Comments on "A Bispectral Method for Cloud Parameter Determination"

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In a recent article, Reynolds and Vonder Haar (1977) have presented an interesting method for determining the cloud cover and cloud height utilizing simultaneous visible and IR scanning radiometer data. A special approach for the case of cirrus has also been developed to account for the semi-transparency of cirrus. The approach involves relating empirically the visible spectral albedo (0.5–0.7 μm) and the IR emissivity (10–12 μm). I am pleased to notice that the authors have referred to my earlier radiative transfer work on cirrus clouds (Liou, 1973, 1974) and have used some of my calculated results in comparisons. The empirical relation between the visible spectral albedo and IR emissivity derived from NOAA scanning radiometer (SR) data shows unsatisfactory agreement with calculated results that they have taken from my previous papers. I would like to point out here a number of possible physical reasons for the discrepancy that they have reported.

Perhaps we should first examine the definition of the visible spectral albedo measured by the scanning radiometer. It is related to the reflected solar radiance N_r as noted in their paper. The reflected solar radiance (energy per area per time per solid angle per wavelength band) generally depends on the solar zenith and azimuth angles ζ and ϕ_0, and the emergent zenith and azimuth angles θ and ϕ at which satellite observations are made. However, a simplified relation, denoted in Eq. (8) of their paper, is employed in defining the albedo which is a function of the solar zenith angle only. The neglect of emergent zenith and azimuth angles dependence on the reflected solar radiance implies that cirrus clouds are isotropic scatterers. Do the authors assume that scattering of visible radiation by cirrus clouds is isotropic? Since the observed NOAA SR data are utilized to establish the empirical relation, the spectral albedo referred to in their paper has to be associated with the reflected solar radiance in which some dependencies of the emergent zenith and azimuth angles are implicitly included.

In my previous calculations for the transfer of solar radiation in cirrus cloud layers (Liou, 1973), the results I presented involved reflection which was defined by

$$r(\mu_0)=F^{\uparrow}(\mu_0)/(\pi\mu_0F_0),$$

where $\mu_0=\cos\zeta$ and $\pi\mu_0F_0$ denotes the solar flux density normal to the top of the plane-parallel atmosphere, and the reflected flux density

$$F^{\uparrow}(\mu_0)=\int_0^{2\pi}\int_0^1 N_r(\mu,\phi;\ \mu_0,\phi_0)\mu d\mu d\phi.$$

Thus, $r(\mu_0)$ and the spectral albedo, which is related to $N_r(\mu,\phi;\mu_0\phi_0)$, used in their empirical study are two distinct quantities. I must point out that the albedo-emissivity relations derived from NOAA SR data in their study and my earlier results involve different physical parameters. In addition, the reflection values for a 0.7 μm wavelength presented in Fig. 5 of my 1973 paper used ice cyclinders with radii of 10 μm and lengths of 100 μm, whereas the emissivity values for 800–1200 cm^{-1} wave-numbers in Fig. 6 of my 1974 paper employed ice cylinders with radii of 30 μm and lengths of 200 μm. The differences in particle sizes would yield higher emissivities but relatively lower reflections for a given cloud thickness, especially for thick cirrus clouds.

The authors also made a comparison with calculations presented by Hunt (1973) who assumed cirrus containing spherical ice particles. The comparison shows an excellent agreement with the observed albedo-emissivity relation. Hunt presented a graph (Fig. 7 in his paper) on the spherical albedo for a 0.6 μm wavelength. The spherical albedo is defined by (see e.g., Hansen, 1969; Sobolev, 1975, p. 18)

$$A_s=2\int_0^1 r(\mu_0)\mu_0 d\mu_0.$$

It is a parameter representing the albedo of the entire spherical atmosphere. I am really surprised to see that the spherical-albedo-emissivity relation from Hunt's calculations had shown good agreement with the albedo-emissivity relation from the authors' observations. Is it possible that the authors misuse the data somehow?

I am in complete agreement with the authors' statement that ". . . detailed *in situ* aircraft measurements, simultaneous with satellite measurements in both the visible and infrared, are needed and should be related to particle concentration and geometric thickness." In my opinion, particle sizes and shapes are also important cloud physics parameters that should also be measured simultaneously in conjunction with radiative transfer and satellite sensing studies. Finally, perhaps, a coherent theoretical calculation which includes both the visible and 10 μm IR radiative transfer in cirrus cloudy atmospheres and takes into account both surface and atmospheric contributions, which both Hunt and myself have neglected, may be valuable. Such a calculation could be used to interpret physically and reliably the empirical relations between the satellite visible and IR radiance observations.

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