

## ***Interactive comment on “Simple Plumes: A parameterization of anthropogenic aerosol optical properties and an associated Twomey effect for climate studies” by Bjorn Stevens et al.***

### **Anonymous Referee #3**

Received and published: 31 August 2016

The authors present a “simple” parametrization of a climatology for anthropogenic aerosols to be used in global climate models. The parametrization is simple in that the authors propose an analytical description, but in fact it is already quite complex in some aspects. The authors have put much more effort into the spatial dimension of their climatology than in its temporal or wavelength dependence. The concept has some value (besides its limitations) and can eventually be published in GMD. I have two sources of concerns: i) some of the assumptions embedded in MAC-v2-SP are not sufficiently justified and could fool the climate community if widely accepted and used, and ii) more clarity is needed in some of the notation and equations.

I am discussing first the issue of assumptions and limitations. Some are not really

[Printer-friendly version](#)

[Discussion paper](#)



discussed while some others are mentioned but are lost in the middle of other assumptions. Overall it makes it difficult for the reader to hierarchize their importance, and I suggest assumptions and limitations are discussed again in the conclusion section.

1/ By definition a climatology does not represent the high-frequency temporal variability. The authors (partly) discuss the implication of this when estimating the aerosol ERF. There can also be implications on the climate response, not so much on the mean climate response, but possibly on the higher moments of the climate response (which is pretty much an open question). I think this limitation should be mentioned somewhere.

2/ MAC-v2-SP assumes constant in time aerosol optical properties. This is a major issue for those interested in the time evolution of the aerosol forcing (which is by the way one of the motivations of the authors, page 2, lines 10-20). Figure 14 illustrates the issue very well and I do not understand why the authors say “on the global scale the effects are not large” (page 16, line 7). Incidentally I think that this assumption alone questions the results of Stevens (2015) as the larger BC fraction in the first part of the 20th century may have suppressed the forcing. Since the authors have already partly done the job (see their Table 6), why not parametrize Eq. 9 accordingly?

3/ MAC-v2 is observationally-based but observations hardly tell which fraction of the aerosol is anthropogenic and which fraction is natural. Hence there is quite a large assumption here, which has some impact on background aerosol levels as well. It seems that what is anthropogenic and what is not is part of MAC-v2 rather than MAC-v2-SP (page 4, line 17). Since Kinne et al (2016) is in preparation, the reader is pretty much left in the dark as to what assumptions are involved here, and why there is such a large change between MAC-v1 and MAC-v2. Incidentally the Editor should make a decision as to whether this manuscript can be published before the Kinne et al (2016) paper that it heavily relies upon.

4/ The authors assume there is no coarse-mode component to the anthropogenic aerosols. This is true in first approximation but should be mentioned somewhere else

[Printer-friendly version](#)[Discussion paper](#)

than just on Figure 1!

5/ The authors assume there is no anthropogenic aerosols before 1850. While I understand they do this because of the CMIP business, it is weird to publish a climatology of anthropogenic aerosols that has zero anthropogenic aerosols in 1850. Again this raises an issue for studies that focus on the early part of the instrumental period (see point 1/ above). The scaling to SO<sub>2</sub> and NH<sub>3</sub> is not well justified and highly debatable.

The notations and equations are generally-speaking well presented but I have a number of questions.

First it is not clear what is  $\tau_a$  in equation (1). It is described as an optical depth and is a function of  $z$  so I assume it is the (anthropogenic) aerosol optical depth from the surface to an altitude of  $z$ , but this doesn't square with whatever is shown in Figure 4 (well part of the problem is that I don't understand exactly what is shown on Figure 4, more later on this. . .). An alternative explanation is that  $\tau_a$  is not an optical depth but an extinction coefficient (unit  $\text{m}^{-1}$ ) that needs to be integrated vertically and indeed vertical integration is mentioned in a few places. Please clarify.

In equations (1) and (3),  $A(\lambda)$  should be  $A_i(\lambda)$  since it says it may vary from plume to plume. Same with  $\alpha$  even though I understand this option is not used in MAC-v2-SP.

In equation (4), the angle brackets do not have any special meaning as later in the manuscript so I suggest they are removed.

I understand that the general form of  $A_{ij}$  in equation (5) only applies to the second Gaussian. For the first (symmetric) Gaussian, the west and east variance are the same, i.e.  $\sigma_W = \sigma_E$ . Is this correct?

Page 6, line 26, in relation to my earlier comment, a column integral only makes sense if  $\tau_{550}$  is in fact an extinction coefficient. Please clarify.

In relation to equations (4) and (7), I assume that  $\int b_i(x, y, z) dz = 1$  but I could not find this. Can the authors confirm?

There must be something wrong in Eq. (9). If I understand correctly  $\Lambda$  takes a 0 or 1 value (or 0, 1, 2, ... value if I take the / to be an Euclidian division). But  $\Lambda = 0$  doesn't make sense in Equation (9) with a negative power of  $\Lambda$ . Please clarify. An illustrating figure of how  $\omega_i(\lambda)$  look like would be useful.

On equation (11), has the angle brackets changed meaning (from vertical integral to vertical average)? It doesn't look like a vertical average in Equation (12).

Below equation (11),  $\tau_g$  should be  $\tau_{bg}$ . More importantly, the  $a_N$  is outside the log in Equation (11) but inside the log in Equation (12). Please clarify.

Isn't it  $\tau_{bg}/b_N$  rather than  $\tau_{bg}$  that more or less corresponds to a background aerosol optical depth (well I know that  $b_N$  is dimensionless but nevertheless. . .).

How does figure 4 relates to the  $b$  function? I cannot make sense of the caption. Is it  $\sigma(z)$  normalized by the aerosol optical depth, i.e. the integral is 1? Please specify wavelength in the caption.

Other minor comments:

Page 2, line 6: I don't understand the logic here. Top-down approach has to factor in observations at some point, not just the "expected response of the climate system".

Page 3, line 21: change "(albedo)" to "and albedo" as scattering cross-section and albedo are two different (related) things.

Page 3, lines 30-31: the natural aerosol is all but well distributed if you think of desert dust and marine sea-salt!

Page 4, line 3: "absorption single-scattering albedo" is a nice oxymoron which I haven't come across before! "single scattering albedo" is enough.

Page 4, lines 26-27: I am not sure what the authors mean by a "lack of evidence of strong coupling between climate and aerosol forcing". If you think of the global mean temperature response, then surely it depends on the aerosol forcing. Are the authors

[Printer-friendly version](#)[Discussion paper](#)

talking about the patterns of T or precipitation response versus the patterns of aerosol radiative forcing?

Page 4, lines 29-30: this is true for the aerosol-cloud interactions, but the opposite would be true for the aerosol-radiation interactions. Overall, it is hard to guess which of time-varying aerosols and climatological aerosols produce a larger RF. It could be also that MAC-v2 has a clear-sky bias if it relies too heavily on satellite or AERONET data. The issues of time variations and representativeness are interesting but would need a more complete discussion than what is provided here.

Page 5 and elsewhere: change “gaussian” to “Gaussian”

Page 6, lines 17-18: please check grammar here.

Page 7, lines 23-24: I don’t understand the logic here. If the individual plumes have their own annual cycle, it doesn’t matter where it comes from. So the annual cycle may also come from an annual cycle in emissions. You just don’t want the annual cycle due to meteorology to vary too much from one year to another.

Page 8, line 30: delete last sentence

Page 12, line 5: the figures do not come sequentially here. Fig 10 should be Fig 9?

Page 12, line 13-14: the impact of the latest Kirkby paper on aerosol formation, CCN number and cloud effects are pure speculation at this stage. Would it make an effect at all given the large amount of nucleation already operating in the pre-industrial atmosphere?

Page 13, lines 9-11: I am not sure what “This” refers to, but a standard deviation ought to be positive.

Page 13, line 16: Ambiguous as the Twomey effect results in brightening. Change “Brightening” to “Switching on”.

Page 13, 20: there is no masking for absorbing aerosols on top of clouds

[Printer-friendly version](#)[Discussion paper](#)

Page 17, line 23: why do you trust the multi-model mean more than your own model here?

Figure 1: I assume the top right number is the global mean anthropogenic AOD at 550 nm. Please make sure the caption provides the wavelength (also on Figure 2). How does it compare to other estimates?

Figures 3 and 5: why “column” here? If you need to distinguish aerosol optical depth (a function of  $z$ ) and the vertical integral, then I recommend to use depth versus thickness.

Figure 5: please specify wavelength.

Figure 6 and 7: what is the meaning of the grey shading? No data?

Figure 8: the caption mentions 4 panels, the figure has only 2. What are the solid and dashed lines?

Figure 10: is 1.7 the average of the ratio or the ratio of the averages?

Figures 11-12: the unit was cropped on the top right of the panels.

Figure 11: The panels are not different measures of the ERF, as there is only 1 ERF.

Figure 13: it would be really useful to have the model range with a grey shading here.

Table 2: Ångström

---

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-189, 2016.

Printer-friendly version

Discussion paper

