

would have a principal line, $\lambda=694.5 \cdot 10^{-8}$, between the ultra-violet and the X-ray regions.

But there is a serious difficulty. If $N=20$, $n=4$, where are the other 16 electrons required to make the atom neutral? Perhaps it is more reasonable to suppose that N for calcium is higher, and given by $N-S_8=19$. In this case, N would not denote the place of the element in the periodic table, but would allow for intermediate and unstable forms of matter—an allowance which may well be necessary. The only alternative is to explain X-rays by the structure of the nucleus. Any internal ring must be one of doublets, such as neutral α particles.

There is one other point to which I must refer. Mr. Moseley states that he has not found a correspondence between the X-ray spectra and the vibrations of the element nebulae treated in one of my papers. This correspondence is not to be expected, for the two investigations are unrelated. The simple-ring atoms which I have used to interpret astrophysical spectra are supposed to have a *simple* nucleus, or to contain no α particles, and to be incapable of giving series spectra. They are not identical with ordinary atoms, into which, however, they appear to change in the stars which follow nebulae in order of evolution, and, as is shown in a paper in the *Monthly Notices of the R.A.S.* for December last, almost certainly by a modification of their nuclei. When this change occurs, they show series spectra, which must depend on the nucleus, and perhaps on tubes of force, in a way which a mechanistic interpretation of Bohr's theory may perhaps explain. In a paper read at the January meeting of the Royal Astronomical Society, these series were shown to lead to the same conclusion as Bohr's with regard to the nature of a hydrogen atom.

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Prof. Turner and Aristotle.

IN *The Times* report of December 29, 1913, of Prof. Turner's lecture at the Royal Institution, his remarks on Aristotle are summarised in a way which will surely appeal to his sense of humour after his astonishment at my letter has abated.

"Aristotle said that a weight of 10 lb., for example, fell ten times as fast as a weight of 1 lb., and the world went on believing it for 2000 years. This raised the question whether it was better to believe things just because people told one, or to try to find out for oneself."

Aristotle never said this at all. Who first fathered it on to him will perhaps never be known now, but since Galileo made the statement notorious 323 years ago, the world has gone on believing it. If anyone wishes to find out for himself, let him consult the Teubner stereotyped Greek edition of Aristotle's "Physics," Book IV., cap. viii., sect. 8-11, or the Leonine edition of St. Thomas Aquinas's "Opera Omnia," tome ii., commentary on Aristotle's physics, texts 71 and 74, pp. 183-7. It is in the British Museum.

Aristotle is discussing the notion of a vacuum, and using the argument from motion. Lecture xi. in "Opera Omnia," containing the argument, begins on p. 180, and is headed, "Ex parte motus ostenditur non esse vacuum separatum." An intelligible paraphrase of the important parts of texts 71 and 74, or sect. 8 and 11, is as follows:—"§ 8: We see that a heavy body is borne (or translated) faster for two reasons, either because of differences in the medium through which it passes, as earth or air or water, or other things being equal, because the body itself differs by reason of its superior gravity or buoyancy.

As regards the medium, the reason is that it resists. . . . If air is twice as subtle as water, then for an equal distance the time of translation in water will be twice that in air. . . . § 11: As regards differences in the body itself. We see that those bodies which have greater potentialities of movement (*δυναμιν*, inclinationem), whether downwards by reason of their weight, or upwards by their buoyancy, other things being equal as regards their shape (*σχήμασι*, figuris) are translated quicker over equal spaces, and this according to their proportionate magnitudes. But why should this be so in a vacuum? Therefore a vacuum is impossible. But why is it that they have different rates of translation? In a plenum it is indeed of necessity, for that body which is the faster, is so by reason of its power or of its shape or of its potentiality of motion whether of translation or projection, whereby it divides the medium more effectively. But in a vacuum all are equally effective, so that all are faster than one another. Which is impossible." § 11 is usually relied upon to convict Aristotle of error, but it is evident that motion through a resisting medium is premised.

The commentary of the Angelic Doctor makes this quite clear. The reader will find, probably to his amazement, that the new and modern notions of velocity were explicitly present to his intellect when he wrote. Special attention may be directed to § 13 of the commentary on p. 187, beginning "Deinde eum dixit, Secundum autem eorum." He actually used the words, "vel propter aptitudinem figurae quia acutum est penetrabilis," just as though he was describing the peculiar property of a modern pointed bullet. In the new and technical language of gunnery "motus" or "motus naturalis" is rendered precisely by the expression, "terminal velocity," the velocity at which the retardation of the medium, air, is exactly equal to the acceleration of gravity, resulting in a constant speed of fall. That Aristotle ever supposed for an instant that a 2-lb. weight fell, in the ordinary sense of words, twice as fast as a 1-lb. weight is an absurdity. What he taught was that the terminal velocity of a heavy body, such as Prof. Turner's sovereign, was greater than the terminal velocity of a light body, such as a feather, in a medium such as air or water. A penny can never fall faster than about 30 ft. a second through air. I performed the experiment last week, dropping pennies from Clifton Bridge, 250 ft., into the Avon. They take eight or nine seconds to reach the water. Sir George Greenhill has often expressed doubts to me as to the correctness of the accusation against Aristotle's common sense, but could never persuade a scholar to find the passage. A year and a half ago he showed me the above reference in the introduction of Mr. Lones's new book on Aristotle's "Natural History," and asked me to look it up. I consulted St. Thomas's Commentary in the British Museum, with the startling result I have mentioned, and fetched my former professor over to the reading-room to verify my discovery. That he did verify it must be my apology as a soldier for intruding into the domains usually preserved for scholars and philosophers of the highest order.

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27 Cranbrook Road, Bristol, January 9.

TEUBNER's edition of Aristotle's "Physica" is out of print, but the equivalent passage is found in his Aristotle's "De Coelo" (C. Prantl), p. 73, where the law is enunciated that the terminal velocity of a body in a medium is proportional to the weight.

Aristotle's law was justified by Newton in his ex-

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 rain-drop 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 Graviorum 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."

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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 $5\frac{1}{2}'$, 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 pre-

viously 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.

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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.

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"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 aerial was only 35 ft. high, and sheltered by other buildings.

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January 19.