EGUsphere, referee comment RC1
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Comment on egusphere-2022-777

Anonymous Referee #1

Referee comment on "Equilibrium climate sensitivity increases with aerosol concentration due to changes in precipitation efficiency" by Guy Dagan, EGUsphere, https://doi.org/10.5194/egusphere-2022-777-RC1, 2022

Summary

The author presents results from a series of idealized simulations of radiative-convective equilibrium that demonstrate a link between aerosol concentrations and equilibrium climate sensitivity (ECS). Specifically, they find that ECS is 0.6-0.7 K higher in simulations with very high concentrations of cloud condensation nuclei (2000/cc) than in simulations with lower cloud condensation nuclei concentrations (20/cc and 200/cc).

The author's experiments are well-motivated and well-designed, the analysis is easy to follow, and the manuscript is well-organized and easy to read. More broadly, the work makes an important advance in exploring connections between cloud-aerosol interactions and cloud feedbacks, two very active areas of research that (as the author points out) are more distinct than they should be. For these reasons, I think this is research that ACP should ultimately publish. However, I do feel that the author may overreach in discussing the implications of their work (see major comment 1), and I feel that the paper would benefit from some additional discussion of robustness (see major comment 2). For these reasons, I'm recommending minor revisions before the paper is published.

Major comments

1. Evidence for a practically-important link between aerosols and ECS: The author acknowledges on line 127 that the range of aerosol concentrations explored in their simulations covers an extreme range of conditions. I think this is great for developing understanding, but one of the author's main conclusions---that there is a strong link between aerosol concentrations and ECS that may have implications for future climate change---seems to rest entirely on a simulation with an extremely high aerosol concentration (2000/cc). (The ECS difference between simulations with aerosol concentrations of 20/cc and 200/cc is very small, and figure 1 shows that ECS is actually

slightly larger at 20/cc than 200/cc when CO2 is increased from 280 to 560 ppm. Robust increases in ECS only appear once aerosol concentrations are increased to 2000/cc.) Despite this, the author seems quite confident in concluding that the results "suggest a strong connection between cloud feedback and aerosol-cloud interactions" (line 380) and that they "might mean that the reduction in global aerosol emissions could lead to a reduction in ECS" (line 386). Are these conclusions really justified, given that extremely high CCN concentrations are required to produce a significant change in ECS? Providing a number for the current global-average CCN concentration would provide some useful context. If it's O(200/cc) and not O(2000/cc), then I think the results of the author's simulations actually suggest that future reductions in aerosol emissions are unlikely to significantly change ECS.

2. Changes in shortwave vs. longwave cloud radiative effects: The author proposes a link between aerosol concentrations and ECS that (as best as I can distill it) relies on the following causal chain:

Higher aerosol concentrations -> autoconversion more sensitive to temperature -> larger increases in precipitation efficiency with warming -> more efficient depletion of cloud condensate -> lower cloud water path -> less cooling from shortwave cloud radiative effects -> higher ECS.

A crucial part of this causal chain is that lower cloud water path leads to less cooling from shortwave cloud radiative effects (which increases ECS) without also leading to less warming from longwave cloud radiative effects (which would reduce ECS). It's not clear to me how robust this asymmetric shortwave vs. longwave response is likely to be, though, and it seems plausible that it might be sensitive to both the microphysics and radiation scheme. I don't think it's necessary that the author include simulations with alternate microphysics and radiation schemes---RCE simulations with interactive surface temperature are expensive, and the simulations the author presents are enough to formulate an interesting hypothesis---but I do think this point deserves some discussion as a potential weak link in the causal chain that deserves further exploration in future work.

Minor comments

1. Lines 71-73: Not all of the papers referenced here use the same definition of precipitation efficiency---Lutsko and Cronin 2018 define it as the ratio of surface precipitation to column-integrated gross condensation, not column-integrated condensed water path. A little bit of discussion about similarities and differences between different precipitation efficiency metrics would be helpful, as would some justification (perhaps later in the paper) of the author's decision to focus on the ratio of surface precipitation to column-integrated condensed water path.

2. Lines 123-124: I think the author should clarify here that S is estimated diagnostically

in SAM. (SAM uses total non-precipitating water as a prognostic variable and diagnoses water vapor and cloud water using a saturation adjustment scheme before calling microphysics routines, so there's no prognosed supersaturation.)

3. Lines 132-133: Just to confirm: by this, does the author mean that they've enabled the option to use effective radii from the microphysics scheme to compute effective radii for radiation? (I ask only because this is not what SAM does by default---you have to edit grid.f90 to enable the relevant flags.) Assuming the answer is yes, a slight change to wording could make this clearer: something like "the model is configured to pass effective radii from the microphysics scheme to the radiation scheme".

4. Line 147: Are fields saved as snapshots or averages?

5. Lines 152-153: What profiles are used for other trace gases?

6. Table 1: It's difficult to interpret this table without knowing definitions for the cloud feedback parameter, hydrological sensitivity, and precipitation efficiency. Could the author provide references in the table caption to locations in the text where these quantities are defined?

7. Line 226-227: Is there a reason the water vapor feedback isn't also listed as a relevant clear-sky feedback?

8. Line 284: The precipitation efficiency metric plotted in figure 5 is different from the metric used in Lutsko and Cronin 2018. Could the author clarify why they expect the two metrics to change similarly with warming?

9. Lines 289-291: I think it's a bit strange to say that a change in epsilon *causes* more efficient depletion---really it's a *measure* of the efficiency of depletion, in that an increase in epsilon means that some combination of processes are changing to produce the same surface precipitation at lower condensed water paths.

10. Lines 326-327: Isn't a simpler explanation just that a similar cloud droplet number concentration (controlled by Na) and larger qc (which increases under warming in RCE, and which Lutsko and Cronin 2018 analyze in detail) implies a larger mean cloud droplet radius?

11. Discussion of figures 6-7: Is there a way to explain *why* there are larger increases

in high qc and large rc in high-Na simulations? Or are figures 6 and 7 a purely diagnostic exploration of why autoconversion is more sensitive to temperature when Na is high? (Either is fine---I'm just not sure how complete an understanding I'm meant to have of the results in figures 6 and 7.)

Typos

1. Line 202: qc -> qv