yellow colour, which gives it the distant resemblance to a hornet from which it derives its name, but this is confined to its colour, for the long, tapering Asilus differs altogether in shape from a hornet.

The species of the next family, Bombyliidæ, are stout and hairy, and those of the typical genus Bombylius have a remarkable resemblance to small Bombi (humble-bees), from which, however, the two wings and the long straight proboscis at once distinguish them. The two remaining families dealt with in this volume are of small extent, and perhaps of less interest than the two first. The Diptera are a somewhat neglected order of insect, but are more studied now than formerly, and we are sure that Prof. Lundbeck's work will be found very useful to English entomologists, for whose benefit it is written in their own language. The order Diptera is probably the largest of the seven great orders of insects except the Hymenoptera, and we wish Prof. Lundbeck long life that he may be able to complete the work which he has so well begun.

Moving Loads on Railway Underbridges, including diagrams of Bending Moments and Shearing Forces, and Tables of Equivalent Uniform Live Loads. By H. Bamford. Pp. iv+78. (London: Whittaker and Co., 1907.) Price 4*s.* 6*d.* net.

THIS is a reprint in book-form, with additions, of a series of articles which appeared in *Engineering* in the autumn of 1906. Those who have had any experience of such work will know how tedious is the process, as usually conducted, of determining the maximum straining actions on a railway girder supported at the ends, due to any given type of train load, and will appreciate the methods here given, which are characterised by directness, simplicity, and comparative brevity. The author uses analytical computation with systematic tabulation, and also, as an alternative method, graphical diagrams based on a clever adaptation of the ordinary bending and shearing force diagrams. By one or other of these methods, and especially the latter, the "equivalent" uniformly spread loads for both maximum bending moments and shearing forces are quickly and easily determined. The investigation is limited to the force actions on the bridge taken as a whole, and does not consider separately the resistances offered by the platform and main girders, but so far as the subject is dealt with the author is to be congratulated on having produced a most useful and practical work.

Practical Floor Malting. By Hugh Lancaster. Pp. iv+211; with numerous illustrations. (London: *The Brewing Trade Review*, 1908.) Price 12*s.* 6*d.* net.

CONSIDERING the economic importance of floor malting in this country, it is somewhat remarkable that no work on the subject possessing any claim to thoroughness has hitherto been published. We hoped to find that the present book filled the void, but although it is a useful addition to the literature of malting, it cannot in its present form be regarded as a complete technical treatise on the subject. The author is evidently thoroughly conversant with the practice of floor malting, but owing, presumably, to lack of literary experience, he has not done justice to his knowledge, and the book is marred by many signs of hasty writing. As it stands, however, the work is distinctly a useful one, and we have nothing but praise for the ten collotype plates it contains which illustrate the differences existing between the various types of barley employed in malting. These plates are of exceptional merit, and add very much to the value of the book from a technical point of view.

LETTERS TO THE EDITOR.

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Students' Physical Laboratories.

IF a protest is not made, I see some danger of the pioneer work done towards organising physical laboratory work for students in University and King's Colleges in London being inadvertently ignored, and everything of that kind attributed to Finsbury. Probably, indeed, the sound work unobtrusively done in early days is known to very few. Allow me to say, therefore, from personal knowledge, that students were admitted to physical laboratory work in these colleges before 1872—in one of them, I believe, in 1866—and that the course of quantitative laboratory instruction through which I was myself put by Prof. Carey Foster, in topographical circumstances of some difficulty, was of high value; and, indeed, reached a standard of accuracy not readily eclipsed in any students' laboratory with which I have since become acquainted.

To take a single instance, Carey Foster described his "bridge" method in 1872, and students were regularly familiarised with it. I remember also making a series of well-designed experiments on moments of inertia, on the kinetic torsion of wires, and on determinations of *g* by falling bodies and chronograph as well as by pendulums. We also used to measure E.M.F. by the potentiometer method, then called Poggendorff's; while other practical subjects were conduction of heat, rates of cooling, specific and latent heats, on the lines of Regnault; absolute density of liquids, by weighing in them a gauged ivory sphere, density of gases, &c.; a long series on magnetic moments and terrestrial magnetism in the light of Gauss's theory; the usual optical measurements and some less usual; Siemens's pyrometer (then under test for a British Association Committee); much work with a tangent galvanometer and resistance boxes—then comparatively new—on Ohm's and Joule's laws; measurements of electrochemical equivalents, &c., &c.; all before 1875. In one of the last-mentioned determinations a platinum basin was used and a weighable deposit obtained, very much on lines afterwards rendered secure and classical by Lord Rayleigh.

Indeed, I went through most of the things done in laboratories to-day which do not involve instruments of more recent date, and in 1875 we published a joint paper, "On the Flow of Electricity in a Plane," wherein the equipotential lines were plotted by an experimental method handier and more accurate than had been possible in previous observations of the kind—a method invented entirely by Carey Foster (see *Phil. Mag.*, December, 1875, §§ 47–50, with an incomplete continuation in 1876).

It is true that in those days attention was paid to the principles of pure physics rather than to technology; and undoubtedly, as technical work became prominent, other laboratories went far ahead in such subjects as the design of practical measuring instruments and in facilities for large-scale work.

But without suggesting for a moment that a word too much has been said in praise of the energetic pioneers in the field of practical work and electrical engineering, it will, I feel sure, be admitted that to say (as on p. 74) that before 1875 only five persons had experimented in electricity in Great Britain, that the Finsbury system was radically different from anything which previously existed, and that before 1879 professors had merely shown experiments at the lecture table, is to make statements which involve a considerable amount of exaggeration, and unintentionally misrepresent the facts.

I take it that the novelty at Finsbury chiefly lay in the permanent installation of a number of ingenious appliances, whereby a crowd of evening students could be put through a useful course of practical work, such as would give them some preliminary idea of measuring physical quantities, and infuse their otherwise abstract notions with something definite and concrete, without the necessity for periodical preparation and clearing away by an impracticably large assistant staff.

But since the students to be educated at Finsbury were largely of the higher artisan class, or at any rate were already familiar with machinery, perhaps I should rather put the matter conversely, and say that the object aimed at was to coax their already too material and concrete ideas towards something more generalised and abstract, by analysing into simplicity the complex machines with which many of them in their daily life had to deal, thus assisting them to grasp something of the theoretical physical principles underlying them all.

An admirable object, excellently carried out! Not a word have I to say towards minimising it: only do not let us minimise the work of others either.

November 21. OLIVER LODGE.

Apparent Decay of Radium.

I WISH to put on record an observation relating to the amount of "electrolytic gas" obtainable from a solution of radium bromide. Some four years ago, about 172 milligrams of radium salts, of which 152 were bromide and 10 sulphate, were enclosed in four small bulbs along with water, which dissolved the bromide, and in which the sulphate was suspended. These bulbs were sealed to a small Töpler pump, and for three years the mixed oxygen and hydrogen gases were pumped off at short intervals—about four days between two extractions. With the emanation accompanying this mixture various experiments were performed, an account of which has appeared in the Proceedings of the Royal Society and the Transactions of the Chemical Society.

In November, 1907, I received from the Vienna Academy what was supposed to be 0·5 gram of pure radium bromide; I was told that that was its weight in 1905. It weighed on receipt only 0·388 gram. This substance was washed into a bulb, and sealed to the pump, along with the other bulbs. The amount of gas collected from the larger quantity, however, did not appear to be proportional to its greater weight, and as analysis of a sample showed that it consisted largely of carbonate, insoluble in water, it was resolved to convert the carbonate into bromide by introducing into the bulb with a pipette some pure hydrobromic acid. (I may mention, parenthetically, that the small sample, converted into bromide, gained in weight to such an extent as to show that the original amount must have weighed 0·4971 gram, as $RdBr_2,2H_2O$.) The gas pumped off after this addition of hydrobromic acid contained much free bromine, but after a few weeks the evolution of bromine ceased, and "electrolytic gas" was produced to the amount of about 30 c.c. a week, always mixed with a small excess of hydrogen. This regular evolution continued from February until November 11. On that day the usual 30 c.c. of gas were pumped off; I have a note that "an unusually small quantity of hydrogen remained after explosion." On November 18 the gas was again pumped off; the quantity was approximately 13 c.c. Although it appeared unlikely that the tubes and taps should have been blocked, it was still possible. On November 25 the gas was again removed; its volume was about 1·5 c.c. At this stage air was admitted into the pump and the connected bulbs, and it was proved that there had been no stoppage. Advantage was taken of this to clean the pump and the connecting tubes, and to regrease the stop-cocks. The air was then removed completely by pumping. To-day (November 30) the gas was again pumped off; its volume was about 0·5 c.c. It still exploded, and left about half its volume of excess hydrogen.

Two alternative suppositions suggest themselves:—either the radium bromide, of which the apparatus contains 0·5071 gram, implying 0·2716 gram of metallic radium, has practically ceased to decompose water (about 25 c.c. of solution are present in the bulbs), or the reverse reaction, viz. the velocity of combination of oxygen and hydrogen to form water, has increased to such an extent as to reverse the decomposition.

It has been assumed that the life-period of radium is very long, say 2000 years, although Mr. Cameron and I, by measuring what we believe to be the true volume of the emanation, arrived at a considerably shorter period. Here, however, appears to be, on the first alternative, a proof that one of the ways in which the radium expends at least a portion of its energy has been stopped. It would be interesting to know if the other ways, say the evolution of heat or the emission of "rays," are similarly affected by time. WILLIAM RAMSAY.

University College, November 30.

Production of Helium from Uranium.

IN a paper in the October number of the *Philosophical Magazine* of this year I gave a preliminary account of some attempts to detect and measure the production of helium from the primary radio-elements, on which I have been engaged since 1905. The results given were few, and referred mainly to the element thorium. The following further results, obtained since the publication of the paper, with the element uranium carry the subject a stage further. The method is described in detail in the paper referred to. By special arrangements the solutions of the substances employed can be freed absolutely from air, and maintained in this condition indefinitely. After any desired period of accumulation the gases can be completely expelled by boiling the solution in a stream of gas from a voltameter. The expelled gases are freed from water by cooling, and then subjected to the action of the vapour of calcium in a special vacuum furnace, whereby all but the inert gases are perfectly absorbed. After cooling the furnace is filled with mercury, and the residual gas, if any, compressed into the smallest possible spectrum tube of lead glass. The minimum quantity of helium detectable in a successful experiment has been found by repeated trial to be 2×10^{-10} gram. Blank tests with a similar apparatus containing sodium sulphate solution were performed, and I feel confident that the data obtained are trustworthy.

I have used two separate quantities of uranium nitrate. The first and smaller had been carefully purified by Mr. T. D. Mackenzie by extraction with ether. It contained 340 grams of the element uranium. When it became evident that the rate of production was too slow to be conveniently estimated with this quantity, a second experiment on a much larger scale was started. The cost of this and similar other large-scale experiments was defrayed by a research grant from the Carnegie trustees. Four kilograms of uranium nitrate of good commercial quality, which had been re-crystallised from water, were employed. It contained 1850 grams of uranium. The preparation of the experiment and complete removal of air were effected by August 15 of this year. The first test for helium was performed after a period of sixty-one days. Helium in several times the minimum quantity detectable by the method employed was proved to be present in the extracted gases. The second test was performed after a period of twenty-seven days. Helium was again present, this time in quantity not much, if any, greater than the minimum detectable. The next test was performed after twelve days. No helium could be detected, although the experiment was a singularly perfect one. An experiment was then performed with the smaller quantity of uranium after a period of accumulation of 128 days. Helium was clearly detected, and its quantity estimated to be not greater than 1·5 times the minimum quantity.

The production of helium from uranium may therefore be considered to be established. With regard to the rate of production, the experiments show that this cannot be far from $2\times10^{-12}(\text{year})^{-1}$. That is to say, about 2 milligrams of helium are formed per year per million kilograms of uranium. The second test referred to shows that the rate is not less than 1·5. The third test shows that it is less than 3·3. The last test with the smaller quantity shows that the rate is not less than 1·7, and probably not greater than 2·5. It is of interest to note that the theoretical rate of production I recently calculated from the disintegration theory is $2\times10^{-12}(\text{year})^{-1}$, on the assumption that one atom of uranium produced but one atom of helium. These measurements, therefore, lend no support to the view, discussed in the paper referred to, that uranium on disintegration expels two helium atoms.

I may mention that I have commenced the observation of a quantity of sylvine (potassium chloride), one of the minerals investigated by Strutt, and regarded by him as exceptional in containing helium which cannot be ascribed to known radio-active changes. The tests so far indicate that the rate of production of helium from this substance, if any, is below $2{\cdot}5\times10^{-12}(\text{year})^{-1}$.

University of Glasgow. FREDERICK SODDY.