Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., community comment CC1
https://doi.org/10.5194/acp-2022-39-CC1, 2022

Comment on acp-2022-39

Albert Ansmann

Community comment on "Wintertime Saharan dust transport towards the Caribbean: an airborne lidar case study during EUREC^{4}A" by Manuel Gutleben et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-39-CC1, 2022

Dear Manuel, Silke, Christian, and Martin! After having a discussion with Moritz Haarig, we decided to write a comment to your interesting paper! In the beginning I should not forget to say that the paper is well written, very intersting, and presents new insight into the long-range transport of polluted Saharan dust across the tropical Atlantic.

Now to our two points:

First point: By reading the introduction (lines 30-36), the reader may get the impression, the authors introduce a new aspect: Winter transport of polluted dust over the remote tropical Atlantic towards, e.g., Barbados or even South America (Amazonia). But this impression needs to be avoided. We at TROPOS (partly together with Munich University, Wiegner, Gross, Freudenthaler) did so much work already in this field (since the SAMUM 2008 and later on in the framework the SALTRACE activities in 2013-2014) that needs to be mentioned.

Ansmann et al., Dust and smoke transport from Africa to South America: lidar profiling over Cape Verde and the Amazon rainforest, Geophys. Res. Lett., 36, L11802, doi:10.1029/2009GL037923, 2009

Baars et al., Further evidence for significant smoke transport from Africa to Amazonia, Geophys. Res. Lett., 382, L20802, doi:10.1029/2011GL049200, 2011.

Rittmeister et al., Profiling of Saharan dust from the Caribbean to western Africa – Part 1: Layering structures and optical properties from shipborne polarization/Raman lidar observations, Atmos. Chem. Phys., 17, doi.org/10.5194/acp-17-12963-2017, 2017.

Ansmann et al., Profiling of Saharan dust from the Caribbean to western Africa – Part 2: Shipborne lidar measurements versus forecasts, Atmos. Chem. Phys., 17, 14987–15006, https://doi.org/10.5194/acp-17-14987-2017, 2017.

Haarig et al., ACP, 2019 (in the references)

Haarig et al., ACP, 2017, https://doi.org/10.5194/acp-17-14199-2017 on dry sea salt depolarization should also be mentioned as a source for uncertainties in the depolarization observations close to Barbados.

You may now realize why I personally was motivated to write this comment!

Second point: We have a severe problem with the PURE SMOKE particle linear depolarization ratio (PLDR) of 0.14 at 532nm in the troposphere! This has never been observed, except for the upper dry troposphere (for cases in which the smoke particles were unable to age quickly…, so that the irregular, fractal-like structures remained for a long time and caused enhanced PLDR values of up to 0.2, Burton et al.). However, in the lower and middle troposphere such enhanced PLDR values for pure smoke have never been observed. Extreme values may be here, 0.07 (Falcon observations during LACE98, and Falcon observation presented by Dahlkoetter et al., 2014, in the upper troposphere). But usually the smoke PLDR values are <0.05. This is the reason that one is able to properly separate smoke and dust contributions to lidar backscatter coefficients (Tesche et al., JGR2009, Tesche et al., Tellus2011). It is general accepted that aged biomass burning smoke particles at heights in the lower to middle troposphere cause PLDRs of <0.05. See Haarig et al., ACP 2018, on smoke in the troposphere and stratosphere...

As long as you cannot demonstrate by lidar observations (or by a proper reference) that the PURE smoke PLDR is about 0.14 one should avoid to mention that. To our opinion, such a statement is not acceptable and even 'dangerous' because lidar scientists may use that in follow-on papers!

All in all: Nice work!