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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.

Anonymous Referee #2

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This paper is a contribution to studying cloud properties in the transition zone between clouds and clear sky from transmitted spectral solar radiance. It is closely related to two previous papers that exploit zenith-viewing radiance measurements of the ARM Shortwave Spectrometer (SWS): Chiu et al. (2009, ACP) and Marshak et al. (2009, GRL) that should, at least in some respect, be regarded as an entity. That said, while the series of papers introduces an original way of looking at aerosol-cloud continua, there are a few incoherences that I will detail out below. I believe that they can be straightened out by minor revisions. The set of figures is well-chosen and gives the paper a nice flow. It is well-structured and short. There are a few minor English problems, for example in the title: It should by "Spectrally-invariant" and not "spectral invariant". The "-ly" suffix is also missing in other places.

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Interactive Discussion
Discussion Paper

Criticism: (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,l21-l26), 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 breaks 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,l1-3 is highly questionable: Especially in the NIR wavelengths, one cannot just "interpolate" over single scattering properties throughout the spectrum.

(2) Marshak et al. 2009 in their linear-mixing hypothesis (which is also the basis for the paper under review) depart 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.

(3) 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, illumina-

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tion, 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:

(3a) 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?

(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 COD$\sim$3; realistic CODs would be more like 30 or so.

(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 -

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maybe in the next paper?

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?

Minor things: Formula 4 - do you want to denote the 'rescaled radiance' as 'transmittance'? Just personal taste. Add "ly" throughout the text, e.g. "spectrally" p3,l8: insert "on" or "the" between "for" and "improvement" p5,l5: "Let's take a quick look" is a bit too colloquial. p5,l5/6: "Similarly" and "firstly", not "similar" and "first" ("secondly" and "thirdly" later) p5,l24: Add "the" between "to" and "difference". p6,l5-9: rewrite, a bit awkward in some places. p7,l10: "shifts are less evident in B1" - not true! Look at the % change in slopes (0.21–>0.24) - quite a lot! p7,l13-16: Simple put: there's more forward scattering for larger drops! p7,l18: Add "almost" before "insensitive". p8,l13: "is unlikely occurred" is not proper English, use "does not occur" p8,l25: replace "vice versa" with "the opposite is true" p8,l26: replace "property" with "properties" p9,l7: something missing after "where" p9,l22: Can you give the value for the two different d*s rather than just giving the difference? p9,l28: "one of these cloud is *moving* with", same thing next line. p10,l3: "cloud base height", not "cloud based height" p10,l13: "of" is out of place - maybe "with"? p11,l6-9: Awkward, reword.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14557, 2010.