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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of absorbing aerosols in cloudy skies: a satellite study over the Atlantic Ocean" *by* K. Peters et al.

Anonymous Referee #1

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The authors propose the creative use of different satellite data sets to identify the presence of absorbing aerosols above clouds to produce quantitative estimates of the radiative forcing associated with these events. The method consists on the identification of overcast pixels using some MODIS retrieved cloud property (presumably cloud fraction or cloud optical depth) not explicitly mentioned in the paper. The authors use the OMI aerosol index to identify those cases when dust or smoke aerosols are located above clouds in the overcast pixels. In the next step they attempt to find a statistical relationship between the CERES-determined shortwave local planetary albedo, the AMSR-E cloud liquid water path (LWP) and the MODIS optical depth. Although the proposed methodology seems reasonable, the outcome of the analysis is totally counter-intuitive. The obtained results indicate that the largest reduction in local planetary albedo (presumably associated with aerosol absorption effects) takes place over the Northern

Hemisphere's West Atlantic Ocean, a region between 10N - 35 N and 50W-95W (or TNWA region using the authors naming convention). The aerosol absorption effect on the TNWA region is about 5 times larger that the effect on the TNEA oceanic region counterpart (10N-35N, 15W-50W). These results are so counter-intuitive (and possibly wrong) that a physical explanation is urgently needed. Unfortunately, the authors just present their findings with no explanation whatsoever and continue to the next part of their analysis as if the reported results were so self-explanatory and deserved no discussion. The atmospheric aerosol load over the north Atlantic region between about 15N and 35N during March to October is predominantly desert dust from the Saharan. While it is true that hematite and other iron oxides contained in the dust absorb in the UV, the absorption in the broadband CERES spectral region is significantly less. So, one should not expect to see any noticeable effect on the CERES measured shortwave fluxes. It is also very surprising that the observed aerosol absorption effect on the TNWA region is even larger that the effect on the TSEA region which is the region where largest effect should be observed. In this region aerosol absorption is mainly due to black carbon, a component equally absorbing from the near-UV to the near-IR. For the sake of argument let's assume that there is significant dust absorption in the vis-near IR region. The gradient of the aerosol absorption effect should be qualitatively similar to the AOD gradient and therefore one should expect a minimum effect on the region farthest away from the aerosol sources. The authors need to provide a reasonable physical explanation for the observed increase in aerosol absorption effect from region TNEA to region TNWA. The seemingly reasonable explanation offered for the reduced effect on region TNEA should also apply to region TNWA except that the effect should be even smaller at TNWA and no larger as reported. I have stopped my review at the manuscript at this point. Without a clear understanding of these very strange results there is no point on continuing to the section 4 of the paper which is based on the questionable outcome of the first part of the analysis.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20853, 2009.