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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative signature of absorbing aerosol over the Eastern Mediterranean Basin" *by* A. K. Mishra et al.

Anonymous Referee #1

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Review of "Radiative signature of absorbing aerosol over the Eastern Mediterranean Basin" by Mishra *et al.*, submitted to Atmos. Chem. Phys.

In this study, the authors characterise the optical properties of aerosols over the Eastern Mediterranean using ground-based and satellite remote-sensing retrievals. Those optical properties are then used in radiative transfer calculations to estimate the direct radiative effect and changes to heating rates exerted by the aerosols. The results are interesting (Figure 10 particularly) and the region is well chosen for his mixture of aerosols with varying absorption properties. The scope is disappointing, however, and the authors could have easily extended their analysis over the full period, rather than just June–August 2010. Contrasting the results with the same analysis, but applied to Western Mediterranean aerosols, would also have been most welcome.

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Nevertheless, my opinion is that the study is worth publishing, provided that the comments below are addressed satisfactorily. In particular, the choice of a single solar zenith angle, and the use of AERONET-retrieved single-scattering albedo, can both lead to an overestimate of absorption and heating rates.

1 Main comments

- Reading the paper, one has the feeling that MODIS could have been put to more use. There are almost two independent analyses in the paper: one with MODIS (and MISR on occasion), which gives an idea of seasonality and inter-annual variability before providing an interesting correlation with AIRS-retrieved temperature profiles. The second with AERONET and CALIOP, which provides the inputs to the radiative transfer calculations. Those two strands could be better coupled. Why not combine MODIS AODs and CALIOP vertical profiles, to give a really three-dimensional view of the situation? Why not use the estimated heating rates to analyse the results from Figure 10 more deeply?

- Section 2, page 2410, line 3: Using a single, daytime-average value of the solar zenith angle neglects the strong dependence of aerosol direct radiative effects (DREs) on solar zenith angle (see Figure 5 of Boucher *et al.*, 1998). Nowadays, a proper integration of instantaneous DRE calculated at regular intervals throughout the day is easy to do, and prevents DRE from being overestimated by the choice of a zenith angle that is close to the maximum instantaneous DRE. This also allows a proper account for seasonality of DRE, which is not only driven by total insolation (as implied by the authors on Page 2411, line 6), but also by different distributions of solar zenith angle.

- Table 2: The AERONET single-scattering albedo and size distribution are only retrieved in specific conditions. One particular requirement is that the AOD is

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large enough (typically larger than 0.2, see section 2a of Dubovik et al., 2002). Level 2 data should take care of that problem (although it is worth checking), but that does mean that retrievals are only representative of thicker aerosol plumes. If those plumes are associated with more absorbing aerosols (mineral dust or large pollution events), then the authors use a single-scattering albedo that is biased low, and overestimate the atmospheric absorption and heating rates.

- Section 3.3, page 2414, lines 10–18 and Figure 9: The discussion of Figure 9 is unclear. What exactly constitutes the "close agreement" claimed by the authors? Are MODIS fine-mode fractions compared to the full CALIOP cross-section, or only the parts highlighted by the black circles? This is important because, if I understand the paper correctly (but clarify the text if I'm wrong), CALIOP's classification is used directly to obtain the profiles shown on Figure 8b.

2 Other comments

Abstract, page 2404, line 4: "model calculations" –> "radiative transfer model calculations"

Figure 3: It would be useful to take the standard deviations (denoting variability) from Table S1 and show them as whiskers on the Figure. That would provide an efficient graphical summary of the AERONET dataset.

Introduction, page 2405, line 16: "temperature increase": surface temperature?

Section 2, page 2410, line 9: Clearly state that radiative transfer calculations cover both the shortwave and longwave spectra. For longwave calculations, how is the temperature of the aerosol layer given? Is it that of the corresponding standard atmosphere level, or does it include any absorption-driven warming of the aerosol layer? Also, the text should note that radiative transfer calculations are done in cloud-free sky.

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Figure 4: Clearly state what the standard deviation measures: interannual variability? Spatial variability? Retrieval uncertainty?

Section 3.3, page 2415, line 12: “no statistically significant changes at 1000 hPa”. Does that mean that temperature changes are statistically significant at the lower pressure levels? At which confidence level?

Section 3.3, page 2416, lines 7–8: “Dust particles are large enough (up to several micrometer) to be comparable to IR wavelength” is an awkward thing to say. I would suggest “Because of their large sizes (up to several micrometers), dust particles are able to strongly interact with IR radiation.”

Section 3.3, page 2416, lines 19–20: “plausible reason have been explained in the Supplement”. The explanation is short enough to be reproduced in the main text, which would remove the need to consult the caption of Figure S4.

Section 3.3, page 2417, line 12: The relative strength of uncertainties merits a discussion. Are some uncertainties weaker because of compensating errors between AOD and SSA?

Conclusion, page 2421, lines 10–14: This paragraph is over-enthusiastic. The “detailed assessment” is in fact the speculative, two-paragraph section 4. The wording should be toned down here. The authors should wait for the results of their EMAC study before making such claims.

3 Technical comments

Introduction, page 2405, line 8: “budget by both directly” –> budget both directly

Section 2, page 2408, line 3: “is board on” –> is on-board

Section 2, page 2410, line 1: Typo: McComiskey

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Please improve the quality of Figures 5, S1, S3, S4, and S5 by plotting thicker lines.

Typo in Figure S2: "Uncertainty"

Section 3, page 2410, line 25: “means” –> mean

Section 3, page 2411, line 17: “the North Africa” –> North Africa

Section 3, page 2411, line 19: Sede Boker

Section 3, page 2412, line 10: “and mixes” –> that mix

Section 3.2, page 2413, line 7: “increase” –> increases

Section 3.4, page 2417, line 11: “rage” –> range

4 References

Boucher, O., *et al.*, Intercomparison of models representing direct shortwave radiative forcing by sulfate aerosols. *J. Geophys. Res.*, 103, D14, 16979–16998, 1998.

Dubovik, O., *et al.*, Variability of absorption and optical properties of key aerosol types observed in worldwide locations. *J. Atmos. Sci.*, 59, 590–608, 2002.

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