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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional and monthly and clear-sky aerosol direct radiative effect (and forcing) derived from the GlobAEROSOL-AATSR satellite aerosol product" *by* G. E. Thomas et al.

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The authors thank the referee for their constructive comments. We have addressed each of their comments, as detailed below. The referee's original comments are included in *italics* before each response for reference.

Main comments

I do not understand why the authors use regional and monthly AOD values to compute direct effect and forcing, instead of the 1x1-degree daily distributions from

GlobAEROSOL. It seems to me that losing the finer details is unnecessary, even considering AATSR's limited sampling and the need for more radiative transfer calculations.
The reviewer makes a valid point here, however as is noted in the paper (Pg. 18471, Section 3, second paragraph), the data used to define the radiative properties and height distribution of the aerosol in the radiative transfer calculations are most definitely not at daily 1x1-degree resolution, as they are a composite of various field campaign and aggregated AERONET measurements. Thus, performing daily 1x1-degree radiative transfer calculations would probably not improve the results and was deemed to be too computationally expensive.

The Section 3 of text has been modified to make this point more clearly.

The author's bottom-up approach to compute total uncertainty is bound to produce large ranges, since it cannot account for compensating errors. Aerosol direct effect is subject to compensating errors, for example at the TOA when AOD is overestimated and single-scattering albedo is underestimated. For the forcing, some errors (such as surface albedo or spectral variability, for example) should compensate since they impact more the absolute values of the radiative fluxes than their change since pre-industrial condition. Model ensembles or Monte-Carlo/Bayesian emulation approaches are able to account for compensating errors, but require more efforts.
The authors' agree that a large ensemble, Monte-Carlo or Bayesian approach would offer the most complete uncertainty propagation. However, as the the referee also points out, this would involve many more radiative transfer computations, which were not feasible in this study. The authors' believe that their approach represents the next best option - compensating effects within each identified error term should be included (the main assumption made is that the error distribution is approximately Gaussian for each term, which is clearly a big assumption in itself). The use of a both random and systematic assumptions uncertainty in combining the separate error terms would reveal any large compensating error effect between the terms.

We accept the referee's point that the error propagation is not the most rigorous possible however and have modified the beginning of Section 4 to better reflect this limitation of the work.

Showing a figure similar to Figure 2, but for the direct effect, would be a nice way to summarise the results and uncertainty analysis.
Unless the authors have misunderstood this comment, the requested figure is already included in the paper (Fig. 5).

Page 18461, lines 20–25: The description of results by Myhre [2009] is wrong. That paper does indeed read that the "change in the aerosol optical properties due to anthropogenic activity is a main reason for the difference between observation-based and model estimates of the radiative forcing", but models are able to reproduce that change, whereas observation-based methods are not, since they lack observations of pre-industrial aerosols.
Thank you for pointing out this rather important typographical error. The missing "not" has been inserted.

Page 18468, line 19: It would be helpful to compare GlobAEROSOL's performance against AERONET with similar studies for other sensors, such as MODIS, MISR, or POLDER. A 0.1 error in AOD seems large when compared to publications for other sensors and when considering that getting the direct effect within 1 Wm$-$2 requires knowing the AOD within 0.02 (McComiskey et al., 2008).
The authors acknowledge that the GlobAEROSOL product shows greater errors against AERONET than the products listed above. However, it should be noted that the biases in the AOD values are much smaller than the RMS, suggesting that the data can be considered accurate on a regional/monthly scale. The discussion of the AOD accuracy at the end of Section 2.1 has been expanded to address this point.

Page 18470, line 20, and 18472, line 5: The radiative transfer calculations are not really state-of-the-art. Nowadays, one can do better than assuming spectral invariance of aerosol optical properties, and using only three values of the solar zenith angle to represent the strong dependence of direct effect with that parameter (see Figure 5 of Boucher et al, 1998). I find those choices difficult to justify, even in terms of computational costs.
We accept that the radiative transfer code used is not "state of the art". However, the point is that we use a radiation code at a level of sophistication similar to that in many climate models and thus we believe that our method better represents the uncertainty that would be introduced into subsequent climate response calculations. We have added a sentence to page 18470, line 23 as follows: "This radiative transfer code, whilst not state of the art, was chosen as it represents the level of sophistication of radiative transfers in climate models, and thus uncertainties identified whilst using this code are likely to be particularly relevant for uncertainties in the subsequent prediction of climate response".

Other comments

Why is and forcing in brackets in the title?
It is an attempt to indicate that the forcing estimate included in the paper is not the primary result (due to the authors' lack of confidence in it) and has been included for interests sake, and to highlight the difficulties in determining its value.

Page 18465, line 18: The surface albedo retrieval is presumably strongly constrained by the MODIS BRDF product used to prescribe surface properties in the first place. Is that correct?

The angular dependence of the surface reflectance (ie. the ratio of the reflectance seen by the two AATSR views in each channel) is constrained by the MODIS BRDF a priori. The magnitude of the reflectance is a free retrieval parameter, with a minimum a priori constraint of 0.02. This information has been included as a footnote in the paper.

Page 18467, lines 22–25: The difficulty here is that this assumption may turn out to be a unreasonable, unless one first identifies the regions which experienced large aerosol trends (independently of the satellite retrievals to be assessed).
This is indeed a potential problem - it is possible that the bias correction calculated for a region with strong trends in aerosol loading will not be appropriate for 2006 conditions. However, in such a case, one would expect corresponding increase in the width of the GlobAEROSOL-AERONET PDF. Thus uncertainty propagated through to ARE and forcing will also be greater, reflecting the increased error. A statement to this effect has been included in the paper.

Page 18470, line 7: It would be useful to point out that this statement only applies to cloud-free conditions.
A good point - the text has been modified accordingly.

Page 18469, section 2.2, and page 18471, equations 1 and 2: The risk with prescribing regional optical properties is that those may be different from the optical properties used when retrieving the AOD, thus making the AOD and direct effect inconsistent with each other. However, the method described in section 3 may correct for this inconsistency. Can the authors confirm that this is the case?
This is a subtle point. The method described in section 3 does indeed ensure that the direct radiative effect is that for the prescribed AOD and aerosol properties. However, it does not guarantee that the radiative transfer accurately reproduces the TOA radiance measurements made by the AATSR satellite. It is this source of error which has been

addressed with the “scattering properties” uncertainty term (see pg. 18473). Section 3 has been slightly expanded to address this point.

Page 18472, line 14: An additional source of error is the radiative transfer code it- self. This could be quantified using the results of the intercomparison by Randles et al. (2012), soon to appear in Atmos. Chem. Phys. Discuss., which includes the radiative transfer code used by the authors.
The referee is correct that the radiative transfer itself is a source of error that has been neglected in this work. However, the authors have used the same version of the code to participate in the AEROCOM intercomparison of radiative transfer codes about to appear in the Randles et al paper mentioned by the referee. In that intercomparison, our model showed a top of atmosphere radiative forcing in good agreeement with other models (for the test cases using sub-arctic winter atmospheres and tropical atmospheres, although it did show a tendency to underestimate the surface diffuse radiation. We therefore believe that the error in the top of atmosphere radiative flux due to the radiative transfer code is smaller than many of the other errors considered here. For the surface flux, the uncertainty due to radiative transfer might be of similar importance. We have added a sentence to the end of section 6.2 as follows: “It is important to remember that any uncertainty due to the radiative transfer code itself is not included here. However, the code used has recently been included in an intercomparison study by Randles et al (2012), and this demonstrated that the uncertainty compared to state-of-the-art radiative transfer models is likely around 1-2% for the TOA radiative flux. This is considerably smaller than most of the other sources considered here. For the surface, the intercomparison suggests that this code may underestimate the diffuse dowanward flux by around 10% and this could contribute an additional uncertainty to those in this paper.”

Page 18479, lines 23–24: Different aerosol sources, rather than air quality measures, *are the cause of the larger importance of photochemically-driven secondary aerosols in the North Hemisphere compared to the South. (In other word, industrialised countries tend to emit aerosol precursors, while biomass-burning areas are dominated by primary aerosol emissions.)*

The authors take the referees point - although the lack of primary aerosol production (from the likes of industrial and domestic coal burning or diesel engines) in developed countries is largely due to air-quality measures introduced in the second half of the 20th century. The manuscript has been modified accordingly.

Page 18479, line 25 to page 18480, line 4: To avoid those artificial positive forcings, the authors could have derived the relative change in AOD using AeroCom present-day and pre-industrial experiments, and applied that change to present day GlobAEROSOL AODs.

The authors' fail to see the point of this, as the result would be rather similar to the radiative forcing calculated from the AerCom models themselves without reference to the GlobAEROSOL AOD.

Page 18481, lines 21–28: Kahn (2011) is a good reference here, as he makes similar points.

Thank you for pointing out this interesting and valuable paper, of which we weren't aware; references to it have been included in our paper. (For the referees own information, this paper is dated 2012 rather than 2011.)

Technical comments

Page 18464, line 18: Typo: Cerrado.
Page 18465, line 27: Typo: AEROCOM.
Page 18470, line 6: Typos: absence, composition.

Page 18482, line 8: Typo: almucantar.
Caption of Table 3: Typo: literature.
These have all be corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18459, 2012.