

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 #2

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This study describes a new climatology, called MACv2-SP, of aerosol optical properties and cloud microphysical perturbations for use in climate modelling. The aim of MACv2-SP is to offer an alternative to online aerosol modelling, especially for studies of climate response where aerosol forcing diversity too often hides robust responses in climate model ensembles. The climatology also aims at being simple to implement while remaining realistic – to capture the essence of aerosol forcing mechanisms, so to speak. When implemented in the ECHAM climate model, the climatology exert an effective radiative forcing of -0.7 W m^{-2} , on the weaker side of the IPCC AR5 best estimate of -0.9 W m^{-2} .

The paper is well written and the climatology is well designed to achieve the aims

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listed above. Interestingly, MACv2-SP is rather complex, a testament to the complexity of aerosol processes in the atmosphere.

Scientifically speaking, the paper is difficult to review because the authors have cleverly described the climatology as a fit to a more comprehensive dataset (MACv2, described by Kinne et al.) while leaving to others the option to investigate the impact of the strong assumptions that are made in the paper. So a reviewer cannot easily criticise the aerosol science in the paper, because it is either a consequence of choices made in Kinne *et al.*, or of simplifying assumptions that can be explored by others if they wish.

In that context, I am satisfied that the paper describes the climatology well and that MACv2-SP is simple, but not too simple. For those reasons I recommend publication, but ask the authors to be a bit more critical in places to improve the discussion, and to explain why their radiative forcing efficiency is so strongly negative – see below.

In the future, it will be important to remember that the climatology is built with assumptions that affect the radiative forcing exerted by the Simple Plumes. Conclusions on climate impacts from simulations that use the climatology will need to be seen in that context.

1 Main comments

- There are assumptions and choices peppered throughout the paper that need to be better discussed because they directly impact the strength of radiative forcing that can be obtained when using MACv2-SP. (1) on page 4, lines 16–17, it would be good to tell the reader here why anthropogenic AOD has been reduced in MACv2. (2) in Figure 1, the centres of several plumes seem offset compared to the locations of industrial activities suggested by emission maps. The Australian and Indian plume positions, and to a lesser extent American, are particularly surprising. Why? (3) Figure 4 shows that compared to MACv2, the fraction of

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anthropogenic AOD is shifted to lower altitudes: non-zero fractions over 7 km are set to zero while the decrease in fraction near the surface is suppressed. What consequences may that have for cloudy-sky RF_{ari} and RF_{aci} ? (4) in section 2.2, why are industrial regions given a small annual cycle? Based on emissions or concentrations? Atmospheric chemistry exhibits an annual cycle, for example because of photolysis, so the basis for the annual cycle influences the results. (5) in section 2.3, optical properties are listed uncritically, yet need to be justified beyond the fact that they come from MACv2. Being small is little justification for an Angstrom of 2 and an asymmetry of 0.63 – ranges are much wider than that, even for “small” aerosols (e.g. see Table 1 of Dubovik et al., 2002). Similarly, the single-scattering albedoes assumed for industrial and biomass-burning aerosols seem to be at the low end of the observed ranges (same reference). (6) Equation 12 on page 10 assumes that τ_{bg} is constant with time, which is wrong for biomass-burning. Does that explain the absence of the South American and South African plumes from the radiative forcing distribution shown in Fig 11? (7) One important limitation to the method used to scale plumes in time (Page 14, Equation 14) is that the extent and “features” of the plumes are more linked to transport and chemistry than they are to emissions. So the choice of scaling with emissions, although simple, is not necessarily physical.

- For cloud-active properties (section 3), I am concerned that the analysis (and probably MACv2 itself) is based on AeroCom 1 models. Those models were run in the early 2000s. Yes: 15 years ago. During that time, progress has been made in modelling aerosols, clouds, and their interactions. I am sure the authors, who are involved in global modelling themselves, would agree that using a 15-year old model to characterise something as complex as aerosol-cloud interactions is unwise. So why not give the models a better chance to be useful and use AeroCom 2 (Ghan et al., 2016)?
- I am really surprised by the strongly negative clear-sky radiative forcing efficiency

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of -27 W m^{-2} obtained for MAC2-SP (Page 13, line 15). Why so negative? Just because of the single-scattering albedo assumptions, I expected a rather weak efficiency. For example, looking at Table 3 of Myhre et al. (2013), ECHAM5-HAM obtains an efficiency of -17 with a similar anthropogenic AOD and single-scattering albedo. Has the ECHAM6 radiative transfer code changed dramatically?

2 Other comments

- Page 2, line 6: “largely” – is that really the case? I had the impression that IPCC aerosol forcing estimates were strongly influenced by AeroCom-style models. Unless the authors are thinking of another kind of bounds?
- Page 2, line 26: the word “ideas” belittles the studies cited. Those studies present good evidence to make their case. More generally, the paragraph could usefully remind the reader that with the possible exception of circulation responses to ozone changes in the southern ocean, no dynamical responses have confidently been linked to a forcing agent, so aerosols are not alone in that regard.
- Page 3, line 10: “computationally-efficient” is not really the right word. Aerosol-chemistry models can be considered computationally-efficient for the amount of work they do – but they are obviously more expensive than simpler representations, like MACv2-SP, that do less.
- Page 3, line 30: The well-distributed nature of natural aerosols is perhaps true for marine aerosol and biogenic sources, but mineral dust or volcanic aerosol distributions share the heterogeneous nature of anthropogenic aerosols.
- Page 4, line 16: Please clarify what is meant by “absolute aerosol optical properties”.

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- Page 4, lines 24–25: It is concerning that a climatology made to better understand aerosol-circulation couplings (page 3, line 15) is based on the premises that those couplings are weak. I would hope that the authors can find stronger arguments to back their assumption on plume shape.
- Page 5, line 13 and Equation 3: Since A varies from plume to plume, shouldn't it be denoted A_i ?
- Page 6, line 11: “transport within the region ... prevailing winds”. Can this statement be clarified? I am not sure what it means.
- Page 7, lines 5–6: What heights below the surface? What kind of vertical coordinates are you using? Is the implementation as universal as you claim?
- Page 7, line 23: That statement is true insofar as human activities are driven by meteorology, I suppose.
- Page 8, line 27: Asymmetry parameter must be weighted by the product $AOD \cdot SSA$. It does not matter in this instance because g is constant, but please ensure that this is the case for future use.
- Page 10, line 35 (at the top of the page for some reasons) and Table 3: There is no such thing as a “coincident” or “corresponding” τ_f . N is retrieved in cloudy sky, τ_f in a clear-sky pixel “nearby” – which may be quite far indeed from the cloud and at a very different altitude. So there is little assurance that the aerosols which provide τ_f and the clouds which provide N have in fact interacted. That has always been the fatal weakness of those correlation studies and the reason why many models prefer to rely on aircraft-derived relationships instead. I am not saying that the method is not good enough for MACv2-SP – what matters here is that all models use same the perturbations. But it is always good to be clear about limitations and assumptions like these.

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- Page 10, first paragraph and Figure 8: The Figure also shows model-derived relationships but that is not discussed at all in the text? Even if models are not retained in the end to derive N , the Figure can still be discussed completely.
- Page 11, line 28: What is the definition of anthropogenic here. With respect to pre-industrial?
- Page 12, line 13: The Kirkby *et al.* paper is valid for ultra-pristine conditions where nucleation is easy. The applicability of CLOUD experiments to the real atmosphere is doubtful, to say the least.
- Page 12, line 31: Not quite the IPCC definition of instantaneous forcing, because it asks for the tropospheric state to be held fixed at the unperturbed (pre-industrial) values – the first call, in your model.
- Page 13, line 20: Cloud masking for aerosols above clouds depends on the absorption properties of the aerosol: RF_{air} can be enhanced by clouds if the SSA is below a critical value which depends on the reflectance of the cloud. I would not call those situations “masking”.
- Page 14, last paragraph of section 4.2 and Figure 12: Caution here: differences between IRF and ERF are also caused by internal variability, which unfortunately dominates adjustments in many cases. So differences should not be over-interpreted.
- Shouldn't section 5 come before the discussion on radiative forcing, which by definition implies the knowledge of a pre-industrial distribution?
- Page 14, line 20: Please clarify what you mean with “almost by definition” here.
- Page 14, line 24: I do not quite understand why “features” (sub-plumes) are involved here. What is the assumption?

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- Page 16, lines 10–12: That sentence is awkward. What is the subject of “would have made”?
- Caption of Fig 14: The caption refers to a value of 0.75, yet 0.73 is highlighted on the y-axis.

3 Technical comments

- Page 8, lines 28–30: Those lines seem to be relics of an older version and can be deleted.
- Figure 9: Adding panel titles to Fig 9 would greatly simplify its caption and improve its legibility.
- Figure 12, line 13: repeated word “has”
- Page 13, line 9: Is Stevens (2015) the correct reference for the 100-member ensemble?
- Page 13, line 16: I would put a comma after “brightening the background”.
- Page 32: Line starting “ISO codes...” looks orphaned from its Table.

4 References

Dubovik, O., *et al.* Variability of absorption and optical properties of key aerosol types observed in worldwide locations. *J. Atmos. Sci.*, 59, 590–608, 2002.

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Ghan et al., Challenges in constraining anthropogenic aerosol effects on cloud radiative forcing using present-day spatiotemporal variability, *Proc. Natl. Acad. Sci. U.S.A.*, doi:10.1073/pnas.1514036113, 2016.

Myhre et al., Radiative forcing of the direct aerosol effect from AeroCom Phase II simulations. *Atmos. Chem. Phys.*, 13, 1853–1877, doi:10.5194/acp-13-1853-2013, 2013.

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