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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 #2

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General Comments

This article provides an interesting sensitivity study exploring the effect of changes in model parameters and aerosol emissions on aerosol composition and vertical distribution of extinction and number concentration, focussing on the marine stratocumulus regions. It analyses separately the impact of changing parameters one by one in the simulations, and concludes on the relative importance of the processes considered, showing that although some of them like the wet scavenging have a strong impact, none is able to reproduce the CALIOP observations.

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I think the analysis could be deepen and the interpretation of the results would gain in being extended. Even if the full chain of processes is very complex to analyses in a climate model, and without providing a full pathway analysis that would need extensive additional work, I think more insight could be gained by crossing the results and trying to interpret them (especially when they are surprising or when there are regional differences).

More direct comparisons with the observations could be provided to asses the effect of parameter changes, and spatial and temporal colocation could increase the robustness of the comparison (although they might not be straightforward to implement). More highlights could be put on answering the question: Could the model possibly represent better the observations if the relevant parameters where adjusted ? Would this set of parameters be realistic? Or are there fundamental discrepancies that cannot be resolved by parameter changes?

I think more simulations could be performed to either better distinguish between processes (convection parameterisation vs. aerosol transport by convection for instance) or investigate other key properties of the model, like its vertical resolution which could be essential in representing the low-level aerosol distribution. More details could also be provided on the model setup, on the characteristics of the parameterisations, and the choices made for the sensitivity study.Please refer to my specific comments hereafter for more details.

The paper is well written overall (some English editing is needed here and there, cf. my technical comments) and is organised in a straightforward way. Although I have numerous comments providing ways for clarifications and improvements, I believe this paper is a good contribution to the literature and I am sure its revised form will de publishable in Atmospheric Chemistry and Physics.

Specific comments

L11-13: is that really resolution that matters here or rather proper colocation? Similarly, not sure about the relevance of interpolation (cf comments hereafter).

L 80: "... underestimation of aerosols near the surface" any reference supporting this statment?

l99: To be fully consistent, model data should be also extracted along CALIPSO overpasses, at the times of the overpasses, before being averaged. Although daily mean works rather well in areas where there is no strong diurnal cycle in aerosols, proper spatial and temporal colocation (of the model data onto CALIOP measurements) reduces errors (cf. e.g. Schutgens et al., 2017). It may not be easily doable to extract profiles along CALIPSO track from the model, but discussions of sampling errors could be included.

L101-102: Is is really interpolation that is used here? As the CALIPSO data are on a finer vertical grid than the model, it would be better to average all the CALIOP data points located inside one model gridbox than to interpolate between two CALIOP levels to get the value at the central point of the gridbox.

L 110-111: could you please show the location of the vertical model levels at least in one of your plots (e.g. adding markers figure 1) or / and give the spacing between levels in the low to mid troposphere?

L 115: "the lowest eight levels" corresponding to what altitude (on average)?

L 121: be more specific: the cloud albedo and cloud lifetime effects are not directly parameterised, but the microphysics parametrisation takes aerosol into accounts and hence aims to represent them.

L 125: why is there a maximum precipitation rate? It seem odd if you do not specify here (as line 205) "before the autoconversion is switched off".

L 127: what means "production-tagged"?

L 129: "for convective clouds an in-plume approach is used i.e. the convective cloud

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cover is calculated explicitly": explain a bit more. What do you mean by "in-plume approach" how is calculated the convective cloud cover? How is it then passed to the large-scale? As convective clouds are parametrised, their cloud cover is surely not fully explicit. As there is no aerosol in the Zhang and McFarlane (1995) scheme, could you be more precise and, if they have been added in a more recent version, cite the relevant literature?

L 134: be more specific on the characteristics of the run, and/or give reference for AMIP setup.

L 159-165: Justify the choice of this emission dataset. Are they more realistic for the simulation period? Why not using realistic monthly emissions for the period of simulations as a control? And then either a different dataset, or a multiplicative factor on emissions for the sensitivity experiment?

L 183: are aerosols also liberated by evaporation of cloud droplets and raindrops? If yes, you could mention it in paragraph 3.1.

l 189: the original convection scheme should be described a bit more (here, or maybe rather section 3.1). Do you mean only deep convection here? What mean the full mixing of aerosols? the aerosol population is the same in updraughts and downdraughts? How about the impact of lateral entrainment then? By "the original scheme" do you mean Zhang and McFarlane (1995) which has no aerosol at all?

L190-196: - What happens in the model when shallow convection is turned off? Is it picked-up by the deep convection scheme? Or by the large-scale as it tends to be when all convection parameterisation if turned off? In any case, turning off shallow convection will not prevent the vertical transport needed to balance surface SW heating and atmospheric LW cooling. - Then, why not turning off only aerosol transport from convection parameterisation (looking at both shallow and deep convection separately)? That would give much clearer results on what is done by the parametrisation in term of aerosols, without having any direct impact on the dynamics, clouds, etc.

l200: what schemes are used?More description of the original scheme is needed (here or in section 3.1) before discussing its perturbations.

L 204-206: You could include the equations for autoconversion (and possibly accretion). This threshold on the radius has been introduced in models historically, partly to compensate for the lack of below-cloud evaporation, but there should not be any threshold as the processes are continuous. The threshold in precipitation is even more arbitrary as cloud droplet should continue to form raindrops no matter how much precipitation there is already (although accretion will then become much more significant than autoconversion, meaning in practice autoconversion might be of little or no effect). Unless the way the equations are written makes it unphysical, I would suggest trying to remove the two thresholds.

L 208: what is the “characteristic subgrid vertical velocity”? What will be the effect of changing it? Explain so that the reader can understand what the chosen values mean.

L 209: “high variability” in what sense?

L 220: is the monthly output obtained from online averaging over the month?

L 222-225: following my previous comment, is that also true for temporal sampling? Schutgens et al. (2016, 2017) suggest the opposite.

L241: again, is it really an interpolation? Averaging would be better.

L 249: what is the average BL height in these regions?

L 256: Indeed model resolution is too coarse (and probably also in the free troposphere up to $\sim$ 5.5 km). From figure 2, I guess AOD is also underestimated by the model? An interesting additional sensitivity experiment could be to refine the vertical grid in the BL and up to about 5.5 km.

Section 4.3: why not include CALIOP extinction profiles in the figures? This would be useful to compare not only sensitivity experiments with the control but also with the observations and see when they perform better than the control. Indeed, one big question is whether or not changes in model configuration can lead to results closer to the observations, so a more direct comparison is needed in the figure and in the analysis.

L 283: “Hence, by changing the size of ...” rephrase to make it clearer, e.g. “Hence, changing the size of emitted particles also leads to changes in emitted aerosol numbers”

l 289-290: “As a consequence . . ..” I do not understand. Something is not right in the way this sentence in constructed. Please clarify / rephrase.

L 303: The differences from tunring off dry deposition are actually almost non-existent, indicating that the dry deposition plays very little role (if any) in your simulations. Although dry deposition will affect mostly the biggest aerosols, I am a bit surprised that the impact is so small. How big is the impact on the total aerosol burden? Using CAMS, Wu et al (2018) show significant impact of the dry deposition scheme on BC burden (cf. for instance their figure4), and I suppose this could also be the case for dust (Johnson et al, 2012). Could you discuss that a bit? Do you think the dry deposition could be underestimated in your control simulation?

L 312: again, it would be helpful to plot the observed extinction profiles on the same figure.

l319-320: can you explain and justify this statement? How do you know the composition changes affects extinction more than the number concentration?

L320: again, I am surprised by the total lack of sensitivity to dry deposition.

Section 4.3.3: more careful description and analysis is needed: -l 326-327: No, there is no decrease in aerosol number above the BL according to fig 8. In all regions and at all heights, there is an increase in both extinction and number concentration. Can you interpret that? It might be related to other changes in the simulation without shallow convection; turning off only aerosol transport by shallow convection would make the interpretation easier. - L 329: do you mean you switched off entrainment completely in shallow and deep convective clouds (no lateral or below cloud entrainment of aerosol, momentum, environmental air, etc)? Turning off only the transport of aerosols by convection, but keeping entrainment unchanged otherwise would be the best way of testing the effect of deep and shallow convection parameterisations on aerosol transport. Reducing entrainment can have a strong effect on the characteristics of parameterised convective clouds (see e.g. Labbouz et al., 2018). -L 331-332: Again, this statement is not true, according to figure 8. More description and analysis should be provided here: noshallowconv leads to an increase in both extinction and number concentrations in all regions, however turning off convective entrainment leads also to an increase in number concentration, but to either no changes or even a decrease in extinction. - L 331-332: Fig. 9 is barely described. Is it really needed in the paper? I would suggest either to remove it, or to go much further in the analysis. What can be gained from it? How can it help in understanding how changing convection affects aerosol vertical distributions?

Figure 8: as comparison between the absolute values of extinction in the different regions is not the main focus here, but rather the effect of changing model configurations, you may consider adapting the scale so that changes in extinction are more visible.

l337: that means no precipitation from warm clouds, hence possibly an overall reduction of wet scavenging.

Figure 11: again, why looking at cloud properties if not to go further in the analysis? The study focusses on aerosols, so I think discussing cloud properties is interesting only if they help better understand aerosol response (or lack of response). Otherwise, figure 11 could be deleted.

Section 4.3.5: why is the effect on extinction so small that it cannot be seen on the figure? You should discuss this result a bit more and try to explain it, especially as it is different from Peers et al., 2016, as you mentioned in your conclusion.

L 367: showing observed extinction profiles on all of your figures would help assessing that more directly. This could be done by showing the markers, or indeed the standard deviation of the CALIOP profiles (based on the monthly-averaged, unless the comparison technique is changed following my suggestion of spatio-temporal colocation).

l395: Correct or clarify as it is almost impossible to see in most of the figures and it seems to be the opposite in the Peruvian BL.

l411-412: Modifying or turning off convective transport only (for shallow convection, deep convection, and both) would be an interesting sensitivity experiment.

L 481-482: some perspective could be added to actually give such a guidance. What should be done to improve the model? What are the next steps?

Technical corrections

The title could be improved, for instance changing it to "What are the processes controlling aerosol vertical distribution on Marine Stratocumulus region? A sensitivity study. . .." or to "Processes controlling the aerosol vertical distribution in five subtropical marine stratocumulus . . .". These are only suggestion, and I let the author decide whether they want to take any of them into account.

I think some commas should be deleted (e.g. l 139 "We note here, that changes ..." the comma is confusing here)

l46-51: Could you try to rephrase this paragraph a little bit? The first part focusses a bit too much on everything being "important". Also, no so clear what is "its". Try to focuss on the main message here and rephrase to convey it in a simpler way.

L73: CALIOP: write what is stands for. L 77: a lidar is made of a laser and detector. You already said that CALIOP is a lidar, so I suggest deleting "using a lidar and detector"

l 84: remove further ; you could ,also remove the reference at the end of the sentence

(or replace “cf.” by “following”)

l148: add “study” (after “sensitivity”)

l149: replace “can not” by “cannot” (here and also in other occurences like l 289)

Figure 4: in the caption, replace the first “deviations” by “differences”, and delete the second one : “Global distributions of differences in aerosol optical depth (left) and absorption aerosol optical depth (right) between the . . . ” l 301, l 307, and other occurrences: avoid the use of “disabling” in this context, replace it by e.g. turning off l 302: I suggest replacing cut off by switched off or turned off (here and in all other occurences) figure 5 : the control simulation is fig 1 not fig 2 l 314: replace “disabled” by simply “no” (or “with wet deposition switched off”) l 318: I suggest replacing by “in response to switching off wet deposition”

l414: “this convective scheme” ambiguous her (which one)?

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