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Discussions

Interactive comment on "Spectral optical layer properties of cirrus from collocated airborne measurements – a feasibility study" *by* F. Finger et al.

Anonymous Referee #3

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The work here utilized aircraft measurements of upwelling and downwelling spectral irradiance to characterize the cirrus cloud layer properties (transmittance, reflectance, absorptance) and albedo. These measurements are compared to modeled values derived from in situ cloud particle sampling combined with a 1-D atmospheric radiative transfer model. The technique of using a retractable instrument pod (AIRTOSS), lowered during flight to sample radiation above and below a cloud layer in a near simultaneous fashion is novel and potentially very valuable. However, the paper requires major revision before it is suitable for publication. It is difficult to follow the text, there are significant problems with the results that are not discussed (both measurements

and modeling), and the relevant problems encountered during previous aircraft studies of cloud layer properties are ignored.

Comments: The measurement of cloud layers properties from collocated aircraft goes back several decades and lead to the "anomalous absorption" problem. Cloud flux divergence is a difficult measurement to make under the best circumstances. The inhomogeneous and often optically thin cirrus clouds makes them especially difficult. The authors correctly point out that collocated aircraft should, in theory, lead to better estimates of cloud layer properties such as absorptance (absorptivity). But the collocation of aircraft is no guarantee that it will. The problem of horizontal flux divergence still exists and substantial errors in the results of airborne flux divergence measurements are common/expected (e.g. Schmidt et al., 2010, Marshak et al., 1999). Other authors have proposed methods to ameliorate the effects of horizontal flux divergence (Cox and Ackerman and Cox, 1981, Marshak et al., 1999), and these techniques have lead to plausible results (Kindel et al., 2011).

It is clear from the results shown in this paper that despite the collocation of the spectral irradiance measurements, there are significant problems with the measurements that are not addressed in the paper. See for instance, Figure 7. The absorptivity, shown in green, exhibits more than 10% absorption in the visible and then decreases as it approaches 1000 nm at which time it becomes negative (non-physical). The absorption of ice cloud (and liquid water cloud) is zero (single-scattering albedo is 1) for the visible wavelengths regardless of the ice particle size or shape (see Figure 7(c)). The absorption begins to decrease at the point it should begin to increase in the near-infrared. The single-scattering albedo drops below one in the near-infrared and some absorption is expected. No discussion of the significant absorption in the visible is given in the text, nor are the effects of horizontal flux divergence or any attempt to compensate for them. There is one sentence remarking on the effect of horizontal photon transport in section 4.2.2. It explains lower absorptivity in the 1000 to 1500 nm range and a result of horizontal photon transport in the cloud layer. I cannot make sense of what is meant here.

Why only 1000 to 1500 nm? In the cirrus layer or the low cloud layer or both?

The measurement of reflectivity has an unusual shape as well. The reflectance goes up in the near infrared (Figure 10(b)). This is not what is expected nor is it what is demonstrated in the modeling. Reflectance goes down in the near infrared for a cloud over a dark surface (in this case water). The authors, in Equation 4, point out that the sum of T, R, and A must equal one (i.e. energy conservation). Why not plot T+R+A? This would be a good test of the validity of the measurements. This would also be a good test of the modeling results as well, which also contain results that seem to be incorrect. For instance, in Figure 12(b), R becomes negative between 1500 to 1800 nm and 2000 to 2200 nm. What is negative reflectivity? Additionally, in Figure 12(b) the water vapor absorption bands (940, 1140, 1400, and 1900 nm) have greater reflectivity than the surrounding window regions. How is this possible? The modeled albedo (Figure 12(d), spectrum in black) Rtop increases with wavelength; the albedo is greater in near-infrared than in the visible. Again, how is this possible? The measurement of Rtop makes more sense than the model in this case. The albedo decreases in the infrared. Additionally, it would be very useful to list the optical thickness and effective radius along with the shape used in all the calculations given in this work. The differences are substantial between the shapes, but it is not clear what the differences are in the optical thicknesses and effective radii.

There are aspects of the measurement technique that are unclear and confusing. The measurement, as I understand it, was made with the aircraft above the cloud layer and the AIRTOSS below the cloud layer (see Figure 3.) This is coupled with a relatively minor offset in time (5 or 6 seconds) because the AIRTOSS trails the aircraft. In Figure 7 the shaded gray area delineates the cloud vertical extent. I estimate the thickness to be about 1.7 km. In the section describing the time correction the vertical extent of the AIRTOSS from the aircraft is given as 914 meters. This, even if flown directly beneath the aircraft, is not long enough to span the thickness of the cirrus layer. Additionally, in Figure 8, the Fdown from above the cloud layer is simulated not from the aircraft

measurement. What is the point of using a model Fdown if a measurement was made? This would help to offset any radiometric calibration errors if all of the spectrometers were calibrated to the same radiometric source. Why not plot the aircraft and the AIRTOSS altitude on Figure 7(b)? It would make it clear exactly where the aircraft and AIRTOSS were during the measurement period. If the measurements were not truly collocated, that is, aircraft measured irradiances above the cloud layer, and AIRTOSS measurements below the cloud layer, measured within five or six seconds of each other, it is difficult to see how this technique differs from a single aircraft experiment.

The irradiance (Fbase up) reported for Time I (Figure 8(a)) the “no low-level cloud” is far too high to be an irradiance spectrum over cloudless ocean. These spectra clearly include the effects of low-level cloud. Over cloudless ocean, the peak of the upwelling irradiance in the near infrared is rarely, if ever, over 0.1 (W m-2 nm-1). Was this period of “no” low level cloud selected on the irradiance values alone or was it confirmed with ancillary data such as aircraft video?

It is not clear to me why this is called a feasibility study. Generally, a feasibility study in this context is meant to denote analysis undertaken to demonstrate whether a particular measurement is likely to be successful given the characteristics of the problem and the performance of an instrument/measurement technique. This is not a feasibility study, as I understand it. Measurements were made and the results are in poor agreement with modeling results and what is generally expected from basic cloud and atmospheric radiative transfer.

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