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Interactive Comment

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

Anonymous Referee #2

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This article describes results from a novel use of a towed, instrumented, airborne platform to quantify the optical properties of cloud layers from radiation measurements above the cloud (aircraft) and radiation measurements below the cloud (towed platform). Ostensibly, this approach would surmount a challenge – that of non-temporally collocated microphysical and radiative measurements – for quantifying these properties. However, the experimental setup does not seem to accomplish the intended aircraft-to-platform formation. In addition, since the measurement results shown are not fully consistent with expected cloud absorption and the inversion procedure can not reproduce measured cloud albedo, it can not be established whether this is because of remaining temporal mismatch between the radiation measurements above and below the cloud layer, or if the strong attempts described to quantify the microphysical properties through the cloud layer were still simply not enough to fully represent the spatial and temporal variability in a cirrus layer.

I begin first with major comments, and include a couple of technical comments at the end.

Major Comments

Experimental Setup: Figure 7: From grey shaded area in this plot (and Figure 5), the cirrus layer would extend beyond 1 km depth. The described experiment, where the AIRTOSS was extended a distance of ∼900 m (∼185 m vertical), would not provide collocated radiation measurements above and below the cirrus layer. In the region prior to that marked I and II, spaced by approximately 100 seconds of flight, was the AIRTOSS lowered even further? If it was not, then much of the supporting points given to support the use of AIRTOSS below and aircraft above (both of which were instrumented with radiation measurements), would describe a theoretical approach – not one that was enacted in practice. This should be made clear, and the title of the manuscript ("…a feasibility study…" should be revised).

In addition, ancillary support (microphysical data, other data?) for the region I as cloud-free below cirrus, and region II as low-cloud below cirrus, is not given in the manuscript text. This would be an important point to address, given that an assumption of the analysis is that there is zero horizontal divergence of radiation – either within the cirrus layer or from high to low optical depth of the underlying (water) cloud. On this note, the assumption of zero horizontal divergence could be better established in the text. Please also note that the level of the cirrus cloud was given different values in the manuscript (please check for consistency).

Deriving Cloud layer optical properties from collocated measurements: Figure 8: As best I can gather, the downwelling irradiance above the cirrus layer came from a model,

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and not measurements. (In most spaces in text and captions, it is identified as a measurement, but in others, it is identified as model results). Besides the fact that it is unclear why a model has been used (given the discussion on a leveling platform in the manuscript), it must be considered that a mismatch between the measured and modeled downwelling irradiation above the cirrus layer would propagate into all of the derived quantities shown in the right hand plots. This mismatch would be an additional source of error that would propagate into the derived quantities because deriving these layer quantities is already subject to large systematic errors, given that the reflectivity and absorptivity are small values derived from differences in large irradiances.

For example, the absorptivity results (subplots c and d) at wavelengths $< \sim 900$ nm (i.e., conservative scattering wavelengths), show non-zero absorption when single scattering albedo at these wavelengths is essentially unity and true cloud absorption is expected to be zero. To achieve (expected) zero absorbtivity results at these spectral bands would require error bars that are 2-3X or more larger than the stated 5-6% uncertainty.

Since the results of Figure 8 are (largely) derived only from measurements (the exception being the downwelling irradiance above cloud), I would ask for a comparison of measured and modeled downwelling irradiance above the cloud, and a percentage difference value between them.

I would also argue that the upwelling irradiances below the cirrus base (subplot a) is larger than what would be expected for over-ocean, clear-sky measurements. What is the justification that low-level clouds were absent– what ancillary measurements did you use? And, how robust is the statement without support from lidar measurements, for example?

Finally, given that the underlying surface albedo is ocean (in the absence of low-level cloud), I would ask that authors please remove all statements suggesting that variability in the cirrus reflectance is due to changing ground conditions given that ocean albedo

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is dark (low albedo) and relatively flat, spectrally.

Comparing Measurements with Simulations: Please ensure that you make clear that the use of 1-D RT simulations will not capture horizontal radiation motion, potentially biasing your results (relative to “truth”, as represented in a model by 3-D). Section 5.1: RT simulations – This section really needs more description to understand, with any certainty, the approach that has been applied. However, I believe this has been the general approach:

a) An (equivalent) particle diameter in conjunction with model single scattering properties and measured number size distribution, to compute bulk, volumetric layer properties. What were the assumptions to go from the non-spherical crystal shape to equivalent particle diameter? Note also that the equations 7-9 should be revised to “dlogD” to correspond with the measurements in Figure 6.

b) The results from a) were used to model cloud optical properties, tau and reff.

c) Derived tau and reff are then used to model upwelling and downwelling irradiance at cloud top and cloud bottom.

d) Cloud layer properties are then derived from the irradiances computed in c).

e) To investigate the cloud layer properties and cloud radiative forcing for different assumptions in crystal shape and habit mixture, a fixed number size distribution is combined with different shape assumptions.

One of my concerns with the results is focused on Figure 10 (albedo panel): None of the simulations, irrespective of crystal habit, reproduce the measured cloud top albedo. If I have understood the experimental process described above in points a-d, the failure to reproduce cloud top albedo (also, cloud base albedo, but that is not shown), calls into the interpretation of the layer properties and the cloud radiative forcing results.

There are two, additional, key variables that are necessary to understand the results, that are I do not believe are provided in the manuscript: approximate optical thickness

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of the cirrus cloud, and solar zenith angle. For optically thin clouds, single scattering will dominate and assumptions in crystal shape will be emphasized (as opposed to an optically thicker cloud where multiple reflections smooths out some of the differences due to crystal shape/habit assumptions). In addition for low sun angles, multiple scattering will also be emphasized.

Interpreting conclusions related to Changing Crystal Shape/Habit: I feel holding the number size distribution constant is an inadequate approach to quantifying differences in cloud layer properties. Understanding differences in cloud radiative forcing due to crystal shape and/or habit changes would require an approach that enforced constant IWC; holding IWC constant was the approach also used by studies cited in this manuscript [Zhang et al., 1999] or to provide another boundary condition to results achieve by holding number size distribution constant [Wendisch et al., 2007]. To support the analysis results, Iplots of the spectral volumetric extinction coefficient, and single scattering albedo (as a function of crystal hsape/habit, for perhaps 1-2 different particle diameters) should also be shown. The authors could also show the derived asymmetry parameter (first moment of the scattering phase function) as well.

Technical Comments:

Pg. 19047, line 14: Please expand text to indicate that the solar irradiance is not impacted by horizontal variability in the cloud, but rather the solar irradiance scatter/absorbed by cloud is. Page 19059, line 14: The number size distribution, integrated over the cloud geometrical thickness, defines the optical thickness of the cloud.

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