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Interactive Comment

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.

N. Benas et al.

benas@physics.uoc.gr

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We would like to thank the Referee for the suggestions and corrections. All comments and recommendations have been taken into account. Please find our point-by-point reply below.

GENERAL COMMENT: Based on the comments regarding the validity and reliability of the results, which were evaluated on a mean daily basis using MODIS data acquired at a specific time of the day, we have repeated our calculations for the specific satellite overpass times, instead of the mean daily. All results reported in the revised manuscript are based on these new calculations.

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 > mean(AOT) + 1SD together with å < mean(å) – 1SD for African dust or 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" (å , g and ω together with mean AOT) should be reported for both African aerosols and anthropogenic aerosols and discussed. For example it could be useful to compare the å , 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 å 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 å 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 å 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?

Mean values and standard deviations of all aerosol parameters (AOT, Angstrom exponent, g and ω) during dust and anthropogenic events have been calculated and are reported and discussed in Sections 4.3 (Page 14, lines 23-32) and 4.5 respectively (Page 17, lines 8-17). Regarding the DRE of dust and anthropogenic aerosols, no

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correlation was found with the Angstrom exponent. Back-trajectory analysis has been performed for anthropogenic aerosols, and the results are discussed in Section 4.5.

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?

A more detailed description of the layers considered in the model was added in Section 2 (Page 5, lines 3-8). Carbon particles are not treated separately by the model. All aerosol particles are considered using their optical properties provided by MODIS Level 2 data.

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

The cloud physical thickness was taken from Peng et al. (1982), who give the parameter for different cloud types, depending on latitude. This is clarified in Section 2 of the revised manuscript (Page 9, line 3-4).

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

Section 3.4 describes the aerosol data which are used as input to the model. The Angstrom exponent is only used for the detection of dust events from the timeseries of the output data.

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

In Section 4.1 of the revised manuscript, MODIS aerosol input data are validated against the FORTH-CRETE AERONET station and the model sensitivity is examined against variations in these data. The aim of both procedures is to assure the validity and reliability of the model input and output results.

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

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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.

Our study examines only the direct effect of aerosols. References and comparisons with other studies have been added in Section 4.3. Aerosols increase multiple scattering within the atmosphere and together with their own absorption increase the probability of photon absorption within the atmosphere, leading to a warming effect. This point is clarified in Page 13, lines 30-32 of the revised manuscript.

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.

More bibliography and comparisons have been added in Section 4.3.

Specific comments:

a) Pag. 19882, Line 21:. . .”corresponding daily peak values”. . .. Do the authors mean monthly?

The word “daily” has been replaced by the word “monthly” (Page 2, line 1).

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).

The comment has been added (Page 2, lines 16-17).

c) Pag.19888, Lines 15-17: Which is the fine mode fraction from MODIS?

The aerosol fine mode fraction was computed by dividing the fine mode AOT at 0.55

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μm by the total AOT at the same wavelength. The statement in Page 19888, Lines 15-17 has been rephrased (Page 7, line 6).

d) How did the authors calculate the water vapour content? Which formula.

The procedure used for the determination of the water vapor content is described in Section 3.1 of the revised manuscript (Page 8, lines 3-9).

e) Pag. 19890, Line 26:“. . .as well scattering optical depth”. Is this scattering optical depth the one given by MODIS?

The scattering optical depth referred in Page 19890, line 26, regards the near-infrared region and was calculated using Mie computations, based on the scattering optical depth value provided by MODIS.

f) Pag. 19891, Line 25. Please add the 870 nm wavelength.

The 870 nm wavelength has been added (Page 10, line 23).

g) Pag. 19893, Line 20. I do not understand how the bias (-17 Wm-2) was calculated.

The bias was calculated by subtracting the mean value of the model output from the mean value of the station measurements.

h) Pag. 19896, Line 12-15. Please, report the numbers of the cited experimental results.

The cited experimental results have been removed, because they cannot be compared with the results of the revised manuscript (monthly means from instantaneous values, instead of daily means).

References

Peng, L. I., Chou, M.-D., and Arking, A.: Climate studies with a multi-layer energy balance model, Part I: Model description and sensitivity to the solar constant, J. Atmos. Sci., 39, 2639–2656, 1982.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 19881, 2011.

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