Comments on "Effects of Cloud Size and Cloud Particles on Satellite-Observed Reflected Brightness"

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The paper in question discussed effects of finite lateral width on the optical properties of clouds. It ascribed the failure of real clouds to attain fully the reflectance levels which theory predicts for conservative, optically thick and plane-parallel (infinitely wide) clouds to the "leakage" of radiation through the sides of real, finite

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clouds. While concurring with that main conclusion of Reynolds *et al.* (1978), I feel that there are some aspects of this paper which call for comment.

First, reduced cloud brightness due to such leakage (an effect well demonstrated in this paper) can lead to an incorrect conclusion about the magnitude of the absorption process within the cloud. There have been several discussions of absorption [especially those of Reynolds *et al.* (1975) and of Rozenberg *et al.* (1974)] in each of which it was inferred that a great deal more absorption was taking place in clouds than would be predicted by theoretical calculations based on accepted values of the various optical constants. The former paper quoted up to 50% absorption of solar radiation by tropical oceanic cumuli, while the latter concluded that water in the clouds was absorbing roughly ten times as efficiently as would be expected. The importance of finite width in producing such "anomalous absorption" has been referred to by Herman (1977) and by others, but it is a pity that Reynolds *et al.* did not explicitly point out this effect and relate it to several earlier inferences—including one by their own group—of large (and unexplained) absorption.

Second, the authors suggest the use of cloud brightness for monitoring liquid water content, based primarily (apparently) on their Fig. 5, which showed appreciably different curves of brightness versus cloud width for two different values of liquid water content. However, *optical thickness* is what fundamentally distinguishes the two curves from each other, and indeed the labeling of the curves and the figure legend confirm this. It is well recognized that brightness depends primarily on optical thickness, but this, in turn, is *not* sufficient to determine liquid-water content unless the drop-size distribution is known. The present writer's experience is that it is integrated droplet cross-sectional *area*, rather than integrated droplet *volume* (i.e., liquid water content), that most closely relates to the optical thickness

$$\int_0^h \int_0^\infty Q_E(2\pi r/\lambda)\pi r^2 n(r,z)drdz,$$

and that seems to accord best with the fact that, at visible and near-infrared wavelengths, the size parameter $2\pi r/\lambda$ for cloud droplets several microns in radius is at least of order 20, and so lies in a region where the extinction efficiency Q_E, although varying in an oscillatory fashion with size parameter, is quasi-constant when averaged over any reasonable range of drop sizes.

Third, Fig. 5 gives the impression that a cloud with optical thickness 20 will be as bright as one of optical thickness 60 and equal width, provided that the cloud width exceeds 40 km. Indeed, the figure suggests that if one extended it to the right, the optically thinner cloud would soon become the brighter of the two! That impression is perhaps mainly a result of the normalization used, but nevertheless it is unrealistic to suggest that a layer of optical thickness 20 will ever equal the brightness of a layer of equal width and three times the optical thickness, other relevant parameters being unchanged.

In spite of these few mildly critical comments, I do not intend to detract from the value and timeliness of this paper. It is to be hoped that others will follow this lead and begin to measure all the major relevant quantities, even if it does involve a mixture of satellite, radiation and cloud physics technologies.

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