periments in St. Paul's, repeated by Desaguliers, as described in the "Principia," lib. ii., prop. xl.

Aristotle is speaking of motion such as of a raindrop or hailstone falling vertically in the air, or of a smoke particle up the chimney; also of a stone dropping in water, or a bubble rising.

But in "De Motu Gravium Naturaliter Accelerato," Galileo is discussing the start of such a body from rest, while getting up speed, like a steamer or train from a station, when the motion is slow enough for resistance to be insensible, as he verified on the Leaning Tower of Pisa, dropping lead weights.

A train starts from the station with the full Galilean acceleration of the net pull of the engine, but as the speed and resistance increases the acceleration falls off, and finally, at full speed for the most part of the journey, Aristotle's state of motion is attained, and the inertia is eliminated, in the language of the engineer.

Galileo versus Aristotle can be shown off in a tumbler of soda-water, where a bubble starts up from the bottom with double Galileo's gravity acceleration, but before it reaches the surface the velocity has attained very nearly the terminal velocity of Aristotle.

I hope Capt. Hardcastle will be encouraged to devote his learned leisure to the preparation of a "Defence of Aristotle's Dynamics," on the lines of Duhem's recent book, "Les précurseurs parisiens de Galilée." G. GREENHILL.

1 Staple Inn, W.C., January 14.

Tungsten Wire Suspensions for Magnetometers.

OWING to the troublesome changes of zero and torsion constant of the silk suspensions of magnetometers, experiments have been made at the Royal Observatory, Greenwich, with the view of finding a satisfactory substitute. Quartz fibres were first tried, but were too rigid in proportion to their tensile strength. Success has, however, been obtained with tungsten wires such as are used in metallic filament electric lamps. These were suggested to us by Mr. F. Jacob, of Messrs. Siemens Bros., who kindly obtained various samples of wire for us; of these a tungsten wire of circular section, and diameter 20 microns, has been adopted as the suspension for our declination magnet, which is of the ordinary Elliott pattern, weighing about 50 grams. This wire, about 25 cm. in length, has now been in use for five months, during which time its zero has not changed within the limits of measurement, *i.e.* certainly less than 10°; the effect of 90° torsion on the wire is to turn the magnet through 4′ (it may be noted that a thicker wire, of diameter 51 microns, which was also tried, gave a deflection of the magnet of more than 2° for 90° torsion).

This success encouraged us to try a similar wire for the vibration experiment in the determination of absolute horizontal force, also with satisfactory results. The deflection of the magnet for 90° of torsion is 5½′, and the zero is constant.

For determining the moment of inertia of the deflecting magnet the latter wire was too weak, the inertia bar doubling the weight carried. A wire of diameter 30 microns is therefore used for this purpose, in a separate box. The advantage of tungsten wire for moment of inertia experiments is that the torsion constant does not vary with the weight borne by the wire; with silk suspensions this is not so.

The ends of the wire are held by simple squeezing, the lower end being gripped between grooved metal cheeks held together by a screw collar just as previously for the silk fibres. Another device was adopted for the top end, consisting of a spring clip with a sliding collar; any method involving soldering is unsatisfactory. The wire used here can be bought for 3d. per foot. S. CHAPMAN.
W. W. BRYANT.

Royal Observatory, Greenwich.

The Pressure of Radiation.

IN his letter of January 1 Prof. Callendar gives his reasons for doubting the formula for the pressure of radiation as it is usually accepted. He makes use of Boltzmann's proof of the fourth power law for the complete radiation, extends it to each separate frequency, and deduces that the energy in every frequency ought to be proportional to the fourth power of the absolute temperature. Since this is known to be untrue he concludes: "Either Carnot's principle does not apply, or E/v is not equal to $3p$ for each separate frequency," and chooses the latter alternative. But it would appear that Prof. Callendar's use of Carnot's principle is somewhat questionable. For, in order to investigate the pressure in an enclosure it is essential to alter its volume, and any change of size will bring the Doppler effect into play and cause a small change in the frequency of the radiation. If this be taken into account, the result leads straight to the displacement law of Wien—$E_\lambda = f(\lambda T)/\lambda^5$—and beyond this gives no information. Moreover, a recapitulation of Wien's work with a different law of pressure fails to give the displacement law, so that this law must be abandoned, if the pressure formula is to be altered.

Prof. Callendar wishes to change the pressure formula in the hope of accounting for the observed radiation curve without making an open breach with our present electromagnetic theory. In his paper in the October *Philosophical Magazine* he extends his conception of caloric from matter to æther, and obtains a formula which fits the radiation curve as well as Planck's. However, his work involves a certain constant, b, the nature of which he does not discuss very fully, and this constant appears to be identical with h/k in Planck's theory, so that "molecules of caloric" are very closely related to Planck's quanta. Thus the work, which has established that the electromagnetic equations lead inevitably to Rayleigh's formula, proves also that according to those equations b should vanish; in fact, that in any finite region of the æther there ought to be an infinite number of molecules of caloric. If my reading of his paper is correct, it would appear that in extending the caloric idea to the æther Prof. Callendar has invented a new and helpful way of regarding Planck's quantum hypothesis. C. G. DARWIN.

The University, Manchester.

"Atmospherics" in Wireless Telegraphy.

WITH reference to Prof. Perry's interesting letter on "atmospherics" in NATURE of January 8, the following experience may be of interest.

Whilst at my instruments on December 12, 1913, I was tuning in the Eiffel Tower signals to read the 7 a.m. press news when the atmospheric disturbances became so great that Paris was entirely unreadable, the phenomenon continuing for fifteen minutes without cessation. The aërial was only 35 ft. high, and sheltered by other buildings.

REGINALD F. DURRANT.

121 Broadway, Cricklewood, N.W.,
January 19.