

Interactive comment on “Radiative forcing of the direct aerosol effect using a multi-observation approach” by G. Myhre et al.

Anonymous Referee #2

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Strengths

- Great impact on a hot scientific issue.
- Many different ways to evaluate the model against data
- Attribution of aerosol direct effects to components (only possible via modeling)
- Well written – in particular the highlight structure of the summary is liked

Weaknesses

- Definition of anthropogenic fraction is less useful if coarse mode aerosol is included

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- The figure captions are often insufficient.

General

This paper is a great effort to document the direct effect by anthropogenic aerosol to the solar radiation at the top of the atmosphere. This contribution helps in providing a more certain range estimate than those suggested by IPCC 2007 and provides a lot of extra detail. The many interesting figures and comparisons of this (timely) presentation are impressive. The only drawback is that the figure captions are often too brief, but this can be fixed. I like this paper and I strongly recommend publication.

Detailed comments

The table and the figures:

Table 1 indicated that the total anthropogenic AOD is at 0.45 (if added correctly). This seems on the high side but still reasonable. Maybe the table should be extended to show that number, so that adding is not required. Similarly, it also might be of interest, how much of the AOD is anthropogenic not only if dust and seasalt are included, but also if natural aerosol (dust and sea-salt) is excluded.

Figure 1 is a nice illustration. I just do not understand the link from meteorology direct to the forcing and would leave it out. I also would be more specific on the aerosol distribution. Clearly since all parameters are changing in time and space here the vertical

Aerosol distribution of all its properties (amount, size and absorption) is meant especially relative to the location of clouds. This I would rephrase by saying 'aerosol vertical placement with respect to clouds'

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Figure 2 shows scatter plots, where the labels are rather tiny. The description fails to indicate the time-scales involved. The comparisons are not too convincing – except for sulfate over the US. Maybe it would be better to compare scatter plots on by combining data-pairs over longer timescales to demonstrate that modeling ‘in general’ gets it right.

Figure 3 appears to be a nice demonstration of modeling skill with respect to aerosol composition. Please be clear to indicate in the captions that it refers to near surface sampling and the lowest modeling layer and also be clear about the averaging (just to make sure that sampling differences are not a major issue).

Figure 4 is very unclear about the comparisons. Are these regional averages (then what regions . . . or are these comparison exactly at AERONET sites? In any case, I am very surprised that the agreement is as good as indicated.

Figure 5 shows interesting comparisons. I assume these are monthly averages (do we have a sampling problem ? –so be clear on what was compared). The overall impression here is that the model underestimates AOD, while satellite remote sensing overestimates AOD. Regional (e.g. ocean/land) aspects might be interesting as well. With respect to Terra comparisons to Aqua are the same years applied (since Terra data are since 2000 and Aqua since 2002) ?

Figure 6. It is unclear to what data (individual pairs, daily averages of monthly averages) these correlation coefficients refer to (correlation of data coincidences during a month?)

Figure 7 is a bit confusing as it contains 3 pieces of information, station ordered by number, the AOD values and the ratios with respect to AERONET. It might be easier to use different symbols (for AERONET #) to easier distinguish the pieces of information.

I am also concerned about the small fonts for the station sites, which also may not always familiar to a reader (a table with site names on lat/lon/alt location would help here).

Figure 8 the widely distributed scatter plots are disturbing and alarming I kind of hate the linear fits and it may be more useful the differentiate the scatter plot local frequency by different color (10 near-by event could be 'black' as are 1000 events)

Figure 9 any good reason, why just these sites are picked and not others? Was there some attempt to stratify by dominant aerosol type?

Figure 10. Are the annual fields based on monthly data or daily data. The model data are apparently sub-sampled at day?, months? Of non-zero satellite data

Figure 11. The comparison of annual averages (here for the single scattering albedo SSA) is only one factor, as there is seasonality especially at sites with biomass burning. More useful would be comparisons of the aerosol optical depth (which focuses on the absorption aspect rather than at questionable SSA (AERONET) values at low aerosol optical depths. It also would be nice to mention how AERONET data were 'interpolated' (were they?) to the reference wavelength in modeling at 0.55 μ m.

Figure 12. Did the SSA comparisons also include AERONET data at low aerosol optical depth (AOD <0.3). If not, stratify by using different symbols. It is also not clear, what AERONET data (I assume version 2 level 2.0) are used. In that case of course the quality 2.0 data do not report SSA values at low AODs which may cause a bias problem in statistical comparison (e.g. monthly averages) to model results. The figure also gives BC absorption data. It is unclear on what data the AERONET values are based on. Please explain!

Figure 13. There is significant scatter in the Angstrom parameter comparison. I suggest to remove the linear regression line, as it seems rather misleading given the amount of scatter. In fact, when removing some outliers, the model suggested Angstrom parameters appear lower. The model seems to be unable to provide Angstrom parameters below 0.5, why? The captions do not indicate, if the Angstrom definitions are identical (or at least comparable) and for what time-scales and sites the comparisons refer to.

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Figure 14. Is the BC mass concentration overestimate in modeling at higher altitude just a sample/measurement problem? I do not recollect that there are many and reliable data available. Unfortunately, available column estimates from AERONET are unlikely to help given the (still) order of magnitude lower concentration at higher altitudes.

Figure 15. Nice figure !, as it shows consistency among different approaches (modeling and data-tied approaches) that the clear-sky ToA aerosol direct effect is well constrained (ca at -5W/m^2 cooling). I imply (please state) that these figures (especially involving data-tied methods) refer to SOLAR radiative forcing since there could be some IR greenhouse effects for elevated dust, especially off Africa.

Figure 16. Now differences start to show. Particularly the results in b) (why are the values so large as values above $-60\text{W/m}^2/\text{aod}$ at the TOA seem unrealistic) and c) (why are the forcing efficiencies so low – e.g. are these related to overestimates in aerosol absorption or do they involve contaminations by clouds?) need explanations.

Figure 17. The large regional differences between a) and d) surprise which was not expected from Figure 16. This needs some explanations. I am also confused as the assignment of the regions (Yu's paper stratifies into 13 regions and you have 14 –as you have a region '0'). The only encouraging things about the figure (despite at time significant (!) differences) is, that regional differences in a relative sense are somewhat maintained. In some regions the coverage by ocean pixels is rather limited, thus it may be useful to indicate the number of pixels involved. In addition, these are annual averages and some of these larger differences may have a seasonal bias. Was the seasonal dependence to the differences investigated?

Figure 18. These are interesting plots. The major problem seems that the anthropogenic fraction refers to all aerosol – including natural aerosol, so that the estimate for anthropogenic aerosol becomes dependent on the near surface winds, which define mass loads for dust and sea-salt. A definition with reference to fine mode aerosol appears more useful. The given estimate also seems to have little consideration for

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(anthrop) pollution to be moved over bright surfaces (e.g. Sahara). I did assume that anthropogenic aerosol only attributes to the accumulation mode ... so it would be reassuring to have it stated.

Figure 19. Nice! Such stratification can only be done by global modeling. This figure is very educational as to expected forcings by particular components. It would be nice to have some supplementary information of the relative placement between aerosol and clouds.

Figure 20. Your BC and OC forcing seems higher ... which may be in part related that the anthrop AOD estimates for those components are relative high in this study. It is also interesting that mainly the considerations of nitrate and dust lead to the overall more negative bias of IPCC 2007.

Figure 21. There are two panels but only one explanation. This is confusing. There is some surprise as to the lack of anthropogenic warming over bright deserts (is a small negative value in that region believable?)

The text

page/line

12825/12 remove 'of'

12827/5 remove 'in the model'

12828/4 ... can exit as ...

12830/18 ... (Yttri...)

12833/15 be more specific than ... 'averaged' (?) values are 'grouped' (weights?) ..

12833/26 for South America, there are large discrepancies between AERONET and

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satellite data on the monthly basis (e.g. different seasonality by MODIS 5) which may go unnoticed when comparing annual averages – well are these representative annual averages what you show in Figure 4? I am concerned here about misrepresentations by (temporal and spatial) sampling biases. For instance, there maybe much more data during particular seasons (e.g. dry season in South America). Also, the satellite and AERONET temporal and spatial samples should be quite different and it is not quite clear (when comparing to modeling), if satellite data (interpolation from what resolution?) were sub-sampled at AERONET sites.

12834/4 ... probably because the heavy polluted Mexico City site was removed... It is unclear, what is meant 'under the same assumption' ... or does it mean that the background rural west US AOD has a high bias. Did you check and remove sub-pixel snow contamination from the satellite AOD data, which can cause significant AOD overestimate artifact especially in spring ... ?

12834/10 do you dare to speculate why the modeled AOD is lower (e.g. dust emission [scheme]?). I am a bit surprised that satellite overestimates are only on the order of 10 to 15%

12834/15 replace 'sampled' with 'sub-sampled'

12834/16+ ahh, now the sampling issue is addressed. All the number an percentages are a bit confusing and I strongly recommend a table summarizing 'fair (same sample)' comparisons, especially between AERONET and satellite data in more detail (at least for bias but possibly also other statistical aspects such as correlation or std.dev., possibly also finer temporal scales).

12835/21 the individual site bias presentation in Figure 7 is quite interesting. Based on that comparison could you indicate some site which (due to an apparent lack in regional representation) should be excluded. It also might we very instructive to see the comparisons of Figure 7 stratified by seasons.

12836/14 Crete would be a good site to test different aerosol type (dust, pollution, sea-salt) and not just overall AOD. Can you say anything on how well the model here reproduces such events?

12836/20 Venice is a site in the Adrian sea off Venice and hardly affected by the Po valley pollution.

12837/1 For Ougadougou you may want to compare the Angstrom parameter to understand the AOD underestimates – most likely biomass burning aerosol

12837/5 ... this begs the question, on how appropriate your assumed near-surface winds and/or the dust emissions parameterizations are

12839/1 sporadic boreal fires significantly contribute to AOD at high latitudes, which may not adequately accounted for in wildfire emissions (I am not quite certain in howfar GFED includes boreal fires).

12841/13 there is some non-negligible aerosol RF sensitivity to the asymmetry-factor, thus to aerosol size. And as mentioned in the next comment, differences in the

Angstrom parameter can fail to demonstrate these differences adequately.

12841/18 differences in the SSA over dust-load regions may also indicate differences in dust size between model and measurements (at larger sizes the mid-vis. Angstrom parameter becomes insensitive. . .)

12851/2 the surface forcings appear a bit on the high side (from what I would have expected) ... but on the other hand the anthropogenic AOD fraction appears on the large side, partly because nitrate and SOA are considered ... thus the values are believable.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 12823, 2008.

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