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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spectral invariant behavior of zenith radiance around cloud edges simulated by radiative transfer" *by* J. C. Chiu et al.

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We thank the reviewer for his/her critical and constructive comments, and for his/her time and effort on reviewing a series of our papers. The following is our responses that address reviewer's comments point by point. (The reviewer's comments are numbered; our responses are started with bullets **.)

1) Within this manuscript, you actually show that solar transmittance is *not* spectrally invariant, at least not across the entire wavelength range. While it appears in Figure 1 that all the model results (or measurements in the preceding Marshak et al. 2009 paper) follow a line. However, as you say yourself (p5, 21-26), the linearity breaks down when splitting the spectrum up into bands B1-B5 which all have individual slopes and intercepts. Figure 4 shows further proof of that. While transmittances ratios in all bands

can be regressed to lines, those lines are different. The grouping into these "line categories" is probably primarily due to changing phase function (asymmetry parameter) and single scattering albedo across the spectrum. While they change rather smoothly in the visible part of the spectrum, there are considerable slopes of both parameters near the wings of liquid or ice absorption bands. It can therefore be expected that the "single slope-intercept" hypothesis break down at least for wavelength ranges where the single scattering properties are quickly changing (e.g., B4 and B5). These issues could/should be explained / discussed in an added "RT background" section of the paper. For these reasons, the statement on p3, 1-3 is highly questionable: Especially in the NIR wavelengths, one cannot just "interpolate" over single scattering properties throughout the spectrum.

** The reviewer is right. There is NO scale-invariant behavior across the entire spectra; the spectrally-invariant behavior occurs only for individual wavelength bands that have similar single scattering properties of clouds (e.g., Bands 1–3 (visible and near-infrared up to 1.3 μm), Band 4 ($\sim$1.6 μm), or Band 5 ($\sim$2.1 μm)). Marshak et al. (2009) did not report this finding from SWS observations because of two reasons. First, observed clear-sky radiance in Band 5 was too small and too uncertain to be divided on, so data at wavelengths beyond Band 4 were excluded in their analysis. Second, the difference in single scattering albedo between bands B1 and B4 is not as significant as that between bands B1 and B5. As a result, Marshak et al. (2009) overlooked the difference in slope and intercept between B1 and B4.

2) Marshak et al. 2009 in their linear-mixing hypothesis (which is also the basis for the paper under review) departs from the assumption that slope (a) and intercept (b) add up to 1 at all times. They find that assumption justified by looking at actual SWS data (they report deviations of less than 5% from unity). While this still holds in figure 1d (B1), it is not true for Figure 1e (B5). How does this violation comply with the original linear-mixing departure point? One can also reverse the question: If linear mixing does *not* hold, i.e. a+b<>1, that means that a and b provide two *independent* pieces of

information, which is indeed what you are doing to derive the effective radius later on in the paper. If b=1-a, you could not have retrieved Reff in the first place.

** The reviewer is right; the original linear mixture hypothesis in Marshak et al. (2009) holds for band B1, but not for band B5. For clarification, we have added the following in section 2.2: "... Zooming into each individual band (see examples from bands B1 and B5 in Fig. 1d and e), we see a remarkable linearity that confirms the spectrally-invariant hypothesis. However, bands B1 and B5 have their own slopes and intercepts. In band B1, the sum of the slope and intercept for each line is close to 1, which is consistent with the finding reported in Marshak et al. (2009). But in band B5, the sum is no longer equal to 1 due to liquid water absorption."

3a) The great thing about this paper is that in the retrieval of cloud properties, this method seems to get rid of the disturbing influence of aerosols, the surface, illumination, and 3D effects all at once! While some aspects of this are indeed shown in this paper, there are some question marks remaining if that's really true: Chiu et al. (2009) state that the slopes and intercepts (I870+I1640 vs. I870-I1640) depend on "sun-cloud-radiometer-illumination" and "aerosol and cloud optical depth, 3-D cloud structure, surface reflectance, and solar zenith angle". I realize that we are talking about different slopes and intercepts here but why should 3D, surface, aerosols etc. matter in the previous (related) study, but not in this one? There are certainly reasons for that but they should be explained! Why do you expect that 3D effects, illumination geometry will not affect linearity?

3c) In the Chiu et al. (2009) paper you actually extracted information about the aerosol from similar measurements (but a different technique). Why did aerosol matter there but doesn't here? In fact, even though Figure 3d shows very nicely that aerosols don't matter (Figure 2b is equally impressive, for surface albedo), I believe that if you had plotted Figure 4 for two extreme aerosol situations (for example, Figure 4a for a cloud scene embedded in a rural aerosol from Figure 3, Figure 4b for a "clean" cloud scene without embedding aerosol), you would have seen a difference, especially if you had

chosen a smaller cloud optical thickness that's comparable to the optical thickness of the surrounding aerosol. Also, it is very likely that the *intercepts*, had you plotted them in Figure 3d, along with the slopes would have been different. Distinguishing by B1-B5 would have shown even larger dispersion. Turning this around, you could probably retrieve simultaneous information about clouds *and* surrounding aerosols - maybe in the next paper?

** We respond these two comments here.

** As the reviewer mentioned, "ratios" in Chiu et al. (2009) are different from those in this paper. In Chiu et al. (2009), we used normalized zenith radiances at two wavelengths only, and plotted them on the sum vs difference plane. Those radiance values were not further rescaled by any clear-sky radiances. Therefore, the locations of those data points on the sum vs difference plane strongly depended on aerosol properties, surface albedo, and sun-cloud-radiometer-illumination. Unlike Chiu et al. (2009), ratios in this paper are values rescaled by clear-sky radiances, and thus the sensitivity to aerosols is significantly reduced. If we rescale radiance using the "Rayleigh-only" radiance (i.e., no embedding aerosols; the reviewer's Fig. 4b), the slope and intercept functions will contain aerosol information, and thus could be used to retrieve aerosol properties. How to retrieve clouds and aerosols simultaneously from here is indeed our goal.

** Preliminary results for 3D effects and illumination geometry are provided in the supplement file (see "Preliminary Results for 3D Effects"). Due to the complexity of 3D effects, we prefer very much to simulate more realistic cases and discuss them in a separate paper.

3b) In Chiu et al. (2009), regime 4 (figure 4) is a "nonlinear" one. How does that translate to this study? Do you expect non-linearities for some constellations of the parameter space (for example, large cloud optical thickness)? I noticed that in the present study you impose a rather tight constraint on COD. Do you expect a limiting "range of validity" of the observed linearity? For example, you don't go to realistically high CODs as in the previous paper - you stop at CODâĹij3; realistic CODs would be more like 30 or so.

** Regime 4 in Chiu et al. (2009) corresponds to a fully cloudy region and is typically associated with a cloud optical depth larger than 5. For Regime 4, we have found that the spectral invariance holds and there is no limit in range of validity. However, because this paper focuses on the transition zone near cloud edges (Regimes 0-2 in Chiu et al., 2009), we exclude cases in Regime 4.

(3d) In sum, you should try to make the series of papers more consistent (e.g., don't contradict the Marshak paper with its linear mixing hypothesis, a+b=1) with this one. Explain why 3D, aerosol, illumination doesn't matter. Explore the ranges of validity of your findings - when does linearity, when does linear mixing break down?

** See response to Comment #2 and #3a–c.

Minor comments

** Most minor comments are taken and corresponding texts have been revised, except the following:

4) Formula 4 - do you want to denote the 'rescaled radiance' as 'transmittance'? Just personal taste.

** The use of "transmittance" in Eq. (4) might be confusing, because the rescaled radiance does not represent a transmittance.

5) Page 7, 10: "shifts are less evident in B1" - not true! Look at the % change in slopes (0.21–>0.24) - quite a lot!

** This sentence emphasizes the shift in the locations of data points, not in the values of slope and intercept. In Band 5, data points for 8 μm are far away from those for 4 μm. On the contrary, in Band 1, data points for 8 μm almost overlap with those for 4

μm. Therefore, we state "shifts are less evident in B1".

9) Page 7, 13-16: Simple put: there's more forward scattering for larger drops!

** We prefer to describe it more carefully. Indeed, larger drops have stronger forward scattering than smaller drops. However, at the scattering angle of 45 degree, the phase functions for smaller drops are actually slightly larger than those for larger drops.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C8124/2010/acpd-10-C8124-2010-supplement.pdf

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