

Interactive comment on “Cirrus cloud radiative effect on surface-level shortwave and longwave irradiances at regional and global scale” by J.-C. Dupont et al.

Anonymous Referee #1

Received and published: 19 February 2010

This manuscript should be rejected. Note the technical issues in (1), (3) and the attached figure.

1. This paper has a scope that is far too broad. It covers several topics, and none of them well. Consider the retrieval of cloud optical thickness (COT) in section 3.1.3 (26789-26790). The faithful retrieval of cirrus COT with lidar poses a serious challenge; this paper stands or falls on the issue. Page 26790 references "Dupont et al. (2008)", which I could not find (I assume it is not the 2008 paper referenced in my paragraph below, because the paper referenced in the paragraph below has nothing significant on the topic at hand), and mentions its use of the Shiobara et al (1995)

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relationship $COT = [2.15 \pm 0.35] \times COT^*$. A single sentence at the end of section 3.1.3 then announces a new relationship $COT = [1.27 \pm 0.12] \times COT^*$ based on "a long period of several hours". "Several hours" is not "long period." The new relationship changes COT by a FACTOR OF TWO, and its application has impact throughout the manuscript. A separate manuscript focusing on the new relationship is needed first to establish the fundamental credibility of this one.

2. The SIRTA and ARM instrumental records used by the authors are valuable resources of prime class. Earlier papers by the authors are well regarded. For example, note the clever Long and Ackerman algorithm for the identification of sky condition with surface measurements and the author's 2008 upgrade (Dupont, Haeffelin, and Long) incorporating lidar. This confusing manuscript is definitely not up to the previous standard of any of its authors. There is no need for simplistic, empirical expressions for the cloud radiative effect (CRE) of cirrus on SW at the surface. CRE depends heavily on the environmental context and is not a cloud property.

3. Radiative transfer calculations for cirrus (diameter 60 micro-meter) at various COT with a Midlatitude Summer atmosphere and $\cos(SZA) = 0.48$ are shown in a single figure attached to this review. The slope of the pair of asterisks (*) is -103.9, evaluated using COT values of 0.1 and 0.2; this slope corresponds to the manuscript's cloud SW effect per unit optical depth $CRE_{sw}^*(Wm^{-2} COT^{-1})$ in Fig. 3. The solid black line of the attached figure represents calculations using an ocean surface albedo as a boundary condition. Fig. 3 in the manuscript shows a $CRE_{sw}^*(Wm^{-2} COT^{-1})$ of about -123 for $\cos(SZA) = 0.48$, indicating reasonable agreement (with my -103.9) for such small values of COT, where most of the measurements were apparently taken (Fig. 2). $CRE_{sw}^*(Wm^{-2} COT^{-1})$ is evaluated again with the attached figure as the slope from the two circles (COT=0.9 and COT=1.0) on the solid line (ocean albedo), yielding -67.9; by using COT values (0.9 and 1.0) generally outside the author's measurement range in Fig. 2, there is now disagreement with Fig. 3 (-123) for $CRE_{sw}^*(Wm^{-2} COT^{-1})$ at $\cos(SZA) = -0.48$. Using the two circles (COT=0.9 and COT=1.0) on the dotted line,

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which has a desert surface albedo, my CRE_{sw}*(W_{m-2} COT-1) based on the slope is -59.1, further still from the author's -123 (Fig. 3). The dashed line on the attached figure represents a snow surface albedo; the slope for the two black circles on the dashed line is -34; this is wildly different than the author's -123 in Fig. 3. Does it imply that the radiative transfer calculations are bad? No. But it does point out that THE AUTHORS HAVE NEGLECTED THE CRITICAL IMPACT OF SURFACE ALBEDO ON CRE.

4. The basic premise of the manuscript is the use of simple parameterizations for CRE_{sw} and also the reliance on COT measurements in a limited range to build them. The paragraph above illustrates that this approach is flawed and will lead to confusion. The community needs thorough reporting of basic cirrus cloud properties like COT and associated factors (cloud height, temperature sounding, surface insolation, etc.) instead. They should be made available in a compact database. The technique for producing COT with such intensive measurements should be documented (CRE could be placed in the appendix). One hopes that the technique makes use of the radiative transfer physics which this manuscript lacks.

5. Global scale results are in section 4.2. Section 2.2 states that they follow Dupont et al. (2009). As I could not find Dupont et al. (2009), and they otherwise "use our parameterizations" (page 26801), I have little confidence in the global scale results.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/9/C10972/2010/acpd-9-C10972-2010-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26777, 2009.

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