

The authors have done an admirable job in finding a better case study to illustrate the measurement of cloud radiative properties. The results in this version are far better than in the initial version of this manuscript. The spectra of absorptance, reflectance, and transmittance are all in agreement with what is generally expected from theory and previous measurements of cloud spectral irradiance. However, the manuscript still requires revision before it is suitable for publication.

Line: 13: The authors list crystal shape, effective radius, and optical thickness as cloud particle properties important on the spectral optical layer properties (optical thickness is not a particle property). Throughout the paper the authors attribute the spectral irradiance differences to crystal shape. Of the three listed, shape has the smallest effect on the spectral irradiance. The optical thickness and effective radius are the two main drivers of cirrus cloud layer properties. The authors need to better delineate and quantify the differences in the results due shape and those due to size. It is imperative to list the sizes alongside the shapes to be sure that differences can be ascribed to shape only.

Line 71: remove the word “applied”

Line 105: change “quiet flying” to “stable flight”

Line 125:132: here begins the the discussion and definition of the radiative forcing (RF). It would be helpful, to explicitly state that the RF in this paper only addresses the shortwave spectrum. The terrestrial spectrum, critical to radiative forcing involving cirrus clouds, is not addressed in this paper (this is discussed only at the end of the paper).

Line: 137: Reword “The measurement areas represent boxes...” Perhaps “the measurement areas were rectangular with areas of 50x80...”

Line 139: I am not sure why the the authors talk about the transport of of tropospheric air into the stratosphere here. It either needs to be followed up with more text as to the importance/relevance of this transport to the work described in this paper or removed. Currently it only distracts the reader.

Line 160-165 The microphysical measurements of the cloud layers are given in mean diameter. There needs to be some description of how these were then related to the radiatively important effective radius. Clearly from Figure 11 the method incorporating the microphysical measurements into the radiative transfer calculations (Approach I and II) has a profound effect and thus needs to be described in detail.

Line 181: Change “dragged” to “towed”. Dragged implies something uncontrolled.

Line 204: Change “no water vapor absorption...” to “little water vapor absorption..” There is undoubtedly water vapor absorption across this layer. The 1400 and 1900 nm band are highly sensitive to very small amounts of water vapor, and water vapor has been measured much

higher (and dryer) in the atmosphere utilizing these bands. Remove or change the line about all of the solar radiation absorption is due to cirrus cloud. This is not the case. In fact, the water vapor absorption is visible in the 940, 1400, and 1900 nm water vapor bands in Figure 10.

Lines 208-211 I have trouble understanding this section. Yes, low-level, optically thick, clouds, have a large effect (dominate the signal) in the upwelling irradiance. Because they are low level the water vapor absorption across the entire spectrum is present. The author talks about liquid water absorption, which no doubt occurs, but is difficult to separate from the overlapping water vapor absorption. This section needs to be corrected.

Line 222 Change “and almost 100%” to “nearly 100%”

Line 222 “The reflectivity in Fig 7 (b) shows very low values of not more than 3% This is due to the optically and vertically thin cirrus layer” Remove “vertically” here, the vertical extent does not affect the reflectivity.

Line 241: “but still within the error bars” What is meant here?

Line 256: Change “needed” to “required”.

Line 275: “The ice crystal shape is assumed “do” [“to” - typo] be constant, further assuming a mixture of particle shapes according to Baum et al., (2005)”. This seems nonsensical, the shape is constant but a mixture of shapes? This should be reworded.

Line 280: Throughout the paper the absolute differences are often left out of the discussion (e.g. a factor of 5 or 10%). Because the values are most often small numbers, the ratios of two small numbers produces large percentage changes but are in fact very small in an absolute sense. This can be misleading to the reader. So please insert the absolute values *everywhere* ratio/percentage differences are given.

Line 286: The author attributes the differences between the measurements and the modeling only to the input parameters in the modeling. It is clear that there are measurement/sampling errors. In fact, the discussion of horizontal flux divergence, one of the main obstacles to making these kinds of measurements, has disappeared from the manuscript. The authors previously cited horizontal photon transport as a motivation for the work. No change in the input parameters will produce 3-4% absorption (Figure 10c) in the visible from ice particles no matter the shape, size, or optical thickness. Horizontal photon divergence will. As the modeling demonstrates, absorption from ice only occurs in the near-infrared. Additionally, the errors bars for the measurements must be included in this figure (which are, no doubt, greater than 3-4%).

Line 302-304 Please expand on the differences between the two approaches. Why is IWC the more physical approach? Why do the two approaches produce such large differences in optical thickness (Table 1.)

Line 310: Following an earlier comment, is this purely a shape change?

Line 314: "A similar spectral trend of the shape effect shows the transmissivity" Not sure what is meant here.

Line 315-325: Again, are these purely shape differences?

Line 355: "It is noticeable that there is a sign changing effect on RF' with negative values for the visible spectral range and a positive radiative forcing forcing in the near infrared range" I think the author is discussing Figure 12(e). Is there really positive forcing in the near-infrared? Hard to see in this plot.

Line 358: Absolute values here please.

Line 381-382 "It is noticeable that the cloud optical thickness of the low cloud in comparison to the cloud top height has a significant effect on the radiative forcing of the above lying cirrus". Re-word here, difficult to make sense of what is being said here. This needs to be better explained.

Line 404: Again, references to shape differences without sizes and only relative differences (a factor of 2) quoted.

Line 415: Differences between modeling and measurements are attributed to shape differences and mixtures of shapes. No mention of the sampling issues, or possible measurement problems.