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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 #3

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In the present study, authors attempt to provide an estimate of the clear-sky aerosol direct radiative effect during Sahara dust intrusions in the central Mediterranean (Lecce, Italy). More specifically, they try to determine the contribution of anthropogenic and natural aerosols during dust intrusions in the Mediterranean basin. To achieve their goal, they have combined modeling and surface-based measurements techniques.

Major comments:

1. The study is of limited spatial and temporal coverage and therefore, the results are just representative of dust intrusions in the central Mediterranean basin since they are derived from 9 selected intrusions having taken place at Lecce, SE Italy, in summer, under clear-sky conditions. This is acknowledged by the authors and

minimizes the importance of the findings for more extended applications.

(a) The most significant finding of the study is the determination of the contribution of anthropogenic aerosols during Saharan dust intrusions, and it is claimed that this contribution is always significant. The determination is performed at two levels: (i) in terms of aerosol microphysical properties, and (ii) in terms of aerosol direct radiative effects. In the former case, the applied methodology has some problems that are partially addressed in the study, whereas in the latter case, there are severe problems and weaknesses that make the presented results significantly uncertain and need to be revised. Both cases are discussed in the following major comments.

(b) The authors have used AERONET-based aerosol refractive indices (imaginary and real, n, k) and volume size distributions (dv/dlnr) to compute with their two-stream radiative transfer model (RTM) the aerosol optical depth (AOD), single scattering albedo (SSA) and asymmetry parameter (g). I do not understand the reason of re-computing these aerosol parameters since they are directly available from AERONET, especially given that, and this is important, they are of better quality because they take into account the non-spherical shape of aerosol particles (which is not accounted for by the authors' RTM method). This is pointed out, but just slightly, in Table 3, whereas it needs to be further addressed. The only reason would be that by using their re-computed aerosol properties, the authors are able to try to separate the effect of anthropogenic aerosols; if this is the case, it should be explicitly and clearly stated. However, yet, I am afraid that the uncertainties induced by the incompleteness of their AOD, SSA and g values are seriously affecting the results, and should be certainly addressed.

(c) Table 3 shows that there is a problem with the accuracy of model-estimated g (underestimated with respect to that of AERONET by 0.04-0.05, i.e. $>$5%), which is due to not accounting for the non-spherical shape of aerosols. The

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effect of this uncertainty on the computed aerosol direct radiative effects (DREs) should be quantified, since it has been documented that it is strong.

(d) The biggest problem is with the validation of RTM fluxes (Figs 4 and 10). The validation is not convincing, for two main reasons: (i) regarding the results of Fig. 4, only two points are not enough for deriving safe conclusions; more points, for other times within the day or other days are necessary for a complete validation, and (ii) assessing the all-wave (AW, i.e. SW+LW) fluxes accuracy is not appropriate; errors in SW and LW have to be quantified separately. Beyond this, another problem is that, and this is very important, an error of 15% referring to fluxes of about 200 Wm^{-2} (as for 15:31) are large in terms of aerosol DREs, making them highly uncertain, since they are of the same order of magnitude. By consequence, this uncertainty can affect seriously the quality of the presented DRE results, preventing thus one of the main objectives of the study (i.e. reducing the uncertainty in model aerosol DREs computations for the region) to be achieved.

(e) Authors ambitiously attempt to estimate the contribution of anthropogenic aerosols to AOD and DRE. They find that significant contributions exist even during dust intrusions. Nevertheless, this is based on Eq. (1), i.e. on AERONET-N_f(r) and on LMD model-f, which are both uncertain. The uncertainty of the former is discussed in pages 13-14, but that of the latter is not discussed at all, and has to be addressed, at least by performing sensitivity tests for f, as done for N_f(r).

(f) The main objective of the study, as indicated in its title, is to estimate the aerosol DREs (natural, anthropogenic and total). Here is a big problem. The accuracy of model-computed DREs, separately given for SW and LW, is not ensured simply because the pre-requisite, i.e. the accuracy of SW and LW fluxes, is not ensured itself. As noted in previous comment 5, the accuracy of model fluxes is examined only for all-wave radiation. And, even at that level (all-wave) there seem to be quite large uncertainties, according to Fig.

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10b, since the order of errors in model fluxes is similar to that of aerosol all-wave DREs. Furthermore, it would be more reasonable to assess only the all-wave aerosol DREs, instead of the SW and LW ones separately.

(g) There is another problem, related with the use of refractive indices: while for the far-infrared (far-IR) spectrum the indices are allowed to vary with wavelength (Table 1), in the near-IR they are kept constant. Furthermore, in page 13 (lines 3-4) it is said that indices were kept constant. What about the treatment of refractive indices in the UV-visible range, where they actually show their largest spectral variability? Even in the near-IR, there is also significant spectral variability, which is much larger than in the far-IR, i.e. the only spectral region treated sufficiently enough.

Minor comments:

1. The authors retrieve AOD at 470 and 675nm. Then, (page 10) they average them to estimate AOD at 550nm. However, it should be clarified how the spectral averaging has been performed.

   (a) To avoid confusions, it should be defined to what exactly the solar and infrared fluxes and aerosol DREs are referring to. It should be specified to which components (e.g. upwards, downwards) at the surface and top of the atmosphere (TOA).

   (b) There is a problem with the terminology used, that is confusing. Thus, in sub-section 5.4, the names used either are inappropriate or at least, are not sufficiently explained, and become thus confusing.

Summary

In summary, because of the above mentioned problems in the applied methodology, the provided estimates of aerosol DREs, which are the primary results of the study,

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are significantly uncertain. These uncertainties are not addressed appropriately in the paper, and improvements should be made. This problem unfortunately minimizes the usefulness of the otherwise interesting findings on separating the effects of anthropogenic and natural aerosols and identifying their roles. The DRE results can be probably meaningful only qualitatively, but not quantitatively. On the other hand, the results referring to the aerosol microphysical and optical properties, despite the problems that were rise above, are more meaningful, though improvements should be made on them as well.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 20673, 2010.