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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Impact of the representation of marine stratocumulus clouds on the anthropogenic aerosol effect" *by* D. Neubauer et al.

**Anonymous Referee #2**

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The study by D. Neubauer et al searches to show that the magnitude of the aerosol effects on climate depends on the choices made in the model ( turbulence description, vertical resolution, etc) which affect the representation of marine stratocumulus clouds. The idea is novel and welcome, as it is important to stress that the magnitude of aerosol effects in climate models depends not only on how the aerosol cloud interactions are parameterized, but also on the representation of the clouds. The study is fairly well written, but several aspects could be further improved before being published in ACP.

General comments



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1. The fact that the aerosol effects, and in particular the aerosol cloud interactions, are poorly represented in GCMs should be stressed out in the introduction. The authors acknowledge that there is a lot of uncertainty in the prediction of these effects among the climate models, and that part of this is related to a poor representation of stratocumulus clouds. But it is important to also point out that the complex interactions between aerosol and clouds are represented very crudely in models, usually through a dependency of the precipitation rate on the cloud droplet number. While other dependencies which have been shown to play an important role in the response of the cloud to the change in aerosol loading in LES studies are still missing, for e.g. the dependency of the entrainment rate on the droplet number, or interactions between evaporative cooling of precipitation and absorption of solar radiation.

2. The authors do not manage to convey clearly enough how much changes in model physics and resolution influence in the end the aerosol effects simulated with the model they use, which is the main focus of the paper according to the title.

3. the authors should as well explain more clearly how much the tuning done in the various experiments to close the radiative budget matters for differences we see between each experiment and the control run. My concern is that the changes in stratocumulus we see between two simulations are not only due to the sensitivity test but also an outcome of the tunning of some parameters. The authors could avoid this controversy by saying that the changes we see between an increased VRES experiment for eg and the control one are not due only to the increased vertical resolution, but to the new model configuration necessary for the increased vertical resolution.

4. The authors should mention that the frequency of occurence of stratocumulus discussed in the paper, depends of course on the way they defined stratocumulus. As well, the authors conclude that the cloud lack in the model because the LTS is too weak. This is certainly not the only reason, the formation of clouds depends on many factors not ony the LTS.



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5. The results and discussions parts could be more focused and concise, bringing forward the lessons learned by the authors rather than describing the results.

Minor comments

1. introduction, page 13863 - the last paragraph feels out of place here

2. page 13687, line 8 to 13 - not clear, it should be rephrased

3. page 13688, point 1 : it should be stated that this formulation is for stable conditions

4. page 13691, lines 14 to 18: not clear, I thought the sst' s were fixed

5.page 13692, first paragraph: other processes that LTS matter for the cloud formation. Same for page 13693 lines 12 to 13.

6. page 13693, what is the LWP climatology from the Univ of Wisconsin based on?

7. page 13694, lines 4 to 14 - not clear what message this paragraph intends to emphasize

8. page 13694, last paragraph - the statement about the diurnal cycle is too strong. It is not that obvious that there is good agreement with the observations. Moreover, these are monthly means, it may well be that if you would look at individual times the model would perform much worse.

9. section 4.2.2 - this whole subsection would benefit from some revisions trying to emphasize more strongly the main points; the discussion about the PBL height could be removed, as the PBL height can be diagnosed in a 100 ways and conclusions that are drawn may depend on the diagnostic that is chosen. If the authors really want to discuss the experiment VRES47+STAB, why not support it by a figure? The last phrase of the section is an important finding, but it is drowned in rest of the text.

10. page 13700, lines 10 to 12 - this conclusion is controversial. The strong negative AAE in stratocumulus regions can be just an artefact of poor parameterizations in



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GCMs.

11. page 13700, last phrase: where is this conclusion inferred from?

12. page 13701, lines 5-6: why is the aerosol load lower in the high vertical resolution experiment

13. page 13702, lines 20-25 - the discussion of the results of the study by Nam et al is not clear.

14. page 13703, lines 25-27, the droplet concentration does not only depend on the aerosol loading but also on the vertical velocity and the available supersaturated water vapor

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