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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Radiative consequences of low-temperature infrared refractive indices for supercooled water clouds" *by* P. M. Rowe et al.

P. M. Rowe et al.

pennyr@uidaho.edu

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[acpd, hvmath]copernicus$_{discussionsamsmath}$

1]Penny M. Rowe 2]Steven Neshyba 1]Von P. Walden

[1]Department of Geography, University of Idaho, 875 Perimeter Drive, Moscow, ID, 83844 USA. [2]Department of Chemistry, University of Puget Sound, 1500 N. Warner, Tacoma, WA, 98416 USA.

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We thank the referee for this thoughtful review. We have responded to the referee's questions (italics) in turn below.

1 Main Questions/Comments

1.1 *One challenge faced by the authors is the coupling of the CRI values from Zasetsky and Wagner et al., which are not the same in the overlapping spectral regions. This coupling needs to be done as there is some inconsistency between the CRI between these two datasets and the typically used temperature-independent CRI from Downing and Williams, and the Wagner et al dataset does not cover the entire thermal IR spectral region. The authors did estimate the uncertainty between the two temperature-dependent CRIs, but this was not propagated into either the radiative flux calculations or the retrieved cloud properties. [Or, if it was, it wasn't clear from the manuscript.]*

We did propagate uncertainties in the temperature-dependent CRIs to uncertainties in both radiative flux calculations and retrieved cloud properties, and regret this was not clear. We have made extensive changes to clarify. To avoid confusion, we have pasted below the entire new section 4.1.

Uncertainties in the calculated flux differences depend on uncertainties in the measured CRI. We use the Downing and Williams (1975) CRI as representative of the ∼300K CRI. It differs from two other commonly-used CRI, that of Bertie and Lan (1996) and Hale and Querry (1973), by 1% for the real part of the CRI and 4% for the imaginary part, for 460 to 990 cm^{-1}. As shown below, this variation is small compared to uncertainties in the temperature-dependent CRI.

The uncertainties in the CRI measurements of Zasetsky et al. (2005) and Wagner et al. (2005) are not well known. We use the comparison between the two sets between 800 and 930 cm^{-1} to get estimates of the uncertainty: differences are $\sim 8\%$, $\sim 6\%$, $\sim 3\%$, and $\sim 5\%$ for the 240K, 253K, 263K, and 273K measurements, respectively.

Uncertainty in the CRI, σ_C, is propagated to uncertainty in the radiative flux calcula-

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tions, σ_F, according to

$$\sigma_F^2 = \left[\frac{\delta F}{\delta C}\right]^2 \sigma_C^2, \quad (1)$$

where we calculate the change in flux with a change in CRI, $\frac{\delta F}{\delta C}$, numerically. For this, it is convenient to use as δF the flux difference that we have already calculated, that is, the change in flux for a change in CRI from that of Downing and Williams at 300 K to the temperature-dependent CRI. Thus δC is then $C_{300K} - C_T$, where C_T is, e.g. C_{240K}. For example, the uncertainty in the flux computed using the 240K CRI, $F(C_{240K})$, is calculated as

$$\sigma_{F(C_{240K})}^2 = \left[\frac{F(C_{300K}) - F(C_{240K})}{C_{300K} - C_{240K}}\right]^2 \sigma_{C_{240K}}^2. \quad (2)$$

The 240K CRI is on average 50% different than the 300 K CRI, so the denominator of eqn. (2) is $0.5C_{240K}$. As previously stated, the uncertainty in the 240 K CRI is estimated to be 8%, so $\sigma_{C_{240K}} = 0.08C_{240K}$. Plugging these into eqn. (2) gives an uncertainty of $0.2[F(C_{300K}) - F(C_{240K})]$, or 20% of the flux difference.

CRI measured at 253 K, 263 K, and 273 K differ from the the 300 K indices by 30%, 20%, and 20%. Thus, as a fraction of flux difference for each CRI temperature, uncertainty estimates made in this manner are 20% for $F(C_{253K})$, 20% for $F(C_{263K})$, and 30% for $F(C_{273K})$.

Uncertainties in cloud property estimates were calculated in a similar manner and are estimated to be about 20%.

Note that the new temperature-dependent CRI are created from interpolating the real and imaginary parts separately. A rigorous treatment would be to recalculate the real part of the CRI from the imaginary part using the Kramers-Kronig transform. However,

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any errors in the real part of the CRI due to this method should be smaller than errors in the measurements of Zasetsky et al. and Wagner et al., from which they are derived.

1.2 *The authors pointed out a large difference in the Zasetsky CRI at 273 K and the Hale and Querry data at 300 K. However, they did not elaborate on the need to resolve this, nor make any suggestions on which may be more accurate. (Certainly there would be no "ice like domains" in at 273, so it would seem that the Zasetsky and Hale & Querry results should be the same at 273 or 300 K.)*

As previously stated, the difference between the 273K and 300K CRI is 18% compared to our uncertainty estimate of 5% for the 273K CRI. Thus the difference is real to within the uncertainty for the 273K CRI. This suggests that there may be a temperature dependence even between 273 K and 300 K. Furthermore, research suggests that low-density domains may exist above 273 K. These are important topics for future research, but are beyond the scope of this paper. We have made the changes to clarify, including the following:

Page 18751 lines 15-16: We have changed "(This behavior is attributed to low density, ice-like domains in supercooled water (Zasetsky et al., 2004)." to "Zasetsky et al. (2004) attribute this behavior to low density domains in supercooled water. They find evidence that the frequency of low-density domains increases from 300 K down to 240 K. Thus low-density domains may also explain the differences between the 273 K and the 300 K CRI."

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1.3 *Furthermore, a previously published work by Cadeddu and Turner (IEEE Trans. Geosci. Remote Sens., 2011) demonstrated (a) with real spectral IR observations that the use of the Zasetsky CRI resulted in poorer fits relative to the Downing & Williams CRI data, and (b) that using the Zasetsky data did not give consistent results with the liquid water absorption coefficients at microwave frequencies. The results of this paper should be connected with the Cadeddu paper, as it would seem to strengthen the discussion here.*

We do not believe discussion of the LWPs retrieved by Cadeddu and Turner is warranted in this paper because the retrievals do not use the same spectral regions. We use 460 to 660 cm^{-1} and 740 to 1000 cm^{-1}, whereas their retrieval algorithm uses 820.0, 901.6, 1128.5, 1145.1, 2455.0, 2610.0, and 2860.0 cm^{-1}. Importantly, the 273 K and 263 K CRI of Zasetsky et al. have large errors from 1000 to 1300 cm^{-1}, and therefore were excluded from our retrieval algorithm. The fact that Cadeddu and Turner retrieved liquid water paths that were significantly higher than results using Downing and Williams is consistent with their inclusion of this unreliable spectral range.

2 Minor comments:

Please note that the line numbers in this section are inconsistent with the line numbers in the online manuscript. We have responded as well as we can, but hope some clarification is possible.

2.1 *Line 11: '...cloud optical thickness...*

We require a clarification on this point.

2.2 *Line 165-166: in tropical atmospheres, the transmission in the IR window is 0.4, and I would not classify this as "weak" absorption.*

(We assume page 18754, line 15). This is a good point. We have changed "absorption from trace gases is weak, so most of the surface flux is transmitted to the TOA" to "gaseous absorption coefficients are weak enough to permit some of the surface flux to be transmitted to the TOA."

2.3 *Line 175ish: A more general statement is that for upwelling cases, the largest impact is where the contrast between Tsfc and Tcloud is the largest.*

(page 18754, lines 20-22). We agree. We have changed, "Thus, for upwelling flux, the greatest sensitivity to supercooled cloud content occurs in warm, wet atmospheres, in the window regions," to "Thus, for upwelling flux, the greatest sensitivity to supercooled cloud content occurs in warm locations, where the temperature contrast between the surface and the cloud is largest, in the window regions."

2.4 *Line 163 and 177: I believe you mean Fig2, not Fig3*

This may be a mistake found in an earlier draft that has since been corrected. If possible, please let us know the page number and line number of the discussion paper. We found two references to Fig. 3 on page 18759 (lines 17 and 18); these are both correct.

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2.5 *Section 4: this is a good discussion of uncertainty, but since you are comparing calculations from DISORT against different calculations, most of these error sources cancel out. Thus, this section could be reduced significantly to focus on only the uncertainties of the CRI, which is the most important part of the discussion (and should be enhanced a bit, see above)*

We have moved most of the uncertainty discussion to the supplemental and included the uncertainty analysis given above for uncertainties in the CRI.

2.6 *Fig 3 caption: Did you mean a LWP of 8 g/m2 (instead of 4 g/m2)?*

Yes, it should be LWP of 8 g/m2 and an effective radius (not particle size) of 5 micron. We have made those changes. Also, Fig 2. caption was changed to (a) Flux upwelling and (b) Flux downwelling.

2.7 *Lines 410-420: It seems like the differences in absorption due to the differences in the CRI are more important that the differences in the scattering that results?*

Without correct line numbers to refer to, we are unable to respond to this comment.

2.8 *Fig. 4: How does these results change if you used the Zasetsky CRI at 273 instead of the temperature-independent CRIs at 300K? This would seem to get directly at the importance of the bias at the warm temperatures.*

This is an interesting point to address in future work. The purpose of this paper was to compare to CRI that are most commonly used. Based on the CRI, we speculate that results would be similar, but errors would be smaller.

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2.9 *Line 426: Why did you compare the surface and TOA fluxes against the fluxes at the tropopause? You can use DISORT to compute the fluxes at the tropopause directly, which can then be compared to show the importance*

(Page 18761, lines 12-16). We wanted to determine errors in fluxes at the surface and TOA. We compared to the value in this reference (Ramaswamy et al., 2001) because it is a useful metric for radiative forcing, despite being at a different height.

2.10 *Section 5.4: The ice and liquid cloud retrievals: are the ice and water particles in the same volume, or was the cloud modeled as a liquid layer over the ice layer or vice versa? If they were considered to be in the same volume, did you model the particles as internal or external mixtures?*

The ice and liquid are in the same volume, modeled as external mixtures. We have added this to the text.

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