Response to Reviewer #2 – Gilbert et al. (under review) GMD

Reviewer comments are in black. Author responses are in blue. *Changes to manuscript are in italic.*

This study investigated the effect of “temperature-dependent cloud optics” on infrared radiation, with a specific focus on the Arctic region. The analysis is done through a combination of a simple mathematical model for two-stream radiative transfer, a single-column atmospheric model, an atmospheric model, and a wind-nudged atmospheric model. The results suggest that the impact of “temperature-dependent cloud optics” is less significant compared to the internal variability in the Arctic region. When model winds are nudged towards reanalysis, the internal variability is partially constrained, and the effect of temperature-dependent cloud optics becomes more prominent.

This study has the potential to update our understanding of the impact of temperature-dependent cloud optics on climate simulations. However, there are a few major issues in this manuscript which I list below. The authors may need to perform additional experiments and data analyses. And based on that, I would recommend major revision.

We thank the reviewer for their time and constructive review. We provide a point-by-point response below.

1. It is not correct to claim that the designed model simulations study the effect of “temperature- dependent cloud optics”. The authors simply switched the cloud optics at 298 K in the original model to the cloud optics at other temperatures. It is essentially cloud optics at a constant temperature (or temperature-independent cloud optics). While it is OK to simply do this in idealized single-column model experiments, because the cloud temperature can be set at any value to quantify the flux changes in the extreme cases, it is not appropriate to do this in the full atmospheric model simulations. Although the authors mentioned in the discussion section that this will be part of future work, “temperature-dependent” is still a confusing term to describe the current approach. I recommend the authors rephrasing it or implementing the physics to the atmospheric model.

   We agree with the reviewer. This issue was also raised by reviewer #1. In response, we have replaced the term “temperature-dependent” with “supercooled liquid”. We have also replaced “temperature-independent” with “room temperature” (i.e. optics at ~298 K).

2. Based on what has been presented in this manuscript, I don’t think the analyses are sufficiently thorough, and the power of model hierarchy on understanding the impact of physical assumptions in climate models is not fully realized in this study. For example, the change of surface downward longwave radiative flux due to the use of temperature-dependent cloud optics is not well quantified. Only spatial pattern of differences between

model runs are shown (Figures 7~10). The ranges given in the manuscript are mostly approximate (e.g., 1~2 W/m^2, 1~3 W/m^2, 1~7 W/m^2, etc.). Also, only surface downward longwave flux changes are quantified here, but the impact on OLR is also important from the perspective of the TOA radiation budget. I suggest that the authors should start from analyzing the global mean and regional mean time series of OLR and surface downward longwave flux, providing an estimate of flux differences, and then go further to analyze the spatial pattern of flux changes.

We agree with the reviewer that we could improve the use of the model hierarchy in the paper and also improve the quantification.

In response to the first point about the power of the model hierarchy, we restructured the paper and the model hierarchy as function of dynamical constraint instead of model complexity. As a part of that restructuring, we have also removed the two-stream radiative transfer model based on comments from both reviewers and the two-stream model not fitting within the revised manuscript framing.

In response to the second point about quantification, we have added spatial averages of the downwelling longwave flux differences. See revised spatial plots and modified Table 3 for Arctic averages.

With regard to OLR, previous work has shown that the supercooled liquid water optics do impact downwelling longwave radiation but had little impact in the Arctic on OLR (Rowe et al. 2013). Similarly, we found very small changes in OLR from the freely evolving climate model run. In the Arctic, the effect of the supercooled liquid water optics ranged from a decrease in OLR (0.04 W m^{-2} – 263 K optics) to an increase in OLR (0.23 W m^{-2} – 273 K optics). Globally, the supercooled liquid water optics increased the OLR 0.08-0.11 W m^{-2}. We added text to the paper but did not add a figure because the effect is small.

L206-211 revised paper:

Although the results thus far focus on downwelling surface longwave radiation, the supercooled liquid water optics that we implemented impact longwave radiation emitted in all directions. Of critical importance, outgoing longwave radiation emitted at the top of the atmosphere (OLR) contributes to the planetary energy balance. Thus, we also assessed the optics impact on OLR from the freely evolving climate model run. We found the globally averaged OLR changes resulting from the optics changes are small (0.08–0.11 W m^{-2}) and not statistically significant. Thus, this short analysis of the OLR provides additional evidence that the influence of the optics change on the freely evolving model is modest.

Finally, we elected to not add timeseries of the fluxes. We think the maps and spatial averages provide ample information to assess the influence of our changes on the mean state. The results are small, and as such, investigating variability seems of second order importance.

3. For the two-stream radiative transfer model described in section 2.3, the authors chose to use a very simple mathematical model to do the calculation. This does not take into account the atmospheric absorption, while it is an important factor that may mask the effect of cloud optics change. The authors may use a more developed two-stream radiative transfer model. For example, RRTMG_LW provides a single-column version that users can specify any profile to test. Using this model, the authors can calculate the flux differences in broad cases and even plot the sensitivity of flux difference to the meteorological factors and cloud properties.

   We agree with the reviewer. This point was also brought up by reviewer #1. In response, we removed the two-stream radiative transfer model from the revised paper.

4. For the single-column atmospheric model, what variables are prescribed by the observations? My understanding is that clouds are not constrained by the observations. For most observational period in Figure 6, the flux difference is very close to 0. Are they cloud-free scenarios? I would suggest filtering out the clear-sky cases and focus on the cloudy scene.

   Here, we clarify the specific variables used to force the single-column atmospheric model. The variables the model relaxed to were observations of temperature and aerosols at every vertical level. The specific variable names listed in the SCAM code were 'T', 'bc_a1', 'bc_a4', 'dst_a1', 'dst_a2', 'dst_a3', 'ncl_a1', 'ncl_a2', 'ncl_a3', 'num_a1', 'num_a2', 'num_a3', 'num_a4', 'pom_a1', 'pom_a4', 'so4_a1', 'so4_a2', 'so4_a3', 'soa_a1', and 'soa_a2', also available in the SCAM namelists we provided. As for the second point about filtering out clear-sky scenes, there were none for the period modeled by the SCAM. This information is provided in Gettelman et al. 2019 and in the SCAM documentation. Thus we do not repeat the specific variables in our paper. Instead, we state generally what is used to force SCAM and point the reader to this paper describing SCAM.

   L93-98 revised paper:

   SCAM has all of the physics parameterizations from the atmospheric component of CESM2, the Community Atmosphere Model Version 6 (CAM), including the radiation scheme RRTMG (Clough et al., 2005; Iacono et al., 2008). SCAM runs the CAM6 physics, including RRTMG, at a single location and prescribes the dynamics state (Gettelman et al., 2019). We forced all SCAM runs with 17 days of observations (temperature and aerosols) from the Mixed-Phase Arctic Cloud Experiment (MPACE) to simulate an Arctic atmosphere with mixed-phase and supercooled liquid-containing clouds (Harrington and Verlinde, 2005).

Specific Comments

1. L19-21: A reference may be necessary to support the statement that "All else being equal, clouds with small particle sizes also scatter more shortwave and emit more downwelling longwave than clouds with large particle sizes.

   We agree with the reviewer's suggestion. In response, we have added Maahn et al. 2021 (https://doi.org/10.1029/2021GL094307) to support the statement "clouds with small particle sizes also scatter more shortwave" and Lubin and Vogelmann 2006 (https://doi.org/10.1038/nature04449) to support the statement "emit more downwelling longwave".

   L19-21 revised paper:

   All else being equal, clouds with small particle sizes also scatter more shortwave (Maahn et al., 2021) and emit more downwelling longwave than clouds with large particle sizes (Lubin and Vogelmann, 2006).

2. L39-40: "Specifically, temperature-dependent liquid water optics are not used in RRTMG." Related to the first major issue, this sentence is very confusing as the authors did not implement the full temperature-dependent liquid water optics in the model, either. The authors may be more specific on what specific cloud optics RRTMG has used (e.g., at 298 K), and point out that this may not reflect the truth in the supercooled liquid cloud regime.

   We agree with the reviewer. In response we have modified the sentence. We changed "temperature-dependent" to "supercooled liquid" and have added sentences to make the reviewer's last point.

   L41-43 revised paper:

   Specifically, supercooled liquid water (240–273 K) optics are not used in RRTMG. Instead, RRTMG uses liquid water optics at one fixed temperature (298 K). Since the RRTMG optics temperature doesn't match supercooled liquid cloud temperatures, the RRTMG optics may not represent radiation emitted by supercooled liquid-containing clouds well.

3. L39: Also cite Clough et al. (2005; https://doi.org/10.1016/j.jqsrt.2004.05.058)

   We agree. In response, we have added the citation to Clough et al. 2005 as suggested by the reviewer.

   L39-41 revised paper:

We identify a cloud optics physics that has not been incorporated into the radiation scheme used by many climate models, RRTMG (Clough et al., 2005; Iacono et al., 2008).

4. L45-47: This long sentence is a bit confusing. “supercooled liquid clouds frequently occur in both observations [...] and the climate model [...] and where the atmosphere is typically cold and dry.” These three are not in parallel. Consider this alternative: “supercooled liquid clouds frequently occur in the cold and dry region, as evidenced by observations and climate model simulations.”

   We agree. In response, we have substituted the reviewer’s phrasing in the paper.

   L51-52 revised paper:

   We focus on the Arctic because it is a cold and dry region where thin supercooled liquid clouds frequently occur in observations (Cesana et al., 2012) and climate model simulations (McIlhattan et al., 2020).

5. L92: For surface, “albedo” is specific for solar radiation. A better term could be “reflectivity”.

   We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant.

6. Figure 2: “reflected ground emission” is ambiguous. A better alternative is “ground emission scattered by clouds”

   We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant.

7. Figure 2: In longwave radiative transfer, it better aligns with the convention to use emissivity rather than reflectivity.

   We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant.

8. Figure 3: For panels (c) and (d), it could be better to visualize the difference between 263 K optics and CESM optics.

   We agree. In response, we have modified Figure 3(c) & (d) to show the difference between the 263 K and CESM control optics. We have also moved Figure 3 to appendix A, so it is now labeled Figure A1, as we think that it fits better there than in the main body of the paper.

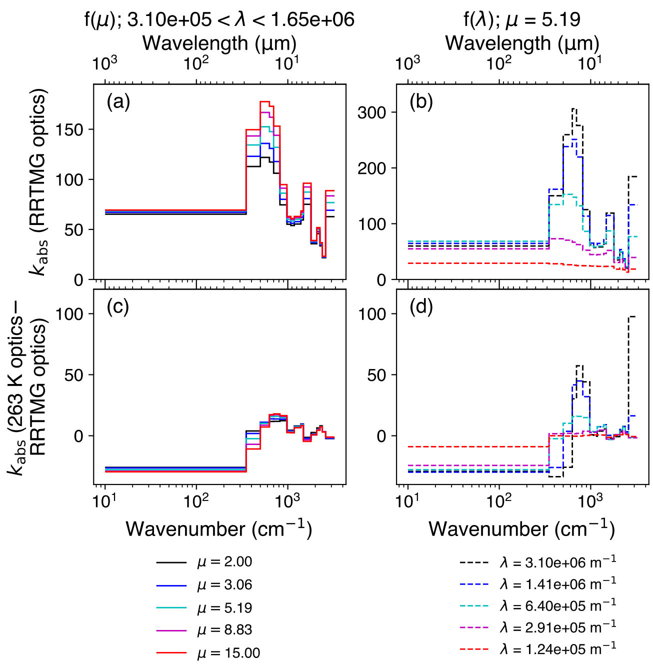


Figure A1. The longwave mass absorption coefficient (k_{abs} (m2 kg−1)) graphed for the current RRTMG liquid optical properties (a) & (b) as function of wavenumber ad wavelength. The difference in longwave mass absorption coefficient between new liquid optical properties calculated from the 263 K complex refractive index (Rowe et al., 2020) and the current RRTMG liquid optical properties (c) & (d) is also graphed as a function of wavenumber and wavelength. In RRTMG, k_{abs} is a lookup table in terms of the parameters μ and 1/λ that describe the droplet size distribution where λ is a function of μ. (b) and (d) are the k_{abs} spectra at a fixed μ and five λ. (a) and (c) are the k_{abs} spectra at five μ and their corresponding λ.

9. Table 1: Do these model runs include model spin-up period? It takes time for the model to adjust to the new state.

   Analysis of timeseries showed little evidence for a need to spin-up the model. When the atmosphere is freely evolving, atmospheric processes spin up within days. When the wind nudging is being used, spin up is not a concern for this work.

10. Table 1: Why is the 263 K run missing in the F1850 experiment? Especially consider that Figure 3 highlights the comparison between 263 K optics and CESM optics, and also the 263 K run appears in all other experiments.

    Initially we wanted to test the extremes of optics set, 240 K and 273 K, and so we only ran those optics sets for the F1850 experiment. After those experiments, we evaluated which optics set was the closest to Arctic cloud temperature and found that 263 K was the closest. Thereafter we used 263 K optics.

All this said - we agree with the reviewer that considering all other experiments have a 263 K optics run, F1850 should as well. In response, we ran and added an F1850 263 K optics run. While this addition does make the study more complete, it did not change the main results.

11. L141: “the next time step”. Note that 6-hourly ERA-Interim reanalysis is used here while the model step is 30 minutes by default. According to the referred literature, this is indeed the next available analysis time, not the next model time. Please be more specific and clear.

We agree. In response, we have clarified our language in this sentence.

L114-120 revised paper:

Nudging is implemented following:

… (Equations)

where F (x) the internal tendency without nudging, F_{nudge} is the nudging term, α is the strength coefficient that is 0 where nudging is not enabled and 1 where nudging is enabled, O(t'next) is the target state at future target time step, x(t) is the model state at the current model time step, and τ is the relaxation time between the next target time step and the current model time step (Blanchard-Wrigglesworth et al., 2021; Roach and Blanchard-Wrigglesworth, 2022).

12. Figure 4: In panel (a), I noticed that there is a smoothing gradient at the boundaries of the latitudinal band. The previous study cited by the authors explicitly mentioned that they applied smoothing (by setting α to a value between 0 and 1 in some region). Did the authors also apply the same technique? Also, in panel (b), a solid line is connected between α = 0 and α = 1 at around 800 hPa. Is the smoothing technique also applied here? To make it clear, instead of using line plot, the authors may choose scatter plot instead to visualize the exact α values at each discrete layer.

Yes, the authors smoothed both at the vertical boundary and horizontal boundary using a sharpness parameter provided in the nudging namelists. We have added a sentence to clarify this for the reader.

L121-122 revised paper:

At both the vertical and horizontal nudging boundaries, we applied smoothing.

13. L164-165: “the downwelling irradiance and flux was higher for temperature-dependent optics than temperature-independent optics” This is confusing. It would be better to state

that the downwelling irradiance and flux was higher for cloud optics at X temperature than the optics at Y temperature.

We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant.

14. L165: "The thinnest clouds [...] showed the largest difference." This statement is not supported by Figure 5, as no results are presented for clouds at different thickness.

    We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant.

15. L167: What is the meaning of "all cloud temperatures"? Rephrase this sentence.

    We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant.

16. L168-169: "However, as cloud thickness increased from 100 to 500 m [...]" This is not shown in any figure.

    We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant.

17. L170~171: "but our model was meant to be a proof of concept and not realistic". Why not use a realistic model, given that a quantitative estimate of the effect is provided above (0.35 W/m^2)?

    We agree. That said, we removed the entire two-stream radiative transfer model section so this change is no longer relevant. Additionally, previous work had used a high resolution line-by-line radiative transfer model to demonstrate an effect from the optics of 1.7 W m^{-2} (Rowe et al. 2013).

18. L177-179: The authors mentioned that when cloud optics at different temperatures are used, the cloud fraction and cloud phase in the simulations are different. I assume that the authors do not prescribe the model simulations with observed clouds. What are the differences in cloud fraction and properties exactly? Having these differences, I don't think this is an apple-to-appple comparison to show the net effect of cloud optics at different temperatures since cloud variability has played a role.

    We plotted the differences in cloud fraction, cloud liquid, cloud ice, and dominant cloud species between all the SCAM runs. We found little difference in all cloud properties between the optics sets. However, those differences in cloud properties concurrently occurred with the large differences (over 10 W m-2) between the different optics SCAM runs. These large differences also drove our decision to subset the downwelling longwave fluxes, only including optically thin low-level supercooled liquid clouds. This subsetting removed any large flux differences caused by cloud property and phase differences.

19. Figure 7: I don't see stippling in the figure, so it is better to say that no significance in the figure caption.

We appreciate this suggestion, but did not add it. We think it is clearer to state the significant results would be stippled. We do not want text that could be confusing saying the double negative of results that are not significant are not stippled.

20. Figure 8: What's the regional mean difference in these plots? The average can be performed over 50oN~90oN, consistent with the given latitudinal band in Table 3, and the values can be added to the panel title.

We agree and have added the regional mean difference to Table 3. However, we calculated the regional mean over 60-90N to match the wind nudging domain.

21. L208: I suggest adding "at 5% significance level" to be more accurate and specific.

We agree. In response, we have fixed the language.

L172-173 revised paper:

Critically, many flux differences were statistically significant at the 95 % confidence level.

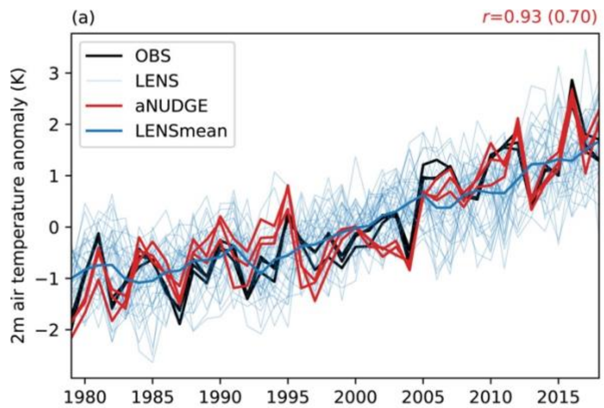
22. L209-210: "because the wind nudging reduced the variability in the annual mean flux between the ensemble members" A figure may be necessary to show this. If there are too many figures, consider combining the information in one figure. For instance, Figures 7~10 show similar information and can be merged into one figure.

This sentence was removed in our revised manuscript. In response, we re-worded:

L174-176 revised paper:

The flux differences were statistically significant in this experiment because the wind nudging reduced noise caused by different atmospheric circulation sequences and emphasized the signal from the supercooled liquid water optics.

Second, wind nudging has been shown in prior studies to reduce ensemble spread between members (Roach and Blanchard-Wrigglesworth 2022).



The figure shown above is from Roach & Blanchard-Wrigglesworth 2022 (Fig. 2a) and plots Arctic surface temperature from observations (OBS, black), the CESM1 large ensemble (LENS, blue), and a wind-nudged ensemble (aNUDGE, red). The 40 CESM1 large ensemble members are dynamically unconstrained and have considerable ensemble spread between members. Additionally, the members do not sync up with the interannual variability of the observations. However, all nudged ensemble members match very closely with each other and observations, substantially reducing spread between members. This example shows that nudging all ensemble members to the same set of winds can reduce spread and variability between the members.

23. L218~220: “no flux differences [...] were statistically significant” Instead of setting some threshold, I suggest providing a *p*-value so that we can understand how far it is from the significance threshold.

For our method of controlling the false discovery rate (Wilks 2016), the critical threshold (normally 0.05), is modified as a function of the p-values. In the case of this experiment, the value for the critical threshold revealed that no flux differences had statistically significant p-values. Additionally, each grid box has its own p-value, so there is no single p-value to provide.

24. L232-236: Given that the authors simply change the cloud optics at another temperature, the effect on mean 2-m air temperature difference should be more prominent than the effect on 2-m air temperature trend, since the temperature-cloud property feedback is muted. Also, considering that no greenhouse gas and aerosol forcings are included in the simulations, it makes no sense to compare to the ERA-I 2-m air temperature trend.

We agree with the reviewer. We also concluded that the temperature time series doesn’t make sense to include in this paper. In response, we removed the figure and any discussion of surface temperature.

25. L246-247: "Whereas for the global cliate model, an effect of a few W m^{-2} is within climate variability and thus relatively small." Note that the historical change in effective radiative forcing from 1750 and 2019 is also a few W m^{-2}.

The reviewer is correct that the results are nuanced. As we wrote to reviewer #1, detecting a signal due to the cloud optics change required strong dynamical constraints. When those dynamical constraints were removed, the signal was not statistically significant at the 95% confidence level above the chaotic atmospheric noise. Additionally here, we note that the effect is small (less than 1 W m^{-2}) and also smaller than the observed change in effective radiative forcing.

26. Table 3: The values in the "Effect of optics" column should be the regional mean values as defined in the "Spatial scale" column.

We agree. In response, we have added the spatial mean values as defined in the spatial scale column to Table 3. Any value ranges in the "Effect of optics" column represent the minimum and maximum effects from multiple supercooled optics sets, i.e. 0.36-0.68 W m^{-2} where 0.36 is the effect of the 263 K optics and 0.68 is the effect of the 273 K optics. The spatial mean differences have also been added to all spatial plots.