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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Saharan dust and biomass burning aerosols during ex-hurricane Ophelia: validation of the new UK lidar and sun-photometer network" *by* Martin Osborne et al.

Anonymous Referee #1

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As the title indicates, the topic of the above-mentioned paper is to show the capabilities and the readiness of the UK lidar and sun photometer network. For this purpose, an interesting case related to an ex-hurricane bringing smoke and dust to the UK is presented.

After having read the first version of the paper, I was a bit disappointed that the extraordinary conditions during this event haven't been more emphasized and its influence on atmosphere (weather, radiation) was not discussed. After having read now the discussion version, I realized that the focus of the paper is to present the network with its capabilities and therefore atmospheric conditions with respect to Ophelia has taken a

back seat. For this reason, I think and suggest that the paper is much more suited for AMT than for ACP. The Editor should thus think about consider moving the paper to AMT.

In general, the paper is well written, and the methodologies are explained in detail. I have some major points of criticism, listed below, with respect to the lidar-retrieved aerosol optical properties. After addressing these points, the paper could be considered for publication in one of the above-mentioned journals.

General remarks:

-In my opinion, the reported lidar ratios at 355 nm for dust and smoke seem to be too low. If I see previous literature, e.g., Fig 9 in Tesche et al., 2011, Fig 12 and 13 in Groß et al, 2015a, or Fig. 9 in Illingworth et al, 2015 (which includes part of the afore mentioned data), lidar ratios for dust of the western Sahara and smoke should always be higher than 50 sr. In turn, the particle depolarization should be not higher than 30 % (mainly not higher than 27%) in the UV. I thus have the feeling the reported particle depolarization ratios are a little too high.

I am therefore wondering if the authors have considered all possible instrumental effects that could affect their data analysis. E.g., polarization dependent transmission (e.g. Mattis et al. 2009) can lead to over/under estimation of the particle backscatter and thus influence the lidar ratio and particle depolarization. Furthermore, also after successful depolarization calibration with respect to Freudenthaler, 2009 (volume depolarization), cross talk errors can occur and can influence the particle depolarization. In the recent paper (Freudenthaler, 2016) more detail is given. As first easy check, one could test if the volume depolarization ratio approaches the theoretical molecular value in particle free regions. Multiple scattering correction might be also needed in dense dust plumes. For the above mentioned reasons, I would encourage to re-check the results with respect to the lidar ratio and particle depolarization retrievals and give a statement on this issue.

-Another major point of criticism is that the aerosol classification based on the lidar data (Sec. 4.4) is performed too subjective without giving evidence. One has the feeling it is solely based on the particle depolarization and not on lidar ratio.

The authors state: “The LR, together with the PDR have been used to attempt a classification of the aerosols based on a classification scheme such as that provided in Groß et al. (2015b) figure 2”. Please argue how you defined your typing and why. Statements like “are consistent with a mixture of marine and dust aerosols” without providing facts are not scientifically convincing. For example, to my knowledge, a measured lidar ratio at 355 nm of 40 sr does not allow to conclude for pure Saharan dust. Please re-check and also give references for your classification.

-Please discuss more intensively the microphysical retrievals of the sun photometers and their respective uncertainties and draw conclusions for your calculations. I would like to see uncertainty estimations for the mass concentrations and the other values given in Table 2.

Specific comments:

All abbreviations need to be explained when used the first time, i.e. also AERONET EARLINET, MPLNET etc...

-2, l9: Please use updated citation (2014) for EARLINET

-2, l19: I suggest to concentrate on one citation for MOCCA as it is not needed for the paper.

-2, l22: exchange “quantity” with mass concentration

-3, l23: “Alignment is ensured using the telecover test described in Freudenthaler et al. (2018).” By doing so, you should also acknowledge ACTRIS, EARLINET etc. . .

-3. L27: as detailed DESCRIBED in . . .

-3, l27: Reference Buxmann et al. not available in web.

-4, l7: What is shot noise? I suppose you mean the strong daylight background within the Raman channels

-5, l1-2: I do not understand this sentence, please rephrase.

-6,l8_ Please delete space within “warm”

-6,l29: Please do not use the term aerosol cloud. Replace by e.g. aerosol plume.

-7.l11: “..wedge shape profile..” Profile of what. Please write more specific.

-7, l14-15: Please refer to the plot of the range-corrected signal here and draw fronts there similar to the depolarization figure. The optically thick aerosol layer is hardly seen in the range-corrected signal, but still visible, but you might consider to correct for molecular attenuation to have more clear temporal evolution of the layering.

-7,l17-18: The thin layer at top of PBL: Could this be dried marine aerosol, like described in e.g. Bohlmann, 2018, ACP, Fig. 4?

-7,l21: “not shown here”: you included the figure, but do not refer to it. . .

-7,l21: “The boundary layer was mostly confined to the lower 1km, rising sightly to 2km after the cold front had passed” : If you would shape up the presentation of the temporal evolution of your elastic channel one could nicely see this feature...

-8,l34: Aeronet forces the size distribution to be zero at 15 mum. Probably SkyNET not. Please discuss this more intensively and also draw conclusions with respect to your research.

8, l10: Please motivate again why you analyze the specific extinction and why it is so important for your paper.

9,l16: 2 times indicating

9,l23: What does “backscatter weighted“ mean? I do not understand this.

9, l30. I cannot agree on the conclusion of the aerosol type with the given reference.

Why not a marine mixture with smoke?

9, l14: “we identify this layer as a mixture of biomass burning aerosols and transported desert dust” But the PDR does not prove confirm this classification, right?

11, l28: “. . .those reported in the literature for transported Saharan dust” Really? Can you provide reference for that, also for the smoke?

13, l24: Buxmann et al, is not findable online, but is essential to proof the high quality depolarization measurements.

18, Caption: Met Office forecast or analysis?

19, Fig. 3: Even though it might be obvious, I suggest to draw in the fronts overlaid to the Modis image

20, Fig 4: A simple map indicating the 4 locations would be great here!

24, Fig 8: X-axis labels are missing

25, Fig. 9: I see a substantial offset between the aerosol layer and the PDR (PDR maximum below aerosol layer). Can you explain this? This is also seen in Fig 10.

Could you also provide averaged lidar profiles (like Fig 8 to 10) for the initial cold sector in Watnall, i.e. on 15 Oct?

References:

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