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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. 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) The outcome of the paper is scientifically relevant and will help improve atmospheric retrieval. However, the core part of the paper, the sensitivity study, is based on too strong assumptions and is incomplete. In particular, the simulations were performed with a Henyey-Greenstein (HG) parameterization of the phase functions for clouds and aerosols. While this approach can be justified when simulating fluxes or heating rates in the atmosphere, it should not be used when calculating radiances, since radiances depend sensitively on the exact form of the phase function. This is especially true in

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the case of low optical depths, as is the case in this study.

** Simulation results using a more accurate phase function (e.g. a Mie phase function) are added in section 3.2 (Sensitivity to cloud properties). In short, the use of the Mie phase function does not change the sensitivity of the spectrally-invariant behavior to surface albedo and aerosol properties. The use of Mie does not change the sensitivity to cloud drop size at visible wavelengths either. However, as shown in the supplement file (see "Results for Sensitivity to Cloud Phase Function"; part of the revised manuscript), the difference in intercepts between 4 and 8 μm is less pronounced using a Mie phase function than that using a HG phase function. Therefore, for purpose of understanding the spectrally-invariant behavior, HG is a proper choice. For retrieval purpose, however, the use of Mie phase functions is necessary.

2) Secondly, the simulations were performed with a 1D radiative transfer solver. However, cloud edges are obviously regions with large horizontal inhomogeneity, and it is quite possible that 3D effects will have an effect on the relationship discussed by the paper.

** Our preliminary simulations show that 3D effects and illumination geometry do not affect the linearity and our main conclusions (see the supplement file "Preliminary Results for 3D Effects"). However, these factors do change the values of slope and intercept. Due to the complexity of 3D effects, we prefer very much to simulate more realistic cases and discuss them in a separate paper.

3) The possible consequences using these two approximations made by the paper, i.e. using HG phase functions and ignoring 3D effects, are not discussed in the paper. This should be done. Even better would be if the paper would drop these approximations. This should be possible at least for the phase function.

** See responses to Comments #1 and # 2.

4) Abstract: In the abstract the relationship is not defined. It would be helpful to explain

ACPD

10, C8121–C8123, 2010

the relationship in one or two sentences. This would also make the second part of the abstract understandable.

** We have added the following in the abstract (Line 4–5) to explain what the relationship means: “Accordingly, the spectral invariance suggests that the shortwave spectrum near cloud edges can be determined by a linear combination of zenith radiance spectra of the cloudy and clear regions.”

5) Page 9, line 16-24: This paragraph is hard to understand, see also below. The authors should rewrite it.

** Revised (see the supplement file "Revised Paragraph").

6) Page 9, line 17: The width of the cloud edge is taken here to be 150m. Can the authors justify this value?

** This value is somewhat arbitrary, because the transition zone could stretch over as little as 50 m to as much as several hundred meters. However, based on SWS data, a transition zone width of around 100 –150 m is quite common. Therefore, this value is reasonable for the purpose of sensitivity tests.

7) Page 9, Eq. 6: The variable "a" is not defined. It is probably not identical to the variable "a" defined in Eq. 3.

** The variable a in Eq. 6 is the slope of the spectrally-invariant relationship, the same as Eq.(3). We have revised it (please see response to Comment #5).

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C8121/2010/acpd-10-C8121-2010-supplement.pdf

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