551.521.3:551.521.1 (545)

Comments on "A STUDY OF ATMOSPHERIC TURBIDITY AND DIFFUSE SOLAR RADIATION AND THEIR CORRELATION AT DELHI" by *Kalipada Chatterjee* and *Hamid Ali*

(*Mausam*, **34**, 2, pp. 223-224)

In their interesting paper with the above title, Kalipada Chatterjee and Hamid Ali (1983), hereinafter referred to as C-A, state that the annual means of the turbidity coefficient β and diffuse radiation D at New Delhi show an increase with time in the periods 1960-64 and 1969-71. However, Table 2 presented by them shows a decrease of β, D and D/G from 1960 to 61 and from 1963 to 1964. Moreover their Fig. 2 does not quite agree with their Table 2. According to Fig. 2, β decreases sharply from 1970 to 1971. Also β for 1964 is not shown in Fig. 2.

Notwithstanding these minor discrepancies there is an increasing *trend* over the years in these parameters as rightly noted by C-A. This would have been confirmed better if data had been available between 1964 and 1969 (The reason for the break has not been stated by C-A). However, their statement that this increase is attributable to the atmospheric pollution caused by urbanisation may need further examination for the following reasons.

Raghavan and Yadav (1966), here in after referred to as R-Y, had computed a dust depletion factor (d) defined by them, for each summer (April to June) from 1961 to 1965 for New Delhi. This factor increased sharply from 1961 to 1962, fell in 1963 and again increased in 1964. and 1965. This, of course, broadly agrees with the results of C-A. Kondratyev (1972) explained the finding of R-Y as due to nuclear tests in the atmosphere and gave some independent evidence to support his explanation. There can of course be other causes such as volcanic explosions, for year to year changes of turbidity. Hence to separate the effect of urbanisation from other factors it may be useful to compare the trends at other radiation observatories not subject to appreciable urbanisation.

Also, R-Y had pointed out that the normal practice of calculating β from Angstorm's formula assumes that the wavelength exponent α is 1.3 and that this may not be valid over north India in summer. Mani *et al.* (1969) have studied this aspect in detail and concluded that α is very low and often negative in north and central India in summer. They recommended direct determination of α from the three filter measurements with the pyrheliometer. It will, therefore, be interesting to know whether C-A have used the same method of computation of β for all the years considered by them or had changed to the method recommended by Mani *et al.* at any stage. If the latter is the case can part of the change in value be attributed to the change in method of computation?

References

Chatterjee, Kalipada and Ali, Hamid, 1983, *Mausam*, **34**, 2, pp. 223-224.

Kondratyev, K. Ya., 1972, "Radiation Processes in the Atmosphere", Second IMO Lecture, World Meteorological Organisation, WMO-No. 309. pp. 4 and 5.

Mani, A., Chacko, O. and Hariharan, S., 1969, *Tellus*, **XXI**, 6, 829-843.

Raghavan S. and Yadav, B. R., 1966, *Indian J. Met. Geophys.*, **17**, 4, 607-612.

S. RAGHAVAN

Cyclone Detection Radar, Madras

29 March 1983