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Letters

Discussion of Paper by E. L. Hill and J. D. Robb, 'Pressure Pulse from a Lightning Stroke'

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Hill and Robb [1968] have described a measurement of shock-front overpressure 35 cm from the breakdown channel of a spark gap placed in series with triggered lightning strokes. They reported, 'Maximum overpressures of the order of 2 atmospheres were observed . . .'

On the basis of this measurement, several conclusions were reached which, if valid, would seriously undermine the whole structure of knowledge about lightning channel characteristics (temperature, electron density, etc.) built up recently from the quantitative analysis [e.g., *Uman*, 1966, 1969; *Orville*, 1968] of lightning stroke slitless spectra [*Salanave*, 1964; *Orville*, 1966]. In particular, acceptance of the conclusion of *Hill and Robb* [1968] about the thermalization time in the lightning channel would involve the rejection of much of the information about lightning derived from quantitative spectroscopy and the substitution of new principles and concepts that would have far-reaching consequences, both theoretical and practical.

Such an important step as this should not go unquestioned. In this letter we show that the conclusions drawn by *Hill and Robb* [1968] are not a logical consequence of their results; that there is apparently no need to question the previous interpretation of the spectroscopic data, and that, in fact, the pressure pulses observed by Hill and Robb are of about the magnitude to be expected under the conditions of the experiment.

The logical development of the paper by Hill and Robb is in essence as follows:

(*a*) The theory of *Drabkina* [1951] and *Braginskii* [1958] describes how large quantities of energy deposited in a linear channel produce sufficient overpressures so that a strong cylindrical shock-wave results.

(*b*) The theory of Drabkina and Braginskii was applied to lightning by *Zhivlyuk and Mandel'shtam* [1961] to predict the pressure pulses to be expected from the lightning discharge.

(*c*) Measurements have been made of actual pressure pulses obtained from a spark gap in series with lightning-stroke currents [*Hill and Robb*, 1968]. At a distance of 35 cm from the spark gap, the measured pressures are considerably lower than the values predicted by the calculations of *Zhivlyuk and Mandel'shtam* [1961].

(*d*) Therefore (it is deduced by Hill and Robb), the channel-heating assumption used by Drabkina and Braginskii (that the energy input to the channel is immediately thermalized and shared by electrons, ions, and neutral particles in local thermodynamic equilibrium) is wrong.

(*e*) It is postulated that the energy input to the lightning stroke, initially absorbed by the electrons, is not transferred to the heavy gas particles for 25–50 μsec. (See (*g*) below.)

(*f*) Hill and Robb state that this 'slow' thermalization of the gas produces a relatively weak shock wave in agreement with the overpressure measurement.

(*g*) Hill and Robb state that, since spectroscopically measured lightning temperatures fall from a peak value of about 30,000° to 5,000°K[1] in about 50 μsec, this time is a measure of the thermalization time.

(*h*) The over-all conclusions drawn are, first, that the channel heating assumption of Drabkina and Braginskii is in error, and, second, that, because of the postulated long thermalization time, heavy particle temperatures in the channel are much lower than the values determined spectroscopically.

It is important to note that (*d*) is not a logical consequence of (*a*), (*b*), and (*c*). Other equally, if not more, valid reasons for the discrepancy could have been suggested. For example, the use of a series spark gap to produce the lightning pressure pulses could have been questioned and so could the assumption of cylindrical geometry of the shock wave. Alternatively, doubt could have been expressed about the general correctness of the Drabkina-Braginskii theory when applied to the problem; in fact, the variation of overpressure with radius according to Drabkina-Braginskii is not in good agreement with recent theories in the weak shock regime. If (*d*) is not a valid deduction, the remainder of the sequence above is needless and without foundation. It should be mentioned in passing that, although (*g*) is the only stated basis for (*e*), no physical justification for (*g*) can be found.

We now take the experimental data of Hill and Robb and reinterpret them in terms of more recent channel expansion theories than that of Drabkina and Braginskii. In a recent paper, *Few et al.* [1967] presented an approximate calculation of the propagation of a cylindrical shock wave and its eventual decay to an acoustic wave. Few et al. used a scaled version of the spherical shock-wave theory developed by *Brode* [1956]. Brode's theory, like that of Drabkina-Braginskii, assumes that all particles in the source have the same kinetic energy. Few et al. were able to predict the variation of the pressure pulse amplitude with distance and the dominant frequency to be expected in the thunder spectrum when the source was a vertical lightning stroke with given energy input per unit length. Agreement with actual thunder spectra was quite good. *Dawson et al.* [1968] measured the acoustic output from the Westinghouse 4-meter spark and obtained very good agreement between the measured dominant frequency and the value predicted by the theory of Few et al. It thus would appear that the theory of Few et al. gives dominant frequencies in accord with observations. Since this dominant frequency depends critically on the amplitude of the shock pressure pulse and on its radial decay, there is good reason to believe that the theory of Few et al. describes, at least fairly well, the dissipation of the cylindrical shock-wave.

Previous work [*Krider et al.*, 1968; *Dawson et al.*, 1968] has indicated an energy input to natural single lightning strokes of the order of 10^5 J/m. For this energy input the theory of Few et al. predicts an overpressure of approximately 3.8 atmospheres at a distance of 35 cm from the purely cylindrical shock source. This, in itself, must be considered fair agreement with the approximate result of Hill and Robb. However, agreement becomes even better when it is realized that the source is not a pure cylindrical shock but results from a discharge 10 cm long [*Newman et al.*, 1967] viewed from a distance of 35 cm. The *actual* behavior of the source will thus lie somewhere between cylindrical and spherical. Let us calculate the lower bound to the overpressure by assuming a spherical source. For an energy per unit length of 10^5 J/m, 10^4 joules is deposited in the 10-cm gap. From the calculations of *Brode* [1956] as plotted in *Few et al.* [1967], an energy input of 10^4 joules would produce an overpressure at 35 cm of about 1.7 atmospheres, in excellent agreement with the approximate measurement of *Hill and Robb* [1968]. If, in the absence of evidence to the contrary, we assume that Brode's detailed calculations for the spherical shock are correct, and if we further assume that the energy input given above is reasonable, the spark-gap source of Hill and Robb appears to more nearly resemble a point source than a cylindrical source such as the lightning channel.

It is therefore clear that, if one takes into account the geometry of the measurement described by Hill and Robb, the results obtained are what would be expected from the calcula-

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[1] The correct minimum temperature given in the referenced publication [*Uman*, 1966] was 10,000°K. See also *Orville* [1966, 1968] for this and additional temperature versus time curves.

tions of *Brode* [1956] and *Few et al.* [1967]. This, in turn, might be considered some confirmation that, at least as far as the shock-wave output is concerned, triggered lightning strokes may not be too different from natural lightning strokes.

Finally, since Hill and Robb have suggested that the electron-ion and electron-neutral kinetic-energy equilibration time in lightning is 25–50 μsec and since many of the quantitative data obtained using lightning spectroscopy were derived with the assumption that the equilibration time is 1 μsec or less, it is appropriate now to determine that time. According to *Griem* [1964], the kinetic-energy equilibration time between electrons and any group of heavy particles is

$$\tau \approx \frac{M}{m} \frac{1}{\langle N\sigma v\rangle} \qquad (1)$$

where M is the mass of the heavy particles, m is the electron mass, N is the density of the heavy particles, σ is the elastic cross section for collisions between electrons and heavy particles, and v is the electron velocity. In the 15,000° to 30,000°K range in air with an electron and ion density of 10^{17} cm^{-3} [*Uman*, 1966; *Orville*, 1966, 1968], the electron-ion equilibration time obtained from (1) is less than 0.1 μsec. (See *Griem* [1964] for details of the calculations, including the expression for the dominant coulomb cross section.)

Although the high-temperature air in the channel is essentially fully ionized, it is instructive to consider the situation envisaged by Hill and Robb in which electrons of several electron volt energy interact with a cold neutral gas. For $\sigma = 10$ A^2, and a neutral gas density even as low as 10^{13} cm^{-3}, the equilibration time determined from (1) is still of the order of 0.1 μsec.

In summary, we suggest that the conclusions of *Hill and Robb* [1968], particularly those questioning the short equilibration time and the validity of the spectroscopic temperatures, were unwarranted. Further, it has been shown that the pressure data of Hill and Robb are consistent with the theory of *Few et al.* [1967] and *Brode* [1956], strengthening the view that the equilibration time is short.

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