

## ***Interactive comment on “Dust mass, CCN, and INP profiling with polarization lidar: Updated POLIPHON conversion factors from global AERONET analysis” by Albert Ansmann et al.***

### **Anonymous Referee #2**

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The paper “Dust mass, CCN, and INP profiling with polarization lidar: Updated POLIPHON conversion factors from global AERONET analysis” presents and discusses the POLIPHON methodology, implemented with an updated set of dust conversion factors, considering the different dust sources around the globe. These different conversion parameters are of critical importance for the POLIPHON methodology in order to compute dust mass, CCN and INP profiles. The paper is not only limited to computing, providing and discussing the novel set of dust conversion factors. A complex case of dust advection over the EARLINET-Dushanbe station in Tajikistan, originating from different desert dust sources is presented. The study falls within the scope of AMT. The authors have done a thorough job, the manuscript is well-written

C1

/ structured, the presentation clear, the language fluent and the quality of the figures high. Furthermore, the authors give credit to related work and the results support the conclusions. However, in order to help improving the manuscript, I would kindly suggest the authors to take into account the following specific comments.

1. The authors refer to the use of the “AERONET data base” in the manuscript. I suggest to provide more detailed information regarding AERONET (e.g. Version) and the use of AERONET data (e.g. level, files, name/list of parameters, units) in POLIPHON method. This is only done in Table 2 (version 3, level 2.0) but I believe it would be useful for the reader if it also stated in the manuscript.

2. I would recommend the authors to use a “world map” figure in the introduction, to give the reader an overview of the AERONET stations used in the study.

3. The manuscript provides a novel dataset of conversion factors for desert dust originating from different dust sources. Table 1 provides the input parameters in the POLIPHON method. However, no discussion is provided regarding the dust extinction coefficient. For instance, desert dust sources around the globe are characterized by different extinction-to-backscatter ratio and in addition different dust particulate depolarization ratio. Since the manuscript aims to provide the POLIPHON conversion factors per different station, has the different dust source per observed case been considered? For instance, how are the observed cases in Dushanbe, where dust originates from different sources as shown, were treated in terms of input dust extinction coefficient values? I assume that the authors have used proper inputs of lidar ratio and dust depolarization per different desert. Thus I would recommend the authors to provide a thorough discussion on the used parameters and in addition a table of the different values used in the methodology, since the accurate computation of the dust extinction coefficient is a critical input for the POLIPHON method per desert region. Have the authors considered the use of HYSPLIT in order to quantify the effect of different desert sources in the computation of the conversion factors for each AERONET station, in order to attribute the provided conversion values in the present manuscript

C2

not confined locally, to a station, but to extend the conversion factors to larger regions?

4. Page 5 – Conversion parameters from the AERONET data base. The authors state at the same time that “We preferred stations with long data records and large numbers of observations . . . for the statistical analysis” and that “We added the Leipzig AERONET observations with a small number of strong Saharan dust outbreaks”– but also stations like “Tuscon, Arizona” (17 dust observations), “White-Sands” (27 dust observations) and “Trelew, Argentina” (21 dust observations). Please consider revising the paragraph, since although the first statement holds for most of the AERONET stations it contradicts the use of other stations in the manuscript.

5. The authors are limiting the available AERONET measurements to dust dominated cases by defining all useful cases to have an Angstrom Exponent (AE) value less than 0.3 and Aerosol Optical Thickness (AOT) value larger than 0.1. My consideration mainly applies to near-coastal regions. Have the authors somehow tried to exclude the marine particles contribution to these cases? Are additional parameters considered when the dust dominated cases are selected? (i.e. the spectral dependence of the SSA?)

6. Table 1. Please consider expanding the Table to include units for the input and the output parameters, while at the same time the use of an additional column with the computed uncertainties (used also in the manuscript) per output parameter would be helpful for a potential user of the POLIPHON method.

7. Table 2. Please consider expanding the Table to include not only Dust AOT but the Total AOT, since the authors provide in addition to dust observations the total number of AERONET observations (dust and non-dust).

8. The authors provide the POLIPHON conversion factors in figures 4 and 5. I suggest the authors to include (on parallel to these figures and per conversion factor), world maps of the AERONET sites used in the study with the computed conversion factors with different color, depending on the computed conversion values, to demonstrate

C3

more clear the spatial distribution of the provided values.

9. Figure 8 and Figure 11. The authors use error-bars in the figures as a metric of the uncertainty, however it is not clear in the manuscript whether the shown uncertainties are computed for the shown cases, or are the more generic uncertainties computed and discussed in previous POLIPHON papers.

10. I suggest the authors to delineate the desert domains related to each AERONET site provided in Table 3, in order to facilitate the use of the conversion factors provided in the manuscript for global studies.

11. Page 2, Line 12: “ice and precipitation formation already at high temperatures of -15 to -35 C”. Please provide relevant references.

12. Page 3, Line 6: It is not clear to the reader what the parameter fss stands for. Same also holds for Table 1. Please revise accordingly.

13. Please provide more information on the temperature values used as input for the INP retrievals with the D15 and U17 schemes. Are those data provided from local radiosondes?

14. Page 4, equations 7 and 8: there is a typo, the values  $u_{df, j}$  and  $u_{dc, j}$  should be in reverse in the two equations.

15. Page 4, Line 11: “with the conversion factor  $c_{v,i,\lambda}$  and the particle extinction coefficient  $\sigma_{i,\lambda}$  measured with lidar at wavelength  $\lambda$ ” Please rephrase so that it is more clear, even to the less experienced reader that the conversion factor is not provided by lidar but from the AERONET measurements.

16. Page 6, Line 4: “by dividing”, do the authors mean by multiplying?

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C4