Response to Reviewer #2 of acp-2020-232

Dear Reviewer #2 (Johannes Mülmenstädt),

Thank you very much for taking your time to review our paper. We think that your comments greatly help improve the manuscript. We have revised the manuscript according to your comments as explained below with point-by-point responses to your comments. We hope that the revision is enough to address your comments to make the manuscript now acceptable for publication in *ACP*.

[RC]: ***Referee comment***
[AC]: Author comment

Reviewer #2 (Johannes Mülmenstädt):

[RC] *I have reviewed "Snow-induced buffering in aerosol–cloud interactions" by Takuro Michibata et al. The authors present an interesting set of sensitivity studies that show that prognostic snowfall in their GCM strongly reduces ERFaci compared to diagnostic snowfall, in large part because the longer residence time of snow leads to greater collection of cloud water by snow, which reduces the relative importance of warm phase ACI. Based on my own work, I think this is a plausible mechanism. There are a few potential weak links in the argument, which I will point out below. I don't think those should hold up publication of this potentially very useful result; after all, no paper is ever the final word on any topic. I recommend minor revisions to clarify the points I list below.*
[AC] We would like to thank Johannes Mülmenstädt for his carefully reading our manuscript and for giving insightful comments. We have revised the manuscript according to the referee comments.
In the revised manuscript, we have added a new estimate of multi-satellite ERFaci which considers the retrieval error based on Ma et al. (2018), as shown in Figure R3 (see **[AC3]** below). Furthermore, we have improved the method for estimating ERFaci in MIROC through providing "clean-sky ERFaci" based on Ghan (2013) to preclude contamination by aerosol-radiation interaction in cloudy-sky condition (see **[AC4]** for details).
The reply and corrections on individual comments are shown below.

Major points

[RC1] *l. 110: The tuning strategy needs to be described in more detail. The worry with retuning is that the ERFaci difference may not be due to the change that was intentionally made, but due to the retuning.*
[AC1] Thank you for sharing your recent study which documents the link between tuning processes and ERFaci as commented in **[RC7]**. In our study, we primarily tuned the warm rain efficiency by modifying scale factor for accretion rate but not autoconversion because the latter can influence the magnitude of ACI due to the direct relation to droplet number (Michibata and Takemura, 2015; Jing et al., 2019) and thus the precipitation initiation (Mülmenstädt et al., 2020). This is effective for modifying SW radiation, but if needed, cloud ice and snow processes were also tuned for modifying LW radiation by changing scale factors for the fall speed of hydrometeors, which may be uninfluential on ERFaci because they are not involved directly in the hydrometeor number densities. We have added these statements in this paragraph.

[RC2] *l. 170 ff.: This is a question rather than a comment. From the conclusion of the paper, I would have expected the relationship between dLWP and dSWP to be the opposite – that when there is more snow, it would lead to more efficient removal of liquid cloud water. Would you mind making these plots separately for supercooled and nonsupercooled water?*

[AC2] This is a very important point. Considering the instantaneous response, the more snow should result in more efficient removal of the underlying cloud water due to collection processes, as the reviewer pointed. However, this does not always mean that the relationship between dCLWP and dSWP should be negative because the increased CLWP with aerosols (dCLWP > 0) contributes to additional sources of rain and snow (dRWP > 0; dSWP > 0), which would make the positive relationship in the monthly mean scale. Although the increased response of CLWP with aerosols through PI to PD is basically the same for both DIAG and PROG due to the cloud lifetime effect, the precipitation-driven collection of cloud droplets in PROG buffers the dCLWP (not mean negative) and thus the magnitude of ACI. The significant buffering of ACI can be seen when the model prognoses snow (Fig. 4) particularly over anthropogenic regions where snow is abundant, which does not conflict the positive relationship between dCLWP and dSWP.
To understand how the dCLWP-dSWP relation depends on the cloud regime, we further looked at the plot for supercooled and nonsupercooled regimes separately (Figure R6). The relationship between dCLWP and dSWP is more robust in the supercooled regime than in the nonsupercooled regime, implying that the mixed-phase cloud microphysics is a key driver to the snow-induced ACI buffering. Although we did not obtain a negative correlation between dCLWP and dSWP from this analysis, a theoretical approach and idealized process modeling should be required for future study to solidify the process-level understanding of snow-induced buffering hypothesis.
We have added the following sentence in the revised manuscript (Line 238): "Furthermore, a theoretical approach (Glassmeier and Lohmann, 2016) and idealized process modeling (Glassmeier et al., 2019) are also required urgently to solidify the process-level understanding of snow-induced buffering hypothesis, which are our important future work beyond the present study."

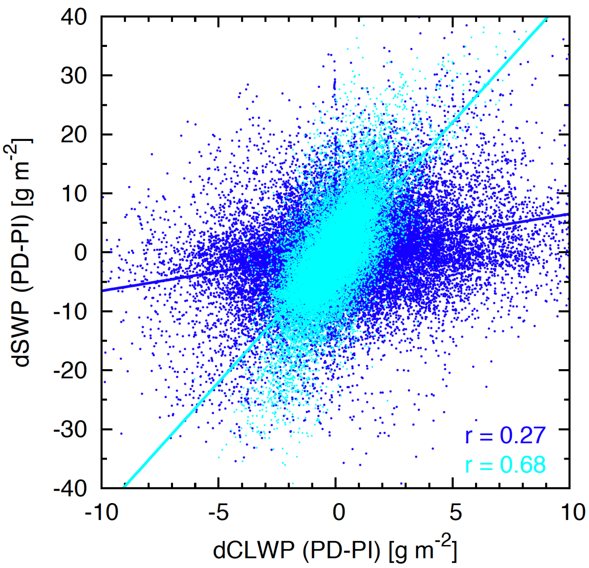

Figure R6. Relationship between the change in annual mean CLWP (nonsupercooled in blue and supercooled in cyan) and that in annual mean SWP, from the change in aerosols from PI to PD conditions, simulated using the PROG scheme. The correlation coefficients (r) are given in the figure.

Minor points

[RC3] *l. 39 ff.: I don't think this argument is logically consistent. First, the authors say ERFaci in GCMs is "too negative" compared to satellite studies (I would prefer "more negative", since satellite studies have their own problems). But then they cite a (problematic) satellite study with a very negative SW ERFaci to argue that the problem is with the models' LW ERFaci. The rest of the paragraph is fine, but I would suggest removing the first two sentences.*

[AC3] Yes, satellite retrievals also include uncertainties which may underestimate ERFaci because satellites undersample the weak-aerosol regime, where the cloud sensitivity to aerosol is largest (Ma et al., 2018). In the revised manuscript, we have added multi-satellite ERFaci which considers the retrieval errors based on Ma et al. (2018) as shown in Michibata and Suzuki (2020), but the DIAG scheme still shows more negative ERFaci (Figure R3). This implies that there might be unknown compensating aerosol warming effects that are missing in current GCMs, possibly through mixed-phase clouds (Lohmann and Hoose, 2009).
We therefore have remained the sentences but modified slightly as follows (Lines 39-42): "As a consequence of the challenges described above, GCMs tend to show more negative ERFaci than that inferred from satellite retrievals (Quaas et al., 2009; Chen et al., 2014) even though retrieval errors (Ma et al., 2018) are considered (Michibata and Suzuki, 2020). This suggests that current GCMs may be missing a compensating warming effect caused by aerosols.".

[RC4] *l. 111: Didn't Ghan (2013) show that the change in cloud radiative effect is not a good estimate of ERFaci because it contains pieces of ERFaci and ERFari?*
[AC4] In the revised manuscript, we have improved the method for estimating ERFaci in MIROC through providing "clean-sky ERFaci" based on Ghan (2013) to preclude contamination by aerosol-radiation interaction in cloudy-sky condition. This revision changes Figures 1, 2, and 4. We have also added a new estimate of multi-satellite ERFaci which considers the retrieval error based on Ma et al. (2018), as presented in Michibata and Suzuki (2020) (please see also **[AC3]** above).

[RC5] *l. 130: It might be worth pointing out that the Heyn et al. (2017) behavior is present in the zonal mean distribution (wherever SW ERFaci becomes stronger [weaker], LW ERFaci also becomes stronger [weaker]), but not in the global mean.*
[AC5] The following sentence has been added in this paragraph (Line 128): "The zonal distribution shows that stronger (weaker) LW ERFaci accompanies stronger (weaker) SW ERFaci, which is in line with Heyn et al. (2017).".

[RC6] *Fig. 3: The legend should say what the aggregation is, i.e., are the box and whiskers calculated based on monthly mean grid boxes? Also, in my mind, "susceptibility" implies susceptibility to a measure of aerosol; I would call the LWP, RWP, and SWP changes dLWP etc.*
[AC6] The following explanation has been added in the caption: "Box-whisker plots represent the 10th, 25th, 50th (black "+"), 75th, and 90th percentiles of the data within each bin based on the annual mean.". The figure legend uses dCLWP, dRWP, and dSWP, instead of "susceptibility".

[RC7] *l. 190: See my comment about retuning above. For example, in Mülmenstädt et. al. (2020), https://doi.org/10.1126/sciadv.aaz6433, we found that ERFaci is fairly insensitive to the cloud droplet number exponent but very sensitive to the liquid water mixing ratio exponent and the overall normalization in the Khairoutdinov and Kogan (2000) autoconversion scheme. If the retuning strategy for the change in Nc exponent involves changing other parts of the autoconversion, that may result in an overly strong apparent ERFaci change. Of course, which parameters ERFaci is sensitive will vary between models.*
[AC7] Thank you for your very important comment. We have added a more detailed description of the tuning (Section 2.2) as answered in **[AC1]**. We have also modified the sentence citing the suggested work.

[RC8] *l. 210: Is this list complete? E3SM has prognostic snow (Rasch et al., 2019), and I believe GISS Model E3 does too. HadGEM3 may do so as well.*
[AC8] Thank you for the information. The authors have contacted several modeling centers to know the latest model spec on the treatment of precipitation. Some replies suggested that now more GCMs include two-moment prognostic precipitation with snow radiative effect (e.g., E3SM, GISS-Model E3). In the revised paper (Lines 210 and 230), this sentence has been slightly changed citing a relevant paper (Li et al., 2020) which overviews the latest model status in CMIP6.

References:

Chen, Y.-C., Christensen, M. W., Stephens, G. L., and Seinfeld, J. H. (2014). Satellite-based estimate of global aerosol-cloud radiative forcing by marine warm clouds. Nature Geoscience, 7, 643–646.

Ghan, S. J. (2013). Technical note: Estimating aerosol effects on cloud radiative forcing. Atmospheric Chemistry and Physics, 13, 9971–9974. doi:10.5194/acp-13-9971-2013

Glassmeier, F., and U. Lohmann (2016), Constraining precipitation susceptibility of warm, ice- and mixed-phase clouds with microphysical equations, Journal of the Atmospheric Science, 73, 5003–5023, doi:10.1175/JAS-D-16-0008.1.

Glassmeier, F., Hoffmann, F., Johnson, J. S., Yamaguchi, T., Carslaw, K. S., and Feingold, G. (2019). An emulator approach to stratocumulus susceptibility, Atmos. Chem. Phys., 19, 10191–10203, https://doi.org/10.5194/acp-19-10191-2019.

Heyn I, Block K, Mulmenstadt J, Gryspeerdt E, Kuhne P, Salzmann M, Quaas J. Is the IPCC AR5 estimate of the aerosol effective radiative forcing too weak? (2017). Geophys Res Lett.

Jing, X., Suzuki, K., and Michibata, T. (2019). The key role of warm rain parameterization in determining the aerosol indirect effect in a global climate model. Journal of Climate, 32, 4409–4430.

Li, J. L. Frank et al. (2020). An Overview of CMIP5 and CMIP6 Simulated Cloud Ice, Radiation Fields, Surface Wind Stress, Sea Surface Temperatures, and Precipitation Over Tropical and Subtropical Oceans. Journal of Geophysical Research, 125, e2020JD032848

Lohmann, U. and Hoose, C. (2009). Sensitivity studies of different aerosol indirect effects in mixed-phase clouds, Atmospheric Chemistry and Physics, 9, 8917–8934.

Mülmenstädt, J., et al. (2020). Reducing the aerosol forcing uncertainty using observational constraints on warm rain processes, Science Advances, 6, eaaz6433.

Ma, P. L., Rasch, P. J., Chepfer, H., Winker, D. M., and Ghan, S. J. (2018). Observational constraint on cloud susceptibility weakened by aerosol retrieval limitations. Nature Communications, 9, 2640. doi:10.1038/s41467-018-05028-4

Michibata, T., and Suzuki, K. (2020). Reconciling compensating errors between precipitation constraints and the energy budget in a climate model. Geophysical Research Letters, 47, e2020GL088340. https://doi.org/ 10.1029/2020GL088340

Quaas, J., Ming, Y., Menon, S., Takemura, T., Wang, M., Penner, J. E., et al. (2009). Aerosol indirect effects general circulation model intercomparison and evaluation with satellite data. Atmospheric Chemistry and Physics, 9, 8697–8717. https://doi.org/10.5194/ acp-9-8697-2009

Rasch, P. J., Xie, S., Ma, P.-L., Lin, W., Wang, H., Tang, Q., et al. (2019). An overview of the atmospheric component of the Energy Exascale Earth System Model. Journal of Advances in Modeling Earth Systems, 11(8), 2377–2411. https://doi.org/10.1029/2019MS001629

Thank you very much again for reviewing our paper.

Sincerely yours,

Takuro Michibata