Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-680-RC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the representation of aerosol activation and its influence on model-derived estimates of the aerosol indirect effect" *by* Daniel Rothenberg et al.

Anonymous Referee #2

Received and published: 30 November 2017

General comments:

Overall this paper provides a nice approach to gain insight into understanding the sensitivities of a climate model, and in particular the Aerosol Indirect Effect(AIE), to the choice of activation scheme. By using a single 'parent' model (in this instance MARC-CESM), they avoid many of the uncertainties associated with the myriad other model differences that are seen in comparisons such as the Aerocom study.

When it comes to aerosol activation schemes - and the effort and complexity that goes into refining them - my major concern is that the uncertainties in the meteorology (as well as the underlying dry aerosol population) might outweigh the detailed behaviour

of the activation scheme. While this paper does provide some information about the meteorology (e.g. LWP, cloud fraction), a little more insight into the mechanisms behind the sensitivities might be seen by exploring those aspects that have a direct impact on the activation (e.g TKE, deep convection), rather than emergent properties. Inclusion of this (see specific comment 1) would make the conclusions of the paper much more robust.

Beyond this, the paper is well written and I would expect it to be accepted for publication after considering these suggestions below.

Specific comments:

1) The benefit of a comparison that uses a common parent model (in this case MARC-CESM) is that a number of the uncertainties relating to the idiosyncratic non-linear interactions and their associated bespoke 'tunings' can be avoided. However, they cannot be removed entirely, even with the common driving model. In particular, the activation scheme is a function not only of the dry aerosol inputs, but also of the meteorology. Figure 5 aims to show some of the variation of meteorology, but it would be good to show those aspects that have a more direct impact, i.e. the updraft velocities (i.e. TKE) and the frequency of deep convection (which provides a different relationship between cloud and activation or scavenging processes). The former may explain to a certain extent the global under-prediction of CDNC, while the latter would shed light on the over-prediction in the tropics. It is apparent (but not directly commented on) that schemes that see an increase in the optical depth in the tropics (Fig 5c) also show a significant increase in the CDNC in the eastern equatorial Pacific (Fig 4). Do you see any changes in the convective activity in this region? Can you supply plots of frequency of convection or partitioning of precipitation between stratiform and convective? Ideally some plots/statistics of the above should be included, but if this information is not readily available, then some discussion relating to the role of these aspects should be included.

2) Also related to statistics on changes to the TKE; in the introduction you reference Hoose et al (2009) who show a (spurious) sensitivity to a lower bound on CDNC. You also have a lower bound in the updraft velocity of 0.2m/s. How often is this bound enforced for the different runs/schemes?

3) Figure 7b shows a significant difference between ARG and other schemes. However, there is much less difference between PCM and Nenes, i.e. schemes that are more 'sophisticated'. I think this should be highlighted, particularly where the key message in the abstract suggests an uncertainty (spread) of 0.8Wm-2. It would be interesting to know what the spread would be if you replaced ARG with other schemes such as those included in Ghan et al 2011.

Technical corrections:

P4, L30: OLS should be bold to be consistent with the other variants.

P4, L30: Could you expand what you mean 'for the supplemental heuristic'?

P5, L23: Missing close bracket

P7, L24: Could you elucidate where the enhancement by anthropogenic aerosol is captured by nenes and PCM please?

P8, L4-5: '..ensemble of aerosol-climate models,. . .' I presume these are the Aerocom models.

P8, L28: Do you mean Figure 7? This only shows the PD-PI. Maybe 5f?

P9, L24-25: Bracketed expression and cite are inconsistent (with spurious comma).

P10,L32: Missing close bracket.

P11,L8: I was slightly confused by this, since the correlation is positive. Perhaps rephrase as something like '. . .shows a correlation between pre-industrial CDNC and the indirect effect, such that Δ CRE decreases as PI CDNC increases'

P12, L3: You reference Ghan et al 2011, I think it should be Ghan et al 2013.

P14, L18: '. . .aerosol within AN adiabatically ascending. . .parcel, provideS. . .' missing words/letters suggested in caps.

P15, L13: 'form' should be from

P16, L9: 'module' should be model?

P16, L13: 'S_{math}' should be S_{max}

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