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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Investigating processes that control the vertical distribution of aerosol in five subtropical marine stratocumulus regions – A sensitivity study using the climate model NorESM1-M" *by* Lena Frey et al.

**Anonymous Referee #1**

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The manuscript presents a sensitivity study of the processes controlling the regional aerosol vertical distribution in the NorESM1-M model, with a particular focus on marine stratocumulus regimes and using satellite lidar retrievals from CALIOP as an observational reference. While the analysis draws significantly on previous studies such as Kipling et al. (2016) which carried out similar sensitivity tests in another model focussing on the global scale, the present manuscript adds a significant and welcome new element in bringing this approach together with vertically-resolved observations. This combination of model sensitivity referenced to observations is then a valuable



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Discussion paper

extension to the existing literature on aerosol vertical profiles, and I'm pleased to recommend it for publication in ACP subject to the following minor comments:

**Specific comments**

**p.2, line 25–26:** the Twomey and Albrecht effects are not the only proposed indirect effects or rapid adjustments contributing to ERFaci – there are several others relating to ice nucleation, glaciation and the invigoration or suppression of convection. Some of these remain quite speculative, but not necessarily any more so than the "cloud lifetime" interpretation of warm rain suppression.

**p.3, line 87:** why is a lower threshold required here rather than only the upper one? Wouldn't a CAD score lower than $-80$ be even more certain to be aerosol rather than cloud?

**p.4, line 101:** please specify the type of interpolation used (linear in height coordinates?)

**p.4, line 115:** please specify approximately how high "the lowest eight model levels" reaches, and the profile applied (equal mass per model level? uniformly in height or pressure coordinates?)

**p.4, line 122:** is the $r_{eff}$ dependence prognostic via a size-resolved cloud scheme, or is it diagnosed separately at each time step from the aerosol?

**p.5, line 140** : please explain briefly *why* the single-process approach is appropriate here, e.g. because many of the tests are not easily framed in a parametric way.

**p.6, lines 166–172:** this paragraph is a bit unclear. Do the terms "emission levels", "model emission levels" and "predefined emission levels" here all refer equivalently to the set of the lowest eight model levels (extending from the surface to

approximately 510 hPa)? In the last case, please specify the approximate height or pressure range spanned by the lowest three levels.

**p.6, lines 184–185:** it should be clarified that in-cloud scavenging refers to nucleation and impaction *by cloud droplets*, while below-cloud refers to *impaction by falling raindrops/precipitation*. It should probably be mentioned explicitly if either in-cloud scavenging by cloud ice particles or below-cloud scavenging by falling ice/snow/hail/graupel is or is not included in the model.

**p.7, lines 209–210:** 10 $ms^{-1}$ is already a very strong updraught velocity outside of deep convection, and 30 $ms^{-1}$ even more so. Given the focus here is on stratocumulus regimes, which are usually characterised by lower velocities, please check if these values are correct and if so consider the impact that this choice might have on the results. (They might be expected to produce large supersaturations and thus activate aerosols down to a smaller size than would occur with a more realistic stratocumulus vertical velocity.)

**p.8, lines 226–227:** the approach taken to checking significance against the variability in the data should be briefly mentioned here (it's very welcome that this is indeed considered as the results are presented).

**p.8, line 241:** again, please clarify the type of interpolation used.

**p.9, line 247:** what is meant by an "increase in magnitude in the boundary layer" here, where the text is talking about a single data set rather than comparing two? Does this mean "increasing with height away from the surface"?

**p.9, line 259:** the limited model resolution may still be important here: even if a layer or plume can be instantaneously represented at that resolution, it may be lost to diffusion too quickly.

**p.10, lines 285–286:** if this is the strongest response, it's surprising that it's not shown.

**p.10, line 303:** it's surprising that dry deposition has relatively little impact even in regions where dust and/or sea-salt are significant components. Do the authors have an explanation for this, given that dry deposition is usually a major sink process for these species? (Unlike the finer particles for which, as is stated, in-cloud wet deposition normally dominates.)

**p.10, lines 310–311:** again, what is meant by "decrease of aerosol extinction in the boundary layer" in the control simulation (not in something else relative to the control)? Does this mean a profile which decreases with height away from the surface? Please clarify.

**p.11, lines 315–316:** might a shift in size as well as composition be significant here?

**p.11, lines 340–344:** Figure 11 also seems to show a change in the cloud top height, which ought to be discussed.

**p.12, lines 358–360:** as mentioned above, increased model diffusion at limited resolution may play a role here.

**p.12, lines 371–372:** if the local maximum simply cannot be resolved at this vertical resolution it's unsurprising that none of the model configurations can reproduce it.

**p.13, line 396:** nucleation scavenging is efficient at removing large particles too (at least the soluble ones like coarse sea salt). Isn't it just that dry deposition and sedimentation are *also* efficient for these, where as they play little role for fine particles?

**p.14, line 413:** deep convection may still be allowed in the model, but does it actually play any role in the stratocumulus regimes that are the focus of this study?

**p.14, lines 429–434:** see also White et al. (2019), who show that the difference between microphysics schemes (and their autoconversion in particular) can be greater than the non-albedo aerosol indirect effects themselves; and West et al. (2014),

who demonstrate the importance of sub-grid vertical velocity variability in another model.

**p.14, lines 441–442:** "aerosol above clouds in climate models underestimate absorption" doesn't make sense. Please rephrase to clarify – it's not the aerosol that does the estimating.

**Figure 4:** do the boxes represent the regions referred to in the text? If so, please state this in the caption and label them. There's also a missing "of" in the caption (should be "Global distribution *of* deviations. . . ").

**Figures 1, 5, 7:** it would be helpful if the boxes for the regions were also drawn on these figures, as on Figure 4, and the control included alongside each set for reference to avoid having to go back to Figure 1 on an earlier page to compare.

**Figures 3, 6, 8, 9, 10, 12:** There are a lot of lines with very similar colours on each of these. While there is a logic to using similar colours for each group of processes, this makes the plots harder to read as the lines on each plot are harder to distinguish. Since the groups are each plotted separately, using contrasting colours on each plot would make them more legible. If it's possible to reduce the number of lines further or adjust the scales to improve clarity that would also be welcome.

**Figures 9, 11:** more than half the vertical extent of these plots is unused – consider adjusting the vertical axis for the plots that don't go above the stratocumulus cloud top.

**Figures 9, 11, 12:** these plots are labelled with "Pressure (hPa)" on the vertical axis, but the same range (0–10) as the others using "Height (km)". Please check and ensure these are all labelled correctly and consistently.

## Technical corrections

**p.1, line 12:** delete comma after “model levels”.

**p.1, line 19:** delete comma after “heating”.

**p.2, line 22:** “amount of liquid water content” $\longrightarrow$ simply “liquid water content”.

**p.2, line 29:** “that requires” $\longrightarrow$ “which requires”.

**p.3, line 80 and throughout:** “cf.” is used repeatedly to introduce citations where it is probably not appropriate.

## References

West, R. E. L., Stier, P., Jones, A., Johnson, C. E., Mann, G. W., Bellouin, N., Partridge, D. G., and Kipling, Z.: The importance of vertical velocity variability for estimates of the indirect aerosol effects, Atmos. Chem. Phys., 14, 6369–6393, https://doi.org/10.5194/acp-14-6369-2014, 2014.

White, B., Gryspeerdt, E., Stier, P., Morrison, H., Thompson, G., and Kipling, Z.: Uncertainty from the choice of microphysics scheme in convection-permitting models significantly exceeds aerosol effects, Atmos. Chem. Phys., 17, 12145–12175, https://doi.org/10.5194/acp-17-12145-2017, 2017.

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