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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The direct effect of aerosols on solar radiation over the broader Mediterranean basin" *by* C. D. Papadimas et al.

Anonymous Referee #3

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The manuscript submitted by C.D. Papadimas and co-authors addresses the important question of the aerosol radiative forcing in the Mediterranean basin, a region which is often described as one of the most sensitive to climate change by Global Climate Models (GCMs). As pointed out by the authors, this calculation has been already made, and results have been compiled e.g. in the IPCC reports (e.g IPCC, 2007). The work by Papadimas et al. is original because their radiative transfer model uses input data derived from measurements rather than aerosol fields obtained from models. This is indeed a valuable effort to approach true values. However, two main questions arise, which are not completely ignored by the authors, but seriously weaken the scientific relevance of this work. First, all the input data are integrated (aerosol optical depth) or averaged (aerosol single scattering albedo and asymmetry parameter) over the whole atmospheric column,while it is well known that aerosol vertical profiles show strong

gradients. At least a sensitivity test should have been performed to determine how the various aerosol direct radiative effects (DREs) discussed in the manuscript are sensitive to changes in the aerosol vertical distribution. Second, the aerosol optical properties (which are retrieved from passive remote sensing instruments) are affected by random and possibly systematic errors, which may be difficult to assess although they are perhaps as large as the errors of the aerosol optical properties calculated by global models. Both these aspects could well hamper the work by Papadimas and co-authors to reduce the current uncertainty of the aerosol direct forcing over the Mediterranean compared to the current knowledge. Yet, this manuscript would have been useful if it had clearly highlighted the role of various variables and parameters in the spatial and temporal variations of the aerosol radiative forcing (as suggested in the abstract). But the discussions regarding this question are too often obscure and not convincing because they are not straightforward enough. For instance, sections 3.2 and 3.3 compare DRE ratios to AOD ratios for various cases, while the comparison of the aerosol radiative efficiencies (E) would provide with the same information in a smarter way. Instead, section 3.4 discusses E as a possible mans to estimate aerosol forcing which is little rigorous. In conclusions, I did not find in this manuscript any piece of information that would improve our knowledge about the aerosol radiative forcing in the Mediterranean basin. In case the authors would consider re-submitting a thoroughly revised version of this manuscript, I have listed below a few specific comments, omitting missing words and spelling mistakes though.

Specific Comments:

- References for MODIS and GADS input data are missing.

- At which altitude was the TOA set?

- Parag. 3.2.1. This is an example where redundancies make the overall demonstration obscure. Sentences 2 and 3 are confusing and should be deleted. Discussing figures is more efficient than general statements.

- Parag. 3.2.1. As an example, this section should have concluded if the regional variations in aerosol forcing at the top of the atmosphere are due to differences in aerosol loadings, optical properties, or surface albedo (not talking about peculiar areas like the Alps or the Sahara).

- Page 3020, lines 6-11: units are swapped.

- Parag. 3.2.2. First sentence is meaningless, because it is not specific enough. Should be deleted.

- Page 30021, line 11: and partly to what?

- Page 30021, line 23: are large DREatm in central and northern Europe due to big aerosol optical depth or small single scattering albedo?

- Section 3.3: The absorbing character of aerosols is described by its single scattering albedo. How can the ratio TOA (DRETOA) / (DREnetsurf) tell more on this?

- Page 30023, lines 2-12: this is all too speculative and little convincing. The role of desert dust in low (DRETOA) / (DREnetsurf) ratios is not confirmed by the spatial variation (Fig 2i) which shows most of the <0.2 values in the northern part of the domain.

- Page 30024, line 10: the “strong dependence” should be specified (slope or regression coefficient)?

- Page 30026, line 15: is the seasonality weak, or the forcing itself?

- Page 30027, line 28 (and elsewhere): “solar brightening” is not the correct concept.

- Page 30028, line 9:what about reduction in aerosol and aerosol precursor emissions?

- Page 30030, lines 13-16: calculating the effect of 10% change in the aerosol optical parameters before stating that such changes “are quite difficult to take place” is another example of confusing thought process. Are these 10% changes in aerosol optical properties visible in the input data or not ? Why not starting straight with the computations

for 1, 3, 5, 7, and 10% error then ?

- Page 30031, line 28 and following sentences on next page is too vague for a conclusion.

- Page 30032, line 6: polluted aerosol is a weird concept.

- Page 30033, line 4: it is stated earlier (p. 30028, line 5) that cloudiness and precipitation in the Mediterranean basin increased over 2000-2007 yet.

- Page 30033, line 5: the sentence of computed data validation is out of place.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30009, 2011.