

Interactive comment on “The direct effect of aerosols on solar radiation based on satellite observations, reanalysis datasets, and spectral aerosol optical properties from Global Aerosol Data Set (GADS)” by N. Hatzianastassiou et al.

Anonymous Referee #1

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This paper discusses a radiative calculation of the global solar direct forcing due to natural plus anthropogenic aerosols in the presence of clouds. Aerosol fields are constrained by satellite products. Results for TOA and surface forcing are compared with those from some other studies.

The paper could be useful but is currently missing a thorough discussion of how aerosol and cloud optical properties are treated. TOA direct forcings are remarkably consistent with results from two studies cited (although the authors appear to provide the incorrect forcing from one of the studies). Results for the surface direct forcing are less

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consistent with results from one of these studies, but that may be due to the treatment of optical properties of absorbing compounds, including soot, soil dust, and certain organic matter. These and additional comments are discussed below.

1) The aerosol optical properties were derived as follows: “We have used a detailed spectral radiative transfer model to match, as best possible, the detailed spectral resolution of aerosol optical properties (extinction, AOT, single scattering albedo, and asymmetry parameter) from GADS, given at 40 wavelengths” More explanation is needed. Did you assume aerosol concentration and composition profiles then perform a calculation to “match” the observed parameters or did you use the observed parameters as inputs into a radiative calculation. If the latter, what was being matched? Further, how did GADS determine these parameters for input? This becomes important since it is possible to obtain the same parameters with different compositions of the same aerosol components simply by changing their mixing state. So, depending on how the satellite inversion was done, derived satellite products could be in error. For example, the global direct forcing of black carbon differs significantly depending on how the mixing state is treated (e.g., Jacobson, Nature, 409, 695, 2001). Thus, for a given measured absorption extinction coefficient, the estimated amount of black carbon present is really unknown in the absence of other measurements. In sum, the authors should clarify how GADs determined properties (e.g., single-scattering albedo, extinction, etc.), what mixing state assumptions were used, and what aerosol inputs were used in the model. The authors should also provide an uncertainty range for both the satellite and their model results.

2) The TOA solar direct forcing from Jacobson (JGR, 2001), provided in Table 2 of the present manuscript, appears to be given incorrectly as -1.23 W/m^2 . That number is stated in Jacobson’s paper to be the solar+thermal-IR direct forcing. The solar direct forcing alone is found in Figure 4a, and is -1.8 W/m^2 , close to the -1.62 W/m^2 from the present study. TOA forcings from the Yu et al. studies are also close to those from the present study. However, the surface direct forcing from Figure 4a in Jacobson’s study

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is about -4.6 W/m^2 , greater in magnitude than that of the current study, -3.22 W/m^2 . Figures 4b, 4i, and 4o in Jacobson's paper indicate that the reduction there between the TOA and surface was due primarily to absorption by soildust, certain organic matter, and black carbon. Again, treatment of the mixing state in the satellite inversion and the model may explain the difference and should be discussed.

3) P. 764. The authors state, "The strong increase in atmospheric absorption of solar radiation is either associated with large mass concentrations of absorbing mineral aerosol components or with significant (but smaller concentrations of strongly absorbing soot or E " This is true, but do you have any way of extracting composition from the satellite products or quantitative way of determining the relative contributions? Again, a better explanation of how the satellite determines products would be helpful.

4) It is not clear how cloud optical properties were treated. How were cloud optical properties filtered from aerosol optical properties in the satellite inversion and model?

Since satellite aerosol products are generally not determined when clouds are present, it would appear that the present method used of determining global aerosol forcing constrained by satellite products must result in some error, particularly in aerosol absorption, since it does not account for the direct forcing of those aerosols within and above clouds. Several studies have shown that clouds enhance the direct forcing of absorbing compounds within and above them by increasing the reflected light entering the particles (e.g., Haywood et al., JGR, 103, 6043, 1998; Liao and Seinfeld, JGR, 103, 3781, 1998). This is particularly important in biomass-burning regions where aerosols are lofted to great heights. The authors should discuss this error if they are excluding aerosol properties when clouds are present. This may also explain some difference between the surface forcing of the present study and that of Jacobson (JGR, 2001) who found enhanced atmospheric absorption due to the presence of clouds (Section 7).

5) P. 756. "Ěan important improvement in this study, compared to other studies, is

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that the aerosol DRE is computed at a very high spectral resolution, while other radiation transfer, general circulation and chemical transport models usually include a few spectral bands in the whole SW range.” The present study included 117 wavelengths between 0.2 and 1 μm and 10 between 1 and 10 μm . Page 1556 of Jacobson (2001) states that 153 UV, visible, and near-IR wavelengths below 4.5 μm and 256 between 4.5 and 1000 μm were used. The authors should put their statement in context with respect to this other study

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