Response to RC1

General Comments:

The manuscript presents a new set of POLIPHON conversion factors for dust aerosols at oceanic or coastal sites. The authors use a depolarization ratio-based metric to identify dust cases in AERONET measurements and further classify them into clusters of pure dust (PD) and dust-dominated mixture (DDM). They estimate the CCN- and INP-related conversion factors for these two clusters and also compare them with the already existing ones. In addition, they discuss the variations of these conversion factors along the transoceanic pathways. The manuscript presents considerable development in the field of lidar-based CCN and INP retrieval and has good potential for publication in AMT only after implementing and addressing the following comments.

Response: We appreciate the reviewer's thoughtful review and constructive comments. All the comments have been addressed in the revised manuscript, and the responses to each comment are given below.

Specific comments:

Comments: Since you mention the significance of how estimating the 3D distribution of CCN and INP can improve the climate models twice, in lines 52–54 and 71–72, could you please elaborate in the text with one or two sentences on how it may do that?

Response: According to the reviewer's suggestion, we have added related sentences respectively for the mentioned two places in the revised manuscript as follows:

'Phillips et al. (2013) stated that the reliable quantification of the linkage between aerosol conditions and ice crystal numbers should be the first step in quantifying cold-cloud indirect effects. Thus, INP concentrations (INPC) are estimated in many studies to predict the initial in-cloud ice crystal number concentration (ICNC) via primary heterogeneous nucleation (Ansmann et al., 2019a; He et al., 2022a; Kanji et al., 2017). Moreover, discrepancies between INPC and ICNC are found to establish the role of secondary ice nucleation (DeMott et al., 2011). Therefore, the constraint of ambient INPC can…' (Please see Line 50-55)

'Therefore, the number concentration of cloud drops and ice crystals can be well quantified (Ramanathan et al., 2001), thus improving the current consideration of ACIs….' (Please see Line 76-77)

Comments: The microphysical properties of dust may change with ageing and mixing with other aerosol types (Kim and Park, 2012; Goel et al., 2020). This implies that the conversion factors estimated close to the main desert sources may not be the same as those estimated far away from the source region. This may be stated in the motivation.

Response: We have rephrased the related sentences as below and the suggested papers have been cited now. **'The dust-related conversion factors can be very different for the downstream areas far from the dust sources due to the possible aging and mixing of dust with other aerosol types during long-range transport (Kim and Park, 2012; Goel et al., 2020). However, for these downstream areas, dust-related conversion factors are still lacking owing to insufficient data points fulfilling the criteria.'** (Please see Line 88-91)

Comments: Some validation studies that use POLIPHON conversion factors to estimate aerosol number concentrations and CCN concentrations from spaceborne lidar measurements highlight the need for improvement, especially for dust and marine aerosol mixtures (Choudhury et al., 2022; Choudhury and Tesche, 2022). Such studies can be included in the motivation for this study.

Response: Thank you for the reviewer's suggestion. We have added a sentence to mention this motivation as below **'The POLIPHON method has been proven to be useful for examining the profiles of CCNC, INPC, and aerosol number concentration retrieved from spaceborne lidar measurements with other algorithms (Choudhury and Tesche, 2022a, 2022b; Choudhury et al., 2022c).'** (Please see Line 62-64)

Comments: Line 136. Ansmann et al. (2019) use aerosol optical thickness (AOT) at 532 nm, not 500 nm. They estimate AOD at 532 nm using Ångström exponent and AOT at 500 nm. Since you compare your results with Ansmann et al. (2019), it is necessary to stay consistent. Also, 532 nm is the usual lidar wavelength. As you aim for application to spaceborne lidar, you should use this wavelength in your analysis. In general, the wavelengths mentioned in the paper for AOT and extinction coefficient up to this point are inconsistent. For instance, it is 532 nm in line 60 and 550 nm in line 139. Did you convert the AOT from 500 nm to 532 nm to estimate the conversion factors? If not, then how do you justify your calculations, as we cannot ignore the wavelength dependency of optical thickness?

Response: In the revised manuscript, to be consistent with the calculation in Ansmann et al. (2019), we have updated the dust-related conversion factors by converting AOD at 500nm into AOD at 532nm using Ångström exponent. It should be noted that only very small changes are found for the obtained conversion factor values. Also, '550 nm' in line 139 has been modified to '**532 nm**'.

Comments: Line 218. It seems like you did not convert the AOT to 532 nm when calculating the conversion factors. Please address the previous comment.

Response: We have converted AOD at 500 nm into AOD at 532 nm by considering the

Ångström exponent. Therefore, all the dust-related conversion factors are updated as seen in Figure 3, Table 3, Figure 4, Figure 5, Table 4, Figure 6, Figure 7, Table 5, Figure 8, and Figure A1. It should be noted that only very small changes are found for the obtained conversion factor values.

Comments: I found the transition from Section 2 to Section 3 to be very abrupt. I suggest adding a paragraph under Section 3 before going to Section 3.1, giving a brief introduction to the section and the analysis you present here so that readers have a basic idea of what to expect. This should have been included in the "Data and Methodology" section. However, looking at the organisation of the manuscript, I suggest adding it at the beginning of Section 3.

Response: Thank you for pointing out this. Taking the reviewer's suggestion into consideration, we have added a new paragraph at the beginning of section 3 (before section 3.1) in the revised manuscript as follows '**In this section, we mainly focus on the calculation of the dust-related conversion factors in the POLOPHON method with the new dust identification scheme, which is based on the particle linear depolarization ratio in the AERONET data product. To verify the performance of the proposed dust identification scheme, the dust-related conversion factors near deserts are first calculated at nine AERONET sites and compared with those obtained by Ansmann et al. (2019b). Then, the dust-related conversion factors** $c_{\mathrm{v,d}}$, $c_{250,\mathrm{d}}$, $c_{\mathrm{s,d}}$, $c_{\mathrm{s,100,d}}$, $c_{100,\mathrm{d}}$, **and** χ_{d} **at 20 oceanic/coastal AERONET sites are derived with the proposed method. Finally, the variations in the dust-related conversion factors along the two transoceanic (i.e., transatlantic and transpacific) pathways are analyzed.**' (Please see Line 232-238)

Comments: Lines 226-228. How can the number of data samples be linked to the level of agreement or difference between pure dust (PD) and PD + dust-dominated mixtures (DDM) clusters? Having fewer samples could only imply that the result is not significant or lacks confidence.

Response: For accuracy, we have removed this sentence.

Comments: Line 231. Can you speculate on the local aerosol sources based on the sites or locations where you found the differences between PD and DDM clusters?

Response: We preliminarily checked the data from other terrestrial AERONET sites across the world with sufficient dust cases available. These strange results are only found for some sites in the following three regions: (1) Middle East (e.g., KAUST_Campus, Bahrain, Dhadnah, Solar_Village, Weizmann_Institute, and SEDE_BOKER); (2) Africa (Blida, Cairo_EMA_2, Dakar, IER_Cinzana, Ilorin, and Tunis_Carthage); (3) polluted Europe sites (ATHENS-NOA, Coruna, CUT-TEPAK, El_Arenosillo, FORTH_CRETE, Granada, IMS_METU_ERDEMLI, Lampedusa, Palma_de_Mallorca, Pairs, and Rome_Tor_Vergata). The strange pattern for the DDM cluster can even extend at the extinction coefficient range of 100-800 Mm^{-1}, such as at Cairo_EMA_2 and Solar_Village. When we remove the DDM data from DDM+PD data, the Pearson correlation coefficient for $C_{s,d}$ and $C_{s,100,d}$ will significantly increase, for example, from 0.7-0.8 to >0.95 (this is the usual situation for most sites as we checked).

Considering that this strange pattern appears at the sites from different regions, it would be difficult to give credible speculation on the local aerosol sources. This strange pattern does not appear at oceanic sites that are focused on in this study. Here we mention this strange pattern so as to remind that more care should be taken when employing DDM data to retrieve conversion factors at terrestrial sites in the Middle East, Africa, and polluted European cities. Therefore, to remind this, we have added a sentence in the revised manuscript as below '**Thus, it should be noted that more care should be taken when employing DDM data to retrieve dust-related conversion factors at terrestrial sites in the Middle East, Africa, and polluted European cities in future work.**' (Please see Line 260-261)

Comments: The comparisons presented in Section 3.1 are all qualitative. I would always prefer and recommend a quantitative analysis. Please quantify the differences in terms of percentages or absolute values.

Response: Considering the reviewer's suggestion, we have updated the text of the third paragraph of section 3.1 by using the percentages to describe the differences in conversion factors between A-19 and this study. (Please see Line 265-270)

Comments: Line 260. PD cases are already included in PD+DDM clusters, right? Please change "PD and PD+DDM" to "PD and DDM". Also, how do you define an adequate sample size? Please include it in the text.

Response: Yes, PD cases are included in PD+DDM clusters. In Ansmann et al. (2019b), the lowermost number of available data points for calculating the conversion factors is 17 at the Tuscon AERONET site. In this study, we provide INP-relevant ($c_{\mathrm{v,d}}$, $c_{\mathrm{250,d}}$, $c_{\mathrm{s,d}}$, and $c_{\mathrm{s,100,d}}$) conversion factors (see tables 3 and 4) with the number of available data points no less than 12 to ensure as many sites as possible can provide dust-related conversion factors with somewhat acceptable reliability. With data point number below 12, the data points may be diverging caused by occasional dust cases, causing very small linear Pearson correlation coefficients R. Here we provide R values for each dust-related INP-relevant conversion factor at each AERONET employed in this study as seen in the following table. It can be seen clearly that most of the conversion factors have corresponding R values exceeding 0.70, except for PD-derived $c_{\mathrm{250,d}}$ values at NR and AS (as marked in red in the table), which can guarantee the robustness of the retrieved conversion factors.

	Site	R for $C_{v,d}$		R for $C_{250,d}$		R for $C_{s,d}$		R for $C_{s,100,d}$	
		DDM+PD	PD	DDM+PD	PD	DDM+PD	PD	DDM+PD	PD
North Africa	CV	0.97	0.97	0.94	0.94	0.76	0.78	0.98	0.99
	DK	0.96	0.96	0.94	0.94	0.71	0.74	0.94	0.97
	IZ	0.98	0.98	0.92	0.92	0.77	0.77	0.99	0.99
Middle East	EI	0.95	0.97	0.91	0.92	0.56	0.82	0.56	0.99
	SV	0.97	0.98	0.96	0.96	0.76	0.78	0.91	0.98
	ME	0.96	0.98	0.93	0.96	0.76	0.82	0.79	0.97
Asia	DU	0.98	0.98	0.95	0.96	0.84	0.83	0.94	0.98
	DA	0.90	0.77	0.76	0.83	0.41	0.93	0.67	0.70
	LA	0.96	0.97	0.86	0.93	0.76	0.73	0.95	0.99
Pacific	TA	0.88	-	0.89	-	0.87	-	0.94	-
	NR	0.93	0.80	0.90	0.32	0.91	0.79	0.96	0.76
	MI	0.92	0.91	0.96	0.97	0.95	0.97	0.97	0.98
	AS	0.89	0.91	0.84	0.50	0.86	0.75	0.92	0.81
	GA	0.80	-	0.86	-	0.89	-	0.93	-
	ML	0.88	0.92	0.92	0.95	0.90	0.95	0.95	0.99
Pacific Coast	HU	0.84	1.00	0.84	-	0.87	-	0.93	-
	OS	0.86	0.90	0.75	0.98	0.66	0.79	0.76	0.87
	SH	0.93	0.99	0.94	0.97	0.89	0.88	0.90	0.98
	SI	0.92	-	0.94	-	0.89	-	0.96	-
	TR	0.88	0.98	0.93	0.98	0.93	0.97	0.95	0.98
Atlantic	AG	0.98	0.99	0.98	0.98	0.95	0.97	0.99	0.98
	TH	0.93	0.98	0.92	0.97	0.92	0.97	0.96	0.99
	ST	0.91	-	0.91	-	0.95	-	0.98	-
Indian Ocean	MG	0.82	-	0.83	-	0.92	-	0.95	-
	AI	0.93	0.82	0.89	0.82	0.93	0.85	0.98	0.92
Arctic Ocean	NA	0.92	-	0.93	-	0.76	-	0.88	-
	TL	0.95	-	0.91	-	0.91	-	0.95	-
	OP	0.76	-	0.94	-	0.87	-	0.91	-
	IQ	0.74		0.72	-	0.84	-	0.88	-

For clarity, we have rephrased this sentence in the revised manuscript as '**The results for only PD cluster and combined PD and DDM clusters are listed. We consider the conversion factors with ≥12 available PD data points valid (provided in Table 4). Moreover, to guarantee robustness, only the retrieved conversion factors with the linear Pearson correlation coefficient *R* exceeding 0.70 are considered valid, except for PD-derived $c_{250,\mathrm{d}}$ values at NR (R=0.32) and AS (R=0.50), which should especially be handled with care in scientific applications.**' (Please see Line 288-292)

Comments: Line 306. You haven't mentioned the cluster you used in Figure 8. Looking at the values, I guess it is for PD cases. Here, I am assuming that the variations in conversion factors along the transoceanic pathways are for PD cases. From Table 2, I can see that the number of samples for stations at MI, ML, and TR are 26, 27, and 18, which are significantly less than other stations. Due to fewer samples, the variations that you report for these stations may not be realistic, as the long-range dust transports may vary seasonally and annually. One thing to look at is the distribution of the small number of sample points across different seasons and years. Are they limited to one season, or one year? In any case, you must mention these limitations in the paper. I would recommend using the DDM cluster, which has more than enough sample space to study the changes in the conversion factors along different transport pathways.

Response: Thank you for pointing out this issue. According to the reviewer's suggestion, we have updated figure 8 by using the conversion factors calculated from the PD+DDM cluster so as to make more data points available. Now, the results may be more reliable and the related statements have been updated accordingly. (Please see Line 357, 364-367)

As for the seasonal and annual variations of the characteristics of transoceanic dust, however, the sample numbers of dust cases are still too small to support us in conducting this analysis. To our point of view, according to the dust activity in the dust sources, the transpacific dust transport from East Asia to the west coast of North America mainly occurs in spring and the transatlantic dust transport from North Africa to the east coast of North America mainly occurs in summer. Thus, seasonal variation in the dust microphysical properties (determining the dust-related conversion factors) may be not significant. Moreover, the annual variation is somewhat out of the scope of this manuscript; we would like to obtain the general conversion factors to reflect the multi-year (please see the period of data given in table 2) average feature.

Comments: Why did you exclude the transoceanic variations of $c_{100,\mathrm{d}}$? I recommend adding a new panel to Figure 8 to show the variations of $c_{100,\,\mathrm{d}}$, and x_{d} and discussing them in Section 3.4 of the manuscript.

Response: Thank you for pointing out this issue. After careful checking, it fails to give the results of $c_{100,d}$ at the four sites (i.e., DK, CV, DU, and LA) before transoceanic transport. The regression coefficients χ_{d} are found to be far less than 0.5 for these sites. Thus, it should be mentioned that using our method to retrieve the CCN-relevant conversion factors seems not robust on the continent. Therefore, we would like not to add the CCN-relevant conversion factors in figure 8. Here we have added some sentences at the end of section 3.3 to discuss the possible problem when retrieving the CCN-relevant conversion factors on the continent as follows '**Moreover, it should be mentioned that using the newly-proposed dust dataset selection scheme to retrieve the CCN-relevant conversion factors seems not robust on the continent. Thus, more care should be taken when retrieving $c_{100,\mathrm{d}}$ and χ_{d} for those polluted city regions in future work.**' (Please see Line 343-345)

Comments: As highlighted in the introduction of the manuscript, the ultimate goal is to apply the conversion factors to spaceborne lidar measurements to estimate global 3D CCN or INP data. How do you suggest applying these new conversion factors to spaceborne lidar? Should it be applied based on the geographical location? Or, as suggested by Ansmann et al. (2019), global average conversion factors should be used?

Response: As mentioned in this manuscript, there will be a following-up work that focuses on the dust-related conversion factors at those polluted city sites and then the global conversion factor dataset can be expected. AERONET sites are serried in some regions (especially the regions with large populations) and can be very sparse in those outlying regions. Thus, the retrieved conversion factors absolutely will be distributed unevenly. Besides the possible use of the global average conversion factor value suggested by Ansmann et al. (2019), we will try to ensure that there are at least regional-representative conversion factors available for most of the geographical locations around the world. A dust-related conversion factor at an isolated site can be applied to a large area around it. Also, geographical interpolation is another possible way to obtain the final global grid dataset of dust-related conversion factors. Nevertheless, the final processing for retrieving the dust-related global conversion factors will be determined only after finishing the following-up work (i.e., conversion factors at polluted city sites), which would be better discussed in detail in our next paper. Here, we have added a sentence to preliminarily explain this issue in the last paragraph of the revised manuscript '**Once those conversion factors at polluted city sites are retrieved, a global dust-related conversion factor grid dataset will be obtained possibly by geographical interpolation.**' (Please see Line 397-398)

Comments: Figure 1 is missing some sites that are included in Table 2. Please modify.

Response: We have updated figure 1 in the revised manuscript to include nine near-desert sites for comparison with Ansmann et al. (2019).

Comments: Table 1 has an expression for n_{CCN} but lacks the expression for $n_{100,\mathrm{d}}$. Please add.

Response: An expression has been added in table 1 as below

$$\boldsymbol{n_{100,\mathrm{d}}(z) = \mathrm{c}_{100,\mathrm{d}} \times \alpha_{\mathrm{d}}(z)^{\chi_{\mathrm{d}}}}$$

Technical corrections:

Comments: Line 41. Please cite the latest IPCC report. And since you quoted the IPCC report, I believe you mean effective radiative forcing and not radiation budget.

Response: 'IPCC 2013' has been replaced by '**IPCC 2021**'. 'radiation budget' has been replaced by '**effective radiative forcing**'.

Comments: Line 60. A two-step dust separation technique for obtaining fine and coarse mode contributions separately, given by Mamouri and Ansmann (2014), has also been used in multiple studies and can be included here.

Response: We have added the following statement **'…as well as from fine and coarse mode components (Mamouri and Ansmann, 2014)'**

Comments: Line 74. Replace "regional-dependent and relevant to" with "regionally variable and dependent on".

Response: 'regional-dependent and relevant to' has been modified to '**regionally variable and dependent on**'.

Comments: Lines 81-82. The term "dust transport pathways" appears for the first time here without any prior explanation. I suggest explaining it briefly here.

Response: We have added a sentence to explain the long-range transport of dust as below **'Dust particles are frequently elevated from the surface of desert regions by wind or thermal convection and can sometimes undergo advective transport over a long range.'** (Please see Line 87-88)

Comments: Line 89. Do you mean "depolarization ratio"?

Response: Thank you for pointing out the mistake. 'polarization ratio' has been modified to '**depolarization ratio**'.

Comments: Lines 87-88. I suggest replacing "we plan to adopt another scheme to select data points that are representative of dust presence from AERONET databases" with "a different scheme to identify the presence of dust in AERONET measurements".

Response: According to the reviewer's suggestion, this sentence has been revised as '**…, we use a different scheme to identify the presence of dust in AERONET measurements.**'

Comments: Lines 101-102. Replace "a previous study" with "the results from Ansmann et al. (2019)".

Response: 'a previous study' has been modified to '**the results from Ansmann et al. (2019b)**'.

Comments: Line 138. Replace "AERONRT" with "AERONET".

Response: 'AERONRT' has been replaced by '**AERONET**'.

Comments: Rephrase lines 201-202.

Response: This sentence has been rewritten as **'The column-integrated dust ratio ($\boldsymbol{R}_{\mathbf{d,1020}}$), representing the contribution proportion of dust backscatter to the total particle backscatter in the atmospheric column, is defined as follows: …'** (Please see Line 219-220)

Comments: Line 208. The abbreviation FMF appears for the first time.

Response: 'fine-mode fraction' has been added before the first use of '**FMF**'.

Comments: Line 209. A flow chart of what?

Response: We have added '**for dust-occurring data point selection and dust-related conversion factors retrieval**'. (Please see Line 227-228)

Comments: Line 241. AERONET.

Response: 'AERONRT' has been replaced by '**AERONET**'.

Comments: Line 249. Remove "scatters regarding the".

Response: 'scatters regarding the' has been removed.

Comments: Line 250. There's no need to mention the colours here. This should be included in the figure caption. Replace "situations" with "cases".

Response: The colors have been moved to the caption of Figure 5 in the revised manuscript. ‘situations’ has been replaced by ‘**cases**’.

Comments: Line 253. Replace "participate" with "are considered".

Response: ‘participate’ has been modified to ‘**are considered**’.

Comments: There were many other obvious language-related errors that I have not included here. Some of them can be corrected during the manuscript's copyediting if it reaches that stage. However, I highly recommend the authors consult a professional language editor to improve the readability of the manuscript.

Response: Thank you for the review’s suggestion. For readability, the language of the revised manuscript has been polished by a professional English language editing service provided by ‘American Journal Experts’.

References:

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