Review of Lorian and Dagan, 2023, revision

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.

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.

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?

   <u>Comments 3-8 refer to the new cloud decomposition and its implications</u>

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).

4. The cloud categories limits may need to be adjusted.
   I.) 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/m2, as thin clouds don't cease to exist at 1 g/m^2, but those thinnest clouds may be less radiatively important.
II.) 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.

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.

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-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?

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?

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?

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

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?

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

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

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.

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.

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:

1. Coper 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)
2. 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).

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 $\mathcal{L}$*and* $\mathcal{I}$*bins"*
And what if it is because low clouds that occur below ice clouds are affecting the result?

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.

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

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!

Page 12, title 3.3:
Please use words.

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.

References:

Cooper et al., 1986; https://doi.org/10.1175/0065-9401-21.43.29
Gasparini et al., 2019; https://doi.org/10.1029/2019MS001736
Gasparini et al., 2022; https://doi.org/10.1175/JCLI-D-21-0211.1
Hartmann and Berry, 2017; https://doi.org/10.1002/2017JD026460
Hong et al., 2015; https://doi.org/10.1175/JCLI-D-14-00666.1
Hong et al., 2016; https://doi.org/10.1175/JCLI-D-15-0799.1
Zelinka et al., 2016; https://doi.org/10.1002/2016GL069917

Best regards,
Blaž Gasparini