Ball Lightning

SIR,—I had just finished reading Powell and Finkelstein's article[1] and Altschuler's[2] chapter on Kugelblitz (ball lightning) when I came across the three recent reports[3-5] on the subject in *Nature*. On May 15, 1970, at a point some 80 to 150 km east of St Louis, an aircraft in which I was a passenger was forced to fly into and through a thick dark storm-cloud mass. Extreme turbulence resulted in a rather violent movement of the wings of the aircraft (a Boeing 727). All this time the aircraft was descending, and an electrical display was lighting the surrounding clouds in a diffuse glow. The frequency of electrical discharges (no actual strokes were seen) increased with the increasing turbulence. At (what seemed) the moment of maximum turbulence and electric discharge, while the aircraft was still descending through the storm, a sequence of events took place that I list below. The whole took not more than 5 s, and I must admit that the order, save for numbers 4 and 6, is not necessarily chronological. 1, The turbulence ceased altogether. 2, The surrounding electrical discharges (glows) ceased altogether. 3, The wing stopped buckling altogether. 4, A white glowing sphere (ball lightning ?) appeared on the port wing tip. I do not know if it was actually touching the wing. Its diameter was less than 1 m and more than 10 cm. Its boundary was "fuzzy" and not distinct. 5, There was a soft "pop". 6, The ball lightning (?) vanished.

Regarding the accompanying noise (5) I recorded it at the time. Shortly afterwards my scientific upbringing and I both decided that an outside noise was not likely to be heard within a moving jet aircraft, and that my eye, seeing the ball go, insisted to my ear that it should do so accompanied by a noise. Nonetheless, I record it now, as I recorded it then.

Yours faithfully,

MURRAY FELSHER

Council on Education in the Geological Sciences,
American Geological Institute,
2201 M Street, NW, Washington DC.

[1] Powell, J., and Finkelstein, D., *Amer. Sci.*, **58**, 262 (1970).
[2] Altschuler, M. D., in *Scientific Study of Unidentified Flying Objects*, sect. VI, chap. 7, London, E. V., Director, USAF Contr. F44620-67-C-0035 (1969).
[3] Covington, A. E., *Nature*, **226**, 252 (1970).
[4] Lilienfield, P., *Nature*, **226**, 253 (1970).
[5] Bromley, K. A., *Nature*, **226**, 253 (1970).

Obituaries

Professor S. Chapman

SYDNEY CHAPMAN, FRS, who died on June 16, made fundamental contributions to many subjects, especially gas theory, geomagnetism and atmospheric physics.

While still a Cambridge student, at Larmor's suggestion he began to work on the problem of viscosity, heat conduction and diffusion in a gas. This problem, posed by Maxwell and Boltzmann forty years earlier, had been solved only for two special types of molecular interaction; Boltzmann had stated that one must almost despair of a general solution. This was true if one sought an exact solution. An approximate solution, correct to any appropriate degree of accuracy, was, however, possible, and Chapman in 1915–17 provided such a solution, based on an equation of transfer given by Maxwell. Shortly afterwards (1917) Enskog independently gave a solution, based on the theory of integral equations. This, though more elegant in formulation, was essentially equivalent to Chapman's; thus one normally speaks of the Chapman–Enskog theory.

Chapman was especially concerned with applications of the theory to actual gases. The theory predicted two new phenomena, gaseous thermal diffusion and a converse thermo-diffusion effect. Chapman was delighted when Dootson in 1917 detected thermal diffusion and when Waldmann later demonstrated the converse effect; still more when Clusius in 1939, by his separation column, realized a dream of Chapman's, dating from 1919, that thermal diffusion might be used to separate isotopes. In 1922 Chapman was applying his results to plasmas (especially in stars); his work on the upper atmosphere, from 1925 on, relied on a background of kinetic theory. After the publication in 1939 of *The Mathematical Theory of Non-Uniform Gases*, written jointly with Cowling, he concentrated less on kinetic theory problems, though he reverted to them as new ideas struck him. It was fitting that the third edition of the book appeared just a month before his death.

When he left Cambridge, Chapman was appointed senior assistant at the Greenwich Observatory. He left Greenwich after only three or four years to devote himself to other interests; nevertheless, those few years in large measure determined his later research. At Greenwich he began by supervising the reconstruction of the magnetic observatory. He found that magneticians were far readier to collect than to interpret data, and set himself to analyse geomagnetic variations. A visit from Schuster, a pioneer in the theory of geomagnetic variations caused by tides in the ionosphere, stimulated him to work both on the dynamo theory of the solar and lunar variations, and on tides in the upper atmosphere. Dyson drew his attention to the apparent connexion between solar flares and magnetic storms, so sparking off yet another series of investigations.

Chapman's research followed several lines, each systematically pursued over a number of years; sometimes he was working along several different lines at once. His work on the analysis of geomagnetic variations covered chiefly the years 1914–31; for it he was awarded the Adams Prize for 1927–28, and the work was later expanded into the two-volume epic *Geomagnetism* (1940), written jointly with Bartels. His main work on lunar atmospheric tides was spread over the years 1918–48. Before it, such tides could be securely inferred from barometric records only near the equator. Chapman isolated the tide from Greenwich records, and went on to determine its properties over the globe; to do so he had (especially with Miller) to devise methods of data processing at a time when mechanical aids were limited. This and the work of others was expounded in the recent book, *Atmospheric Tides* (1970), written jointly with Lindzen.

Chapman's work on magnetic storms began in 1919 with a paper, the theoretical part of which he later acknowledged to be invalid. During the next ten years he cautiously surveyed various aspects of the problem, but not until 1931 did he, in company with Ferraro, reach the idea that magnetic storms occur because streams of solar particles compress the geomagnetic field. This idea, predating observations of the magnetospheric cavity by thirty years, explained only the initial phase of a storm; not until after the discovery of the van Allen radiation belts could it be completed. Chapman shared with others in its completion, this time working with Akasofu (1961). He also introduced Akasofu to work on aurorae, which had long fascinated him, and took a lively interest in the solar wind, the continuous flow of which he had, in 1955, narrowly missed predicting.

In his Bakerian lecture (1931) Chapman gave the theory