Reply to the referee's review of: On the sensitivity of aerosol-cloud interactions to changes in sea surface temperature in radiative-convective equilibrium

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We would like to thank the Editor and Reviewer #1 for their constructive suggestions. Please find below a point-by-point reply to all of the comments (in blue).

1 Editor

I would also like to see some consideration given to the recent opinion piece by Varble et al. (2023) "A critical evaluation of the evidence for aerosol invigoration of deep convection" where they cast a critical eye on any possible role of convective invigoration through latent heat release. Presumably SAM accounts for the effect on buoyancy of increased condensate loading as a counteracting force, but I don't see this mentioned.

Reply: Thank you. Convective invigoration refers to the case in which an increase in aerosol loading drives an increase in vertical velocity in convective clouds. In our article, we don't relate the latent heat release to changes in vertical velocity, in fact we are not dealing with vertical velocities at all, thus, making convective invigoration process irrelevant to our paper. We mention convective invigoration in the introduction section, where we specify that convective invigoration through latent heat release is highly questionable: "*Under this hypothesis, which remains highly questionable (Varble et al., 2023; Romps et al., 2023), increasing aerosol concentrations have been suggested to drive stronger latent heat release and hence stronger vertical velocities. In addition, under high aerosol concentration conditions, the smaller hydrometeors are transported higher into the atmosphere for a given vertical velocity (Koren et al., 2015; Dagan et al., 2018, 2020), and their lifetime at the upper troposphere is longer, due to a weaker sedimentation rate (Fan et al., 2013; Grabowski and Morrison, 2016). However, it is important to note that these proposed aerosol effects are still highly uncertain (Stevens and Feingold, 2009; Varble, 2018; Romps et al., 2023; Varble et al., 2023).*

In our case we use the enhanced latent heating at the upper troposphere under polluted conditions to explain the increase static-stability, which affects the radiatively-driven divergence and thus the anvil cloud fraction. An enhanced water loading, which is accounted for in SAM and will impact the buoyancy (and hence the vertical velocity), will not directly affect the static-stability, and hence is not relevant for the mechanism explained in our paper (which is again different from "convective invigoration" as we are not examining the effects of aerosols on vertical velocities).

Also, in the conclusions, a passing mention is given to how only a small domain is considered, but not speculation given to what the intuitively might be expected were a larger domain simulated. Presumably something can be said building on prior SST sensitivity studies using a channel domain. Alternatively, there's physical or observational arguments (e.g. DeWitt et al. 2023 in ACP) that suggest aerosol loading doesn't affect distributions of cloud sizes, and hence cloud behaviors (tentatively) may be quite robust if sufficiently large time and space scales are considered.

Reply: Thank you. A recent study (Dagan, 2024), focusing on a larger domain simulations (long-channel domain), showed that an aerosol perturbation in these simulations intensifies the large-scale circulation, dominating the domain-mean cloud and radiative response. These effect are not accounted for in small-domain simulations as used in our current study, which focus on the local response. Following this comment, the following was added to the conclusions section: "*In a larger domain, the circulation is suggested to intensify with an increase in N_a (Dagan, 2024; Dagan et al., 2023). In this case, the large-scale circulation changes dominate the change in the domain-mean cloud and radiative properties. In our simulations we are focusing on the local response and these larger-scale effects are not accounted for.*"

2 Reviewer #1

I thank the authors for significantly improving their manuscript and addressing most of my comments well. This adds much value to the manuscript. Nevertheless, I think that the new and very informative analysis is not yet at a good enough level and should therefore be revised before the manuscript can be published in final form. I also added a few more rather minor comments.

Reply: We would like to express our gratitude to the reviewer for his extremely constructive comments and the time he spent suggesting ways to improve our manuscript. It is truly remarkable how much these comments (in the previous round and in this round) have helped us improve our paper. Thank you very much!

2.1 General comments

1. It appears that the main radiative response is the Twomey effect. However, this effect comes mainly from ice clouds, which is plausible but rather unusual. What do such clouds look like compared to unperturbed/clean clouds? Also, to be fair, this could simply be an artifact of the lack of aerosol coupling in the freezing part of the code, which should be clearly stated somewhere.

   Reply: Thank you. One can get a feeling of how these clouds look like from Fig. 3 from the main text (and presented below) and from the newly added snapshots of outgoing longwave radiation (OLR) to the supporting information (Fig. S1, SI below).

   A reference to the snapshots was added to the methods section: "*The SST ranges from 290 to 310 K in 5 K intervals. Snapshots of the different simulations is presented in Fig. S1, SI.*"

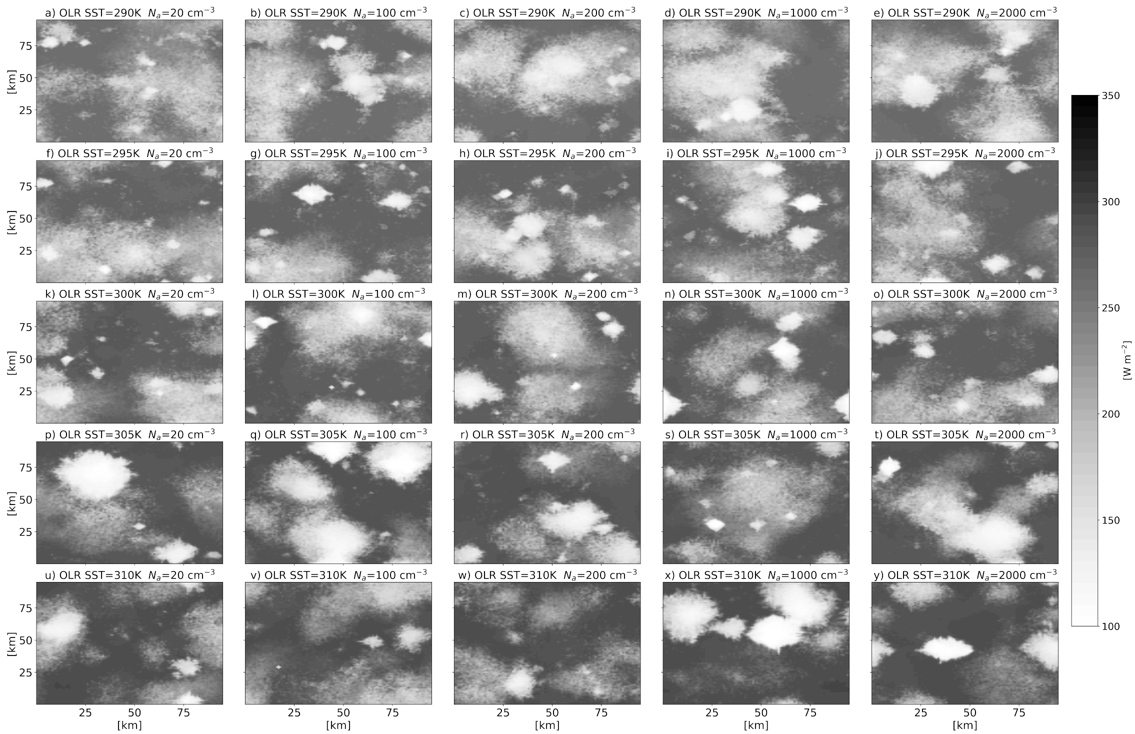

Figure S1. Snapshots of the outgoing longwave radiation (OLR) of the different simulations.

Furthermore, a caveat was added to the results section: *This term represents an increase in the reflectivity of the ice clouds for a given $\mathcal{L}$ and $\mathcal{I}$ distribution, and can be explained by a similar mechanism to the Twomey effect but for ice particles. We note that this result might differ under coupling of N_a to ice nucleating particles, which is not considered here.*

2. The warm rain inhibition mechanism is mentioned a few times in the manuscript. Is warm rain really important under RCE conditions? Isn't most of the rain we see just from melting ice hydrometeors?

Reply: Thank you. In our simulations, under very clear conditions (and especially under high SSTs for which the warm section is deeper) the warm rain production is not negligible. For example, under SST of 310K and N_a of 20 cm^{-3}, almost 20% of the rain is from shallow clouds (according to the cloud regimes definition presented in the paper). Thus, an increase in aerosol loading has a potential to significantly reduce this non-negligible fraction. Indeed, as can be seen from Fig. 3 from the main text (presented below), rain is reduced in the warm phase with an increase in aerosol concentration for all SSTs. The warm section acts as the boundary and initial condition for the mixed-phase section, thus any changes in the warm phase affect the mixed phase. Specifically, the delay in warm rain formation leads to higher production rates of graupel and snow ((Chen et al., 2017); as can also be seen in Fig. 3 below).

Comments 3-8 refer to the new cloud decomposition and its implications

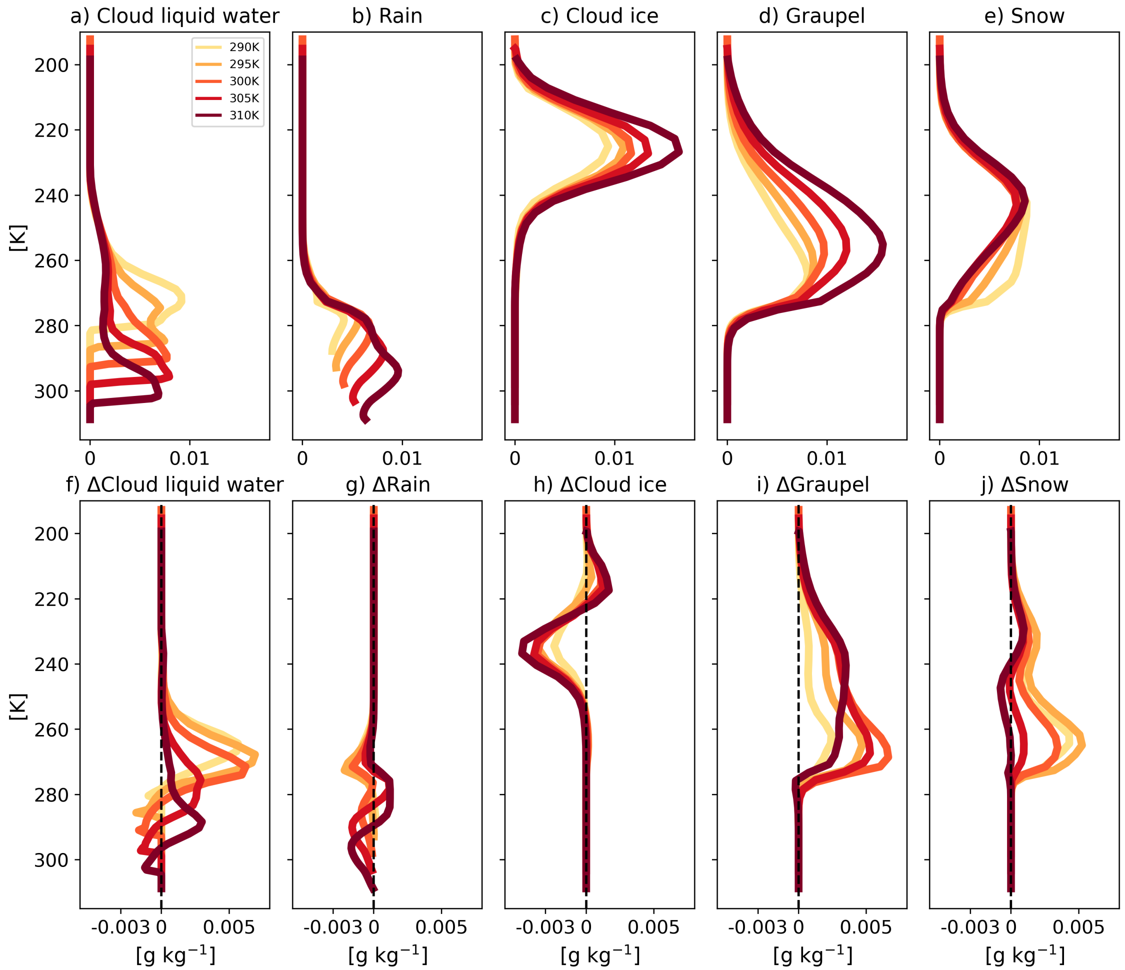

Figure 3. Domain and time mean vertical profiles of the different hydrometeors for the cleanest runs ($N_a = 20\ \mathrm{cm}^{-3}$): (**a**) cloud liquid water, (**b**) rain, (**c**) ice, (**d**) graupel, and (**e**) snow, and their response to increasing N_a to $2000\ \mathrm{cm}^{-3}$, relative to the cleanest run for each SST (**f** – **j**). Here we only present the cleanest and the response of the most polluted runs for clarity. The full range of N_a is presented in Figs. S3-S7, SI.

3. The authors follow the decomposition of Sokol et al, 2024. However, I think the way they use this decomposition is confusing, mainly because of the naming/interpretation of the individual terms. What the authors call the "shift term" is considered in Sokol et al., 2024 to be a combination of the area and opacity terms, plotted in black in their Fig. 3c. Sokol goes further and decomposes this into the area and opacity terms, shown in pink. It would be nice if you could do that too - but it's just a suggestion; it could be more elaborated due to your 2D phase space.

   What the authors call "opacity term" is in Sokol et al. described as "The second term on the right-hand side accounts for changes in CRE(IWP), which may occur due to changes in clear-sky fluxes or cloud microphysics, temperature, and altitude. This term encompasses the entire ice cloud altitude feedback, as well as the part of the opacity feed- back related to changes in τ at fixed IWP, which may result from changes in cloud microphysical structure." So it is likely a combination of several factors. However, I would agree that intuitively most of it should come from the increased opacity at the fixed ice water path (i.e. "ice Twomey effect"). But you should mention that other factors could influence it.

Ultimately, not much needs to change in the decomposition, but the terms need to be more clearly described, not only in comparison to Sokol et al. 2024, but also in comparison to the more widely known feedback decompositions (e.g., Zelinka et al. 2016).

Reply: Thank you for this comment. Following this comment, a better explanation for each term was added: "... *we decompose the mean ΔR into three contributions:*

$$\begin{aligned}\Delta CRE \approxeq \Delta R \\ &= \underbrace{\int_0^\infty\int_0^\infty \Delta CRE(\mathcal{L},\mathcal{I}) CF(\mathcal{L},\mathcal{I})\, d\mathcal{L}\, d\mathcal{I}}_{Opacity} \\ &+ \underbrace{\int_0^\infty\int_0^\infty CRE(\mathcal{L},\mathcal{I}) \Delta CF(\mathcal{L},\mathcal{I})\, d\mathcal{L}\, d\mathcal{I}}_{Shift} \\ &+ \underbrace{\int_0^\infty\int_0^\infty \Delta CRE(\mathcal{L},\mathcal{I}) \Delta CF(\mathcal{L},\mathcal{I})\, d\mathcal{L}\, d\mathcal{I}}_{Nonlin} \end{aligned} \tag{1}$$

In this decomposition, the first term on the right-hand-side, the "Opacity" term, represents changes in ΔR due to changes in the CRE per $\mathcal{L}$ and $\mathcal{I}$ bin, while the distribution of $\mathcal{L}$/$\mathcal{I}$ are held fixed, i.e., this term is calculated by multiplying Fig. 4a with Fig. 4k. This term represents changes in the cloud's opacity (reflectance and absorption) for a given liquid and ice amount (for example by the Twomey effect). We note that this term could also be influenced by changes in clear-sky fluxes (Sokol et al., 2024). The second term on the right-hand-side, the "Shift" term, represents changes in ΔR due to changes in the distribution of $\mathcal{L}$/$\mathcal{I}$ occurrence, while the CRE per $\mathcal{L}$ and $\mathcal{I}$ bin is held fixed, i.e., this term is calculated by multiplying Fig. 4b with Fig. 4l. The Shift term is contributed by both changes in the total CF and by a shift between the different cloud regimes (for example thinning of ice clouds). The last term on the right-hand-side, the nonlinear ("Nonlin") term, represents the combined effect of changes in the CRE and the cloud occurrence in the different $\mathcal{L}$/$\mathcal{I}$ bins, i.e., this term is calculated by multiplying Fig. 4k with Fig. 4l.

4. The cloud categories limits may need to be adjusted.

   (a) In table S1, "no clouds" category goes to ice water path of 5. This limit should be corrected to at least 1 g m^{-2}, which corresponds to a cloud of a cloud optical depth of approximately 1, which is clearly not negligible in radiative terms. The range could even go down to 0.1 g m^{-2}, as thin clouds don't cease to exist at 1 g m^{-2} , but those thinnest clouds may be less radiatively important.

Reply: Thank you for this important comment. To further understand the effect of the selected boundaries of the "No Cloud" regime on the cloud fraction (CF) trend under the different simulations, we have calculated it with varying boundary definition in the range of $\mathcal{L}$ < (1, 2, 4, 8) and $\mathcal{I}$ < (0.5, 1, 2, 4) (Fig. S2, SI, presented below). As can be seen from this analysis, the trend in total CF with N_a doesn't vary much for the different values of $\mathcal{L}$ described above, but exhibits different trends for the different $\mathcal{I}$ values described above. This figure is consistent with the thinning of ice clouds with an increase in N_a, as explained in the manuscript. Following this comment (and the following comments), the different cloud regimes' boundaries were changed and specifically, we now follow the reviewer's suggestion regarding the "No Cloud" regime. The new boundaries of the different regimes can be seen in Table S1, SI, presented below.

Table S1. Cloud regime's liquid water path ($\mathcal{L}$) and ice water path ($\mathcal{I}$) boundaries.

Cloud regime	$\mathcal{L}$ [g m^{-2}]	$\mathcal{I}$ [g m^{-2}]
No clouds	$0<\mathcal{L}<1$	$0<\mathcal{I}<1$
1) Thick ice	$0<\mathcal{L}<1$	$16<\mathcal{I}$
2) Thin ice	$0<\mathcal{L}<1$	$1<\mathcal{I}<16$
3) Shallow	$1<\mathcal{L}$	$0<\mathcal{I}<16$
4) Deep	$1<\mathcal{L}$	$16<\mathcal{I}$

(b) Deep convective clouds occur in nature at IWP larger than about 1000 g m^{-2} . Clouds with IWP between 20 and 200 g m^{-2} are certainly not deep convective towers (unless something is very weird/wrong in the model). Therefore, your category 4 might rather include cumulus congestus with frozen cloud tops reaching heights above 5 but below 10 km in the tropics and representing the third peak in cloud fraction (see e.g. Fig. 5a in Hartmann and Berry, or Fig. 1 in Gasparini et al., 2019). In any case, the number of the cloud regime should be added to Table S1, and the name of the cloud regime should be added to the caption of Fig. 4.

Reply: Thank you. We completely agree with the reviewer. Following this comment a caveat was added to the manuscript: "*We note that the shallow and deep cloud regimes may also consist of other types of clouds, such as cumulus congestus in the deep regime and two-layer cloud conditions with cirrus clouds with relatively low $\mathcal{I}$ above shallow clouds.*"

Furthermore, the number of cloud regimes was added to Table S1, SI above, and the following addition was added to the caption of Fig. 4 in the manuscript: "*Four different cloud regimes are marked in red in panel **a**: (1) thick anvil clouds, (2) thin anvil clouds, (3) shallow clouds and (4) deep convective clouds, while the clear-sky regime is painted in tan.*"

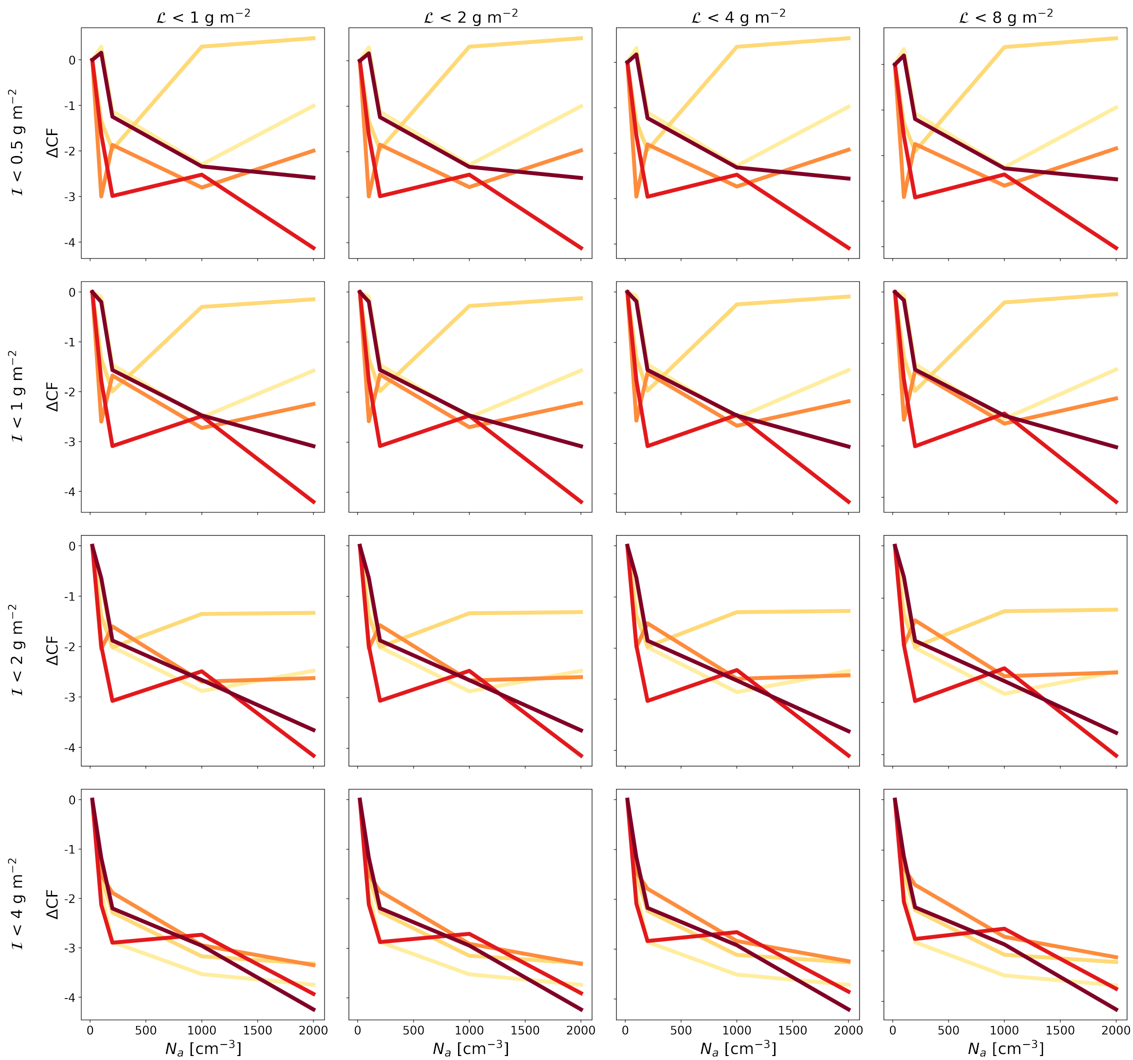

Figure S2. The response of domain and time mean cloud fraction (CF) to an increase in N_a. The values are presented relative to the cleanest run (N_a = 20 cm^{-3}) for each SST, as indicated by the Δ sign. Four different limits of liquid water path ($\mathcal{L}$) and ice water path ($\mathcal{I}$) are considered for the "No clouds" regime to examine its sensitivity.

5. It would be great if the authors could add another column to your Figure 4, with values of CRE*CF, which would show the radiative significance of each ice-water path bin in your 2D space.

   Reply: Thank you for this suggestion. Following this comment and comments 6 and 8, Fig. 4 was revised as suggested in these comments (see below).

6. What is plotted in the first column is not cloud fraction. It is simply a 2D PDF of the frequency of occurrence (ok, cloud occurrence may be ok, but not cloud fraction). So please call it that, especially since in Figure 2 you are using the domain-

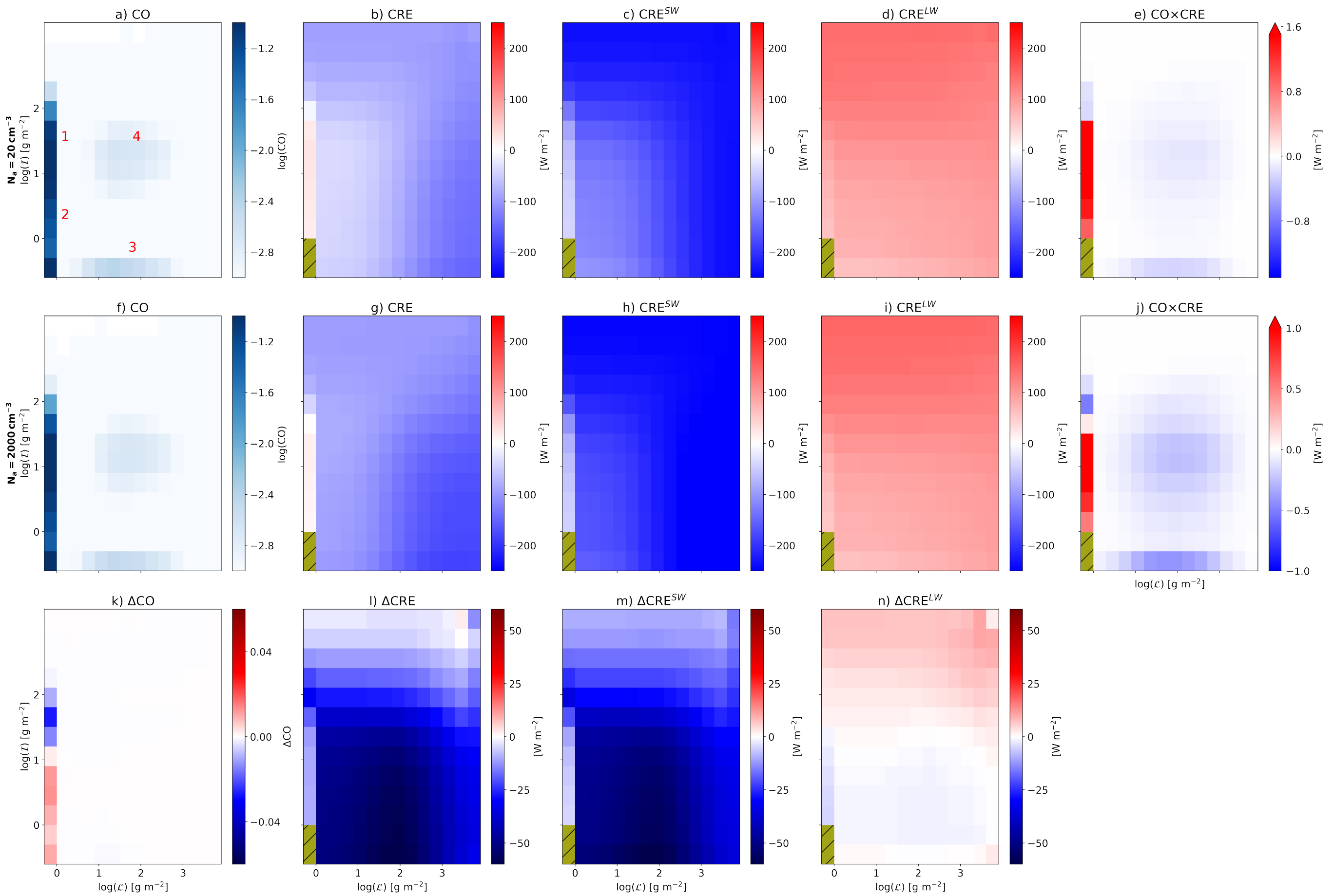

Figure 4. Domain and time mean two-dimensional histograms of cloud occurrence (CO; **a** and **f**), at different bins of liquid water path ($\mathcal{L}$) and ice water path ($\mathcal{I}$) and the average total (**b** and **g**), shortwave (**c** and **h**) and longwave (**d** and **i**) cloud radiative effect (CRE) at these different bins. Furthermore, the radiative significance (CRE×CO) of each bin is illustrated in panels **e** and **j**. These quantities are presented for two simulations using the lowest (N_a = 20 cm^{-3}; **a**-**e**), and the highest (N_a = 2000 cm^{-3}; **f**-**j**) N_a, under SST = 290 K. Four different cloud regimes are marked in red in panel **a**: (1) thick anvil clouds, (2) thin anvil clouds, (3) shallow clouds and (4) deep convective clouds, while the clear-sky regime is painted in tan. In addition, the difference between the highest and lowest N_a conditions is presented in panels **k**-**n**.

averaged cloud fraction, which is something completely different. Also, is the sum of the frequency of occurrence over the whole phase space equal to 1?

Reply: Yes, the sum of the occurrence over the whole phase space equals to 1. All instances of cloud fraction (CF) when referring to the histograms were changed to cloud occurrence (CO) as the reviewer suggested.

7. I am confused why the "shallow" category seems to be as radiatively important. In figure S6 we see that the shallow cloud fraction is about 0.2%, compared to the ice cloud fraction of 40% (let's assume that splits equally - 20/20 to thin and thick ice). That's 2 orders of magnitude difference. The difference in CRE (column 2 in Figure 4), however, seems to be at most 1 order of magnitude. Why is therefore the decomposition for shallow leading to same magnitude size of effect in Figure 6? Am I missing something?

   Reply: Thank you for spotting it. Following this comment, a mistake in the code calculating shallow CF was found, and is now fixed. Fig. S8, SI (presented below) illustrates that shallow clouds consist of 4.5-6.5$\%$ of the domain, i.e., an order of magnitude smaller than ice clouds. Even with these differences in CF, shallow clouds have a more negative CRE (Fig. 4 b and g), and are susceptible to changes in N_a (Fig. 4l), thus contributing a significant amount to the changes in ΔR.

8. It's very hard to see the occurrence frequency values in column 1. Maybe plotting as pcolor instead of contourf could help? Also, is it really important to go to values as low as 0.0001? Couldn't the colormap stop at log(cf)=-3? And start maybe at -1?

Reply: Thank you. We have followed these suggestions. Please refer to our answer to general comment 5.

2.2 Specific comments

1. You may want to update the Sokol et al., 2024 citation; it should appear in final form in the coming days in Nat. Geosci.

   Reply: Thank you, the citation was updated as suggested.

2. Page 1, lines 6-7: What does the sentence "The changes in..." really mean? I thought you explain the key radiative difference with the Twomey effect, not changes in cloud fraction?

   Reply: Thank you. Indeed, we explain the key radiative effect with the Twomey effect, which is dependant on the baseline cloud fraction. Specifically, the larger the baseline cloud fraction, the larger the magnitude of the Twomey effect. The baseline CF decreases with an increase in SST (Fig. S8, SI). Following this comment, the abstract was revised: "*The changes in TOA shortwave flux exhibit greater sensitivity to underlying SST conditions compared to longwave radiation. To comprehend these trends, we perform a linear decomposition, analyzing the responses of different cloud regimes and contributions from changes in cloud's opacity and occurrence. This breakdown reveals that ice and shallow clouds predominantly contribute to the radiative effect, mostly due to changes in cloud's opacity, due to the Twomey effect, which is proportional to the baseline cloud fraction.*"

3. Page 1, line 13: "decline in TOA longwave energy gain" I guess this is a very complicated way to say "more outgoing longwave radiation".

   Reply: Thank you, the sentence was reworded as suggested: "*... iris–stability effect, resulting in an increase in outgoing longwave radiation.*"

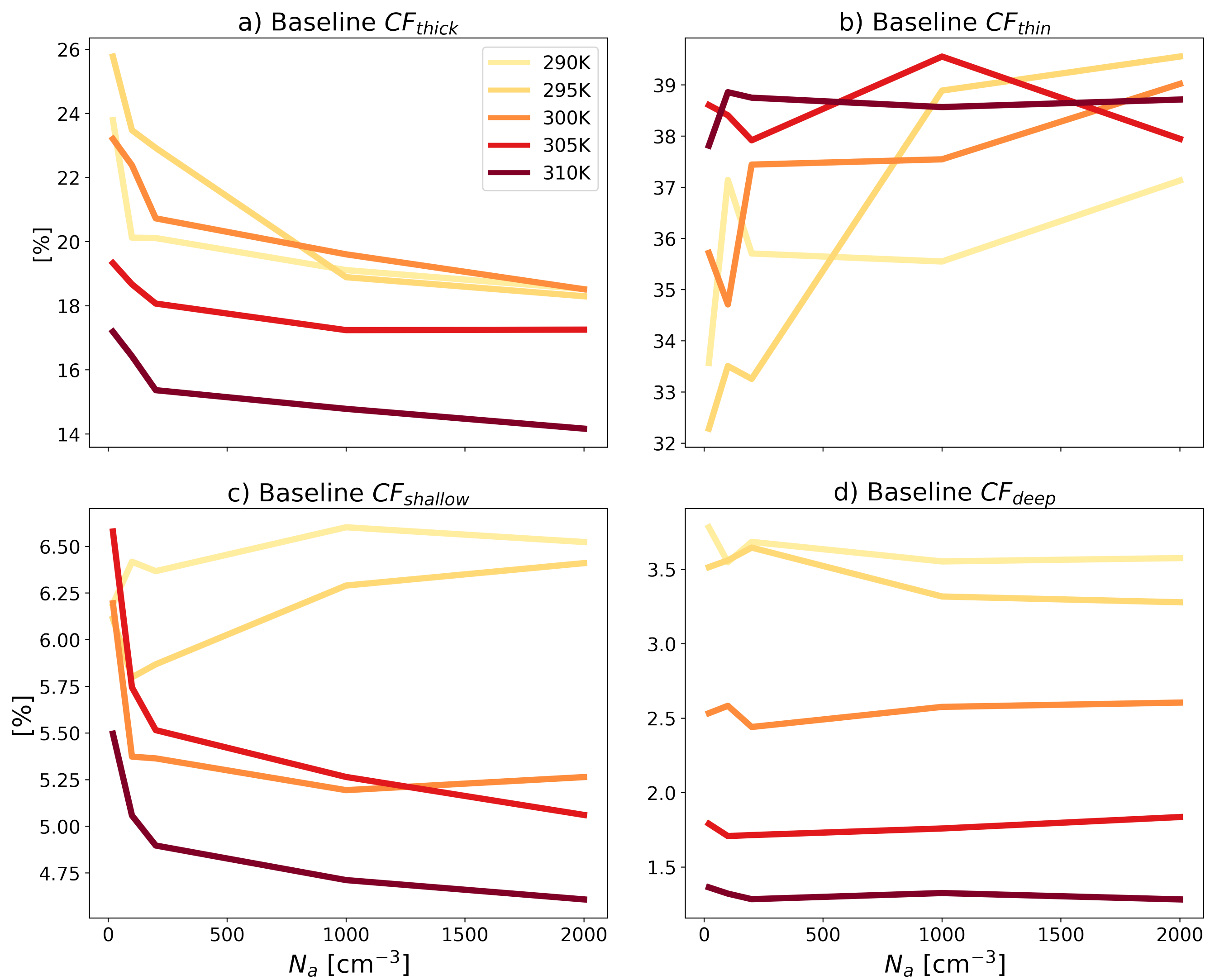

Figure S8. Changes in domain and time mean cloud fraction of thick ice (CF_{thick}; **a**), thin ice (CF_{thin}; **b**), shallow ($CF_{shallow}$; **c**) and deep convective clouds (CF_{deep}; **d**) due to an increase in N_a, for each SST.

4. Line 61: generally => often (they are indeed not always opaque in infrared; most frequent high clouds at COD<1 are not)

   Reply: Thank you, "*generally*" was changed to "*often*" as suggested.

5. Page 3, line 81: Delete Lindzen et al., 2001 and Mauritsen and Stevens, 2015 reference if you strictly describe the stability iris hypothesis. Lindzen's Iris hypothesis is different; it is a microphysical iris, and not the stability iris you describe; a similar iris formulation was also considered by Mauritsen and Stevens, 2015.

Reply: Thank you, the articles mentioned above were removed following this comment.

6. Page 4, section 2.1: I imagine that a reference to the microphysical scheme would make more sense than the cited paper, which seems to be about processes and not parameterization. The two-moment bulk microphysics of Morrison et al. (2005) probably uses the Cooper et al., 1986 formulation for deposition freezing. Indeed, the best way to confirm this is to search the microphysics scheme in the code.

   Reply: Thank you. The microphysics scheme used in the model is indeed the one described in Morrison et al. (2005). The cited paper of Morrison et al. (2005) is indeed about the parameterization and its description (and not about processes, as the reviewer suggested).

7. Page 4, Lines 108-110: In our simulations, heterogeneous nucleation dominates for temperatures higher than approximately 238 K, while ice formation is dominated by homogeneous freezing for temperatures lower than approximately 233 K (Rasmussen et al., 2002).

   I don't think that's necessarily true (unless you've checked it yourself). Homogeneous freezing of cloud droplets is only active at the homogeneous freezing temperature of water, not below/above it. So I suggest deleting this sentence and just mentioning which parameterizations are used for freezing. I assume:

   (a) Cooper et al., 1986 for deposition freezing, which is also active at T<233 K (should not be the case in reality, but that's what the model likely does)

   (b) Homogeneous freezing of water droplets (no need for a reference, as it's simply a statement of the kind: "if cloud droplets present at T<233 K, freeze them")

   I imagine there is no physical process that would be able to nucleate ice at T<233 K. Instead, and contrary to what is known about ice nucleation, Cooper et al., 1986 are allowed to be active at such conditions (probably along with some strong artificial limits on nucleated ice crystals to prevent the model from getting crazy numbers of ice crystals).

   Reply: Thank you. The cited paper should've been Morrison et al. (2005) and not Rasmussen et al. (2002). Following this comment, this sentence was revised: "*In our simulations, freezing occurs through homogeneous freezing and heterogeneous freezing by contact or immersion freezing (Morrison et al., 2005). Ice nucleation directly from vapor is not considered here, but depositional growth of cloud ice is enabled.*"

8. Page 7, line 174-176: "We note that the average CRE of thin anvil cloud is small but not positive as in previous assessments (Sokol, 2024), probably due to the use of a relatively coarse resolution of and bins" And what if it is because low clouds that occur below ice clouds are affecting the result?

   Reply: Thank you. Following this comment and some previous ones, we have conducted these calculations with higher resolution of $\mathcal{L}$ and $\mathcal{I}$ bins. The new calculation with the higher resolution shows positive CRE of thin anvil clouds, as known from previous studies. Thus, this sentence was removed.

9. Page 7, line 175: Sokol et al., 2024 just analyzes RCE simulations. Other studies look at satellite retrieved CRE, and may deserve to be mentioned. E.g. Hong et al., 2015, Hong et al., 2016, Fig 1 in Gasparini et al., 2019, etc.

   Reply: Following the previous comment, this sentence was removed.

10. Page 12, lines 231-232: The net effect of the shift term is not negligible for this ice clouds, in Fig 6.

    Reply: Thank you. Following this comment and changes to the bin's resolution, the shift term of ice cloud is indeed not negligible, and a new explanation was added: "*Thin ice clouds exhibit an opposite trend to thick clouds in the shift term, due to them increasing in CO with an increase in N_a (Figs. 4k and 5b). However, the combined net effect of thick and thin ice clouds on the shift term is low, due to them being similar in magnitude but opposite in sign (Fig. 6d).*"

11. Page 12, lines 235-248: I thought the definition of shallow clouds is that they don't reach the freezing level. But around line 245 I see explanations that involve changes in the freezing level. Please clarify!

    Reply: Indeed the definition of shallow clouds in our case is that they have little ice water path and thus are likely to not reach the freezing level. However, the freezing level becomes deeper with an increase in SST. Thus, under higher SSTs shallow cloud (which do not reach the freezing level) can form deeper. In these relatively deeper, but still warm, clouds, forming under high SST, warm rain inhibition by aerosols is less likely (the clouds are deep enough so that warm rain in inhibited at the lower part of the clouds but is formed at higher, still warm, sections of the clouds). To make this clearer, we have replaced "freezing level" with "warm layer depth" in this section:

    "*The contrasting response of $CF_{shallow}$ to N_a under the different SSTs can be explained by warm rain inhibition at varying depths of warm layers. As was noted above, with an increase in SST, the warm layer depth increases, while an increase in N_a acts to push warm rain formation to higher levels (Rosenfeld, 2000; Freud and Rosenfeld, 2012; Heikenfeld et al., 2019). Thus, under lower SSTs, for which the warm layer depth is relatively shallow, an increase in N_a can inhibit warm rain (see Fig. 3g) and hence lead to an increase in $CF_{shallow}$. In contrast, under higher SSTs, for which the warm layer depth is relatively deep, an increase in N_a drives warm rain inhibition at the lower levels, which is compensated for at higher levels of the warm section (Fig 3g), thus eliminating the positive effect on $CF_{shallow}$.* "

12. Page 12, title 3.3: Please use words.

    Reply: The title was changed to: "Mechanism behind the ice cloud fraction's response to N_a".

13. Page 16, section 3.4: I think the paper is already dense enough that you could remove this section to keep focus on the radiative fluxes.

    Reply: Thank you. Although the paper might seem dense, we feel that this section adds important physical understanding. Considering the section's short length, we've decided to keep it in the manuscript.

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