

I looked at the Figure 9 of Bundke et al. 2002, since I assume it is meant by the authors to support their SSA estimates. It is rather difficult to interpret a single day from that figure. Moreover, one has to be sure how the Julian day has been defined. In LACE 98 data Julian day starts always from 0 (see Figure 2 of Ansmann et al. for example). In other words, Julian day of 221.5 refers to August 10th 12 UTC, for instance. By a careful look at the figure 9, I would say that w_imgf in August 10th was 0.84 and the next value was 0.83. However, the uncertainties in the Figure 9 are about 0.1. Moreover, in August 10th, only one value per day is shown, so it is difficult to evaluate the very short-term changes in SSA. The reason is that the figure 9 of Bundke et al. 2002 shows the mean values for filter sampling periods. So I would argue that it is not possible to extract morning and afternoon values from their Figure 9. In a rough sense, it seems that SSA decreased around August 11th, but more precise timing is not clear.

From Ansmann et al. 2002 I get an impression that it took place in "early morning" of August 11th.

To summarize the above discussion, I think there are three reasons why the case in Figure 2) is not the most appropriate for the verification of SSA estimation: 1) low AOD values in August 10th => uncertainty of SSA is more than 0.2 2) Ansmann et al. 2002 and 3) Bundke et al. 2002 do not give clear and unequivocal support.

One additional point about Figure 2): if the Brewer 323 nm irradiance measurements were not cosine corrected, it deserves to be mentioned as an additional source of uncertainty. The cosine error of Brewer is typically 5-10% and it depends on sza, so now when global irradiance values (and not the ratio of direct and diffuse) are used to retrieve SSA, it is a source of uncertainty. How about the spectral data of Thessaloniki used in this study, were they cosine corrected?

About the three cases in Figures 4-6. I think generally they make a rather good set. However, in my mind, in case 2 there is some contradiction. Is there a reason why the authors do not show LR in October 29 below 1 km? It seems that there is high vertical variability in the LR in that day and the values of backscatter (divisor in LR) are peculiarly small. Moreover, despite of the authors' argument (in block 4683 from line 22 "We have to emphasize ..."), the clear contradiction between SSA and LR remains. (By the way, this argument applies to any case and should be stated earlier.) But in this case 2, I think it cannot explain the disagreement between SSA and LR. If one takes a look at the Lidar measurements at the around 1 km and below (layers that are affected by the local boundary layer development) I think relatively speaking in October 29 there must have been stronger absorption in the layers below 0.8 km than in September 13. But still the SSA (which is affected by lowest layers as well) in September 13 is smaller. So I would argue that both SSA in September 13 and LR in October 29 look rather peculiar and the contradiction remains. Also, I think it is an optimistic statement to say that the trajectories in September 13 support the maritime component. At least, there has been mixing during 4 days with the polluted continental air mass in central Europe.

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I would make a bit different interpretation about the case 3. Based on the direction of trajectories only, October 4 seems more like "local pollution". October 11 could be maritime, but maybe mixed with the components of Saharan dust (if the trajectories went further back in time). But in the former case, there has been a large-scale subsidence and cleaner air from higher layers are transported downwards. This is not necessarily the most precise interpretation, but the one by the authors is not strongly supported by the trajectories either.

In Wenny et al. 2002 and Petters et al. 2003 (JGR, doi:10.1029/2002JD2360), no strong correlation between air mass type (based on the trajectories) and single scattering albedo was found. I think that from the cases in this study clearly opposite conclusions cannot be drawn. Trajectories in Figure 7 versus SSA (or cases 1-3) are not very unequivocal.

Minor point: in the figures there are four trajectories, in September 25 there are six?

About the Figure 7. In Figures 4-6 the authors wrote "we examined only spectral measurements performed during late afternoon hours in order to minimize the time difference between the two measurements". Is this same true in Figure 7 as well? What kind of time differences are there between Brewer and Lidar? In the case SSA variability was really as rapid as in Figure 2), it would affect the kind of comparisons shown in Figures 4-6.

35 clear sky days are included in the Figure 1 - in Figure 7 there are 18 cases. How about the rest?

I think Figure 8. should be explained in more detail. There are 40 cases, 5 for each aerosol type. Eventually it became apparent that for 8 aerosol types 5 different humidity classes were used. However, this figure should be introduced with more detailed description.

Block 4685, line 2 ("... the mean of the values ...). This was not mentioned before, but

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maybe it should.

Technical corrections:

The reference of Wenny et al. 1998 is missing.

Location of Lindenberg (in lat and lon) should be given.

First sentence in block 4683: case 12 should be case 2)

In Figure captions, mention Thessaloniki in Figure 1) and Figure 7).

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 4671, 2003.

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