

Interactive comment on “Ensemble daily simulations for elucidating cloud–aerosol interactions under a large spread of realistic environmental conditions” by Guy Dagan and Philip Stier

Michael Diamond (Referee)

diamond2@uw.edu

Received and published: 12 March 2020

This study is a follow-up to a paper exploring two case studies from the NARVAL campaign that uses ensemble simulations of two months (of which the original cases were a subset) to analyze the robustness of inferences regarding aerosol–cloud interactions that can be made on the basis of a small number of cases. Certain changes (such as in shortwave reflections and boundary layer deepening that lowers lower tropospheric stability) appear robust whereas others (such as cloud fraction and precipitation changes) appear less so. Seasonal differences in response can be explained via

C1

different responses in different cloud regimes, particularly due to ice-phase effects in deep clouds during the summer.

The manuscript is in very good shape and only requires some very minor revisions, in my estimation. If not for the comment below regarding the reasonableness of the “lower bound” language, I’d be happy to accept as is.

Specific comments:

Page 2, Line 45: “As the anthropogenic activity...” is phrased somewhat awkwardly. Perhaps you can simplify to something like “Anthropogenic aerosol emissions may thus perturb Earth’s radiation budget both directly by scattering and absorbing light and also indirectly through these cloud-mediated mechanisms.”

Page 4, Line 116: I’m glad you address this point. However, did you mean “interactions” or “feedbacks” instead of “involve”? Also, a relevant citation for the aerosol scavenging idea:

Yamaguchi, T., Feingold, G., & Kazil, J. (2017). Stratocumulus to Cumulus Transition by Drizzle. *Journal of Advances in Modeling Earth Systems*, 9(6), 2333–2349. doi:10.1002/2017MS001104

Page 5, Line 138: I’m not convinced this is a reasonable lower bound, given that the relatively small domain size with fixed boundary conditions (which you argue would lead to an underestimate of aerosol effects) is not the only potential source of error, or necessarily the largest. I’d either like to see a fuller explanation of why the estimates should be seen as true lower bounds or a weaker statement simply explaining this particular source of error would tend to underestimate the effect compared to a simulation with a larger domain.

Page 6, Figure 2: It would be helpful if “LP” were defined somewhere in the text in addition to in the figure captions.

Page 9, Line 226: I would add “likely” between “would” and “further” given that ice-

C2

phase microphysical changes can be quite complex.

Page 11, Line 275: How significant is 12 versus 8 in this context? Is there any way to quantify the variability we could expect in deep-cloud days due to chance?

References: There are some typos and weird formatting issues with some references. A quick proofread should sort most of those out.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-949>, 2019.