Response review #1

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 focusing 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 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:

We would like to thank reviewer #1 for his/her comments, which improved the manuscript. We address each of the comments in the following.

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.
  Thank you for your comment. We rephrased the sentence.

- 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?
  Yes, no lower threshold is required and we used here only values below -80. Apologies, this was a mistake in the manuscript. We have tested different CAD scores along the project and failed to update the used score in the manuscript.

- p.4, line 101: please specify the type of interpolation used (linear in height coordinates?)
  Yes, we used linear interpolation in height coordinates and added this information in the manuscript.

- 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?)
  The lowest eight model levels reach up to approximately 5,5 km on average. We have added this information to the manuscript. The default IPCC emissions are distributed following the recommendations by Dentener et al. (2006), see Seland et al. (2008).

- 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?
  The $_{reff}$ dependence is prognostic and depends on the cloud droplet number concentration. We rephrased.

- 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.
  To simply identify processes which are controlling the vertical aerosol distribution, a simple on/off approach is more feasible. Once important processes are identified a parametric way would help to improve certain processes by testing parameter ranges. The aim of our study is to identify processes and emphasize their importance. We clarified this in the manuscript.

- 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.
  We rephrased to clarify and added the pressure range for the lowest three model 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 fallingice/snow/hail/graupel is or is not included in the model.
  Thank you. We clarified in-cloud and below-cloud scavenging according to your suggestion. Scavenging in NorESM1 is only included for precipitation of liquid water, see Seland et al. [2008].

- 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.)
  Yes, indeed, 30 ms^{-1} is an extreme scenario and not a realistic case for the chosen stratocumulus regimes. We have clarified this in the manuscript. We chose this high velocity, since lower values within a more moderate range did not lead to a significant change in the simulated profiles.

- 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).
We have moved the explanation of our approach to test the statistical significance of sensitivity changes to Section 3.3.

- p.8, line 241: again, please clarify the type of interpolation used.
We specified 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"?
Yes, we meant an increase in magnitude with height. We clarified this.

- 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.
Thank you. We have added your comment to the manuscript.

- p.10, lines 285-286: if this is the strongest response, it's surprising that it's not shown. The experiment with increased sizes of primary emitted particles shows a strong response in the Canarian region compared to the other regions, but it has not the strongest response in the Canarian region compared to other experiments. We rephrased the sentence and also show the results of this experiment in Figure 3.

- 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.) In the experiment with dry deposition turned off, the model compensates the missing dry removal with an increased wet deposition. However, the opposite is not true for the experiment with wet deposition turned off, since dry removal is more efficient for larger particles. We have added this explanation to the manuscript.

- 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
Yes, we meant a decrease in aerosol extinction with height away from the surface. We rephrased the sentence.

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

- p.11, lines 340–344: Figure 11 also seems to show a change in the cloud top height,which ought to be discussed.
  Yes, you are right. We removed Figures 9 and 11, following the recommendations by reviewer #2. We decided that it is easier to follow the manuscript by focusing on the vertical aerosol extinction distribution, rather than to elaborate on cloud properties.

- p.12, lines 358–360: as mentioned above, increased model diffusion at limited resolution may play a role here.
  Thank you. We included your comment in the manuscript.

- 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.
  Yes, you are right. We removed this part of the sentence.

- 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?
  Yes, you are right. We rephrased the sentences.

- 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?
  In general, deep convection does not play a role in the stratocumulus regimes. However, one should be aware that switching off shallow convection still allows deep convection and transport of aerosols.

- 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.
  Thank you for the references. We included them in the manuscript.

- 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.
  Thank you. We rephrased the sentence.

- 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. . . ").
  Yes, the boxes represent the regions. We added labels and adjusted the figure caption.

- 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.
We added boxes indicating the regions and also included the control simulation in Figures 5 and 7.

- 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.
Yes, you are right. We intended to have similar colors within one experiment category. We have now chosen more contrasting colors for the different profiles.

- 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.
We have chosen 10 km as an upper limit on the vertical axis following Koffi et al. (2016). But since the study by Koffi et al. (2016) has a different study domain and we focus only on marine stratocumulus regions, we have adjusted now the vertical axis.

- 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.
We removed Figures 9 and 11, following the recommendation by reviewer #2. We corrected the label on Figure 12 (now Figure 10).

Technical corrections

- p.1, line 12: delete comma after "model levels".
Done.

- p.1, line 19: delete comma after "heating".
Done.

- p.2, line 22: "amount of liquid water content"→ simply "liquid water content".
Done.

- p.2, line 29: "that requires"→ "which requires.
Done.

- p.3, line 80 and throughout: "cf." is used repeatedly to introduce citations where it is probably not appropriate.
Done.

- 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, 369–6393,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.Interactive comment on Atmos. Chem. Phys. Discuss., h
  Thank you for the references. We included them in the manuscript.

References

Ø. Seland, T. Iversen, A. Kirkevåg, and T. Storelvmo. Aerosol-climate interactions in the cam-oslo atmospheric gcm and investigation of associated basic shortcomings. *Tellus A: Dynamic Meteorology and Oceanography*, 60(3):459–491, 2008. doi: 10.1111/j.1600-0870.2007.00318.x.