Response to reviewer comments on “The relative importance of macrophysical and cloud albedo changes for aerosol induced radiative effects in stratocumulus” by Daniel P. Grosvenor et al.

Comments from anonymous Referee #1

We sincerely thank the Referee for taking the time to review our paper and for providing constructive suggestions for improvement. We hope that we have fully addressed the Referee’s comments – our responses are listed below in non-bold font.

Specific comments

1. The authors use the model to partition the cloud response to aerosols into “macrophysical” (cloud fraction and liquid water path) and “microphysical” (droplet size) responses. Being able to use the model to understand the various mechanisms at work is one of the major benefits of having a reliable model, so I feel this is an important part of the paper. Since the authors point out that their model works to their satisfaction only in closed-cell SCu (p. 13 l. 30f.), the title and abstract should reflect that fact. (The title and abstract should also reflect that the results are based on a model and reflect a case study.)

In response to this comment, and also one made by Referee #2, we have changed the title to :

“The relative importance of macrophysical and cloud albedo changes for aerosol induced radiative effects in closed-cell stratocumulus: insight from the modelling of a case study.”

, to include the fact that this was a modelling/numerical study of a case study and that we primarily are concerned with closed-cell stratocumulus.

The first sentence of the abstract is also modified from :-

“Aerosol-cloud interactions are explored using 1~km resolution simulations of SE Pacific stratocumulus clouds that include realistic meteorology along with newly implemented cloud microphysics and sub-grid cloud schemes.”

to :-

“Aerosol-cloud interactions are explored using a 1~km resolution simulations of a case study of predominantly closed--cell SE Pacific stratocumulus clouds. The simulations include realistic meteorology along with newly implemented cloud microphysics and sub-grid cloud schemes.”

2. In Sec. 4.2.1, the distinction between LWP and LWPic is made. This leads me to assume that LWP refers to gridbox-mean LWP throughout the manuscript. If this is not the case, the manuscript should be changed where appropriate.

Apologies, but we should have referred to this as the "domain mean LWP" rather than the grid-box mean since we were comparing the overall LWP change to that in the cloudy grid boxes only (rather than considering the sub-grid cloud fraction). This has been changed. The model LWP values in the rest of the paper are based upon the grid-box mean values for each 1km grid cell.

3. The high model bias in LW fluxes is attributed to low bias in cloud altitude or cloud fraction (p. 20, l. 25). What about the cloud thickness? I realize that the effect of LWP on the LW flux probably saturates pretty quickly, but the modeled LWP peaks at pretty small values.

Thanks for the suggestion, this has been added as a possibility :-

shifted to too high LW flux values, indicating either clouds that are too low in altitude, cloud fractions that are too low, or clouds that are too thin. However, we note that cloud thickness would only be relevant for very thin cloud regions ($<\sim 20$ gm^{-2}) since the increase in LW flux with LWP saturates at low LWP values (Miller et al., 2015). As for the daytime results, there is a much

4. p. 15, footnote 1: more explanation is needed here; I assume "Poisson counting statistics" means that the uncertainty scales as $\sqrt{n}$, but that doesn't tell me whether the ranges quoted are 1σ, 90%, 95%, etc. confidence intervals.

We estimated the Poisson error bars for the frequencies of each histrogram bin based on $\pm\sqrt{n}$. The ranges quoted were then the minimum and maximum (vice versa) differences in frequencies between the model and the observation. The minimum difference was calculated as the upper bound of the error bar for whichever was lowest out of the observation and the model minus the lower bound for whichever was highest. And vice versa for the maximum.

However, based on the request to simplify this section by Referee #2 we have removed the information about the degree of agreement using the Poisson statistics as we felt it was adding unnecessary detail.

5. The authors are right to point out that the subgrid cloud scheme may play an important role even at fairly high resolution. However, one of the drawbacks of case studies is that it is difficult to tell which conclusions generalize (see my first specific comment above). Changing "demonstrates" to "suggests" on p. 32 l. 10 would make me feel more confident in the conclusion.

We have changed this sentence to :-

"This study suggests that it may be necessary to employ a sub-grid cloud scheme within the UM model for stratocumulus, even at 1~km horizontal resolution. This finding may also apply to other models."

Technical corrections

The manuscript, while well written, would benefit from thorough proofreading. In

addition, units are consistently italicized when they should be roman; I believe

copernicus.cls provides the \unit command for this purpose

Thanks, we will ensure that the final manuscript is proofread and we have used the \unit command throughout.

References

Miller, N. B., Shupe, M. D., Cox, C. J., Walden, V. P., Turner, D. D., and Steffen, K.: Cloud Radiative Forcing at Summit, Greenland, Journal of Climate, 28, 6267–6280, doi:10.1175/jcli-d-15-0076.1, https://doi.org/10.1175%2Fjcli-d-15-0076.1, 2015.