Reply to the review of:

Equilibrium climate sensitivity increases with aerosol concentration due to changes in precipitation efficiency

I would like to thank the reviewers for their constructive and thoughtful reviews that helped me improve this paper.

Below please find a point-by-point reply to all of the reviewers' comments (in blue). In addition to the changes listed below, the colours schemes used in the figures were adjusted to ensure that they allow readers with colour vision deficiencies to correctly interpret them. Furthermore, the use of "rain efficiency" in the title was replaced with "precipitation efficiency", which is more commonly used.

Reviewer #1:

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.

Reply: I would like to thank the reviewer again for providing these constructive comments. I am happy that the reviewer found this work to make an important advance.

Below I detail the changes made to the revised manuscript following the reviewer's comments.

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 CO_2 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.

Reply: Thank you for this comment. I agree with the reviewer that the aerosol range covered here is probably too extensive for most places over the globe. This is used, as the reviewer mentioned, to develop physical understanding. I also agree that the typical aerosol range observed in the atmosphere would affect the magnitude of the total ECS sensitivity to aerosol loading. Hence, following this comment, a caveat was added to the revised manuscript. In addition, in number of places in the abstract and conclusion sections the wording was modified to better reflect the uncertainties involved in the main conclusions (see details below).
Please note that, as aerosols are highly variant in time and space (varying over more than 3 order of magnitudes), the global-average CCN concentration is probably not a

very meaningful quantity. In addition, in the real atmosphere different locations would have a different dominated cloud regime. It was shown before that ACI mechanisms are cloud regime dependent (Altaratz et al., 2014; Dagan & Stier, 2020b; Gryspeerdt & Stier, 2012). Thus, aerosol pollution at different geographical locations (dominated by different cloud regimes) would drive a different cloud response, and hence could also results in a difference ECS sensitivity. Integrating all these variables, i.e., accounting for the local aerosol range and cloud sensitivity, would be an important but very challenging task which future research should aimed at.

The additions to the revised conclusion section following this comment:
"In addition, the results presented here suggest that the sensitivity of ECS to aerosol loading might not be linear (Table 1). Hence, the dynamical aerosol range present at different geographical locations would affect the total ECS trend."

In addition, in order to better reflect the uncertainties involved, the word "strong" was replaced by "possible" in the sentence the reviewer is referring to in this comment:

"The results presented here suggest a possible connection between cloud feedback and aerosol-cloud interactions."

Similar changes were applied to the Abstract:

"In this paper, using idealized cloud resolving, radiative-convective-equilibrium simulations, with a slab ocean model, we show that aerosol-cloud interactions could affect cloud feedback."

"These results indicate a possible connection between cloud feedback and aerosol-cloud interactions."

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.

Reply: Thank you. Indeed, a-priory, an increase in precipitation efficiency with warming can result in either negative cloud feedback (due to less longwave warming by high anvil clouds, e.g., Lindzen et al. (2001)) or positive feedback (due to less shortwave cooling, e.g., Li et al. (2019)). This is explained in the introduction:

"An increase in ϵ with warming represents more efficient depletion of the water from the clouds, thus affecting the radiation budget. On the one hand, increase in ϵ with warming was suggested to reduce the anvil cloud coverage and hence increase the outgoing longwave radiation (Lindzen et al., 2001; Mauritsen and Stevens, 2015), thus producing negative feedback. On the other hand, however, it was recently shown that the longwave effect of an ϵ increase is over-compensated for by changes in the shortwave flux (Li et al., 2019), i.e., a large reduction in the cloud optical depth, driving a reduction in the shortwave cooling effect of clouds, dominates the response."

In our case, consistent with a recent study (Li et al., 2019), the positive shortwave feedback dominates over the negative longwave feedback, thus, the ECS increases. While a similar trend was shown with a different model (Li et al., 2019), I agree that it is not yet a consensus that the shortwave feedback always dominates over the longwave feedback, and that the net effect of an increase in precipitation efficiency with warming could be model dependent. Hence, a caveat was added to the revised conclusion section:

"We note that the increase in the total (shortwave plus longwave) cloud feedback parameter with the increase in precipitation efficiency is a result of a stronger shortwave effect (Li et al., 2019) than a longwave effect (Lindzen et al., 2001) in the simulations presented here. Future work should examine the robustness of this trend in different models, and with different microphysical and radiative schemes. Moreover, the response of precipitation to changes in aerosol concentration might be microphysical representation depended (White et al., 2017), and hence should be examined in the future under different microphysical schemes (conceivably in a multi-model intercomparison project focusing on aerosol effect on RCE simulations)."

Furthermore, I believe that examining the co-dependency of aerosol-cloud interactions and cloud feedback with a wide variety of models could be very informative. Hence, currently I am initiating (together with Philip Stier, University of Oxford and Sue van den Heever, CSU, and with the help of Allison Wing, Florida State University) an aerosol-focused RCEMIP (radiative convective equilibrium model intercomparison project) stage (using fixed SST rather than interactive SST as used here for reducing the computational demand). This will enable us to examine the results reported here under a wide variety of models including different microphysical and radiative schemes.

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.

Reply: The definition of precipitation efficiency used here follows the definition of Li et al. (2022), which is, indeed, slightly different from the definition of Lutsko & Cronin (2018). However, Li et al. (2022) showed that these two definitions are highly corelated (r=0.86). Hence, the exact definition used here is not expected to change the main conclusions. In addition, the definition used in Li et al. (2022) and here can more easily be applied to observational data-sets as well as to global climate models (and not just to cloud-resolving simulations). Hence, using Li et al. (2022) definition better set the stage for future work examining our results with other data-sets. For this reason, the definition used in Li et al. (2022) is adopted here.

Following this comment, a clarification and justification were added to the revised manuscript:

"Please note that the precipitation efficiency definition used here, following Li et al. (2022), is slightly different from the definition used in Lutsko and Cronin (2018). However, the two different definitions were shown to be tightly correlated (Li et al., 2022), thus, the exact definition used is not expected to change the main conclusions. In addition, the use of this definition will enable easier comparison with observations and global climate models in the future."

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.)

Reply: Thank you. In the revised manuscript this is now explicitly mentioned:

"The cloud droplet number concentration source assumes that the number of activated CCN depends on the super-saturation (S – which is estimated diagnostically in the model as the model assumes saturation adjustment) according to a power-law: CDNC = $N_a S^k$*, where* N_a *is the prescribed concentration of CCN active at 1 % super-saturation, and k is a constant (set in this study to 0.4 - a typical value for maritime conditions)… The activation of CCN at the cloud base is parameterized following Twomey (1959), using the vertical velocity and CCN spectrum parameters."*

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".

Reply: Yes, we have enabled the option to use effective radii from the microphysics scheme to compute effective radii for radiation.

Following the reviewer's suggestion this sentence was revised:

"The model is configured to pass cloud water and ice-crystal effective radii from the microphysics scheme to the radiation scheme; thus, the Twomey effect (Twomey, 1977) of both liquid and ice is considered."

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

Reply: Thank you. The relevant information was added to the revised manuscript:

"The output resolution for all fields is 1h (3D fields are saved as snapshots while domain statistics are saved as hourly-averages)."

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

Reply: The following information was added to the revised manuscript:

"The O_3 vertical profile is similar to Wing et al. (2018) and represents a typical tropical atmosphere. The effect of other trace gases (such as CH_4 and N_2O) is neglected for simplicity."

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?

Reply: Thank you for this suggestion. A reference to the main text was added to the figure caption.

"Table 1. Average equilibrium climate sensitivity (ECS), cloud-feedback parameter (λ_{cloud}), hydrological sensitivity (η), and change in precipitation efficiency ($\Delta\epsilon$) of the three

combinations available for each N_a condition [$2xCO_2$-$1xCO_2$, $4xCO_2$-$2xCO_2$ and $4xCO_2$-$1xCO_2$]. For the calculation of the average ECS, the difference between $4xCO_2$ and $1xCO_2$ is divided by 2. The rest of the quantities are normalized by the SST change between the relevant simulations. Please refer to the text for the definitions of these quantities."

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

Reply: Thank you for this comment. The water vapor should be mentioned here as well, and in the revised manuscript it does:

"Thus, the different decrease rates in R^{LW} with CO_2 concentration for the different N_a conditions (Fig. 3b) must be driven by clear-sky changes (specifically, the plank, the lapse-rate and the water vapor feedbacks – see Fig. 2 above)."

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?

Reply: As explained in the answer to minor comment number 1 above, the definition of precipitation efficiency used here follows the definition of Li at al. (2022), which is, indeed, slightly different from the definition of Lutsko & Croning (2018). However, Li et al., 2022 showed that these two definitions are highly corelated (r=0.86), hence they are expected to change similarly with warming. Please see the reply above for more details.

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.

Reply: Thank you for this comment. Following this comment this sentence was revised as follow:

"The much larger (more than double- Table 1) rate of increase in ϵ with the CO_2 concentration under the highest N_a conditions represents more efficient depletion of the

cloud water from the atmosphere, leading to a faster reduction in CWP with CO_2 concentration (Fig. 4), which in turn leads to higher λ_{cloud} and ECS."

A similar change was applied to the introduction section:

"An increase in ϵ with warming represents more efficient depletion of the water from the clouds, thus affecting the radiation budget."

And to the Summary and conclusions section:

"The ECS increase is explained by a faster increase in precipitation efficiency with warming under high aerosol concentrations, which represents a more efficient depletion of the water from the cloud and thus is manifested as an increase in the cloud feedback parameter."

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?

Reply: Thank you. I agree with this proposed explanation and think that it is consistent with the explanation provided in the text. Hence, in the revised manuscript I now include both:

"This could be explained by the increase in the availability of water vapor (Fig. 2), which, for a given N_a conditions, enable larger diffusional growth of the droplets. This trend could also be understood from the increase in q_c with warming (Fig. 6, Lutsko and Cronin 2018), which under a given N_a conditions implies larger $\overline{r_c}$."

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.)

Reply: Thank you. A possible explanation for why there is a larger increase in large $\overline{r_c}$ in high-N_a simulations is related to the fact that under higher aerosol conditions the droplets growth is limited by the availability of water vapor, which increase with the CO_2 concentration, as explained (in a more elaborated manner in the revised manuscript compared with the previous version) in the following:

"Here again, the highest N_a conditions demonstrate the largest sensitivity of $\overline{r_c}$ to CO_2 concentration, especially at the right-hand side of the distribution (Fig. 7b). This could be explained by the fact that under these high N_a conditions, the cloud droplet growth is primarily limited by the availability of water vapor, as large number of droplets compete for the available water vapor (Koren et al., 2014; Dagan et al., 2015a; Reutter et al., 2009). Thus, an increase in the availability of water vapor with CO_2 concentration (Fig. 2) under polluted conditions results in a larger increase in $\overline{r_c}$ compared with clean conditions."

However, I think that the reasons behind this trend, as well as behind the larger increases in high q_c in high-N_a simulations, deserve further exploration in the future. This is now mentioned in the revised text:

"However, the reasons behind this trend, as well as behind the larger increase in q_c in high-N_a simulations deserve further exploration in the future."

Typos

1. Line 202: qc -> qv

Reply: Thank you. Corrected.

Reviewer #2:

The author explores in a series of modeling experiments the synergistic effects of an increase in CO2 (global warming) and changes in aerosol loading. He takes the two most important contributors to the climate prediction uncertainties and explores their joined effects on clouds (forcing and feedback) from the perspective of a radiative-convective equilibrium assumption using an idealized SAM model with two-moment bulk microphysics. He shows that the equilibrium climate sensitivity increases with an increase in aerosol loading and explains it in an increase in the shortwave cloud feedback driven by an increase in precipitation efficiency in a warming climate.

The study is important, interesting, and well-presented. However, since the core of this study relay on the way by which the model can capture the right cloud trends in the phase space of CO2 vs. aerosol concentrations, more description and discussion on the model's assumptions and limitations is needed.

Reply: I would like to thank the reviewer again for providing these constructive comments. I am happy that the reviewer found this paper to be "important, interesting, and well-presented". Below I describe the added descriptions and discussions on the model's assumptions and limitations following the reviewer's comments.

More details on the RCE model are needed with respect to the type of clouds that are considered. Is it only precipitating clouds? What about the portion of the non-precipitating and specifically the shallow clouds? Are they considered in the model? If not how it is justified? After all the effects on shallow clouds are considered a major source of uncertainty. Is it negligible because the model mimics tropical conditions? If yes, we know that even on tropical thermodynamics shallow clouds form. Does the study consider feedback between clouds (i.e. preconditioning and/or how changes in one type affect another)?

Reply: Thank you for this comment. Clouds of all depths are formed in the simulations presented here, including shallow and deep, precipitating and non-precipitating clouds (see Fig. R1 below as an example). Without applying a large-scale forcing on the domain (such as large-scale subsidence and temperature and humidity tendencies) the atmosphere eventually develops sufficient instability for deep convective formation. Hence, at equilibrium, i.e., in RCE, deep convection must be present. However, this is not meant to say that shallow convection is neglected or not present in the domain. In fact, shallow clouds are probably playing a role in the grow of the instability mentioned above by affecting the thermodynamic conditions (Correia et al., 2021; Hohenegger & Stevens, 2013; Seifert et al., 2015; Spill et al., 2019) – effect which is referred to as "preconditioning" as the reviewer mentions. These effects are included in the RCE simulations presented here.

However, since the grid spacing used here is 1 km, the model is unable to solve all of the small-scale processes related to shallow clouds. Hence their representation is limited in this configuration. Using interactive SST in RCE simulations requires very long and computationally expansive simulations. Increasing the grid resolution to the required resolution needed to better capture shallow clouds (order of 100m) for multiple simulations (sampling different CO_2 and aerosol conditions) is, unfortunately, beyond our current computational ability.

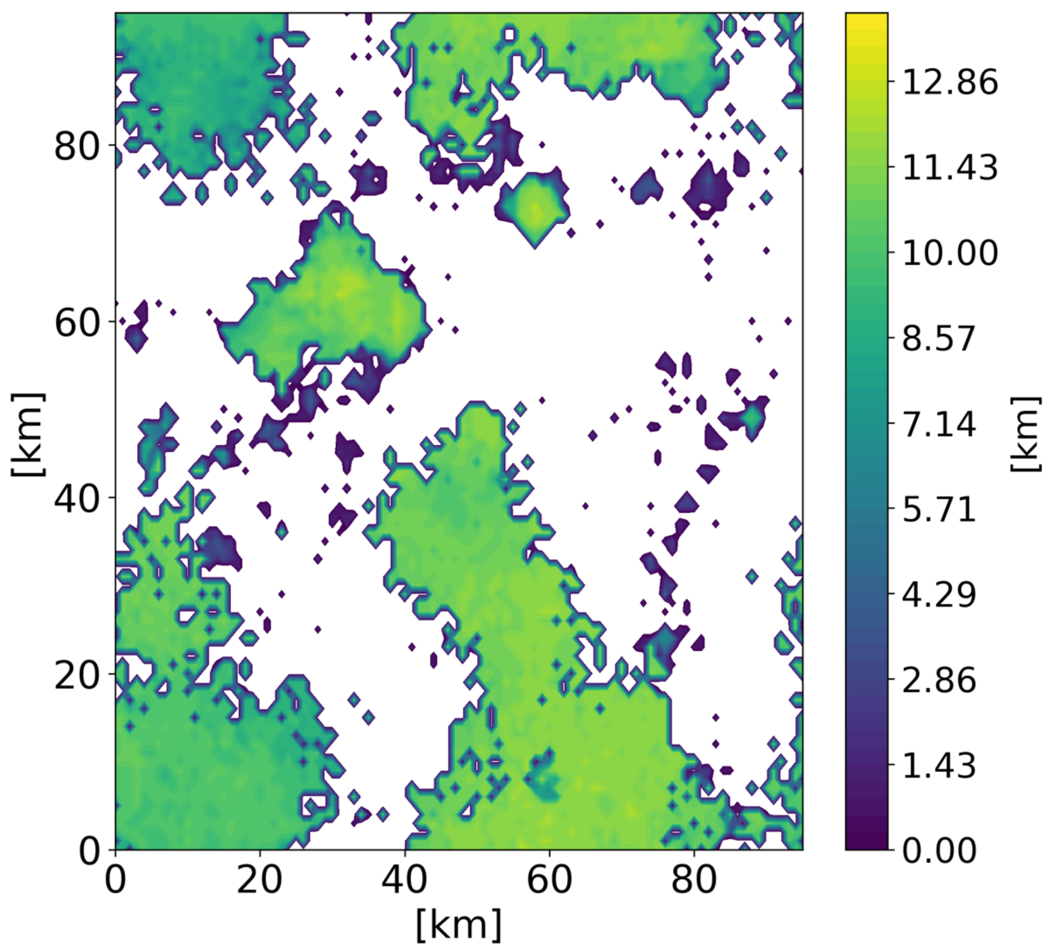

Figure R1. A snap-shot of the cloud top heights in the domain of the RCE simulation with 1xCO_2 concentration and a 200 cm^{-3} aerosol concentration, presented as an example.

Following this comment, the limitation of the model in representing shallow clouds is now explicitly mentioned:

"We note that while shallow clouds are present in the simulations, the grid spacing used here is too coarse for a full representation of these clouds."

Is the model use saturation adjustment? If yes, can it capture all the delicate aerosol effects on the microphysics right? Can it represent the onset of rain and rain efficiency right? This is important as rain processes play a key role in the study.

Reply: Thank you. Yes, the model uses saturation adjustment. This is now mentioned in the revised manuscript:

"The cloud droplet number concentration source assumes that the number of activated CCN depends on the super-saturation (S – which is estimated diagnostically in the model as the model assumes saturation adjustment) according to a power-law: CDNC = $N_a S^k$*, where* N_a *is the prescribed concentration of CCN active at 1 % super-saturation, and k is a constant (set in this study to 0.4 - a typical value for maritime conditions)."*

Please note that in the simulations presented here I use a temporal resolution of 10 sec (which is, as explained in the previous reply, already computationally expansive). The phase change relaxation time of condensation and evaporation is usually on the order of a few seconds, and even under extremally clean conditions is not more than 10 sec (Pinsky et al., 2013). Hence, even if we would use a microphysical scheme that explicitly resolves super-saturation, the humidity is expected to get back to saturation on shorter time scales then the temporal resolution of the model, and hence, practically we will be in "saturation adjustment" conditions anyway.

However, I agree with the reviewer that the results presented here could be dependent to some degree on the microphysical representation used, and hence, should be examined in the future using other models and microphysical schemes. Hence, currently I am initiating (together with Philip Stier, University of Oxford and Sue van den Heever, CSU, and with the help of Allison Wing, Florida State University) an aerosol-focused RCEMIP (radiative convective equilibrium model intercomparison project) stage. This will enable us to examine the results reported here under a wide variety of models including different microphysical schemes.

Following this comment the following caveat was added to the revised Conclusions section:

"Moreover, the response of precipitation to changes in aerosol concentration might be microphysical representation depended (White et al., 2017), and hence should be examined in the future under different microphysical schemes (conceivably in a multi-model intercomparison project focusing on aerosol effect on RCE simulations)."

What about cloud invigoration by aerosol? Such an effect was not discussed in the paper. I miss the discussion on some of the aerosol effects on the buoyancy and vertical velocities, and mobility of the hydrometeors. As well as a discussion of the aerosol effect on the mixed phase. Does to model show invigoration? Are the polluted clouds reaching higher levels of the atmosphere? Does the model show larger transport of condensate to the upper parts of the cloud?

Reply: Since the SST is allowed to react in the current simulations, higher aerosol concentration results in lower SST (for a given CO_2 concentration), which in turn drives a decrease in the anvil cloud top (consistent with the fixed anvil temperature paradigm (Ceppi et al., 2017; Hartmann & Larson, 2002; Zelinka et al., 2012) – see Fig. R2). That is to say that in our simulations polluted clouds actually reach to lower levels in the atmosphere, due to the aerosol effect on the SST. This trend is the opposite of the predicted trend by the invigoration hypothesis. However, it is important to notice that the original invigoration hypothesis discuses a fixed SST condition, while in our simulations the SST react to the aerosol perturbation (and dominate the response).

Following this comment, the invigoration mechanism is now mentioned in the revised manuscript:

"In addition, aerosols were suggested to enhance the vertical velocities and the cloud top heights of deep convective clouds (due to the so-called invigoration mechanism (Abbott & Cronin, 2021; Koren et al., 2005; Rosenfeld et al., 2008)), which in turn can results in precipitation enhancement (Koren et al., 2012). Therefore, aerosols could affect ϵ *(Khain, 2009)."*

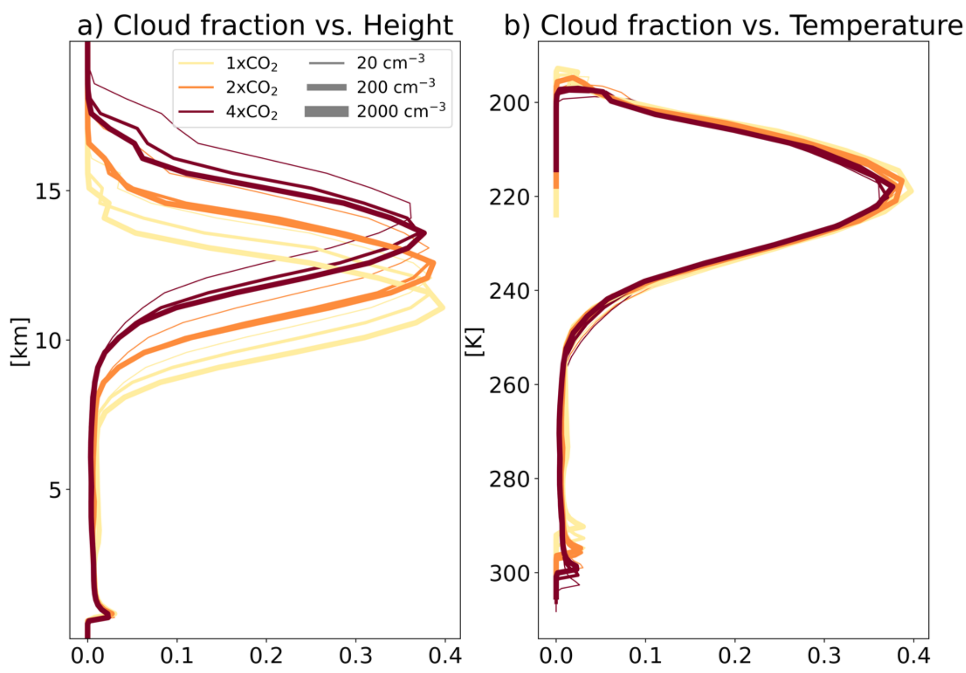

Figure R2. Time- and domain-mean vertical profiles of the cloud fraction in the different RCE simulations vs. height (panel a) and vs. air temperature (panel b).

Last question - delay on the onset of rain by aerosol was shown to have an opposing effect later for deep clouds. It was suggested that when rain processes start in deep polluted clouds, higher rain yields are possible due to a larger integrated collection of falling drops in the invigorated clouds. Is it considered or maybe the model does not show it?

Reply: Thank you. The mechanism the reviewer mentions here maybe operating in our simulations when examining individual clouds response on short time-scales. However, for long time-scales and under equilibrium conditions, aerosol effect on the SST dominates the total precipitation response, and is demonstrated to drive a decrease in it (i.e., under a given CO_2 concentration an increase in aerosol concentration drives a decrease in the domain- and time-mean precipitation - Fig. R3).

The focus in this research is on equilibrium conditions under which the total amount of precipitation is constrained by the water and energy budget of the atmosphere (Dagan & Stier, 2020a; O'Gorman et al., 2012). Since, aerosols are known to decrease the amount of surface-evaporation [the so-called diming effect (Li et al., 2022; Norris & Wild, 2007; Ramanathan et al., 2001)], at least form the water budget perspective, we can anticipate a decrease in precipitation with an increase in aerosol loading (as demonstrated in Fig. R3 below). Similar arguments can be presented from the energy budget perspective (Dagan et al., 2019; Richardson et al., 2018).

Please also see the reply to the comment above, which describes the relevant additions to the revised text.

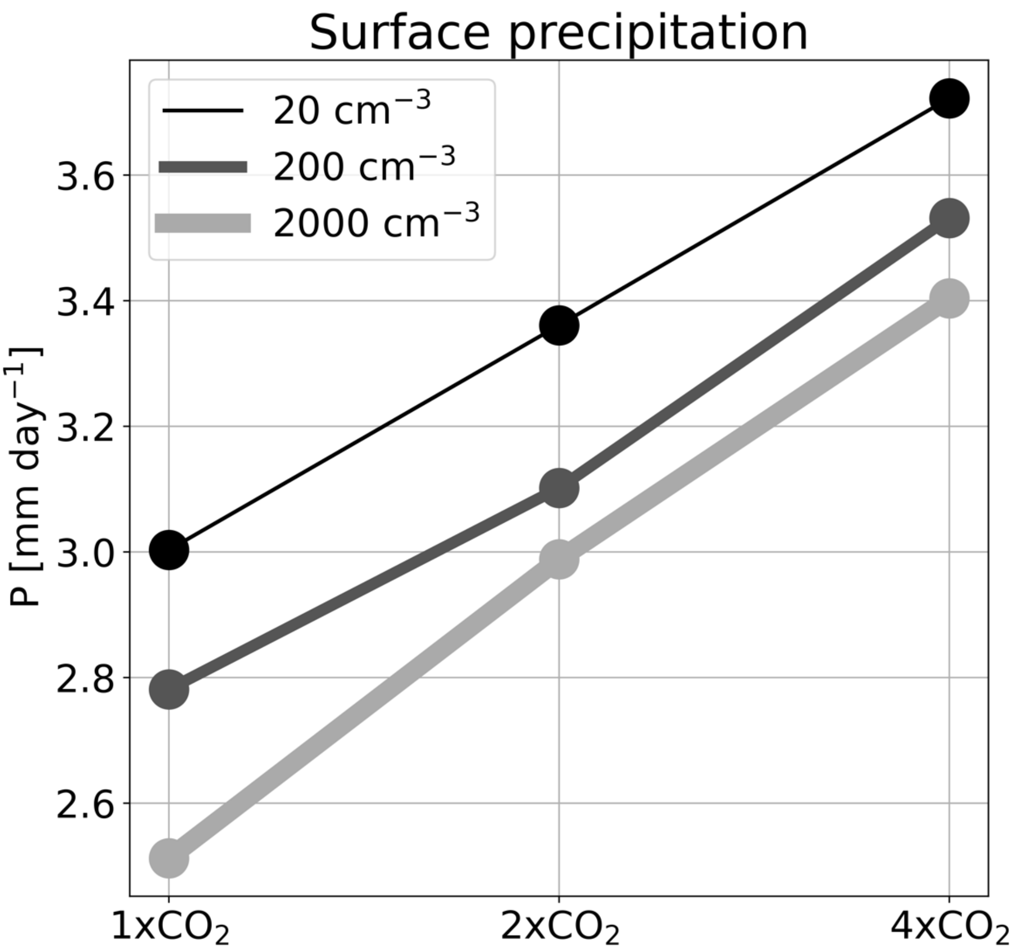

Figure R3. Time- and domain-mean surface precipitation in the different RCE simulations as a function of the CO_2 concentration. The different curves represent different aerosol concentrations.

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