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Atmospheric Chemistry and Physics Discussions

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 #4

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The paper presents a detailed analysis on the aerosol shortwave radiative effect for an area, the Eastern Mediterranean, which is considered one of the most sensitive ones concerning the climate change. The analysis is based mostly on MODIS level-2 data and their use by a radiative transfer model. The analysis mostly demonstrates a methodology that can applied for estimating the daily radiative effect using as an example Crete. As a consequence the numerical results presented are not put in a global perspective, mainly through comparisons with other studies. Morever the manuscript in many cases is rather confusing, concerning aspects dealing with the methodology applied. Therefore I suggest that before accepting the manuscript to be published in ACP certain modification/improvements should be considered by authors in a revised

version, which are outlined below in my report:

Page 1-2, Abstract: The inclusion of so many numerical results without any comment on the numbers shown is rather confusing than informative.

Page 2-3. Introduction. Many parts of this section do not belong to an introductory section but mostly belong to the next sections describing the data and the model, and should be moved there. One should expect here from the authors to outline the major objectives of their study. In addition there is a confusion (also later on in the manuscript) with the use DRE and DRF (the latter considered only as the effect of anthropogenic aerosol, but never used again in the manuscript).

Page 4. Model description. The authors use actually a two stream model using the delta-Eddington approximation, obviously due to its rapid computation time. However there are nowadays multiple stream solvers that are included in most of the radiative transfer models. The authors should make a comment of the accuracy of their radiative transfer calculations and the expected uncertainty introduced in their estimates due to the simple approximation they use.

Page 5; Lines 1-3. Is there any refernce for what the authors mention as standard diffusivity approximation?

Page 7: Lines 20-27. This paragraph is very confusing as written. The authors should better provide equations for the various DF's they calculate. In addition the subscripts used are not consistent throughout the manuscript.

Page 10> Aerosol particles. The correction of winter GADS SSA estimates with the ration of summer ratio of AERONET/GADS seems completely unjustified. At least a range of the values (SSA and ratio) should be given in order to be able to have an idea of the uncertainty included in this assumption.

Page 10. Section 4.1 The validation of MODIS AOT is not necessary. There are already many validation papers (even for Crete) and providing reference to them should be

sufficient.

Page 12. Lines 20-29. This paragraph is rather confusing as written. Actually the authors provide DSR with standard deviation estimates using MODIS optical depths which have a certain uncertainty, expressed by the error in the measurement. It should better to show the DSR together with the standard deviations, rather than presenting and discussing them in a different figure. Please consider to revise. Page 13. Lines 7-8. The terminology here (NetSurface) is not consistent with the one used in page 7.

Page 13. Section 4.3. There is no information in the text on what calculations I sbased the daily DSR mean. In addition by checking figure 9 one can detect many outliers which result to large DF's. Have the authors checked these values. Can these be due to AOD contaminated by clouds? Are these consistent with estimates using AOD from AERONET?

Pages 14-15. Section 4.4. The authors do not provide any significance estimate for the trends they calculate.

Page 15. Lines 10-23. The only criterion for distinguishing anthropogenic particles is the fine mode fraction set to 0.7. Any reference for that? Is this consistent with backtrajectory analysis? Do the authors claim that all the fine mode aerosols in the area exlusevily of anthropogenic origin? Please comment.

Page 16. Lines 1-5. The seasonality presented here is not consistent with earlier discussion in the same paper. April maximum due to anthropogenic aerosols seems unjustified, as well February maximum due to dust.

There is no reference to Figure 15 in the text.

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