genera and species. In a work with such a title one expects rather to find a detailed description of the methods of procedure as regards capture, habits, habitats, &c., and not to require the young collector to plunge *in medias res* without such knowledge. The introduction certainly attempts to deal with these points, but it only consists of eleven pages of large print; and the all-important subject, in such a work, of "the habits of beetles and how to catch them" is dismissed in about thirty lines.

The plates are worth the cost of the work, which may be found useful for a somewhat more advanced student, but which hardly appears to realise our idea of a "Young Collector's Handbook."

Exercises in Practical Physiology. By Augustus D. Waller, M.D., F.R.S. Part iii. Pp. 91. (London: Longmans, Green, and Co., 1897.)

THE exercises and demonstrations contained in this and the two preceding parts are primarily intended to facilitate class work in physiology, and for use in conjunction with such a text-book as the author's "Introduction to Human Physiology." The present part contains sixty-eight instructive experiments on the physiology of the nervous system, and descriptions of the instruments used in investigations in electro-physiology generally. The subject is one which the author has made peculiarly his own; so that the experimental details will be found sufficient to enable students and demonstrators to set up the required apparatus satisfactorily and obtain good results. The book affords a strong argument for the teaching of the principles of physics to students of physiology; for without this fundamental knowledge it would be impossible to perform the experiments intelligently.

Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. Fourteenth annual issue. Pp. 270. (London: Charles Griffin and Co., Ltd., 1897.)

THIS work, in addition to being a convenient handbook of our scientific societies, contains lists of the papers read during 1896 before societies engaged in fourteen departments of research. It is thus a very useful index to scientific progress, as well as an indispensable book of reference to the officers, places and times of meetings, publications, and membership fees, of British Societies for the advancement of knowledge of every kind.

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.*]

The Trotting Horse.

IN "The Primary Factors of Organic Evolution," Prof. Cope, whose recent death has taken from us an untiring worker and suggestive writer, adduces the evolution of the trotting horse as an illustrative case of the inheritance of characters due to the exercise of function (p. 426). Prof. Brewer, of Yale, is quoted at some length. He says: "There is every appearance and indication that the changes acquired by individuals through the exercise of function have been to some degree transmitted, and have been cumulative, and that this has been one factor in the evolution of speed. . . . There is nothing whatever in the actual phenomena observed anywhere along the line of this development of speed that would lead us to even suspect that the changes due to exercise of function had *not* been a factor in the evolution, and there is not a particle of evidence, other than metaphysical deductions, much less proof, that it would or could have gone on just the same by mere selection and adventitious variation" (pp. 429-430)

Mr. A. J. Meston, of Pittsfield, Mass., has recently discussed this question statistically in a pamphlet entitled "The Common Sources of 2·10 trotting and pacing speed." The results of this seemingly very careful investigation are of such general biological interest, that I have no hesitation in requesting space to draw attention to Mr. Meston's conclusions.

The first point that is especially noteworthy is the predominant influence of one horse, Hambletonian 10 (1849-1876). "While we have extreme speed without the aid of Hambletonian, it is, nevertheless, a fact that the influence of Hambletonian has been exerted amongst 92 per cent. of the 2·10 trotters, and 84 per cent of the 2·10 pacers [that is to say, trotters or pacers who can cover a mile in two minutes and ten seconds or under] We have pacing speed, apart from Hambletonian, within two seconds of the best record; but trotting speed without Hambletonian is four seconds behind the fastest mile. No mile has yet been trotted faster than 2·07¾ without the aid of Hambletonian. . . . Furthermore, the majority of both the trotters and pacers that descend from Hambletonian have more than one cross of his blood. . . . A very superficial examination of the blood of the 2·10 list shows that Hambletonian has exerted a predominant influence in its formation" (pp. 6-7).

The second point is the conclusion to which Mr. Meston is led with regard to the transmission of acquired speed. "It appears from the table," he says, "that some stallions and mares, after having been trained to fast records, have got foals that made fast records. It also appears that demonstrated capacity for speed and the ability to beget speed are qualities possessed in common by many stallions and mares, but the relative dates of 'making the record' and 'getting the foal' exclude the affirmation, if not the probability, of cause and effect between the two occurrences. It does not appear that a line of trained ancestors is more successful in producing speed than a line of untrained ancestors, or a mixed line of trained and untrained ancestry. Therefore, the evidence is negative upon the question whether increase of speed acquired by the individual through training, habit, or experience, is passed on to the foal, in any degree, by the force of heredity. On the other hand, the evidence is positive and convincing that congenital capacity for speed and innate plasticity for the development of speed are transmitted hereditarily to progeny, and that, by judicious or fortunate crossing, the capacity and plasticity have been vastly increased" (p. 23).

As this is the most careful statistical investigation of the kind with which I am acquainted, it appears to me that Mr. Meston's conclusions (which, he informs me, were not those that he anticipated at the outset of his inquiry) are worthy of careful consideration.

C. LLOYD MORGAN.

Bristol.

Fire-fly Light.

IN *Wiedemann's Annalen* for December last, Prof. H. Muraoka published an account of the rays which he found to be emitted by a fire-fly (described by him as a "Johanniskäfer"), and which resemble the rays which Dr. Dawson Turner has found to be emitted by glow-worms, in that they can pass (like Röntgen's rays and uranium rays) through aluminium. Can any reader of NATURE state what species of insect is known by this name? Muraoka describes them as on the average 13-15 mm. long; the largest being 20 mm. long. He says they have two (or in smaller insects three) rows of luminous spherules on the under side of its body, but that the whole body is photographically active. He used about 1000 insects at a time, with exposures of two to three days.

June 6. SILVANUS P. THOMPSON.

THE LIQUEFACTION OF FLUORINE.[1]

THE physical properties of a large number of mineral and organic fluorine compounds led to the theoretical prediction that the liquefaction of fluorine could only be accomplished at a very low temperature.

Whilst the chlorides of boron and silicon are liquids at the ordinary temperature, the fluorides are gaseous, and well removed from their boiling points. The same difference is noticeable in their organic compounds,

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[1] "On the Liquefaction of Fluorine," by H. Moissan and J. Dewar. (Translated from *Comptes rendus* of the Paris Academy of Sciences, May 31, p. 1202).

ethyl chloride boiling at 12°, ethyl fluoride at −32°, propyl chloride boiling at +45°, ethyl fluoride at −2°.

Similar observations have been previously made by Paterno and Oliveri, and by Vallach and Heusler. These facts can also be connected with the experiments of Gladstone on atomic refraction. Finally, although clearly a member of the chlorine group, fluorine in some of its properties also presents some analogies to oxygen. The whole of these observations appear to clearly establish that fluorine would only with difficulty be reduced to a liquid, and it has already been shown by one of us that at −95°, under ordinary pressure, it does not change its state.

In the new experiments that we now publish, the fluorine was prepared by the electrolysis of potassium fluoride in solution in anhydrous hydrofluoric acid. The fluorine gas was freed from the vapours of hydrofluoric acid by passing it through a small platinum spiral cooled by a mixture of solid carbon dioxide and alcohol. Two platinum tubes filled with well-dried sodium fluoride completed this purification. The liquefaction apparatus consisted of a small cylinder of thin glass, to the upper part of which was joined a platinum tube. The latter contained another small tube of the same metal. The gas to be liquefied arrived by the annular space, passed into the glass bulb, and passed out again by the inside tube. This apparatus was united to the tube which led in the fluorine.

In these experiments we have used liquid oxygen as the refrigerating substance. This oxygen was prepared by the methods described by one of us, and these researches have necessitated the employment of several litres of this liquid. The apparatus being cooled to the temperature of quietly-boiling oxygen (−183°), the current of fluorine gas passed into the glass bulb without liquefying; but at this low temperature the fluorine had lost its chemical activity, and no longer attacked glass.

If now the pressure on the boiling oxygen be reduced, it is seen, as soon as rapid ebullition is produced, that a liquid trickles down the walls of the glass bulb, whilst no gas issues from the apparatus. At this moment, the exit tube is closed with the finger to prevent the entrance of any air. Before long the glass bulb becomes filled with clear yellow liquid possessing great mobility. The colour of this liquid recalls the tint of fluorine seen through a layer a metre thick. According to this experiment, fluorine becomes a liquid at about −185°. As soon as the little condensation apparatus is removed from the liquid oxygen, the temperature rises and the yellow liquid begins to boil, furnishing an abundant evolution of a gas which presents all the energetic reactions of fluorine.

We have taken advantage of these experiments to study some of the reactions of fluorine upon bodies maintained at very low temperatures. Silicon, boron, carbon, sulphur, phosphorus, and reduced iron, cooled in liquid oxygen, and then projected into an atmosphere of fluorine, do not become incandescent. At this low temperature, fluorine does not displace iodine from iodides. Its chemical energy, however, is still sufficiently great to decompose turpentine or benzene with production of flame even at −180°. It would seem that the powerful affinity of the fluorine for hydrogen is the last to disappear.

Finally, there is one other experiment that we ought to mention. When a current of fluorine gas is passed into liquid oxygen, there is rapidly produced a white flocculent deposit, which soon settles at the bottom of the vessel. If the mixture is shaken and poured on a filter, this precipitate is separated. It possesses the curious property of deflagrating violently as soon as the temperature rises. We are pursuing the study of this compound, as well as that of the liquefaction and solidification of fluorine, in which further experiments are required.

A NEW DETERMINATION OF THE GRAVITATION CONSTANT AND THE MEAN DENSITY OF THE EARTH.

An account of a new determination of these quantities, carried out in a very careful manner by Dr. C. Braun, S.J., at Mariaschein in Bohemia, has just been published in the *Memoirs* of the Vienna Academy (Bd. lxiv., Math. Nat. Classe).

Dr. Braun has been engaged on the work since 1887. He used the torsion-rod method, and though his apparatus was considerably larger than that of Prof. Boys, it was still much smaller than the older apparatus of Cavendish, Reich, or Baily. The rod was about 24 cm. long, and was suspended from a tripod by a brass torsion wire, nearly 1 metre long and 0·055 mm. in diameter. The whole torsion arrangement was under a glass receiver, about a metre high and 30 cm. in diameter, resting on a flat glass plate. The receiver could be exhausted, and in the later experiments the pressure was about 4 mm. of mercury, and the disturbances due to air currents were very greatly reduced. The attracted masses at the end of the rod were gilded brass spheres, each weighing about 54 grammes. Round the upper part of the receiver, and outside it, was a graduated metal ring, which could be revolved about the axis of the torsion wire, and from this were suspended, about 42 cm. apart, the two attracting masses. Two pairs were used: one a pair of brass spheres about 5 kgms. each, the other, a pair of hollow iron spheres, filled with mercury, and weighing about 9 kgms. each.

To determine the position of the torsion-rod, a mirror was fixed on the centre of the rod, and immediately in front of it was a mirror at 45° to the horizontal, throwing the reflexion down through the base plate on to the horizontal objective of the observing telescope; another mirror, immediately under the lens, was inclined at 45°, and sent the beam horizontally on to a graduated glass scale in the focal plane of the eyepiece. The object of which the image was viewed was an index mark on a plate placed horizontally just below the scale, and the light from it was made to traverse the axis of the telescope outwards by reflexion at a parallel plate of glass at 45° to the horizontal. As the index mark was nearly at the same distance from the objective as the scale, the rays fell nearly parallel on to the torsion-rod mirror, and the angular value of the scale divisions was determined from their length and the distance of the scale from the objective. It was also determined by a theodolite, viewing the scale through the object-glass, and found to be about 3½ min.

The instrument was fixed on a stone slab, in the corner of a room with very solid walls, and protected from temperature variations and electrical effects by a casing of cloth and tinplate.

As there was a continuous creep of the torsion-rod in one direction, amounting in the course of years to several lengths of the scale, it was necessary to have some method of moving the torsion-head. This was effected from outside the receiver in a very ingenious manner. A plate was fixed on a part of the torsion-head which did not revolve, and to this was attached a clock from which the escapement was removed, and on the axis of the escapement-wheel was fixed a small magnet. On the axis, where the driving spring had been, a pinion was fixed, gearing with a large wheel attached to the torsion-head. The magnet could be turned round by moving a magnet outside the receiver, and so the torsion-head could be slowly revolved. The gearing-down was such that, if the minute finger of the clock moved one minute, the image of the index in the telescope moved one scale division.

Vibrations of the torsion-rod were started by a light magnetised fork, which could be made to softly touch the rod on either side by the motion of a magnet outside the receiver.