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Atmospheric Chemistry and Physics Discussions

Interactive comment on “” by

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We want to take the time to thank Reviewer #2 for his/her careful review, and apologize for the length of time it has taken us to respond.

1) Please describe the instrumentation used in the CLAIM campaign, and provide references for the campaign itself.

Both reviewers made this request, and we agree that the omission in the original manuscript was a mistake. We have added a paragraph in Section 3.1 describing the instruments and the measurement methodology. CLAIM was a small-scale, single aircraft experiment. There is no official documentation on the experiment itself.

2) “The cloud side scanning geometry implies that the instrument records solar photons predominantly in the backscattering direction. Scattering phase functions for individual spherical and non-spherical water or ice particles do differ particularly in the side and backscattering directions. Given a fixed amount of cloud water or cloud ice, how big is the influence of particle shape on the reflected radiation? Have any sensitivity studies

been performed by the authors with respect to this issue?”

We performed a sensitivity of the 2.10/ 2.25μm ratio to different ice crystal shapes, and that is shown in Figure 9. The different color curves represent different ice shapes. We did not perform a similar analysis for retrievals of effective radius. However, the retrieval in Figure 7 was done first assuming all water droplets, from bottom to top of the cloud and then using the ice LUT from bottom to top, and finally using the water LUT for the water part, the ice LUT for the ice part and proportionally mixing the two for the mixed phase part. The ice LUT was the average of all microphysical models that we had available for ice. The results did not seem to matter for this particular cloud and geometry of illumination and observation. Other situations may be more sensitive to choice of particle shape.

In nature one rarely finds a situation where only one ice habit can be identified, usually a combination of multiple crystal habits is a better approximation to describe the ice particles found in the glaciated phase of a cloud. For this reason the simplified algorithm described in this work considers an average phase function in the glaciated part of the cloud, which represents an average distribution of ice particles of multiple habits.

3) “Feasibility assessment of radiometrically scanning the side faces of realistic clouds. Which portion of a typical cloud scene does allow ’visible access’ to the cloud sides? Certainly, cirriform and all types of stratiform clouds would not be candidates to be observed. What would happen in a situation for which individual clouds are located above a lower cloud layer? Would cirrus clouds located above a particular cloud target hamper the retrieval of the vertical profiles of effective drop radius and temperature?”

The reviewer is correct in pointing out that the method requires a clear view of the illuminated sides of clouds and sufficient vertical extent for the sensor to make sufficient measurements in the vertical. Exactly how tall the cloud needs to be depends on the spatial resolution of the sensor. From space we may be limited to deeper convective elements, but applied to aircraft, shallower clouds may be possible. We also need to avoid cirrus clouds and will need to develop a cirrus screen to apply to retrievals from space. We have added a paragraph in the Summary and Discussion section addressing this limitation.

4) "Most likely, the cloud scanning instrument would be not the only instrument on the proposed platform. Please discuss other instrumentation that would provide complementary information."

We envision a satellite mission with the cloud scanner providing measurements in at least one visible and two bands near 2.1 μm, as well as thermal infrared measurements at 11 μm. In addition we would hope that the platform would include a multi-angle polarimeter with wide spectral range. This was described in the original manuscript in Section 4 and continues there in the revised manuscript, "In addition to the cloud side measurements. . ." There has been some revision to the paragraph in the new version because Reviewer 1 prefers for us to focus on the cloud measurements and not become overly focused on the aerosol piece.

5) "The side viewing geometry needs a retrieval algorithm that converts all slant views to altitude or pressure levels in a consistent manner. It is not clear to the reader how this can be achieved, i.e. how can the distance between the detector to a particular area element on the walls of an individual cloud be measured accurately? Note that the satellite will be typically 800-1000 km away from the cloud, while the aircraft's view in Fig. 7 was just 20-30 km away from the target."

We are not proposing an algorithm to retrieve altitude or pressure levels, although we could adapt algorithms currently used by MODIS and applied to cloud top measurements. Instead we are simply using observed brightness temperature of various sections of the cloud as a proxy for altitude.

6) "p. 4487, last paragraph of the introduction: Reference is made to the 'current model of the cloud scanner'. Please provide more detail on this instrument with respect to: i) calibration issues for wavelength accuracy, ii) absolute radiometric accuracy, iii)

accuracy of measured spectral reflectance, iv) signal to noise ratio versus wavelength, v) calibration of radiances in thermal atmospheric window region, vi) spatial resolution. - Please also elaborate on the time resolution of the scanning instrument versus the platform velocity."

We agree that this information was a mistaken omission in the original manuscript, and we have added this information in Section 3.1.

7) "p. 4490, lines 16-20: It is stated that the conventional geometry of making cloud-top nadir reflectance measurements has to rely on certain assumptions, for example, that the observed or retrieved quantities at a certain point/altitude within the cloud is to be taken as valid for all points within this cloud having the same altitude. It is not clear, which assumptions have to be employed in order to interpret the proposed cloud side scanning observations. Please explain this issue in more detail, since any cloud, when viewed from the side, is not transparent in the entire visible to thermal infrared wavelength region."

The single most important assumption in this type of remote sensing is the assumption that effective radius of the particles measured at cloud sides are applicable to what is going on inside the cloud. We address this assumption several times in the manuscript and offer ample references to support the assumption. On the other hand, the cloud-top remote sensing approach used by the Rosenfeld papers, relies on the assumption that the vertical profile of droplet sizes inside a cloud can be approximated by the microphysical properties of different cloud tops, for clouds under several development states, observed in a given area. Our point was that the methodology described in this work does not require these strong assumptions needed for the Rosenfeld remote sensing approach, but only the ones discussed in Sect. 2.5. We have added an explicit statement in the Summary and Discussion section pointing out that the assumption that side retrievals are applicable to the core of the clouds is one of the limitations of the method.

8) “p.4491, lines 8-14: Reference is made to the penetration depth of near infrared wavelengths. Please quantify the “domain of influence” within the cloud for the measured cloud side radiances. Please give some examples for photons having different near infrared wavelengths to make the capabilities of the cloud scanning instrument with respect to vertical resolution more illustrative.”.

While an important aspect of cloud remote sensing we feel that the phenomenom is not the focus of the present manuscript. Previous studies exist in the literature that explicitly address the reviewer’s interest and these are well-referenced in our paper. We point the reviewer to the following references: Platnick , 2000, 2001; Chang and Li, 2002, 2003; Zhang et al., 2010.

9) “p.4492, lines 3-8: Reference is made to the uncertainties of ground-based observations with respect to large background noise as related to water vapor emissivity effects or cloud shadow effects. But, any particular observation geometry has its strengths and weaknesses. For example, for spaceborne observations both shadow effects as well as uncertainties with respect to surface temperature and water vapor may negatively influence the spectral cloud side scanning observations, too. The authors are asked to comment on this and to provide clear arguments supporting the proposed strategy.”

First of all the reviewer is mistaken in one of his/her assertions. There will NOT be shadow effects when the cloud is viewed from space if the instrument points correctly. This is because we can keep the spacecraft continually pointed at the sun using a sun sensor and make Earth-view measurements on the opposite side of the space craft. This means that the Earth view (including the cloud) will be perfectly illuminated for the cloud scanner with no shadowing at all. This is the major strength of a space-based platform, and the advantage cannot be over stated. Of course, multi-layer clouds create difficulties, but they would from any platform. The other advantage to a space-based platform is that when you look down, you look down through the thinnest atmosphere with the smallest concentrations of aerosols and water vapor. Contrast this with looking up from sea level. The viewing path would intersect the highest concentrations of

aerosols and water vapor just to reach cloud base. These points are mentioned in the manuscript.

We apologize to the reviewer but we do not see any scientific weaknesses to the space-based platform, although there may be administrative or programmatic considerations.

10) “p. 4493, lines 5-7: One particular point to be discussed in this context is the fact that real clouds have no such smooth vertical side walls like those used for the SHDOM simulations, i.e. idealized cylinder clouds. Please describe the uncertainty of associating slope information and/or gradient information -df/dx to altitude dependent quantities like retrieved temperature and retrieved effective drop radius.”

It is a matter of scale. An instrument with a very fine spatial resolution will resolve all of the jumps and bumps in a natural cloud, while a coarser spatial resolution may be too coarse to retrieve the vertical profiles in the cloud. We have added several sentences at the end of Section 2.3 that address this issue.

Also the text was adapted to include a new reference to clarify this issue: Zinner et al. (2008) show excellent results for the retrieval of cloud droplet effective radius and thermodynamic phase considering the cloud side radiance measurements performed with the instrumentation discussed in this work. The authors used a realistic cloud field with 3-D structures of ice, water, and mixed phase clouds, and temporal evolution from early stages of convective activity up to mature deep convecting clouds, while the radiative transfer was solved by a rigorous 3-D Monte Carlo model. The results by these authors, in particular their Fig. 5, are consistent with the results we show in our Fig. 5 considering the simpler SHDOM model, indicating that our approach indeed captured the essential physics needed to be addressed in the problem. As for the uncertainty in retrieving particle size profiles using the spatial gradient technique, Zinner et al. (2008) show that for water droplets the uncertainty is about 1 μm, while for ice particles it is about 8 μm.

11) “p. 4498, lines 16-18: Here describe the accuracy of the calibration for the 2.1

um cloud reflectance. A further explanation needed in this context is a brief paragraph describing the main steps and assumptions employed for retrieving the profile of the effective drop radius.".

We have reworked this section, added a description of the instrument used to make the measurements and added a few sentences walking a reader through the process of a retrieval. This is all in Section 3.1.

12) "p. 4500, lines 24-26: Here the authors discuss the match and mismatch of the refractive index for ice in Fig. 8. Frankly speaking, the used argument is very much guesswork. There are several other unknowns influencing the reflectance behaviour of ice cloud populations. For example, the observation angle versus the solar incidence direction (solar zenith angle and solar azimuth angle) along the outer faces of the cloud may have a strong impact, too. Please discuss possible cloud orientation effects. Indeed the real cloud shown in Fig. 7 does not resemble the cylinder idealization."

In this work and in the proposed satellite mission the solar illumination geometry (zenith and azimuth angles) is constant for a given cloud, and as close to 180o scattering angle as possible. The instrument view angles have small variations of about 5 degrees. In addition to this, only the well -illuminated parts of clouds were considered, so any possible shadows were filtered out from the analyzed pixels. Under these conditions we took only pixels for which the measured temperature was below -38°C, so we can be sure they correspond to ice particles in the target cloud, and then we plotted their 2.10/2.25 μm ratio as indicated in Fig. 10. For the water droplets, we considered only pixels with measured temperatures above 0°C. We explored the issue of different crystal shapes in Figure 9, and they offer some variability, but not enough to explain the mismatch. We cannot prove entirely that the imaginary refractive index used to calculate the curves in Figure 9 are wrong, but the resulting ratios of the calculation do not match the experimental values for ice. What could it be? Geometry? Geometry was constant. Too simplified cylinderical model? Later simulations using a more complex model (Zinner et al., 2008) produced the same sensitivity of the reflectance ratio to

thermodynamic phase. We cannot prove it conclusively but the imaginary index is the most likely culprit. All of this is described in the text.

Because we cannot prove it, we have softened the concluding sentence.

Typographical errors

We thank the reviewer for the time spent and identifying these errors, and have fixed each of them.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4481, 2007.