Review of Aerosol radiative effects with MACv2 by S. Kinne for ACP

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I have examined the submitted manuscript Aerosol radiative effects with MACv2 (file name acp-2018-949-manuscript-version4.pdf) and the companion manuscript in review at *Tellus* The MACv2 Aerosol Climatology (file name MACv2_clima_2019_06.pdf)

To great extent the manuscript under review, and also the companion manuscript, read like a reports, stating what was done, to support model calculations using these results. Specifically the present manuscript reports the influence (direct, indirect, i.e., Twomey effect) of aerosols (natural and anthropogenic) on atmospheric irradiance (at the top of the atmosphere and at the surface) as a function of location (1˚ x 1˚), in monthly average and annual average and in global average, based on a similarly resolved aerosol climatology described in the companion manuscript; and also historically, at 40-year intervals from 1865 to 2065. There is no question that such a report is essential to understanding the forcing that would be employed in climate model calculations using this climatology and is thus essential to understanding the output that would result from such climate model calculations, and in this sense is an admirable activity and one to be encouraged. Although such documentation is essential to bolster confidence in subsequent calculations, the question remains whether publication of this documentation in the refereed scientific literature is the appropriate means of providing this documentation to the interested community, as opposed to simply being made available in online files. On reflection my decision is to favor publication. There is much that was done that goes well beyond "turn the crank" in terms of decisions that were made how to treat this or that issue and justification thereof. That said, there is also a lot of "turn the crank," but I must say that the crank seems very well turned.

As anyone who peruses this manuscript will immediately discern, that, including the appendices, it contains several dozen multi panel figures with each panel consisting of multiple global maps of various aerosol radiative quantities. In general I am a great fan of what Tufte (1983) refers to as "small multiples", multiple images in the same format that in the aggregate tell a story that cannot be told by a single image. Decipher the image once, and the deciphering pays off in the ready comprehension of the multiple images.

Where do all these numbers come from? And there are a lot of numbers. Each map presents results at 1˚ lat x 1˚ long or 64800 numbers. Of course these numbers cannot be accurately read off the figures, but the figures do serve to give a very valuable two dimensional picture of the pattern of the quantity being plotted.

The numbers are available at as netcdf files

ftp://ftp-projects.zmaw.de/aerocom/climatology/MACv2_2018/

so the user can download and manipulate (and use) them to his or her heart's content to bring out the results in much finer detail, or to use in subsequent calculations, compare with observations or other model calculations, etc. The website is alive at the time of writing of this review although some of the files seem to have been entered or updated subsequent to submission of the manuscript. I did not attempt to download or examine any of the files. Presumably the files will be frozen as of some date, and any changes will be noted, with prior versions remaining available.

In this context the question might be raised whether it is worth the (electronic) journal pages to present all these figures. I would say yes. I found it very interesting, for example, to compare TOA forcing and surface forcing efficiencies, (W m^{-2} AOD^{-1}), for different aerosol components and try to understand the reasons behind the spatial variation. The author does present some discussion to some of these spatial variations, though perhaps not enough. Maps are presented for both direct and indirect aerosol effects, and total aerosol effects and anthropogenic enhancement, by species (e.g., sulfate) or groups of species (e.g., organics), annual, and monthly. So quickly this adds up to a lot of maps and a lot of data files and a lot of bytes per file (I saw some files as large as 146 Mbytes).

From the perspective of a reviewer I must address the question of how the numbers are generated and the extent to which they can be believed, that is their accuracy. The approach taken by the author is to combine information about the distribution and properties of aerosols from both modeling and observations. It seems that the author places more confidence in aerosol amount (extensive variable: optical depth, column burden) as constrained by observations, but uses models to apportion to aerosol substances and to infer intensive properties (phase function, asymmetry parameter, effective radius, composition, single scattering albedo). This seems reasonable. Observations (from the surface or satellite) cannot give much information about the intensive variables, which variables greatly influence the forcing efficiency. On the other hand, available aerosol models yield a diversity in extensive quantities that would render them useless without constraint.

Here I might register a concern. The aerosol models used for the present study date from the mid 2000's as presented in the first AeroCom study (of which the present author was lead investigator) as documented in Kinne *et al*. (2006) and Textor *et al*. (2006), two enormously important papers. To my thinking one of the insights of the that study (perhaps not intended by the investigators) is shown in Figure 3 of Kinne *et al*. paper, which shows the total annual global AOD from some 16 models agreeing (for all but one) with surface based and satellite based products within 10% or so, but the components contributing to the total AOD differing substantially from model to model, e.g., a factor of 3 for sulfate, a factor of 5 for organic and black carbon. Similarly also for the intensive variable mass scattering efficiency, with Max/Min 6.7 for sulfate, 3.5 for black carbon, 2.8 for organics. Pretty clearly the agreement with observation of global annual AOD for each of these models within about 10% or so is a consequence of various compensations. Another enormously valuable figure in the Kinne *et al*. paper is Figure 4, which presents maps of spatial distribution (annual average) what the authors denoted "central diversity", max/min ratios of the central 2/3 of the modeled quantities, for yearly averages, which are several fold for several of the quantities, and more than an order of magnitude for aerosol water over continental regions. Variations in other model output quantities such as residence times of modeled aerosols are presented in the Textor *et al*. paper. One can only surmise that the divergence in maps of monthly distributions would be even greater, and more importantly that there is a large amount of compensation in the models for them to be able to obtain such accurate global annual mean AOD.

I go into this detail regarding the results of the AeroCom study because the chemical transport model results that serve as the basis for the present analysis, as described in the companion *Tellus* manuscript, are in fact taken from the 2006 AeroCom intercomparison, specifically as the monthly median values of 14 different models that participated in that study. So the model diversity reflected in the 2006 papers should raise if not a red flag, at least a yellow flag, as to the confidence that can be placed in the apportionment of optical properties, radiative effects, and CCN properties that are used in the study under review and that are the basis for attributing these effects to anthropogenic and natural sources.

Given this situation, should the present analysis be rejected out of hand? I do not think so. I think the study presented in the manuscript under review (and the companion paper submitted to *Tellus*) probably represents the state of the art in such analyses, although I do wonder whether there are not more recent (not necessarily more accurate) aerosol chemical transport models the results of which might have been used in the present study

I called attention above to the large divergence in aerosol water across the models, an order of magnitude max/min ratio in the central 2/3 of the models over continental regions in annual average. This should be viewed as a further red flag. The effect of relative humidity on the amount of condensed phase water in an aerosol and the resultant radiative effect are well known from a theory perspective (given knowledge of particle composition; e.g., Nemesure *et al*., 1995) but enormously difficult to represent in large-scale models. A brief period during which there is a relative humidity over 90 or 95% at the top of a boundary layer containing hygroscopic aerosol can dominate the instantaneous optical depth and contribute substantially to the 24-hour forcing but would never be captured models absent high 3D spatial resolution and temporal resolution. As a result, forcing based on average RH will certainly underestimate actual forcing.

A further question with respect to the publishability of the present manuscript is the extent to which the manuscript presents scientific findings. Here "findings" can be taken broadly. For example does the climatology developed change or confirm present quantitative understanding of aerosol forcing, at various levels of spatial and temporal resolution? Turning to the abstract, the second paragraph indeed reports such findings:

> Likely present-day global annual radiative effects for anthropogenic aerosol are (1) a climate cooling of -1.0 W/m2 at the top of the atmosphere (TOA), (2) a surface net flux-reduction of -2.1 W/m2 and (3) by difference an atmospheric effect of a +1.1 W/m2. This associated atmospheric solar heating is almost entirely a direct effect. For the climate relevant TOA response the indirect effect (-0.65 W/m2) on average dominates the direct effect (-0.35 W/m2). In contrast, at the surface the direct effect (-1.45 W/m2) dominates on average the indirect effect (-0.65 W/m2).

Of course, such estimates have been provided previously, importantly in the IPCC AR5 Assessment, which summarizes a large body of previous work. However a key distinction between that assessment and the findings quoted above is the absence in the abstract of any statement of uncertainty associated with these numbers, and as well comparison with prior estimates. To be sure, these uncertainty estimates are provided in the body of the manuscript, (p. 24, line 29),

> Considering these different uncertainties, a [-]0.7 to -1.6 W/m2 range is estimated for present-day aerosol forcing (assuming a year 1850 reference), with the best guess value aerosol forcing at about -1.0 W/m2. Hereby the less negative lower bound is more certain than the more negative upper bound.

(a minus sign before the 0.7 appears to be required but is missing), with some justification provided in the subsequent paragraphs. To my thinking such an uncertainty range needs much stronger justification than is provided, especially given the strong sensitivity of forcing to uncertainties in AOD, single scattering albedo, asymmetry parameter, and surface reflectance (e.g., McComiskey *et al*., 2008) as well as uncertainty due to aerosol water noted above and the uncertainty in attribution to anthropogenic components arising out of the model diversity. Similar considerations apply to the estimate of uncertainty in the indirect effect that is folded into the total forcing and uncertainty reported. I would hope that the author would re-think his estimate of uncertainty in light of these considerations, and also that his (revised) estimate of uncertainty be included in the abstract.

More broadly, I would hope that the author would reflect on the comments I have provided and perhaps elaborate in the manuscript on the issues raised or appropriately revise.

Finally, I commend the author on the enormous body of work represented in these two manuscripts. I would hope that others preparing such a forcing climatology would do such a careful and well documented job.

Additional comments, by page and line

1,22. At TOA?
1,38. ... is poorly defined; maybe better ... is quite uncertain.
3,28. radiative properties --> optical properties
16,36-7. Do not understand what is meant.

References:

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