Atmospheric Chemistry and Physics Discussions

Interactive Comment

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

Anonymous Referee #3

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The paper is part of a series addressing the estimation of the direct effect by natural and anthropogenic atmospheric aerosols. A first paper, published in 2004, studies the effect in the UV and visible parts of the solar spectrum, in clear-sky conditions. A second paper, dated 2006, adds the near-infrared solar spectrum. This paper completes the picture by considering all-sky conditions.

The paper is well written and an interesting contribution to the study of the direct effect. Especially commendable are the focus on the surface level and local aspects of the direct effect. However I hereafter list five issues that I reckon should be addressed or

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discussed to improve the reader's confidence in the authors' results. Consequently, I recommend major revisions.

1. Sign convention

The sign convention used by the authors for the definition of the direct effect is disturbing. The perturbation made by the aerosol-induced increase of outgoing radiation is counted as positive. An opposite convention is used by the vast majority of similar studies (including those cited in the paper) and the IPCC reports. Choosing another convention is therefore unhelpful.

Equation 1, page 758, is defined for net fluxes, i.e. difference between incoming and outgoing radiation. So

DF = Faerosol - Fno-aerosol = (F_downward_aerosol - F_upward_aerosol) - (Fdownward_no-aerosol - F_upward_no-aerosol).

As F_downward, when computed at the top of the atmosphere, is independent on the contents of that atmosphere, the above relation reads

DF_TOA = F_upward_no-aerosol - F_upward_aerosol.

Following this, the increase of outgoing radiation by aerosols will translate into a negative perturbation. Defining net fluxes as the difference between outgoing and incoming radiation is not forbidden, but it goes against the accepted convention. I strongly advise for a change of sign throughout the paper, although it will unfortunately be inconsistent with the earlier papers published by the authors. Note that both surface and atmospheric perturbations have the correct sign in the paper.

2. The Global Aerosol Data Set

The aerosol climatology used by the authors is the Global Aerosol Data Set (GADS). Very few information on the dataset is given in the paper, and the reader is referred to Hatzianastassiou et al. (2004a) where the dataset is described. Numbers describing

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the dataset (e.g. global average of the optical depth, values of the single-scattering albedo and asymmetry parameter by species, at 0.55 microns) would gain at being repeated here, as they are of prime importance when discussing the estimated direct effect.

For example, in the 2004a paper, the global-averaged total aerosol optical depth is given at 0.08 at 0.55 microns. This is significantly smaller than the current climate model estimates (ranging from 0.11 to 0.14 in AEROCOM models, Kinne et al. (2006)), AERONET average (around 0.14) and satellite retrievals (around 0.15). This potential underestimation casts doubts on their direct effect estimates: Figure 1b, for example, does not show the strong biomass-burning cooling pattern I would expect in the Congo Basin in July.

Another example is the single-scattering albedo of mineral dust. In the 2004a paper, Figure ii-a suggests is it around 0.80. The authors rightly state that mineral dust absorption is a debated issue, but their "sensitivity analysis" is poor. They write (p762) that they increased the SSA by 6%, without giving the original and new values. They write that the increase changed the sign of the forcing. What is the new value? Without those pieces of information, the reader cannot make any conclusion more useful than the well-known fact that there exists a critical SSA, depending on the underlying surface reflectance, at which the direct effect changes sign.

3. Radiative transfer calculations

The authors take great care at having a high wavelength resolution when integrating aerosol optical properties, using 117 wavelengths and 10 wavebands to cover the solar spectrum. To do so, they have to interpolate and extrapolate the aerosol optical properties given by the GADS, as stated on page 757. Is it is really worth it? Couldn't they simply use the 40 wavelengths provided by the GADS? It would already be much more than what is used by other studies and would make sense compared to the actual (lack of) knowledge of the wavelength dependency of aerosol optical properties (especially

the single-scattering albedo).

In contrast, the asymmetry parameter is used to describe the angular dependence of scattering. This is known to potentially introduce significant errors in the computed perturbations (Boucher 1998, Marshall et al. 1995). Those errors could potentially cancel the gain of using 117 wavelengths. Also worth mentioning is the impact of the dependence of the direct effect on the solar zenith angle. How does the authors' radiative transfer code deal with that? It is unclear whether their solver uses a two-stream method, or more accurate techniques. Obviously, using a "mean solar zenith angle" would introduce significant errors in the computed perturbations.

Another important parameter is the surface reflectance. It is poorly described in section 2.3. Does it depend on wavelength? Over ocean, does it include the solar zenith angle dependence?

Finally, the distribution of the direct effect estimated at the surface, as shown in Figure 4 and discussed in section 3.3, surprises me. Why is there such a strong discontinuity between land and ocean surfaces? I would expect the surface properties to play a second-order role here. Atmospheric absorption is the dominant effect and should not exhibit such a strong change over ocean, unless the aged (transported) aerosol properties change radically.

4. Comparison against other estimates

In Table 2, the authors compare their estimates against those by other studies, some being observation-based, other being modelling-based. This is not a straightforward job, and, again, including the global-averaged optical depths would help a lot. For example, the global, all-sky direct effect at the TOA is -1.0 Wm 2 with an optical depth of 0.12 by Reddy et al. (2005). The authors' estimate is -1.6 Wm-2 with an optical depth given at 0.08 in the 2004a paper. The authors must use very optically active aerosols to get that. Why is that?

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I was also unable to find in the cited papers the numbers quoted in Table 2 for Yu et al. (2006) and Yu et al. (2004). I may have missed them. Could the authors point me to their location?

5. Direct effect above clouds

When observation-based studies try to get an "all-sky" estimate of the direct effect, they have to assume a cloudy-sky effect of zero. This is not true, but current satellite instruments cannot do better. Here, the cloudy-sky effect can be derived, as the authors have both the aerosol and cloud vertical profiles. This is one of the assets of the study, but the authors are silent about it. What is the fractional area where aerosols overlie clouds? What is the cloudy-sky forcing? Both numbers are certainly of great interest when assessing the validity of the "zero cloudy-sky effect" assumption (it should be noted that they strongly depend on the quality of the assumed/retrieved vertical profiles and on aerosol absorption).

Other comments

The abstract is misleading when it reads that the direct effect is "obtained by combining satellite measurements and reanalysis data". The aerosol optical depth field from GADS is neither. It is output from a chemical transport model.

In the introduction, the sum of the natural and anthropogenic direct and indirect effects is said to be comparable to the radiative forcing of greenhouse gases. This is irrelevant, as only the aerosol forcing (i.e. exerted by anthropogenic aerosols only) can be compared against the greenhouse gas forcing, as stated by the authors on the very same page.

In the introduction, Chin et al. (2002) is given as a reference for model-based assessments of the direct effect, but this paper does not give such estimates. This is a validation study on aerosol optical depths.

In the introduction, Remer and Kaufman (2006) is not a good reference for the wide

range of estimates seen in climate models. Schulz et al. (2006) is more relevant.

In the introduction and again in section 3.4, it is said that satellite-based methods cannot provide estimates of the surface and atmospheric perturbations. They can: once the aerosol optical properties are known, a radiative transfer code can yield those estimates (e.g. Bellouin et al. 2003). True, those are not "direct" estimates, in the sense that fluxes are not directly measured (studies using CERES fluxes are arguably more "direct"). But the authors' estimates are not direct either.

In section 2.2, it is written that the GADS has been used by "a number of climate model studies", with King et al. (1999) and Chin et al. (2002) as references. King et al. (1999) is not a climate model study: It is a review of remote-sensing techniques. They cite an earlier version of the GADS (d'Almeida et al., 1991) as being a dataset commonly used for getting aerosol optical properties (before "advertising" the AERONET dataset!). As for Chin et al. (2002), they do not use the aerosol optical depth fields of the GADS.

In section 3.1, sea-spray production is said to be intense in the South Hemisphere in January. It is more intense in July (SH winter), as shown in Figure 1.

In section 3.1, Europe and North America "are found to have relatively small values of DF_TOA despite relatively large AOT values". Writing "relatively" is not especially accurate, and there is no direct dependence between the extinction AOT and direct effect anyway: It strongly depends on the aerosol absorption and surface reflectance, as correctly stated by the authors elsewhere.

Throughout the paper, the direct effect estimates are given at one or two decimal digits accuracy. One digit is enough - currently, the other cannot be significant.

The paper cites a lot of previous studies. It sometimes reads like a review! I reckon the reference list can be trimmed down to include only the most recent/significant papers and direct the reader to references therein.

References:

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D'Almeida et al. Atmospheric Aerosols, Global Climatology and Radiative Characteristics. A. Deepak Publ., 561 pp. 1991.

Bellouin et al. Aerosol absorption over the clear-sky oceans deduced from POLDER-1 and AERONET observations. Geophys. Res. Lett., 30, 1748, doi:10.1029/2003GL017121, 2003.

Boucher. On aerosol direct shortwave forcing and the Henyey-Greenstein phase function. J. Atmos. Sci. 55, 128-134, 1998.

Kinne et al. An AeroCom initial assessment - optical properties in aerosol component modules of global models. Atmos. Chem. Phys., 6, 1815-1834, 2006.

Marshall et al. Relationships between asymmetry parameter and hemispheric backscatter ratio: Implications for climate forcing by aerosols, Appl. Opt., 34,

6306-6311, 1995.

Schulz et al. Radiative forcing by aerosols as derived from the AeroCom present-day and pre-industrial simulations, Atmos. Chem. Phys., 6, 5225-5246, 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 753, 2007.

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