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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol direct radiative forcing during Sahara dust intrusions in the central Mediterranean" *by* M. R. Perrone et al.

Anonymous Referee #1

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General Comments

This is a paper in a series on aerosol effects on solar and infrared radiation in the Central Mediterranean. In this paper, the authors attempt to determine the direct effect of aerosols (DRE) on solar, infrared (IR) and net-allwave (Net) radiation at Lecce-Italy during dust outbreaks from Sahara. The paper shows that during such events the aerosol effect is due not only to (natural) dust but also to anthropogenic aerosols. The contribution of both is quantified at the surface and also at the top of atmosphere (TOA). The most interesting contribution of the paper is the separation between natural and anthropogenic aerosol optical depth (AOD), and mainly of aerosol DREs. Generally, the work is good and worthy of publication, based on the solar direct radiative effects of aerosols for the specific location of the study. I do have some serious reservations

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concerning the thermal infrared calculations. These I discuss below, along with other comments that I hope will help to improve the paper.

Specific Comments

1. For the infrared region the model uses refractive indices for tropospheric aerosols taken from the book of Paltridge and Platt 1976. These properties would certainly not be representative of the actual study site. Have the authors looked into more recent measurements or estimates? 2. The model uses AFGL standard atmosphere vertical profiles for temperature and water vapour, parameters that play a crucial role in determining atmospheric infrared radiation. These atmospheric properties are highly variable in space and time. How reliable are the infrared calculations given this fact. 3. Table 2 gives about 10 Wm-2 for the DRE on net infrared flux at the surface and about 2 Wm-2 at TOA. I presume that this is extra thermal emission by the aerosol layer? If so, it would depend on the emissivity of the layer, which of course depends on the data from Paltridge and Platt. 4. The simulated net all-wave and solar fluxes shown in Fig. 10a & b, show that the solar (10b) is well modelled, but the all-wave shows a lot of scatter, which presumably is the scatter in the infrared surface net flux. Why not plot the net infrared flux separately? Given that net infrared flux at the surface is small, about 50 Wm-2 , the uncertainties in the infrared flux are masked by adding them to the larger solar component. 5. The computation of aerosol radiative effects, and also of the radiation fluxes themselves, is done using a two-stream radiative transfer model. Probably, this is not the most adequate tool for aerosol radiative effect studies. 6. The applied methodology to identify dust events during the period 2003-2006 is not explained. Although it is not the primary subject of this study, it should be done. Moreover, the applied methodology may be problematic, since it results in aerosol Angstrom values as high as 1.5 (e.g. Fig. 5b) which are certainly not indicative of coarse dust aerosols. Probable problems with the methodology could affect the magnitude of computed aerosol AOD and DREs. 7. Section 3 seems to be unreasonably placed before section 4. It refers to a unique dust event in 22 June 2006, and it is less important than

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sect. 4, which refers to the total number of identified dust events. The importance of sect. 3 probably consists in explaining the methodology applied on every event, but if this is the case, then sect. 3 should be renamed pointing to the Methodology. On the contrary, the use of term "methodology" in the name of sect. 4 should be avoided.

8. The most interesting contribution of the paper is the separation between natural and anthropogenic aerosol optical depth (AOD), and mainly of aerosol DREs. Formulas are derived and given, which are claimed to be representative for the Mediterranean. However, the possibility of the more generalized application of these formulas to other sites as well, should be assessed and discussed.

Other specific comments

Abstract

1. Page 22540, Line 11: Replace "to intialize radiative transfer simulations . . ." by "to perform radiative transfer simulations . . .".

2. Page 22540, Line 13: The range of values of AOD "0.2-0.7" has an upper limit (0.7) which seems to be somewhat low for dust outbreaks in the Mediterranean according to the existing literature. This is likely due to the use of AERONET AOD data. During dust events, the ground sunphotometers become saturated so that larger AOD values are missing from timeseries. On the contrary, corresponding satellite-based estimates (e.g. MODIS-based) report quite larger values. Thus, the reported here AOD, but also DRE values are affected and bounded. Even larger values are possible.

1. Introduction

1. Page 22542, First paragraph: Why the range of values of aerosol DRE at TOA given by Haywood et al. (2003) and Meloni et al. (2003) are so much different? (-44 to -129.2 W m-2 against -1.2 to -6.2 W m-2). There is difference by an order of magnitude.

2. Page 22542, third paragraph: Why so much emphasis is given to a specific aerosol event (22 June 2006)? An entire section (sect. 3) is devoted. This has to be explained

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in the Introduction.

2. The two-stream radiative transfer model and input data

1. Page 22542, Lines 25-27: How accurate is the use of 2-stream models for computing aerosol radiative effects? More sophisticated models are a better choise (see general comment).

2. Page 22543, Lines 5-6: Are the numbers given the centers of the spectral bands? It should be specified. Also, 8 solar and 20 infrared bands seem to be unbalanced in terms of aerosol optical properties and forcings. A larger number of bands in the solar is more suitable, since aerosol properties and effects are highly variable in the solar, and especially in the ultraviolet-visible wavelengths.

3. Page 22543, Lines 8-9: "... the optical properties ... of the 20 subbands": do they remain or are they set constant, and why?

4. Page 22543, Lines 16-17: "... Sahara dust intrusion ... to 2006 year": how these events have been identified over the study period? It should be specified. What are the criteria that have been applied in order to derive the 26 dust events listed in Table 1?

5. Page 22543, Lines 28-29: "... are averaged ... spectral range": How are they averaged? Why to average over 0.3-0.7μm? It is reported above that there are 4 solar subbands around 0.35, 0.45, 0.55 and 0.65 μm. This is inconsistent with the band (0.3-0.7) reported here. Do you assume constant surface albedo values over the entire solar range of wavelengths?

6. Page 22544, Line 1: "... are averaged ...": similarly to the previous comment.

7. Page 22544, end of sect. 2: What about clouds? It should be specified that aerosol DREs are computed under clear-sky conditions.

3. Dust outbreak of 22 June 2006 and aerosol DREs

1. Page 22544, sect. 3: Although references are given, a few sentences about the

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methodology will be helpful to the readers of this study.

2. Page 22545, sect. 3.1: The introduction of this section here seems unreasonable or at least not justified. The aim of the paper is to evaluate aerosol DREs at Lecce over the period 2003-2006. Why focus on a specific dust event and study it separately?

3. Page 22546, Lines 28-29: Similar bi-modal structures have been reported for other Mediterranean sites as well (e.g. Fotiadi et al., ACP, 2006).

4. Page 22547, Lines 1-2: "0.87 ... over Lecce. ...": The coarse mode in the bi-modal distribution could be also attributed (at least to some extent) to maritime sea-salt aerosols (see e.g. Fotiadi et al., 2006). This is also supported by the back-trajectories (Fig. 1).

5. Page 22547, Lines 13-15: The results of Table 2 are discussed later on. They should be discussed here. Also, how does one explain that the surface DRE decreases in magnitude from 15:31 to 16:27, while it increases at TOA?

6. Page 22547, Lines 22-25: Comparison-validation for two points only is a problem.

7. Page 22548, Line 6: It would be interesting to examine the role of the IR DRE during night.

8. Page 22549, Lines 2-3: "... A discussion ... Bergamo et al. (2008a)": Is this assumption ivalid, since we generally know that the refractive index depends on wavelength?

4. Methodology and results of 2003-2006 dust outbreaks

1. Page 22549: see general comment about the name of this section.

2. Page 22549, Line 19: "The dust events that have been selected ...": Again, how was this achieved?

3. Page 22549, Lines 22-25: "... Analytical back trajectories ... previous paragraph":

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Authors must be more specific.

4. Page 22550, Line 8: “black line in Fig. 5a . . .”: the curves cannot be distinguished between themselves. Symbols or different colors could be used together with lines.

5. Page 22550, Line 14: “. . . However, along with . . . aerosol burden”: To what extent? It can be computed.

6. Page 22550, Line 16: “plotted in Fig. 5b.”: it would be useful to relate the points in Fig. 5b to the curves of Fig. 5a, at least, the points corresponding to fine aerosols in Fig. 5b (black circles and red rectangles).

7. Page 22550, Line 18: “. . . span the 1.5-0.23 range. . . .”: dusty days with Angstrom values as high as those shown in Fig. 5b are hard to believe. Probably, this reflects the problem with the selection procedure of dusty days.

8. Page 22551, Line 1: “. . . of n and k is: <n>=1.48ïĆś0.01”: values of <n> could be also given separately for fine and coarse aerosols.

9. Page 22551, Line 11: “Instantaneous AODt, SSAt, and gt values . . .”: how do these properties compare with those directly given by AERONET?

10. Page 22552, Line 9: “. . . of the AOD fraction used . . .” (also in caption of Fig. 9): explain clearly what is AOD fraction.

11. Page 22552, Line 12: replace “lidar on 24 June 2003 . . .” by “lidar on 24 July 2003 . . .”.

12. Page 22553, Line 5: According to the two Figures, the differences in the net flux between model and measurements (Fig. 10a) are due to the IR fluxes. Differences as much as 50 W m-2 can be seen.

13. Page 22553, Line 12: replace “increase . . .” by “decrease in magnitude . . .”. Attention should be made to avoid confusion, since the values are negative.

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14. Page 22553, Line 19: replace ". . . smaller than that . . ." by ". . . larger than that . . .". It is better to talk about magnitude.

15. Page 22553, Lines 20-27: "The solar . . . on monitoring time": It is better to remove Figure 12a (it does not add much to the information already given) and incorporate Fig. 12b into Fig. 11. Also, the IR and net DREs are not discussed at all.

16. Page 22554, Lines 1-10: The entire discussion of Fig. 13 (dependence on solar zenith angle, z) has a problem and merits further discussion. It s well known that AOD (and hence DRE) depends on the atmospheric optical mass, i.e. solar zenith angle. However, other factors like the suspended amount of particulate matter and the associated scattering and absorption properties are also responsible for AOD and DREs, so they have to be discussed in order to explain the features shown in Fig. 13. For example, note the changing dependence of AOD and DREs on z in some cases (e.g. solar DREt,TOA in 22 June 2006).

17. Page 22554, Lines 15-16: "The variability range . . . of large dust particles": Here it is claimed that n does not affect significantly AFE, opposite to what happens with DRE. An explanation for this should be given.

18. Page 22554, Lines 17-18: "We observe that . . . at the surface": similar to the previous comment.

19. Page 22554, Lines 20-22: "It is worth noting . . . ToA and surface.": A reason for this should be given. What are the differences between the two studies?

20. Page 22554, Lines 23-29: "Figure 11b,e . . . dust intrusion events": this discussion should be moved to the previous page (22543). Concerning the percentage 47%, what are the cases (conditions) in which the IR aerosol DRE becomes comparable to the solar DRE? It might be worthy to examine and discuss this, before the conclusion in the following (last) sentence. Also, concerning the last sentence of the paragraph, it is not valid generally, r at least at the same significance level. It should be more specific,

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taking into account what was noted just before.

21. Page 22555, sub-section 4.5: This section contains the most interesting finding of this work. It provides derived equations relating the anthropogenic and total aerosol DREs based on linear regression fits.

22. Page 22555, Line 22: A comment should be made about the validity of the given formulas to other locations in the Mediterranean basin as well, or even outside of the Mediterranean.

5. Summary and conclusion

1. Page 22557, Lines 10-12: "Aerosol optical and microphysical ... transport pathways": This sentence is vague and should be rephrased.

2. Page 22558, Lines 4-7: "In particular ... Mediterranean dust events": This seems to be in contradiction with the range of the reported values in the 2nd paragraph of this section (34-85%).

3. Page 22558, Lines 8-9: "To a first approximation ... of aerosol present": This sentence should be rephrased.

4. Page 22558, Lines 25-27: "Nevertheless, we believe ... to Bergamo et al. (2008a).": Why would they not be representative for other sites (affected by local pollution) as well?

Table 1 The title of Table 1 is incomplete. Here, sets of values are given for a series of specific days corresponding to dust events at Lecce from 2003 to 2006. This should be specified.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22539, 2009.