

Author's Response – “On the representation of aerosol activation and its influence on model-derived estimates of the aerosol indirect effect”

We would like to thank the reviewer for their helpful suggestions and feedback on our manuscript. To streamline the comment/dialogue process, we have reproduced the comments from the reviewer and added responses inline, using [blue text](#). We have addressed a list of “Specific Comments” directly, in similar fashion, along with recommended technical corrections

Reviewer #2

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 behavior 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.

[We acknowledge this key point as raised by the reviewer, and address it in more detail as noted in Specific Comment \(1\).](#)

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.

Unfortunately, we don't have the diagnostics for frequency of convection from these runs (we prioritized high-frequency aerosol and microphysical diagnostics). We do note that we've made an effort in past work (Rothenberg and Wang, 2016) to rigorously quantify the influence of meteorology (through vertical updraft velocity) on CDNC using the activation schemes included here, through its interactions with the dry aerosol input parameters, but we agree that in the context of online climate model simulations, the uncertainties arising from interactions with meteorology are a key part of the story here.

To comment specifically on Figure 4 – the “hot spots” of CDNC in the equatorial Eastern Pacific here are not necessarily increases in CDNC; rather, they are areas where the model-derived CDNC is much greater than that estimated from MODIS (Bennartz, 2007). Complicating the interpretation here, this is an area where the MODIS-derived inventory likely under-predicts CDNC itself (and shows very low CDNC values in Figure 4) because it uses an idealized cloud model and set of assumptions in its inference. The large “relative error” in CDNC here is partially a numerical artifact given the small values present in the observational dataset.

To zero in a bit more on potential changes in convective activity in this region due to the choice of activation scheme, we plot the present-day, annual average convective precipitation rates for the **arg_comp** case, followed by the absolute difference between this field and those associated with each of the other 6 activation schemes (similar to Figure 7b):

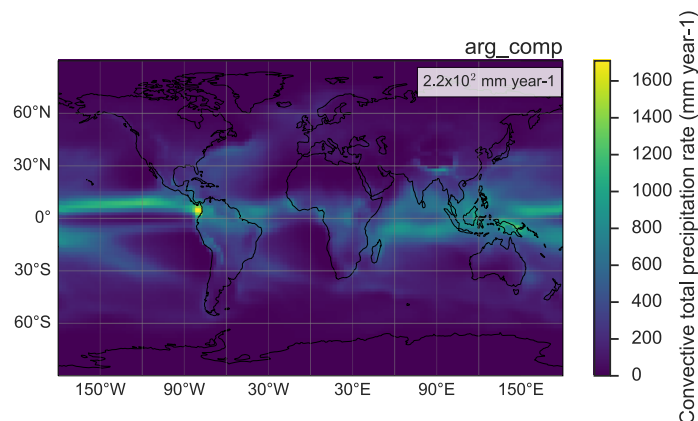


Figure 1) Annual average convective precipitation rate in the **arg_comp** case

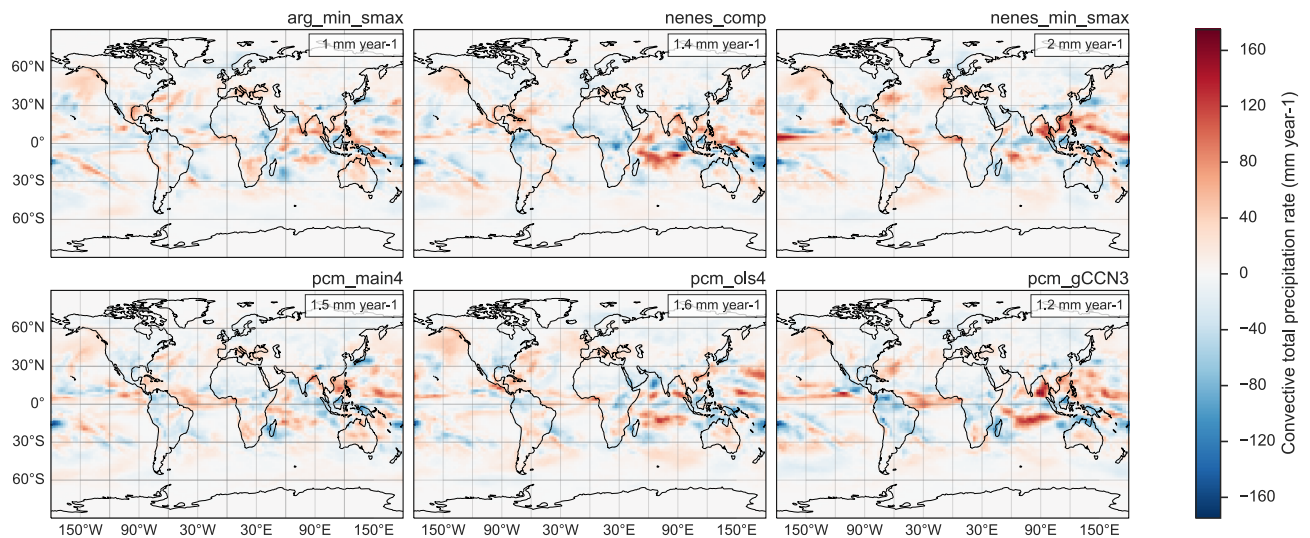


Figure 2) Difference in annual average convective precipitation rate between *arg_comp* case and cases indicated on each panel

In the Eastern equatorial Pacific, the difference in convective precipitation activity between each scheme is very small. We clarify some of these results in the discussion of Figure 5c in the manuscript. Regarding the potential distribution of TKE/sub-grid scale vertical velocity, we defer this point to the following comment.

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?

For a more complete discussion of updraft vertical velocity, TKE, and its relation to activation within MARC, we refer the reviewer to Section 2.1 of Rothenberg and Wang (2016). To directly answer the reviewer's question, we reproduce here Figure 1 of that work, which shows PDFs of updraft vertical velocity for near-surface grid cells (below 700 mb), broken down into land (red) and ocean (black) regimes, for a reference MARC simulation using the *arg_comp* activation scheme:

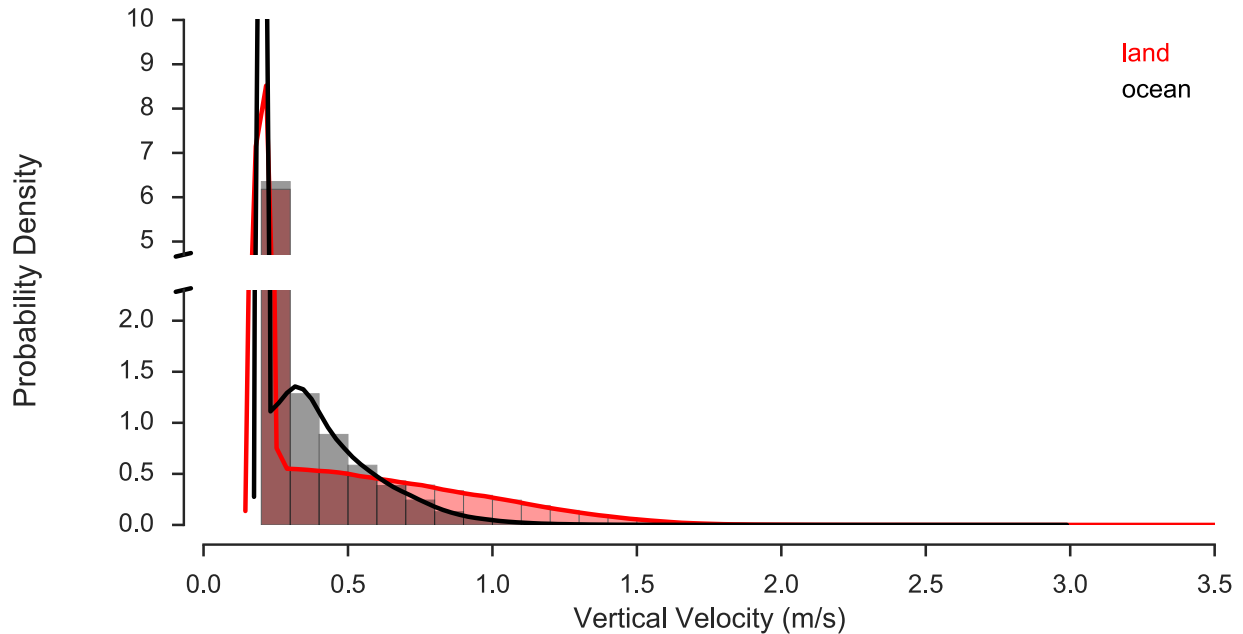


Figure 3 - Distributions of model-predicted instantaneous subgrid-scale vertical velocity for near-surface (below 700mb) grid cells broken down by land (red) and ocean (black) regimes.

The lower bound of 0.2 m/s is frequently invoked over both land and ocean regimes, although it is more frequent over land than ocean. We note that West et al. (2014) showed that TKE-based parameterizations do a good job of reproducing the spatio-temporal variability of updraft velocity, but tend to produce an unrealistically high frequency of any minimum threshold imposed on that velocity.

The results we have previously reported looking at offline activation diagnostics across the complete aerosol-meteorology parameter space sampled by MARC suggest that MARC's under-prediction of CDNC arises from a systematic bias towards fewer, smaller aerosol particles. Put another way, we simulate fewer CCN and as a result, fewer CDNC. A follow-up work rigorously comparing MARC to the CESM/MAM3 and CESM/MAM7 which is in preparation for submission for peer review looks at this issue in more detail.

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 0.8 Wm^{-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.

We have made some changes to the Discussion and Conclusions to emphasize this point, although we are careful not to suggest that 'sophisticated' here is equivalent to 'better', especially with regards to producing a more reliable estimate of the indirect effect. We agree

that it would be useful and very interesting to supplement the analyses presented here with additional simulations incorporating the full bevy of activation schemes documented by Ghan et al (2011); however, we faced some difficulties in obtaining implementations and/or independently reproducing some of the results previously reported by those schemes, hence the abbreviated list we studied here and in the works preceding this manuscript.

We hope to conduct this exercise in the future with additional activation schemes and aerosol-climate models.

Technical corrections:

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

Tweaked the phrasing in the sentence to clarify that this refers to **pcm_ols4**.

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

Re-worded the sentence to explicitly mention that this is the minimum-Smax heuristic.

- P5, L23: Missing close bracket

Fixed typo.

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

In this comment, we wish to emphasize two locations: the coast of eastern Asia, and eastern United States. We have added a clarifying remark in the manuscript.

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

Yes; we have added a remark to clarify this.

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

We mean Figure **7b**, which we use to compare the spread in SW CRE for the present-day between each scheme versus the change in SW CRE between PI and PD (shown in 7a). We have updated the reference as such.

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

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We fixed an error in the LaTeX parenthetical citation macro which led to this typo.

- P10,L32: Missing close bracket.

Fixed.

- 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’

Fixed using a phrasing similar to the proposed modification.

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

We do mean Ghan et al (2011) here; the reduction we’re referring to is summarized in Section 5, P. 19 (second paragraph), where it is reported that the ARG scheme yields SW CRE of -1.76 W/m^2 but the Fountoukis and Nenes (2005) scheme produces a smaller one of -1.60 W/m^2

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

We’ve included the suggested additional words/letters.

- P15, L13: ‘form’ should be from

Fixed.

- P16, L9: ‘module’ should be model?

Yes; fixed.

- P16, L13: ‘ S_{math} ’ should be S_{max}

Fixed.

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