This is an interesting and useful study that examines how the assumed aerosol concentration N_a impacts cloud characteristics for a variety of sea surface temperatures (SST's) in idealized radiative-convective equilibrium simulations. This study builds on previous studies that focused on either the SST effect on clouds or the aerosol effect on clouds, but not the combination of both effects. It is shown that increased aerosol concentrations enable liquid cloud droplets to loft higher in the atmosphere as they do not grow and rain out as quickly. For the cooler simulations, which have shallower warm layers, dropets are lofted above the freezing level before the droplets are large enough to form rain. This decreases rain rates but increases ice crystal growth and therefore frozen precipitation production. For the warmer simulations, thicker warm layers allow rain to still form, although at higher elevations, and so the precipitation phase is not as sensitive to N_a. This finding was made possible because the study considered both a range of both SST's and values of N_a, rather than one or the other, which illustrates the novelty of the study.

Interpretation of some of the results is problematic, which I have outlined further below. In particular, a central result of the study was that anvil cloud fraction is reduced with increasing N_a, but the authors do not clearly state how anvil cloud fraction is calculated and two plots showing anvil cloud fraction are inconsistent with each other. Furthermore, one plot appears to show anvil *height* changes with increasing N_a, but anvil *coverage* does not. Both observations call the anvil result into question.

The authors may need to revisit their analysis of simulation output in order to correct the inconsistency in the anvil cloud fraction. If the results change, the discussion and conclusions may need to be significantly revised. There are also some smaller claims that appear to be unsupported in plots, outlined below, which also necessitate a revision of some of the results/discussion and conclusion sections.

Once the issues with anvil cloud fraction are addressed, I believe there is significant potential for novel results about anvils that would attract a wide audience. In particular, it is possible that both FAT and the stability iris hypothesis are supported for a given N_a, but FAT does not apply across different N_a. Rather, increased N_a raises and thins anvils. These guesses are based off a tenuous reading of the plots and would need to be examined further.

General comments

1. The claim that increasing N_a decreases anvil cloud fraction (CF) is first made a line 157, in reference to Fig. 2c. Fig. 2a shows a maximum in CF at about 220K, and Fig. 2c shows increases in CF above this level and decreases below with increasing N_a. This indicates a rising of the level of maximum CF. The increase in CF above 220K is smaller in magnitude than the decrease in CF between 250K and 220K, which indicates the anvil depth decreases with an increase in N_a. Furthermore, if the zero-crossing in plot 2c is precisely at or below the maximum in plot 2a, as it appears, the value of the maximum CF could not be decreasing. However, it is difficult to tell in the plots.

   Anvil CF is further investigated at line 161 and Fig. 6a, which shows a relationship between anvil CF and maximum mass divergence. The plots are inconsistent given that all points in Fig. 6 have anvil CF< 0.24 but that all profiles in Fig. 2a have a maximum CF> 0.35 at about 220K. The authors need to describe carefully how anvil CF is calculated. My guess is that, for Fig. 6, anvil cloud fraction is calculated as the time mean of

   $$\frac{\text{number of cloudy grid points above 245K}}{\text{total number of grid points above 245K}}$$

   This is not the anvil cloud fraction, but rather the anvil cloud volume. If true, this would explain why Fig. 6a shows a decrease in anvil CF, but Fig 2 does not: anvil depth decreases, decreasing their volume, but not necessarily their horizontal extent. The authors should calculate anvil cloud fraction as the maximum in the vertical profile of cloud fraction above 245K, which would be the same definition as Beydoun et al., 2021 and similar to the definition in Saint-Lu et al., 2020.

2. In multiple locations, a distinction is made between the cloud radiative effect specifically and the overall changes to radiative effects generally. But aerosols were only allowed to change cloud microphysics and

not interact with radiation directly, and thus the CRE was effectively the only thing that was allowed to change in the simulations. Indeed, the top and bottom rows in Fig. 1 appear nearly identical.

Specifically, the following statements are misleading and should be reworded:

(a) L5: "we show that increasing N_a leads to a decline in top-of-atmosphere (TOA) energy gain across SSTs due to changes in the cloud radiative effect"

(b) L130: "the CRE ... is identified as the main driver of ΔR variations, while changes in clear sky radiation has a minimal impact"

(c) L218: "increasing N_a decreases top-of-atmosphere (TOA) energy gain ... as a result of changes in the cloud radiative effect."

In all cases, I suggest it be made clear that the aerosol concentration affects the CRE, which is the only component of the overall aerosol radiative effect that is examined.

3. The introduction presents a nice overview of research on how clouds respond to changes in SST, including the FAT hypothesis and the stability iris effect. There is also a nice overview of existing work on how aerosols affect cloud properties. However, while there is a clear motivation for investigating higher SST's, the reader may be left wondering how realistic the aerosol concentration scenarios used are, if and how global aerosol concentrations are expected to change under warming, and how the results of the study would modify the aerosol concentrations further.

   For example, the study finds increased aerosol concentrations may increase precipitation rates. One may hypothesize that such increased preciptiation would feed back into the aerosol concentration as the aerosols get rained out faster, and thus moderate the concentration. Even if these effects are highly uncertain, they could be discussed.

4. The supplement presents plots in the same format as the plots in the main text, but every plot has all combinations of SST and N_a. Including all 25 profiles on each plot makes them impossible to read. I would create a new plot for each value of N_a so that these plots can be read. The plots would be exactly as the top row (or only row) in Figs. 1,3,5,8,9, but for different N_a.

5. Most vertical profiles in the study are plotted with temperature as the vertical coordinate. I find this quite useful in some cases, e.g. fig 2a appears to support the FAT hypothesis. In other cases, it is slightly more confusing, e.g. in determining the altitude of rain production in figs. 3b and 3g.

   The only change I would request here is that there appears to be an artifact in most profiles that could be removed. For example, the cloud ice profile (Fig 3c) is bivalued between $\sim$ 200k and $\sim$ 230K. I assume one value is from the stratosphere while the other is from the troposphere. I suggest truncating the profiles where the temperature begins to increase, thus ensuring each T is uniquely associated with a single z.

Specific comments

1. L5: ", even at equilibrium conditions.": Things not at equilibrium include the SST with insolation and N_a with aerosol removal via deposition. I suggest explicitely stating the equilibrium is RCE.

2. L133: Please describe how grid points are defined as cloudy for the purpose of calculating cloud fraction.

3. L135: Does "total water" refer only to total liquid and ice? If so, please clarify. "total water" to me reads as including water vapor.

4. L146: " under lower SST ... an increase in N_a can completely suppress warm rain (see Fig. 3g)." This appears unsupported in the plot. The cleanest 290K run shows a maximum for rain of ~ 0.004g kg^{-1}, while the change in rain in the polluted scenario shows a maximum change of ~ -0.002g kg^{-1}. Please quantify this statement.

5. L151: "The stronger increase in total water with Na under lower SSTs leads to the stronger SW reflectivity,": I'm not sure that just the result of increased total water is enough to establish causality here, with the role of anvils and changes to cloud morphologies uncertain. Perhaps just "consistent with".

6. L156: "higher N_a and lower SSTs ... thus producing more graupel (Fig. 3i)" In Fig. 3i, the coldest run shows the smallest increase in graupel production. The largest increase is from the middle 300K simulation, so there is not a clear trend.

7. L169: "A reduction in D_r with N_a could be attributed to changes in Q_r": For me, it's less intuitive that N_a would affect Q_r than D_r. Throughout this section, it is not clear to me that causality can be estabished, though I don't understand the calculation. Please explain the calculation and consider whether it establishes causality.

8. L178: "for a given SST, an increase in Na drives strong warming of the upper troposphere and a weak cooling of the lower troposphere.": The two coldest simulations do not appear to cool in the lower troposphere. They warm in the upper troposphere, but both have small regions of cooling in the upper troposphere.

9. L179: "the increase in S with Na (Fig. 5a)": Fig. 5a does not show the change with N_a, only the values for the cleanest simulation.

10. L190: "driven by a stronger latent heat release,": As well as weaker advection (since the advection term is negative), which appears to be a similar magnitude.

11. L190: "is in turn driven by higher production rates of graupel and snow ": I would say "consistent with". The causality implied here is hard to see and may flow the other direction.

12. L199: Kirchoff's Law implies $\Delta LWC \approx -\Delta R^{LW}$ because net surface LW radiation flux is zero if LW surface reflection is negligible. Is this true? The plots of ΔLWC and $-\Delta R^{LW}$ appear almost identical.

13. L219: "lower SSTs, contributed mostly by a stronger SW response.": Fig. 1 shows, especially in the colder simulations, quite similar SW and LW components to the CRE.

14. L228: I'm not sure this is shown clearly enough to be a conclusion. Cloud morphology, overlap, and anvil coverage may also impact the SW reflection.

15. L245: This paragraph generally nicely explains the limitations of the simulation. I would perhaps also add that the finding that increased N_a increases precipitation could lead to moderation of N_a through faster rates of wet deposition, but that here N_a was prescribed.

Figures:

1. Fig. 7: Please explain how this is calculated. It is not obvious to me how Q_r could be held fixed in any given simulation.

Minor comments

1. L74: "vertical pressure velocity" → "clear sky vertical pressure velocity"

2. L81: "and observations to decrease with surface temperature " → "and observations to decrease in coverage with surface temperature "

3. L112: "solar-insulation" → "solar insolation"

4. L115: "is fixed at pre-industrial level (280 ppm)" → "is fixed at the pre-industrial level (280 ppm)"

5. LL118: "The vertical profile of O_3's represents": I suggest simply O_3 or just "ozone"

6. L141: "the isotherm height" → "freezing level"

7. L160: " reduction in cloud ice at the upper troposphere (Fig. 3f),": I think you meant 3h.
8. L199: "calculated as the TOA's LW radiation flux" → "calculated as the TOA's net LW radiation flux". Right?
9. L227: "Hence, the liquid water content in clouds" → "Hence, the column liquid water content in clouds"
10. L256: "ACI": Spell out explicitly here please.

Figures:

1. Fig. 3: Some x-axis labels are overlapping and difficult to read.
2. Fig. 4: "and ice water path (IWP; a)" → "and ice water path (IWP; b)"
3. Fig. 5: "c) vertical pressure velocity" → "c) clear sky vertical pressure velocity"

Suggestions

The comments in this section are intended to be more suggestive. The authors are welcome to take them into consideration but need not feel obligated to.

1. L80: "Beside increasing in height": This is the first explicit mention of the prediction that anvils will rise with FAT. If a reader was not familiar with FAT, it could be nice to introduce it in the paragraph beginning at line 67.
2. Fig. 4 and 10: Perhaps my personal preference, but I would plot on the y-axis the values rather than the difference with the cleanest run. The point would remain the same as this amounts to a vertical shift. If you choose not to, make this more clear in the captions ("relative to the cleanest run for each SST" → "Values are shown as a difference with the cleanest run for each SST." or similar)
3. I might add a thin dashed line on all plots that present changes along the $\Delta\phi = 0$ line where ϕ is the variable being plotted (similar to Fig. 10).
4. The font sizes in plots are inconsistent. Assuming these are generated in Python, this could be fixed by setting `figsize` to a constant value in `plt.subplots(figsize=(width, height))` and then setting the font sizes to constant values in `ax.set_ylabel()`, `ax.set_title()` etc.
5. L182: Perhaps define h_L mathematically to make clear this includes latent energy from freezing.
6. L220: "pushes warm rain initiation to higher levels of the cloud" → "pushes would-be warm rain initiation to higher levels of the cloud" or something similar. In the colder simulations, these "higher levels" are above the freezing level, so rain does not form as well since the ice crystals "steal" water off the droplets that would become rain. In Fig. 3g, it is only the warmest 2 simulations that show increased rain near the freezing level. This is explained well in the following sentences.
7. L253: "allow us to confront our conclusions": Did you mean confirm?

References

Beydoun, H., Caldwell, P. M., Hannah, W. M., & Donahue, A. S. (2021). Dissecting anvil cloud response to sea surface warming [e2021GL094049 2021GL094049]. *Geophysical Research Letters*, *48*(15), e2021GL094049. https://doi.org/https://doi.org/10.1029/2021GL094049

Saint-Lu, M., Bony, S., & Dufresne, J.-L. (2020). Observational evidence for a stability iris effect in the tropics [e2020GL089059 10.1029/2020GL089059]. *Geophysical Research Letters*, *47*(14), e2020GL089059. https://doi.org/https://doi.org/10.1029/2020GL089059