LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Students' Physical Laboratories.

I AM truly sorry that the obituary notice published in NATURE two weeks ago should seem to Sir O. Lodge to minimise the work of Prof. Carey Foster and others. I feel sure that nobody can value Prof. Foster's work more than I do, but he had neither the money nor the other opportunities that Prof. Ayrton had in Japan. I admit a little overstrain in the statement that at the time when he created his Japanese laboratory "there were not half a dozen people in Great Britain who had experimented in electricity." I ought to have said that there were only a *few* workers in electricity. I had in my mind that before starting for Japan early in June, 1875, I had the curiosity to count the number of electrical papers published before the Royal Society, and now printed in vols. xxii. and xxiii. of the Proceedings. I had no knowledge of meetings after May 13, 1875, as I lived in Glasgow. At the forty-one consecutive meetings from December 11, 1873, to May 13, 1875, there were in all only five papers read having a bearing on electricity. These were two by Dr. Gore, one by Prof. Adams, one by Messrs. de la Rue, Hugo Müller and Spottiswoode, and one by Prof. Balfour Stewart. I was on my way to Japan when my own first published electrical investigation was described at the Royal Society on June 10, 1875.

I do not think that with a record like this it is worth while to cavil at my statement, for it is to be remembered that Royal Society papers, not electrical, were numerous. For example, at the meeting on June 18, 1874, there were twenty-eight papers, and on June 11 there were eight papers, and not one of these thirty-six papers had anything to do with electricity. I have not referred to a few papers during the year on terrestrial magnetism. It was with impressions due to this knowledge that I first saw the Japanese laboratory, and when I wrote the obituary notice my old feeling of overpowering admiration had come back.

In writing about Finsbury I ought perhaps to have expressed myself more clearly. Sir Oliver Lodge misunderstands me. Everybody knows that at King's and University Colleges, and at many other colleges, students were allowed to work in laboratories, and I can imagine that it was a great privilege to Sir Oliver to work under Carey Foster, whose record as a pioneer, as a teacher, and as a writer is so high that it is almost an impertinence in me to refer to it. Volunteer boys did excellent work in my own laboratory at Clifton College in 1871, just as Kelvin's students had worked much earlier in Glasgow; but I think I was right in saying that in all such cases the students were few in number, and that they were volunteers. My point was that *all* the students at Finsbury had much laboratory work, and they were made to think that laboratory work was much more educational than attendance at lectures. I still think that the reform effected at Finsbury was exceedingly great, and that it was of quite a new kind, for it was not only in the nature of the laboratory work, but in its combination with many other kinds of work, that the reform consisted. I cannot hope to carry Sir Oliver with me, for it is quite evident that he knows of Finsbury only at second or third hand. He seems to think that there were only evening classes. It is true that many of the evening students were of the artisan class; but the day classes were of much greater importance than the evening classes, and students of the ages of sixteen to eighteen coming from secondary schools will not fit into his description.

I am glad to think that Sir Oliver approves of that small part of the Finsbury work of which he has heard. No doubt much may be said for and against some of the Finsbury methods, but I do not care to continue a discussion founded on an obituary notice. I know of no obituary notice which might not be the subject of controversy.

JOHN PERRY.

December 5.

A Model Atom.

THE following attempt to construct a kinematics of an atom may prove of interest to readers of NATURE.

Let a sphere of a certain radius (depending on the given circumstances) be described round each charged particle as centre, and let the radii of these spheres be such that some of the spheres are in contact. The spheres may be called *spheres of interference*, and the points of contact *nodes*. The spheres associated with two oppositely charged particles may be supposed to touch internally, and those associated with two particles carrying similar charges may be supposed to touch externally.

A model atom may now be built up of spheres touching one another in this way. We shall suppose that there is one sphere surrounding all the others, which we shall call the atomic sphere. Within this sphere there may be other spheres which completely surround a number of others. Such groups will be called subatoms.

As the electrons within the atom move about we shall suppose that in general their spheres of interference adjust themselves so that the contacts are preserved; such a motion may be called a *steady motion*, and may be obtained by applying a continuous succession of conformal transformations to a given configuration of the spheres or set of spheres.

When an atom is in a normal state we shall suppose that the outer shell contains either a ring of electrons the spheres of interference of which touch one another in succession, and also touch two other spheres, one internally and the other externally, or a system of electrons at the corners of a polyhedron, the spheres being now arranged so that each one touches all its neighbours and two other spheres as before.

If the two extra spheres are kept fixed the electrons can move round an ellipse, so that the contacts of the spheres are preserved, the radius of a sphere being at any time proportional to the distance of its centre from the radical plane of the two fixed spheres (Steiner's porism). If now the mass to be associated with a given electron or sphere of interference belonging to the ring is inversely proportional to the square of the radius of the sphere, the total mass, kinetic energy, and position of the centre of mass will remain invariable so long as all the contacts are preserved.

When an atom is ionised we may suppose that there is one sphere missing from the ring if the charge be positive, and an extra sphere in contact with two spheres of the ring, but not belonging to the ring, if the charge be negative. If the number of degrees of freedom is calculated by allowing three for each electron and subtracting one for each contact or other geometrical condition, there will be a gain of one degree of freedom for each additional charge, whether it arises from the gain or loss of an electron.

We may suppose that a line spectrum is emitted when a given arrangement of nodes or geometrical conditions is preserved, and a continuous spectrum when the geometrical conditions are violated.

The group of infinitesimal conformal transformations seems the natural one for describing the kinematics of a system within a sphere; it may be built up from successive inversions with regard to spheres, just as the group of displacements of a rigid body may be built up from successive reflexions in different planes; it should be noticed, however, that an even number of inversions are required to produce an infinitesimal change.

An inversion does not alter the type of contact of two spheres when the centre of inversion is external to both, but when it lies in the space between two spheres the type of contact changes, and the spheres become external to one another. This may be regarded as a kinematical description of a radio-active process, for a subatom may be thus brought outside the atomic sphere by a continuous succession of changes. According to this view, an atom would break up whenever one of the centres of inversion happened to lie within the atomic sphere.

We suppose that in general the arrangement of spheres within the atom is not symmetrical; if, for instance, the atom forms part of a molecule, the field of force is not symmetrical, and there seems no reason why the arrangement of the electrons should be so.