Response to Reviewer #1 – Gilbert et al. (under review) GMD

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

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

This manuscript seeks to understand how a change in the specification of the optical properties of liquid clouds might affect the simulation of Arctic climate. The change is motivated on physical grounds - the index of refraction of water is temperature dependent but this dependence is usually ignored when mapping cloud physical to cloud optical properties in broadband codes. Mie calculations are performed for drop size distributions consistent with the CESM2 climate model using indexes of refraction valid at 273, 263, and 240K; these are used in an off-line radiative transfer model across limited spectral ranges in the infrared, as well as in single-column simulations, uncoupled and coupled freely-running ensembles, and in an ensemble in which winds are nudged towards reanalysis at high latitudes. Small differences in spectrally-resolved fluxes in the offline simulations; differences in integrated fluxes are lost in the variability in single-column and free-running model simulations, becoming large enough to be distinguished from noise, if still small, in the nudged simulations.

The manuscript has two goals: 1) to assess the possible impact of an elaboration of cloud optics on simulations in the Arctic, and 2) to develop methods for such an assessment. Both goals might be reached more effectively by revisiting the experimental design to more clearly delineate the perturbation that might be expected from such a change from any subsequent impact on simulations.

The motivation for the study is well-grounded: the index of refraction of liquid water does indeed depend on temperature, so the degree to which fluxes might be systematically biased in some circumstances is not know a priori. What can be anticipated, however, is that the impact on fluxes will be restricted to thin clouds, since (band-wise) fluxes will only change when (band-wise) optical thickness is in the range of roughly 0.5 - 3 i.e. where the clouds are neither optically thin or thick. (That this is not illustrated using the two-stream model is a missed opportunity.) The impact of changes in the index of refraction thus depends on the population of clouds and atmospheric states. The magnitude of this change for a given population of clouds, such as those produced by a particular climate model, could be evaluated with off-line broadband radiation calculations.

That this impact of the optics is limited to thin clouds is illustrated in the paper by the two-stream radiative transfer model. From the unrevised paper L164-169:

As expected from Rowe et al. (2013), the downwelling irradiance and flux was higher for temperature-dependent optics than temperature-independent optics. The thinnest clouds (100 m thick with optical depth $\tau \sim 1–1.5$) showed the largest difference in downwelling flux between the temperature-dependent and temperature-independent optics (Fig. 5). For the 100 m thick cloud, all cloud temperatures had a 0.35 W m−2 flux difference between the temperature-dependent and temperature-independent optics. However, as cloud thickness increased from 100 to 500 m (τ ~ 4–8) and 1000 m (τ ~ 10–15), the difference caused by our cloud optics change was negligible.

We appreciate the reviewer's framing for the estimation the supercooled liquid optics effect and have modified the paper to reflect it:

L43-46 revised paper:

For instance, using a high spectral resolution line-by-line radiative transfer model applied to case studies in the Arctic, Rowe et al. (2013) found that these supercooled liquid water optics can increase modeled longwave fluxes emitted by thin (liquid water path < 10 g m^{-2}) supercooled liquid-containing clouds by up to 1.7 W m^{-2}.

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).

The title and framing of the manuscript is misleading: tests in the dynamical models do not use temperature-dependent cloud optics; rather they replace cloud optics computed with the index of refraction used at a single temperature with optics computed at a different temperature. Whether accounting for the temperature dependence of cloud optical properties would impact fluxes and/or other simulation characteristics can not be assessed with the current information.

We appreciate this important communication issue brought up by the reviewer, which was also brought up by reviewer #2. We agree with the reviewer's comment that the authors are not using "temperature-dependent" cloud optics, but optics at a single temperature applied to liquid water at all temperatures. Therefore, we have changed the language "temperature-dependent" optics in the paper to "supercooled liquid" optics to accurately reflect our methodology, results, and conclusions. We have also changed the language "temperature-independent" optics to "room temperature" optics.

The authors assert that the computational cost of implementing cloud optics that depend on temperature would be "immense" (lines 282-287) but this is unlikely to be true: cloud optics are usually a tiny portion of the time spent in radiation calculations.

We agree that using the word "immense" is unnecessary and have removed it, but fully implementing the temperature-dependent optics would add time & complexity. In climate model development, it is best practice to not add time and complexity to the model unless necessary.

Considering the modest impacts these temperature-dependent optics would on climate, implementing them is not a first priority.

Fully implementing the temperature-dependent optics would involve taking the grid box temperature, which would presumably fall between two sets of supercooled liquid optics, and linearly interpolating the two sets of optics the temperature fell between to create a set of liquid water optics to perfectly match the grid box temperature. This process would have to be repeated for each grid box in the atmosphere containing a cloud, at every time step the radiation code is run. This implementation would not add immense time and complexity to the radiation code, but it wouldn't be negligible either. This cost is why we recommend the implementation described L285-287 of the unrevised paper. It involves matching the grid box temperature to the temperature of the closest supercooled liquid optics set available and using that optics set. This implementation does add time and complexity, but considerably less than the first option.

L251-258 revised paper:

Finally, fully implementing the supercooled liquid water optics would increase the model computational cost. In our study, we switched out the liquid optics lookup table, which didn't change the computational cost. Ideally, the model would match the cloud temperature and optics temperature by interpolating the optics properties. This implementation would involve the model performing that interpolation at every timestep and grid cell, increasing the cost of the already costly radiation scheme significantly. One possible compromise to these two implementation approaches would be to find the optics set closest to the cloud temperature and use that lookup table. We expect this third approach would be easy to implement and nominally increase the radiation scheme's computational cost.

Interpretation

The motivation for the "model hierarchy" is not made clear. The answers to the questions on lines 74-83 are tautological, i.e. the single column model is motivated by asking what the impact is at a given location during a finite time frame. Linking each set of simulations to a testable hypothesis will help readers make sense of results.

We agree with the reviewer and the motivation and clarity of the model hierarchy was also brought up by reviewer #1. In response to both reviewer comments, the authors have decided to restructure the paper and the model hierarchy as a function of dynamical constraint, not model complexity. As a part of the restructuring, we removed the questions in the model hierarchy outline, as they no longer felt appropriate to the authors.

Parametric sensitivity studies, as in Rowe et al. 2013 and as might be done with the spectrally-resolved model are useful in motivating the work. A missing step is broadband calculations analogous to those used in the global model simulations - say, offline calculations with RRTMG over the distribution of Arctic clouds produced by CESM - to understand how those parametric sensitivities convolve with the population of Arctic clouds in the model to be examined and whether one might expect systemic differences in interactive simulations. It is unclear what is gained from the wind-nudged simulations, which are motivated by trying to constrain internal variability, that wouldn't emerge more clearly from calculations applying changed optics to the clouds produced by the unperturbed model.

We are concerned that the reviewer thinks that it is "unclear what is gained from the wind-nudged simulations". Our results show that the only simulations in which the optics change is detected are when the wind-nudging has been applied. In the "unperturbed" model (i.e., the freely evolving global model), the signal from the optics change is hard to detect and small compared to the model generated variability. We explain more here for the reviewer to address this confusion.

In the freely evolving global climate model, simulations with have different sequencing of atmospheric events. These differing sequences of atmospheric events can make it hard to detect a difference due to a cloud optics change. For example, the control optics simulation might have produced an extra-tropical storm in the Arctic in August of year 15 whereas the 240 K optics simulations might not have due to differing atmospheric sequencing due to inherent chaos (i.e., atmospheric internal variability). So, when we compare the mean downwelling longwave at the surface between the control optics and supercooled liquid optics simulations, there are different sequences of atmospheric events in each mean downwelling longwave timeseries in addition to any difference caused by the optics alone.

The wind nudging is an attempt to remove the "noise" of different atmospheric sequences due to the chaos from the signal of the supercooled liquid optics. By dynamically constraining both the control and supercooled liquid optics simulations to the same atmospheric sequence, both simulations produce the same sequence of the winds and large atmospheric circulation. Therefore, the noise caused by different sequences of storms and clouds is reduced and the signal from the supercooled liquid optics is emphasized. In short, the wind nudging increases the signal-to-noise ratio for the longwave effect of supercooled liquid optics.

We believe this confusion has arisen from our text in the original manuscript not being clear. Thus, we have also modified the text to make these points clearer as well at multiple points in the revised manuscript.

L30-31 revised paper:

A key advantage of prescribing the winds using nudging is that the time evolution of the prescribed and modeled large-scale circulation is synchronized to the prescribed wind time evolution.

L36-38 revised paper:

These studies show that wind nudging is a powerful tool to amplify a radiative signal above chaotic atmospheric noise by constraining the time sequence of the modeled atmospheric circulation.

L55-59 revised paper:

We anticipate using this hierarchy of constraint on the modeled atmospheric circulation sequencing will be of value. We expect the most dynamically constrained models will enable the easiest detection of the optics change. In contrast, dynamically unconstrained models will have more noise from internal climate variability and that noise may make it hard to detect the optics change signal.

L123-126 revised paper:

Nudging the winds constrains the internal variability of the modeled climate system to a specific sequence of atmospheric circulation, which was the ERA-I winds in our experiments. Since all experiments were constrained to the same atmospheric circulation sequence, they were all likely to model the same sequence of clouds.

Single-column model simulations can be expected to diverge somewhat in response to even tiny changes, making the interpretation of changes on particular days in a long simulation ambiguous. (Did the authors consider doing ensembles of single-column model simulations to see if the cloud optics change can be teased out?).

We thank the reviewer for raising this issue. The single-column model we used was relaxed to temperature and aerosol observations and the dynamics were prescribed. Therefore, given the constraints on the single-column model, the internal variability in the model is negligible and no ensembles are necessary. This specific model, SCAM, was designed to evaluate physics parameterizations (Gettleman et al. 2019; https://doi. org/10.1029/2018MS001578). We have modified the paper to clarify this for the reader:

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).

What is the motivation for simulations with global models? Such simulations are useful when scales interact - here, if the change in cloud optics might be expected to systematically impact interactions between the Arctic and rest of the world. Is that expected? If not why wouldn't assessment with regional model be more informative?

We ran global models to assess the impact globally since the optics were applied globally. However, we focused on the Arctic because we know optically thin clouds occur there and the optics have the largest effect for those cloud types.

We clarified this important point multiple times in the revised manuscript:

L43-46 revised paper:

For instance, using a high spectral resolution line-by-line radiative transfer model applied to case studies in the Arctic, Rowe et al. (2013) found that these supercooled liquid water optics can increase modeled longwave fluxes emitted by thin (liquid water path < 10 g m^{-2}*) supercooled liquid-containing clouds by up to 1.7 W* m^{-2}*.*

L51-53 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). Thus, we anticipate the clouds optics change may have a substantial impact on Arctic longwave fluxes.

The claim (repeated seven times) that the approach represents a "novel model hierarchy" is not well-founded. "Model hierarchy" refers to sets of equations representing the same underlying system with different levels of complexity. It's a stretch to call a configuration in which winds are relaxed to time-varying empirical values a separate element and the idealized radiative transfer calculations are clearly a different beast. As the authors note the use of wind nudging is not novel. The work can stand on its own without claims to greater generality than are supported.

We very much appreciate and agree with the reviewer's comments. In response, we changed our entire approach to describing the model experiments that we use. Instead in our revised paper, we removed the two-stream radiative transfer model and only present the influence of the cloud optics change in the RRTMG radiation scheme. We then test the influence of the optics change in models with differing levels of dynamical constraint.

See lines 54-60 and section 2.2 of the revised manuscript for an overview of the new approach:

L54-60 revised paper:

A novel aspect of this study is using a hierarchy of models to assess the relevance of this cloud optics change. All models use the same radiation scheme (RRTMG), but vary in the degree to which the atmosphere is dynamically constrained. We anticipate using this hierarchy of constraint on the modeled atmospheric circulation sequencing will be of value. We expect the most dynamically constrained models will enable the easiest detection of the optics change. In contrast, dynamically unconstrained models will have more noise from internal climate variability and that noise may make it hard to detect the optics change signal. While this study focuses on *one specific cloud optics change, the methods used here are applicable to any model physics change and therefore should be of broad interest to the model development community.*

Section 2.2 – L69-83 revised paper:

2.2 Model hierarchy

In this work, we evaluate the effect of changing the liquid water optics from room temperature to supercooled on longwave radiation across a range of dynamically constrained models, while keeping the radiation scheme the same. The models in our hierarchy proceed from the most to least dynamically constrained atmosphere:

*1. **Single-column atmospheric model:** a completely constrained model at a single location on a daily time scale*

*2. **Wind-nudged global climate model configurations:***

- *(a) **Atmosphere-only (short time scale):** a global dynamically constrained model on an annual time scale*
- *(b) **Atmosphere-only (long time scale):** a global dynamically constrained model on a decadal time scale*
- *(c) **Fully coupled (short time scale):** a global fully coupled dynamically constrained model on an annual time scale*

*3. **Freely evolving global climate model:** an unconstrained global climate model on a decadal time scale*

For each model, we compared the longwave radiation produced using room temperature water optics against longwave radiation produced using supercooled liquid water optics. Then, we evaluated whether the difference in radiation was detectable and statistically significant. Finally, we assessed at what time and spatial scales and degree of dynamical constraint the supercooled liquid water optics mattered. Primarily, we focus on the downwelling longwave flux at the surface (W m^{-2}) to evaluate if the optics changed.

Some more minor comments

Most figures could be more carefully crafted to emphasize the narrative points being made. Some figures (2, 12) illustrate concepts that emerge clearly from the text. Others contain information that's visually hard to parse. Figure 3, for examples, requires readers to mentally subtract lines from two different panels, in addition to showing variations with respect to two related by hard-to-interpret quantities, while the information density of figure 4 is low. The authors might fruitfully review each figure and refine those that do not advance the story being told.

We appreciate the reviewer's feedback on these figures. We agree with these concerns. In response, we have moved Figure 3 to the appendix and changed panels (c) and (d) to be the difference between the supercooled and current CESM2 optics. We have also removed Figure 4, agreeing with the reviewer that it adds little to the paper.

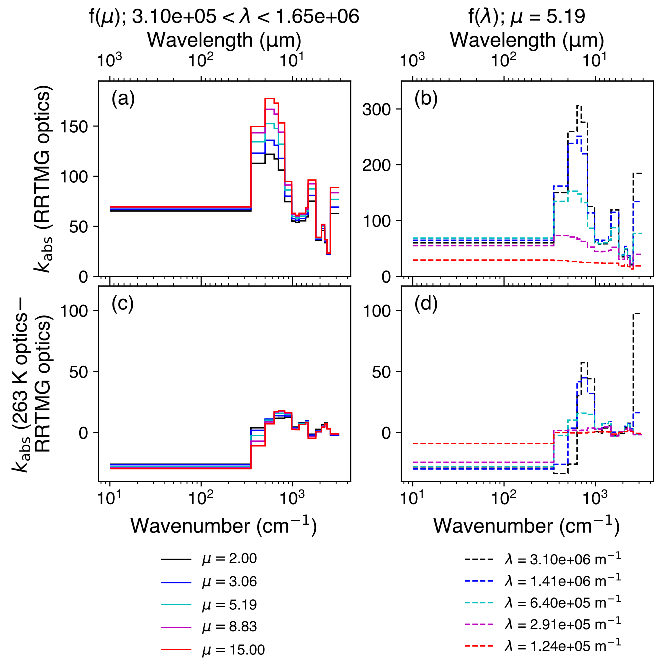

Figure A1. The longwave mass absorption coefficient (k_{abs} (m2 kg−1)) graphed for the current RRTMG liquid optical properties (a) & (b) as function of wavenumber ad wavelength. The difference in longwave mass absorption coefficient between new liquid optical properties calculated from the 263 K complex 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.

The captions of Figure 7 and later note that statistical significance is assessed "following Wilks (2016)" but the text provides no elaboration. Is significance computed accounting for false discovery rate? If so this should be noted more clearly in the main text.

Yes, we did use Wilks (2016) to account for the false discovery rate. We have added language in all the relevant figure captions to clarify that for the readers.

One example of an added sentence about Wilks (2016) from the Figure 3 caption in the revised paper:

False discovery rate was controlled for using Wilks (2016).

The authors are quite free with advice to others (e.g. line 238, line 249, line 279). This may be worth revisiting given the nuanced results obtained.

We agree that the results are nuanced. 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. Therefore, we think these changes should be added to RRTMG, just not as a first priority.

The formulation of the offline radiative transfer model in appendix A is confusing. The offline model is used to compute longwave fluxes. Liquid clouds do not scatter longwave radiation so it's not clear why one would use two-stream equations representing multiple scattering (A7-A11). It would be far simpler to use Schwartzchild's equation, potentially accounting for intra-layer temperature gradients as in section 2.1 of Clough et al. 1992 (doi:10.1029/92JD01419). Indeed that's what models like RRTMG do.

We agree that the offline radiative transfer model is not needed and added unnecessary confusion. As such, we removed the two-stream radiative transfer model and thus Appendix A1.

Please note, however, that liquid clouds do scatter longwave radiation. We agree that this effect is very small for downwelling longwave radiation, but multiple scattering by liquid clouds may be important for upwelling radiation, as it causes biases due to using the incorrect CRI for even the thickest clouds, as noted by Rowe et al. (2013). While we do not explore upwelling radiation in this work, since the effect is largest in the tropics and small in the Arctic, we point out in the discussion that the effect on upwelling longwave radiation is a topic of interest for future work.

The equations in both appendicies are well-known and the tables are available in the original literature. Since neither sheds light on the problem at hand they can be safely omitted.

We agree with the reviewer about appendix A1. In response, we have removed it.

However, the equations in appendix A2 describing the calculation of CESM2 liquid water optics were hard to find, understand, and reconstruct from CESM2 CAM6 documentation. Therefore, we retained these equations in the appendix to make our study reproducible and well documented.