

Interactive comment on “Aerosol shortwave daily radiative effect and forcing based on MODIS Level 2 data in the Eastern Mediterranean (Crete)” by N. Benas et al.

Anonymous Referee #3

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1) The criteria used by the authors for the identification of days affected by the presence of African dust or by dominance of anthropogenic particles were based on specific thresholds (for example $AOT > \text{mean}(AOT) + 1SD$ together with $\hat{a} < \text{mean}(\hat{a}) - 1SD$ for African dust or $\text{fine}(AOT)/AOT > 0.7$ for anthropogenic aerosols.) When anthropogenic aerosols were detected the parameters g and ω (taken from MODIS and AERONET respectively) were regarded as characteristic of the fine mode. Hysplit was used to confirm the presence of air masses coming from Africa. This is a good methodology for understanding (over a big database) when ambient aerosols were characterized by the presence of coarse (African) or fine (anthropogenic) mode aerosols. However, the mean ($\pm SD$) values of these “parameters” (\hat{a} , g and ω together with mean AOT) should

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be reported for both African aerosols and anthropogenic aerosols and discussed. For example it could be useful to compare the α , g and ω values used in the manuscript with previously published values for the same parameters. For example: what is the mean value (+/- SD) of the α exponent (and AOT) for the days detected as “African”? Is the Angstrom exponent around 1? And for anthropogenic aerosols? ...and so on. Moreover, it could be also useful if the authors could provide the values of DRE as a function of the α exponent when African aerosols and anthropogenic aerosols were present. Did the authors find any correlation between these variables? For example absolute DRE higher for lower α under African outbreaks?...and so on. Finally, as for the African aerosols, the authors should use Hysplit to detect clear East Europe pollution episodes in order to characterize them as well. Thus, the authors should study not only the presence of anthropogenic aerosols in general (which may be of more local origin), but they should also characterize well-defined European episodes. For example, how much these episodes contribute to the AOT and DRE compared with African episodes?

2) In my opinion Section 2 is incomplete. More details (including equations) should be added to this section. How many vertical levels have been considered in the proposed model? Did the authors include the light-absorbing carbon particles as well?

3) Paragraph 3.3: Please, clarify how the cloud physical thickness was calculated.

4) Paragraph 3.4. Why is the Angstrom exponent not included in this section?

5) results: Paragraph 4.1. I do not understand the need of a MODIS AOT “validation” by using the AERONET data.

6) Paragraph 4.3. Is the aerosol indirect effect included here? Moreover, the author should add references to previously published papers dealing with the radiative effect of aerosols (also on different regions, not only Mediterranean) and compare the results reported in Figure 9 and 10 (for example) with the bibliography. In my opinion the positive radiative effect of aerosols within the atmosphere is too high. The bibliography

is very important here. Moreover, explain in more detail why the aerosol radiative effect of aerosols is positive within the atmosphere.

7) In general the authors should add more bibliography dealing with the same topic. Previous modelled results should be reported in the manuscript and the results compared.

Specific comments:

a) Pag. 19882, Line 21: ...”corresponding daily peak values...”. Do the authors mean monthly? b) Pag. 19883, Line 11-13. Please, add that also the aerosol chemical composition (and not only aerosol mass) is highly variable and that aerosols differ from GHGs not only for the different lifetimes but also because of the huge number of aerosol sources (and modification processes changing aerosol properties). c) Pag. 19888, Lines 15-17: Which is the fine mode fraction from MODIS? d) How did the authors calculate the water vapour content? Which formula. e) Pag. 19890, Line 26: “. . .as well scattering optical depth”. Is this scattering optical depth the one given by MODIS? f) Pag. 19891, Line 25. Please add the 870 nm wavelength. g) Pag. 19893, Line 20. I do not understand how the bias (-17 Wm⁻²) was calculated. h) Pag. 19896, Line 12-15. Please, report the numbers of the cited experimental results.

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