Mr. T. C. Hepworth and Mr. W. T. Bashford trace the history and describe carefully the various processes of photography. Mr. J. S. Keltie has an excellent article on Polar exploration, illustrated with a North Polar and a South Polar chart. A short but very good paper on protoplasm is contributed by Mr. J. A. Thomson; and Prof. Sorley makes the most of the few pages set apart for psychology. Rain is discussed admirably by Dr. Buchan, and the rainbow by Mr. W. T. Omond. Reflection and refraction are dealt with by Dr. Alfred Daniell. The main facts relating to the Red Sea are presented by Dr. John Murray; and Dr. Hugh R. Mill sets down all that is likely to be wanted by students who have occasion to refer to the article "River." Altogether, the various papers we have examined may be commended as in every way worthy of the high reputation secured for the present edition by preceding volumes.

La Place de l'Homme dans la Nature. By T. H. Huxley. (Paris: B. B. Baillière et Fils, 1891.)

MORE than twenty years ago a French translation of Prof. Huxley's well-known work, "Man's Place in Nature," was published. The translator was Dr. E. Dally. In the present volume this rendering is reissued, and along with it are associated translations of three papers in which Prof. Huxley has presented his ideas on various ethnological subjects. These papers have been translated by Dr. Henry de Varigny, to whom Prof. Huxley expresses thanks for the care he has taken to represent clearly and faithfully the meaning of the original. The volume will be very welcome to French students who desire to understand the methods and tendencies of English scientific thought.

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.]

Smithsonian Standards for Physical Apparatus.

ON the occasion of a scientific expedition of which I had charge many years ago, the need of common standards of size for the parts of different astronomical and physical instruments was brought forcibly to mind; for the instruments used, while of the latest and best construction, were necessarily dismembered, and then transported in fragments to their scarcely accessible destination by numerous independent bearers; and if any accident happened to any fragment of any piece of apparatus, it was found, as a rule, that the whole was rendered useless, since it could not be replaced from the like parts of other pieces which were spared. The weapons of attack of the little scientific force were, then, in one important respect, far inferior to those of modern warfare, in that there had been no attempt to make their parts interchangeable.

My attention having been drawn to the matter, I was led to examine astronomical and physical instruments in all cabinets accessible to me, with a special view to this feature. I found that, as a rule, no draw-tube, screw, or other piece from one instrument would fit the corresponding parts in any other, there being no attempt to make them interchangeable even where they came from the same maker.

This experience must be confirmed by that of most others, who will probably agree that this is a cause of incessant, but quite avoidable, loss and delay, even where apparatus is used under ordinary conditions, and it has led to inquiry for some scheme which would assimilate different parts of the work, not only of the same, but of different makers. Some of the plans suggested are well matured, and in themselves apparently commendable, but all are too complex, the ambition of the authors being, as a rule, to make them so complete as to cover all possible demands of future progress.

What has been wanted by many others doubtless is some simple and practicable plan for immediate use, which shall yet

be found in accord with the larger scheme which may be under consideration hereafter.

When it fell to me to meet the somewhat varied wants of the Smithsonian Institution by a plan which should at least enable a beginning to be made in the right direction, it seemed that this should be with such simple and general conditions, that common consent to them might almost be counted on, at least on the part of all ready to use the metrical standards.

To provide for the immediate practical wants of this Institution, advice was sought of several of the best instrument makers, and a considerable number of tubes and screws by English, French, and German, as well as American makers were examined to find out the sizes which long-established use in these countries had shown to be practically convenient, and the forms of screws which the best modern practice of scientific instrument makers concurred in; and this having been done, dimensions having a metrical unit, and as near these sizes as practicable, were adopted—not as a finality, but as a beginning.

In the hope that others may consider this very modest attempt to be in the right direction, and that these standards may fall into use for immediate needs, and thus tend to bring about the adoption of that much more complete system of international standards which most will admit to be (at least in the abstract) desirable, I beg leave to inclose a circular which has been sent to all instrument makers employed by this Institution, trusting that you may find it of sufficient interest to bring it to the attention of the readers of NATURE.

S. P. LANGLEY,
Secretary.

Smithsonian Institution, Washington, D.C., December 16.

Circular to Instrument Makers.

In all apparatus used by the Smithsonian Institution a series of standard sizes for metal tubes and for the screws chased on them has been adopted. The metric division is employed, and all tubes ordered are to be finished to some even number of half centimetres in diameter, unless this cannot be done without great difficulty. The series of diameters and corresponding threads to be used is, for a diameter of

10	centimetres,	5	threads to a centimetre.
9	"	5	" " "
7.5	"	7	" " "
6	"	7	" " "
4.5	"	10	" " "
3	"	15	" " "

When any new tube has to be ordered, it should be made one of these diameters and chased with one of these threads, if this can in any way be done.

New eye-pieces are to be as far as possible made to fit the three-centimetre plug gauge supplied by Pratt and Whitney, and in fitting them to old work, this size is still to be adapted wherever possible. The Institution is preparing standard plugs and gauges of the diameters given above, and has on hand chases of 5, 7, 10, and 15 threads to the centimetre. All screws have the 60° thread, with flattened top and bottom. These it will supply at first cost to any instrument maker engaged in its work.

Plug gauges are to be had of great accuracy and at moderate cost from several standard tool makers. Those here referred to have been made for the Smithsonian Institution by the Pratt and Whitney Company of Hartford, Connecticut, and are within a limit of error of two-hundred-thousandths of an inch at 62° Fahrenheit. The hobs are from the same makers.

Pigment in Yellow Butterflies.

Apropos of the interesting discussion on comparative palatability and warning colours (NATURE, November 19, p. 53; November 26, p. 78), it may be of interest to your readers if I restate in your columns some of the properties of the yellow pigment contained in the wings of the common brimstone and many other butterflies; the possible significance of which in conferring protective unpalatability is suggested by Mr. Beddard. My paper on the subject, to which Mr. Beddard refers, was read before the Chemical Society in June 1889; but, being more or less of a preliminary nature, it was published only in the abstracts of that Society's proceedings (Abst. Proc. Chem. Soc., vol. v., 1889, p. 117; *vide* also NATURE, vol. xl. p. 335).

The pigment is freely soluble in hot water, though quite insoluble in cold water, and in most organic solvents. Its aqueous solution is strongly acid to litmus; and, though it appears to be quite innocuous to frogs when injected under the skin, it may well be ungrateful to the ranine palate. At the same time it must be noted in this regard that its solubility in the secretions of the frog's mouth is but very slight.

The substance is, as I have shown, undoubtedly a derivative of uric acid, yielding the latter body as one of its products of hydrolysis. It gives the murexide reaction direct. It forms quite definite salts with metals, its compounds with the alkalis being soluble bodies.

Having regard to the wide-spread presence of the body in the scales of diurnal Lepidoptera, I have ventured to call it *lepidotic acid*. In its physical properties it closely resembles mycomelic acid, a yellow derivative of uric acid; and, in my original paper, I ventured to suggest a formula for the body. I hope shortly to publish a more complete account of the subject, and to assign a formula to lepidotic acid based upon fuller evidence. Meanwhile, in common with many others of your readers, I am looking forward to the appearance of Mr. Beddard's book. The literature of the subject of animal coloration is not easily accessible, and a text-book thereon will be a valuable acquisition. We have, it is true, the interesting work of Mr. Poulton; but the subject is there treated from what is, perhaps, a somewhat limited standpoint.

F. GOWLAND HOPKINS.

Sir Wm. Gull Research Laboratory,
Guy's Hospital, December 16.

The Chromosphere Line λ 6676.9.

In response to Father Cortie's implied question as to the identification of this line as belonging to the spectrum of iron, I would refer him to Appendix G of Roscoe's lectures on "Spectrum Analysis" (third edition). It is an extract from a joint paper by Ångström and Thalen, giving a list of several hundred (then) new identifications; among them appears K 654.3, ascribed to iron.

The original memoir was presented to the Stockholm Academy of Sciences in February 1865, and an English translation of it appeared the next year. I am unable to assign any reason why many of the identifications given in this memoir fail to appear in the map published three years later; but they do, and K 654.3 is among the missing.

C. A. YOUNG.

Princeton, N.J., U.S., December 15.

Grafts and Heredity.

I HAD not thought of grafts when I wrote my paper, and I have to thank Mr. Beeby for reminding me of an excellent illustration of my views; though I cannot gather from his letter whether he considers the "individuality" for which he contends to be represented by matter or force. Adopting his phrase I would apply it to both. The material form, *e.g.*, of the leaf of the scion, is due to molecular motion, set up by a group of forces acting in a way peculiar to the life of the scion; which forces, together with the resulting form, constitute its individuality—somewhat as a man is known by his mental and moral characters as well as by his face.

Now, no two individual plants could be fed more alike than a stock and its grafted scion; since they both receive identically the same food through the roots of the former. All I contend for is, therefore, that it would seem to be more probable that the organic molecules constructed out of this food are all alike, only differently arranged in the leaf of the scion and in that of the stock respectively. These arrangements must be due to molecular forces; while it is difficult to conceive in one's mind how any special kinds of matter can be concerned in the construction of the special forms of leaves; to say nothing of the total want of evidence of the existence of germ- or other plasm.

There is, however, a deeper question still which Mr. Croll asked:—"What determines molecular motion?"¹ He observed that although physical forces are not only interchangeable but can pass into those which, for want of a better expression, we may call vital energies; yet, as he says, nothing we know of in the properties of physical forces can

throw the smallest degree of light upon the above question. There is always, he adds, the "object" which runs through the whole of organized nature; which cannot be accounted for by means of the known properties of physical forces. In concluding his paper he says:—"If one plant or animal differs from another, or the parent from the child [and, we may add, the scion from the stock], it is because in the building up process the determinations of molecular motion were different in the two cases; and the true and fundamental ground of the difference must be sought for in the cause of the determination of molecular motion. Here, in this region, the doctrine of natural selection and the struggle for existence can afford no more light on the matter than the fortuitous concurrence of atoms and the atomical philosophy of the ancients." This observation seems to agree with the following remark of Sir J. D. Hooker on the origin of secretory glands of *Nepenthes*:—"The subsequent differentiation of the secretory organs of the pitcher into aqueous, saccharine, and acid would follow *pari passu* with the evolution of the pitcher itself, according to those mysterious laws which re-ult in the correlation of organs and functions throughout the kingdoms of Nature; which, in my apprehension, transcend in wonder and interest those of evolution and the origin of species."¹

The nearest approach to an answer to Mr. Croll's question is, as it seems to me (though it be but cutting the Gordian knot after all), that there exists a *responsive and adaptive power inherent in living protoplasm* which is called into action by external forces; so that by a change of environment—especially if the old and the new one be strongly contrasted—a plant, as a rule, at once begins to alter its structure so as to re-establish equilibrium with its new surroundings; and further, if these be maintained long enough, the altered structures become fixed and hereditary, while more or less of readaptation can commence again at any time.

We can no more discover the ultimate cause of this power which determines or directs molecular motion in living beings, than we can that of crystallization or gravity, reflex action or instinct. Innumerable facts, however, justify the full recognition of its existence.

To apply this to grafts. It is obvious that, whatever determines the molecular motion in forming the leaf of the scion, it is different from that which determines the molecular motion in forming the leaf of the stock, since the resulting forms of the leaves are different; and it is just this ultimate determining power, which is unknown and apparently unknowable, which characterizes the individuality of the scion on the one hand, and of the stock on the other. Form is but the outward and visible expression of this power. It is this, too, which underlies the responsiveness of protoplasm, and determines a new form in adaptation to, or in equilibrium with, a changed environment.

GEORGE HENSLOW.

Mental Arithmetic.

THE very simple method of multiplying large numbers, published in NATURE (p. 78) by Mr. Clive Cuthbertson, is mentioned by Pappus, Book II. (ed. Hultsch), 2-29, as an invention of Apollonius. The same method was known to the Hindoos under the name *Vajrabhāṣa* (Algebra with Arithmetic and Mensuration from the Sanskrit Brahmagupta and Bhāscara, translated by H. Th. Colebrooke, London, 1817).

The method may be enlarged to multiplying three and even more numbers all at once in the following manner:—

$$\begin{aligned}
 & (100a_1 + 10b_1 + c_1)(100a_2 + 10b_2 + c_2)(100a_3 + 10b_3 + c_3) = \\
 & \quad c_1c_2c_3 \\
 & + 10(b_1c_2c_3 + b_2c_3c_1 + b_3c_1c_2) \\
 & + 10^2(a_1c_2c_3 + a_2c_3c_1 + a_3c_1c_2 \\
 & \quad + b_1b_2c_3 + b_2b_3c_1 + b_3b_1c_2) \\
 & + 10^3[a_1(b_2c_3 + b_3c_2) + a_2(b_3c_1 + b_1c_3) \\
 & \quad + a_3(b_1c_2 + b_2c_1) + b_1b_2b_3] \\
 & + 10^4(a_1a_2c_3 + a_2a_3c_1 + a_3a_1c_2 \\
 & \quad + a_1b_2b_3 + a_2b_3b_1 + a_3b_1b_2) \\
 & + 10^5(a_1a_2b_3 + a_2a_3b_1 + a_3a_1b_2) \\
 & + 10^6a_1a_2a_3.
 \end{aligned}$$

¹ "What Determines Molecular Motion?—The Fundamental Problem of Nature" (*Phil. Mag.*, July 1872).

¹ Address to the Department of Zoology and Botany of the British Association, Belfast, 1874.