

***Interactive comment on* “Effective radiative forcing in the aerosol–climate model CAM5.3-MARC-ARG” by Benjamin S. Grandey et al.**

Anonymous Referee #2

Received and published: 30 May 2018

In this study, the authors compare components of the aerosol effective radiative forcing and other relevant variables simulated by the CAM model using two (or three for some variables) different aerosol schemes, MAM3 (and MAM7 for some variables) and MARC. Differences depend on the component and whether one takes a regional or global view.

The paper is well written and richly illustrated. I like the decomposition by forcing components following Ghan (2013). I also like the way relevant aerosol physical distributions are compared before moving to comparing each forcing component. But the paper has three major flaws. First, the description of effective radiative forcing needs

[Printer-friendly version](#)

[Discussion paper](#)



to be sharper and refer to recent results on adjustments. Second, the paper is very diagnostic and lacks the deep explanation of causes of differences that the reader would like to see. Finally, the authors interpret differences between the two aerosol models by guessing they may be due to differences in lifetime / hygroscopicity etc. when they should instead rely on quantifying those differences in lifetime etc. The authors have chosen to make no comparisons to observations, which can only be acceptable if they provide deep understanding of the differences between the different aerosol schemes.

To address those flaws, I recommend major revisions.

1 Main comments

- The paragraph starting on Page 2 Line 7 should be sharper and reflect the precise definition of effective radiative forcing. That means introducing the concept of instantaneous radiative forcing and rapid adjustments. For more information, see Sherwood et al. doi:10.1175/BAMS-D-13-00167.1 . The results of the PDRMIP project would also be useful to frame the analysis of the paper, see for example Stjern et al. doi:10.1002/2017JD027326 - that is particularly true in section 3.2.2, where rapid adjustments (semi-direct effects) are not discussed at all for the moment.
- The paper is a list of differences and similarities between MAM3 and MARC, but the causes for those differences and similarities are never identified in a convincing way. Why are sulphate burdens different in the subtropics and mid-latitudes? What justifies the low hygroscopicity in externally-mixed OC in MARC (Page 7 line 30)? Why the area of positive DRE_{sw} by biomass-burning aerosols over the Sc deck of southeastern Atlantic disappears in MARC? (Figure 7) What are the changes in single-scattering albedo or absorption aerosol optical depth that would help explain differences seen in sections 3.2.2 and 3.2.3? In section 3.3.4

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

(Page 14 lines 6-8), the authors need to go beyond being surprised at the agreement between globally-averaged DeltaCREsw. The pre-industrial baseline is important for DeltaCREsw. So the explanation of the “surprising” agreement must lie in differences in both DeltaCDNC and DeltaLWP and year-1850 CDNC and LWP.

In addition, it is important to remember when discussing differences in sea salt (3.1.4), mineral dust (3.1.5), and surface albedo (3.4) that 2000-1850 differences are caused by feedbacks from changes in aerosol emissions, not changes in climate. So we are really looking at the impact of internal variability on wind speeds and precipitation. In that context, statistical significance is important. It would be useful to run the model several times with different initial conditions to ascertain which of the changes are noise and which are signal. That would also answer the authors’ question about the causes of mineral dust changes (Page 9 line 15).

- Section 3.1 (and all aerosol modelling papers really!) should open with a Table showing the mass budget of each aerosol component (sources: emissions and chemical production; burdens; sinks: dry and wet deposition; lifetimes). This is important because it provides clear explanations for the differences that are discussed later in the paper. Too often the authors resort to guessing what the differences are (for example: Page 7 line 27; Page 8 line 14; Page 9 lines 9-10; Page 11 line 5; Page 16 line 8) when they should be able to put hard numbers on them. In addition, publishing mass budgets allows the reader to check that the schemes are balanced!

2 Other comments

- Page 1, lines 15-16: The first “default” is not needed.

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

- Page 1, line 20: The abstract should also mention the comparison to MAM7.
- Page 2, line 4: Ghan (2013) is not the best reference for aerosol-surface interactions. More relevant would be Jiao et al doi:10.5194/acp-14-2399-2014
- Page 2, line 15: “of particular importance”. The pre-industrial aerosol state has also been cited as an important driver of ERF uncertainty, see Carslaw et al. doi:10.1038/nature12674
- Page 3, line 12: “Within each of these modes” That is not strictly correct, since not all species can be present in all modes. See Figure 1 of Liu et al. 2012. I suggest rephrasing to “Depending on the mode, ...”
- Page 4, line 3: The description of MARC in the Rothenberg paper is incomplete. Is there a paper that describes the current version of the scheme in more details, including a diagram like Plate 2 of Wilson et al. doi:10.1029/2000JD000198? If not, the present paper might be a good opportunity to do it. What are the mixing assumptions for emissions? I count 7 modes for MARC, excluding mineral dust and seasalt bins. Is that correct?
- Page 4, line 18: What is the mixing state of resuspended aerosols?
- Page 5, line 8: “due to a lack of year-1850 emissions files for MAM7” That is a poor excuse. Couldn’t the authors make those files?
- Page 5, line 18: “from some sources”. Be more specific.
- Page 5, line 31: Could also cite Lohmann and Feichter doi:10.5194/acp-5-715-2005 where ERF calculations methods originated. See their section 7.2.
- Page 6, line 6: Should reference Ghan (2013) again here for the formulas.

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

- Page 7, line 4: “reveals”: Aerosol column burden does not “reveal” total mass, it *is* by definition the column-integrated aerosol mass.
- Page 9, line 30: And MODIS collection 6 is also different from MODIS collection 5.1, making comparisons to satellite retrievals a moving target...
- Page 9, line 31: “insufficient to explain the differences”. How can the authors tell? It could be useful to compute the ratio aerosol optical depth to total burden to see if mean aerosol optical properties have changed.
- Page 10, line 24 and Figure 7: It is surprising to see DRE_{sw} peak over the Mediterranean, but it is possible that adjustments compensate instantaneous radiative forcing over the European continent.
- Page 11, line 30 and Figure 9: The peak in CCN_{conc} over the Middle East in MARC is also surprising. Why is it there?
- Section 3.4, Page 14 line 21: Why give up on the logic of looking at relevant distributions before looking at the forcing? It would make sense to discuss Figures S11-12-13 within that section before discussing DeltaSRE_{sw}.
- Page 14, line 27: Wouldn't aerosol-induced changes in precipitation be captured in DeltaCRE_{sw}?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-118>, 2018.

Printer-friendly version

Discussion paper

