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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Saharan dust intrusions over the northern Mediterranean region in the frame of EARLINET (2014–2017): Properties and impact in radiative forcing" *by* Ourania Soupiona et al.

Anonymous Referee #2

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The authors present a study using a combination of ground-based aerosol profiling and models. Statistics of lidar and depolarization ratio values, aerosol optical thicknesses, Ångström exponents, and geometrical properties of (mixed) Saharan dust layers over western, central, and eastern Mediterranean are reported. As the main result, the lidar measurements of the dust layers were used to calculate their radiative forcing with a radiative transfer model. The results were partly validated with ground-based radiation measurements. Additionally, using a conversion technique, the measured optical properties were used to retrieve microphysical properties (dust mass concentration

profiles), which were compared with a dust model.

The manuscript covers an important aspect of aerosol remote sensing with lidar, as the authors correctly state, namely using type separated aerosol profiles in e.g., dust forecast as well as radiative transfer models.

Nevertheless, I have some concerns which should be addressed before publishing (see comments below and in the attached pdf).

Major comments:

1. The authors state, that they estimate the dust radiative forcing based on 51 selected cases with respect to a clear sky background. But in fact, they calculate the aerosol radiative forcing of dusty aerosol layers- These aerosol layers are partly pure dust cases, but also dust cases mixed with other aerosols. Thus it cannot be claimed that the dust radiative forcing is calculated. As the authors use well-known techniques to separate the dust contribution in the observed lidar profiles, it would have been interesting and very innovative, to discuss if this dust-only profiles have could have also been used to really determine the dust radiative forcing. Then one could also discuss the contribution of the other aerosol types mixed with the Saharan dust, i.e. the non-dust radiative forcing.

2. Concerning the reported radiative forcing effects of the different schemes applied, there is too much simple reporting of values instead of a real discussion. So please address the following questions

2a. What conclusion do you draw from your three used schemes? The first conclusions is, that the Scheme A underestimated the mass concentration and thus radiative forcing. But what do we learn from the comparison of Scheme B and C?

2b. With respect to the evaluation with ground based radiation sensors: Is scheme B more correct than scheme C? Or the other way around, or no nothing of the both? Why did you use these three schemes?

2c. Are the layer geometrical properties from the model (Scheme A) equal to that of the lidar or have there been significant differences in the layer heights and extents also influencing the radiative forcing? Please discuss

2d. Concluding: You present the results from the 3 schemes intensively, but a proper discussion is missing. What is the most appropriate scheme, what are the weaknesses and strengths of the schemes and what is your recommendation for future research?

3. The language is partly sloppy, i.e. not clearly scientific, please see comments attached and check for the whole manuscript. I.e. put special emphasis when writing about dust, if you refer to dust-only properties or to aerosol layers containg dust.

4. Sometimes proper references are missing, see pdf.

5. Also the explanations are sometimes unclear and a rephrasing /extension is needed. See pdf as well. E.g. for the SphInX software tool

6. Minor, mainly textual suggestions, can be found in the attached pdf.

Considering the amount of minor comments, major revisions are needed. But I am confident that the authors can address all issues!

Specific comments:

1. Page 5, line 178:

"For LRd the mean values of 52 $\pm$ 8 sr, 51 $\pm$ 9, 52 $\pm$ 9 sr and 49 $\pm$ 6 sr were used per site, respectively as calculated from our findings."

So you used these lidar ratios as the pure dust lidar ratios in the conversion from optical to microphysical properties. Furthermore, you say you calculated them from your findings. I do not understand how. They are not a result of this conversion, they are an input parameter. Based on which criterion were they calculated? Are these the averages of only those layers having a large particle linear depolarization ratio of >0.31 at 532 nm wavelength? How many cases of such layers occurred and were analyzed?

Or are these the averages of all your cases? I suggest to describe this in more detail and make a reference to Sect. 4. There you present average values, but there you describe also mixed dust (dusty) cases, based on the criterion introduced on page 4, line 125.

2. Page 8, line 277:

"Before using the aerosol mass concentrations vertical profiles…"

Do you calculate and compare the whole aerosol mass concentration or only the mass concentration of the dust fraction? If it is the latter, you have to state that correctly. If it is the first, then you have to describe the calculation (list the conversion parameters) for the non-dust fraction, especially the used non-dust lidar ratio. In dusty layers with a particle linear depolarization ratio of 0.16 at 532 nm wavelength (the lower boundary of the selection criterion), a significant amount of non-dust mass can be expected.

3. A more detailed explanation of Fig. 3 is needed, in Caption and text.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-611/acp-2020-611-RC2-supplement.pdf

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