**Atmospheric
Chemistry
and Physics
Discussions**

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.

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MAJOR COMMENTS

Point 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)

relationship COT = [2.15+/-0.35]xCOT*. A single sentence at the end of section 3.1.3 then announces a new relationship COT = [1.27+/-0.12]xCOT* 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.

Response 1 : We agree with the referee that the COT calculation is a major point to precisely estimate the cirrus cloud radiative forcing with our methodology. However, we have made a mistake is our manuscript because the "long period" corresponds to several months of measurements, i.e several hundreds of hours of collocated measurements between lidar and sun-photometer. I agree with the referee concerning the factor two between our new relationship and the previous (Shiobara et al, 1995) equation. However, our equation accounts for the average microphysic properties of cirrus clouds over SIRTA and SGP site contrary to the (Shiobara et al, 1995) equation. These microphysic properties are able to significantly modulate the cirrus cloud radiative forcing (Schlimme et al., 2004; Wendisch et al., 2005). So, I consider that our new parameterization is much more representative of the cirrus cloud climatology over SIRTA and SGP site.

Point 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.

Response 2 : The shortwave downwelling flux at the surface is directly driven by the solar zenith angle and by clouds so it seems to be logical to consider the cloud proper-

ties in order to estimate the cirrus cloud radiative effect on SW irradiance. I do not well understand this point cited by the referee. Either we know precisely with a high temporal resolution the vertical profiles to use some well known Radiative Transfer Codes or we use parameterizations based on control parameters [Dupont et al., JGR 2007; Long et al., JGR 2006].

Point 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 cloud SW effect per unit optical depth CREsw*(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 CREsw*(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). CREsw*(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 CREsw*(Wm-2 COT-1) at cos(SZA)=-0.48. Using the two circles (COT=0.9 and COT=1.0) on the dotted line, which has a desert surface albedo, my CREsw*(Wm-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.

Response 3 : I agree with the referee that our Equations 1 and 2 do not account for the surface albedo. However, when we consider the Barrow site dataset we tend to obtain a stronger slope CRESW* (paragraph 27). This site is located where we have

often snow surface albedo. Hence, our result and your radiative transfer calculation tend to show opposite result. I think that the microphysic properties of ice crystal are likely to have a significant impact on the slope (Schlimme et al., 2004; Wendisch et al., 2005). In our dataset, 90% of our dataset owns a cloud optical thickness smaller than 0.5 what tends to increase the slope as suggested by your figure. I have added some details about this albedo effect on our manuscript in order to add this uncertainty aspect. Do you have a reference showing the results of your figure? See modifications section 3.1.4.

Point 4 : The basic premise of the manuscript is the use of simple parameterizations for CREsw 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.

Response 4 : The main advantage of using parameterizations is to use a limited set of control parameters. In fact, for the calculation of the CRESW, we need COT, SZA, AOT and WVOT and not cloud height or temperature vertical profiles. When we have made sensitivity tests to establish Equation 1, only these 4 parameters seem to have a significant impact. So, here, and for the calculation of CRESW, the cloud height and the temperature profile are useless whereas these two variables are very important for the CRELW calculation (Equation 2). COT derived from lidar observations used Morille et al. (2007) and Cadet et al. (2005) algorithms (see paragraph 42). For the other COT, we use the equation developed in paragraph 26 based on the sun-photometer dataset corrected by multiple and forward scattering.

Point 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.

Response 5 : We agree with the referee but the article Dupont et al. (2009) is now published with the reference Dupont et al. (2010) since June 10th 2010. We have added this reference in the reference section. See modification page 45.

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

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