THURSDAY, AUGUST 18, 1870

MR. DARWIN AND THE FRENCH INSTITUTE

THE judgment of foreign nations gives the best clue to that of posterity; and it is therefore with peculiar interest that the countrymen of Mr. Darwin have watched the reception of his works in France and Germany. In the latter country his theory of the origin of species has been more or less completely accepted by those best qualified to judge, including men like Gegenbaur and Haeckel; and it has produced a complete literature of arguments and facts "fur Darwin," without encountering any very serious opposition. In France, the truth of the theory is far less extensively admitted, and has been lately the subject of prolonged discussion in the Academy of Sciences. The debate on Mr. Darwin's claims has now been adjourned for three months, but so far as it was reported in our last number it furnishes much ground for reflection.

At the present time, Imperial France is, perhaps, the most conservative in science of any country in Europe. It is not, therefore, surprising that, with a few exceptions like M. Claparède, French naturalists refuse to accept the theory of Natural Selection, and do not see (as others, and notably the Germans do) that it has already made a new epoch in human knowledge. Some, like M. Robin, object that it is not "demonstrable," and therefore not scientific at all; as if gravitation or the atomic theory had been, or could ever be, demonstrated like a proposition of Euclid. The Darwinian theory offers an explanation of acknowledged facts by the help of others equally indisputable, and it will only be "disproved" when it ceases to furnish an adequate explanation, or is superseded by a more simple and equally sufficient hypothesis. Meanwhile it fulfils, at any rate, one object of every theory, by stimulating research in all directions, and awakening new interests for the fresh investigations which it suggests. The speeches of MM. Milne-Edwards and de Quatrefages are especially worthy of note; though opposed to the conclusions of "The Origin of Species," both these distinguished men discuss them with the intelligence and clearness of Frenchmen and the tolerance of philosophers: and both accord to the earlier works of Mr. Darwin the hearty praise which they deserve.

Two points come out very clearly in the course of this debate. First, that the wide extent of a scientific man's labours may hinder them from being properly appreciated. Geologists, who know well the value of Mr. Darwin's writings on the formation of coral islands, are ignorant of his admirable series of observations on the fertilisation of orchids; and many who are startled by the boldness of his speculations in zoology would feel more confidence in his judgment, were they acquainted with his work on barnacles—"ouvrage, selon M. Milne-Edwards, qui depuis sa publication suffit à tous les besoins de la science." The field of natural history is now so cut up, that a great dipterist may be almost ignorant of many orders of insects, and quite so of other animals. Few professed geologists have an adequate knowledge of physiology, botany, and geology, and thus we fear science often loses in breadth what is gained in minuteness. One cannot, of course, hope even to touch upon all the provinces which Aristotle "ruled as his demesne," and the extent of Hunter's or of Cuvier's range is probably too great for modern times; but, after all, the theory of natural selection would scarcely have been framed by one who had studied nature in a less comprehensive spirit than Mr. Darwin.

Another remarkable point, which was brought forward in the Academy as a reproach to the English naturalist, is that his researches are those of an amateur. M. Milne-Edwards met the accusation in the best possible way,—by admitting it: "Si l'on entend par amateur un homme qui aime passionnément la science, et qui s'y consacre tout entier sans en attendre aucune rétribution—oui, M. Darwin est un amateur, un grand amateur." But for us in England it is a serious consideration how far it is wise to rely so exclusively as we do upon a continued succession of such amateurs. The result upon the English character of our entire disregard of science as a pursuit of national importance is what might have been expected. When with us a man by his own independent energies comes to the front rank, he is pretty sure to make an epoch in science, be he never so much an amateur in French eyes. Such were Hunter and Faraday. Of such men we can boast at the present moment among the greatest living historians, philosophers, and political economists of Europe.

But, on the other hand, we have never had a continuous "school" in any branch of learning, and as a distinct result the mass of our scientific work is much below the standard of average excellence abroad. Nowhere, probably, are text-books so slovenly and imperfect as in England; nowhere are even good workers in the rank and file of science so narrow and unenlightened; and nowhere does the attainment of equal results so little affect the character of the nation. Whether we shall ever succeed in uniting the discipline and organisation of French science with the freedom and individuality of our own is hard to say, but unless something be done in this direction, we must be content every now and then to see a whole branch like Physiology become almost extinct in this country. The success which has attended the efforts made in Germany (chiefly within this century alone) to establish scientific research in a durable manner, and as a part of the national organisation, may at least encourage us to forward the attempt here. In the meantime, as we have no system of establishing and fostering schools, we must hope for men who cannot be repressed.

Another point introduced by M. Robin was, that considered in respect of demonstrable facts which Darwin had introduced into science, there would be a hundred zoologists who should have precedence over him. "Si des publications de M. Darwin, on élimine les vues dont ni la réalité ni la fausseté ne sont démontrables, et qui dès lors ne sont plus objets de science, il lui reste un ensemble de titres qui est inférieur à celui que représentent les données scientifiques bien démontrées introduites dans la science par M. Bischoff; il lui reste même moins de titres à nos souffrages qu' à quelques-uns des savants qui sont placés *ex æquo* avec lui sur notre liste de présentation. Ce sont là les raisons qui m'ont conduit à ne pas porter M. Darwin au premier rang, et il m' a paru qu'elles ont influé sur le vote des autres membres de la majorité de la section."

THE ICE AGE IN SWITZERLAND*

IF one might venture to use the word "romantic" in reference to the history of any past geological period, it would certainly with most fitness be applied to that time, so recent and yet so remote, which we know as the Ice Age. The story of the old glaciers, like that of the living ones, has a perennial interest. We listen to it over and over again without wearying, much as we used to do with some of the standard tales of childhood. For even though we are now familiar with the evidence which proves that, at no very distant date, the northern parts of Europe and America, including nearly, if not the whole, of our own country, lay buried under a vast sheet of ice, the fact is so strange that every fresh presentation of it comes even yet before us with not a little of the charm of novelty. Hence every description of new facts which tends to elucidate the history of the Ice age in any one locality possesses a more than local interest and importance. More particularly is this the case when the description relates to Switzerland. The Swiss glaciers of to-day have become in a manner the common property and fighting-ground of the geologists of all countries; and all fresh observations which bear on the ancient extension of these glaciers are welcomed as additions to the common fund of geological knowledge.

But there is a peculiar interest attaching to the publication the title of which is given below. It is not only a most meritorious contribution to geological literature, showing in great detail the history of glacial phenomena in one of the Swiss cantons, but it may be taken by all local scientific societies as a model of what enthusiasm and industry, well directed, may accomplish. Its origin and growth may be briefly stated. In December of 1867 the Natural History Society of Aargau determined that the year 1869, being the fiftieth of its existence, should be marked by some fitting celebration. It was suggested that, besides the usual festivities, it would be well if the society would engage in some piece of scientific work, so as to present the result at the anniversary; and it was finally resolved to make an extended and detailed survey of the erratic formations of the canton. General attention had been roused to this subject by the Swiss Naturalists' Society in 1867. That body had issued a circular inviting all authorities and private individuals throughout Switzerland to preserve the erratic blocks which were rapidly disappearing before hammer and gunpowder. MM. Favre and Soret, of Geneva, had likewise pointed out the desirability of having an accurate map made of the distribution of erratics in the country. Accordingly communications were entered into with the federal and cantonal authorities, who cheerfully lent their assistance. A circular was sent out inviting all and sundry to give their aid towards the ascertaining and mapping of the erratic blocks in the canton Aargau. In that document were likewise given directions as to the nature of the observations to be made. Each of the fellow-workers was requested to note in a schedule the erratic blocks (or *foundlings*, as they are happily termed in German) of his neighbourhood, with an exact record of the locality of each block, its local name, height above the sea, proprietor, the nature of the rock of which it consists, the legends, if any, connected with it, such human marks as might have been carved on it, and any other remarks that might seem important. To conduct the whole inquiry a reporter or secretary was appointed. His duties were to receive and reduce into connection and order the reports of the various volunteer surveyors; and that he might the better perform this task, he was authorised at the public cost to visit the more remarkable localities, to mark those blocks which he considered it desirable to preserve in the interests of science, and to preserve two type specimens of all the more important blocks, the one specimen to be deposited in the Aargau Museum, the other to be sent to M. Favre at Geneva. The blocks lying on State property were to be considered thenceforth inviolable, and for the conservation of those resting on other lands the secretary was authorised to treat with the proprietors with a view to the acquisition of the blocks by the State. By thus taking advantage of the general movement the Aargau Society not only aided in a work of national interest, but gained an opportunity of celebrating in a singularly excellent way its own anniversary.

The result of the undertaking is now before us in the form of an octavo volume of upwards of 200 pages, with a map showing the distribution of erratics and the extension of the old glaciers over the canton. A more admirable piece of work has not been done in glacial geology for some time. The society deserves the warmest thanks, and more especially the reporter, Herr Mühlberg, who first suggested the task, and on whom the main share of the labour has fallen. The course of each of the ancient glaciers is traced by the lines of "foundlings" which it has left in its path. The evidence furnished by the Canton Aargau as to the retreat of the ice is carefully collected. The limits of the second glaciers are mapped out, and abundant details are furnished as to their moraines. It has long been familiar knowledge that the great glacier of the Rhone, after abutting against the Jura mountains, pushed an immense body of ice eastwards over the lowlands of Switzerland as far as the district round Soleure. But we now learn that rocky *débris* borne along by the huge glacier has been traced even up to the Rhine at the confluence of the Aar. Swelled by the numerous streams of ice which came down from the Bernese Oberland, the united glacier of the Rhine and Rhone must have poured an enormous mass of ice down the Rhine valley, between the Black Forest and the Jura.

The sequence of events in Switzerland since the close of the Tertiary period is thus epitomised by Herr Mühlberg:—

1. Close of the Tertiary period, elevation of the Alps, &c.
2. Erosion of the valleys in the area of the molasse.
3. Renewed elevation of the Jura, formation of the barriers at Aarburg, Wildegg, &c., and consequent lake-basins.
4. First and greatest extension of the glaciers even to the highest crests of the Jura and over the whole canton. Northern limit of the ice unknown; accumulation of bottom moraines (Grundmoräne, *moraines profondes*) upon the "Stoss-seite" of the hills.
5. Retreat of the glaciers, with deposit of earthy rubbish, angular blocks, scratched stones, sand and gravel; filling up of the lakes behind the above-named barriers (save

* Ueber die erratischen Bildungen im Aargau, &c. Ein Beitrag zur Kenntniss der Eiszeit, von F. Mühlberg. (Aarau: H. R. Sauerlander.)

the still-existing lakes over which the retreat of the ice, if it took place at all, was rapid); erosion of the river terraces.

6. New advance of the glaciers to a line drawn from Dagmersellen to Baden; overlaying of the older glacial deposits with fresh erratic materials; formation of large moraines; origin of the small cross-valleys in the molasse hills outside of the ice-covered region.

7. Periodic retreat of the glaciers (with perhaps an occasional renewed advance) until the ice had finally retired from the canton; newer deposits of moraines, &c., and formation of lakes and marshes where the moraines were laid down across valleys.

8. Changes which are still going on.

There is nothing, indeed, which is novel in this synopsis, but it is satisfactory to find that it is borne out by so large an array of evidence as the author has here brought together. On one or two points we could have wished for information. There is no record given of the direction of the striæ on the rocks. No doubt the molasse is not well adapted for the preservation of such markings, though they would remain even on that rock when protected by superficial clayey deposits. We can hardly doubt that if looked for, striated rock surfaces would be found in Aargau as good as those in the adjoining canton of Solothurn. Herr Mühlberg says nothing of any warmer interglacial epoch, as indicated by lignites lying between the deposits of the first and second glacier periods. Is it that no such evidence exists in Aargau, or that the attention of the observers was not specially directed to this subject?

We cordially commend this little volume to the notice of local scientific societies in this country. Such societies often fail of success, either because on the one hand they are too ambitious and seek to emulate the greater societies in such a manner as to ensure certain failure; or, because, though they possess the will and the ability to work, they lack the strength and the enthusiasm which spring out of well-directed and hearty co-operation. Let them take a lesson from the way in which the Aargau Natural History Society has celebrated its fiftieth anniversary. ARCH. GEIKIE

PRIMITIVE MAN

Primitive Man. By Louis Figuier. Revised Translation. 8vo. (London: Chapman and Hall, 1870.)

M. FIGUIER has been singularly fortunate in the mode in which his books have been received by the French public. "Le Monde avant le Deluge" had a large circulation, and even in England the translation revised by Mr. Bristow met with considerable favour. It is almost needless to remark that it reproduces the wild catastrophic doctrines that were given up in England some thirty years ago, with a grace and elegance which lead the *dilettante* reader to believe that he is learning at the feet of a modern Gamaliel. "L'Homme Primitif" takes up the narrative where it was dropped by the preceding work, and tells the story of the early races of men that have lived in Europe, with that vivacity and idealism which is only to be found in perfection among the people for whom it was written. It is undoubtedly true that M. Figuier's works give a faithful outline of the present state of science in France; but it is none the less true that in England catastrophism is practically extinct, and that many of the inferences of French Archæologists are received by English *savants* with a considerable amount of reserve. We, therefore, hold that Science is not really advanced by these books being put before the non-scientific English reader. Of the two books the latter is by far the best, but it is disfigured by many and grave mistakes. There can be no doubt that it will become almost as popular in England as in France, although the elegance of style cannot be preserved in plain straightforward English.

"Primitive Man," the English editor tells us, is intended to "fill an open place in the literature of Prehistoric Archæology," although it covers precisely the same ground as Sir John Lubbock's "Prehistoric Times," and rivals that work in size. If this means that "the Raffaelesque idealism" of the illustrations is intended to find its way to the hearts of a British public that is given to sensational literature, we think that the editor is right. The frontispiece, representing a family of the Stone age, is admirably adapted to arrest the attention of the mother of a family; and the feasting during the Stone and Bronze ages is fitted to strike gourmands, the pictures of the chase, sportsmen; while the cultivation of gardens during the Bronze epoch is a touching scene of rural happiness. A man is hoeing the ground, while a woman is sowing seed, and hard by stands Phyllis with a basket on her head, and leading a favourite goat that is reaching forward to eat a tempting vegetable (also of the Bronze age), while Corydon up an apple tree is shaking its riches into Galatea's lap, and behind are the sheep tended by a shepherdess, and kine and long-snouted pigs are looking on from the palings. All this is extremely pretty, and will doubtless attract many readers. We doubt, however, whether an appetite for archæological knowledge created by such stimulants would be satisfied with the sober logic of the English standard works on the subject. In the letterpress we have failed to detect any fact of high importance which is not to be found in "Prehistoric Times," or in "The Antiquity of Man." The English edition of "Primitive Man" has been corrected and altered from the French original, and therefore may be treated as a purely English work. In his preface the editor is so anxious to give reasons for translating the book, that he seems conscious that he holds a bad brief. We shall proceed to point out some of the more glaring mistakes.

M. Figuier gives an account of the celebrated controversy about the Moulin-Quignon jaw, in which he states that Dr. Falconer, Dr. Carpenter, Professor Busk, and Mr. Christy "unanimously agreed in recognising the correctness of the conclusions arrived at by the indefatigable geologist of Abbeville," viz., that the flint *haches* and the human jaw were of the same Quaternary age as the extinct mammalia found in the same gravel pit. So far from this being the case, Dr. Falconer stated in the *procès-verbaux* that "the character which the jaw presents, taken in connection with the conditions under which it lay, are not consistent with the said jaw being of any very great antiquity," while Prof. Busk wrote that "the internal condition of the bone is wholly irreconcileable with an antiquity equal to that assigned to the deposits in which it was found." Mr. Evans also afterwards came to the conclusion that the suspected flint *hâches* are modern fabrications, and that an ingenious and successful fraud had been practised by the workmen. The editor, however, allows M. Figuier's version to stand, feebly qualified by a note in small print, that "it should rather have been said that the ultimate and well-considered judgment of the English geologists was against the authenticity of the Moulin-Quignon jaw." Why, then, let M. Figuier's error be allowed to mislead the English public? The high antiquity of man is amply proved by the genuine flint *hâches*, without involving the apocryphal Moulin-Quignon jaw, which throws a certain degree of doubt on the whole question.

In 1823 Dr. Buckland advocated the theory of a great diluvial flood which swept over the earth, destroyed the extinct mammalia, and conveyed large blocks of stone far from their parent rock. This he shortly after withdrew. From that time down to the present no attempt has been made in England in any scientific work to account for facts which the author could not understand, by the hypothesis of a deluge. The glaciers and ice-bergs are supposed to have transported the blocks of stone, and the presence of the remains of men and animals in caves is accounted for either by the fact of their having lived and died in them, or by their having been swept in by the action of water. Now, however, M. Figuier brings again the old exploded theory to the front. He speaks of the "cataclysm of the European deluge of the quaternary epoch," as if it were an article of geological faith; "the European diluvial inundation was, as we know, posterior to the glacial epoch, a great catastrophe, the tradition of which is preserved in the memory of all nations, marked in Europe the end of the quaternary epoch." In the first place there is not the slightest proof to be adduced of anything of the kind having ever happened in Europe, and in the second there is proof direct that it did not happen at the end of the so-called quaternary epoch. And the proof direct is furnished by the fact that the quaternary mammalia die out one by one, gradually and not all at once. There is an unbroken sequence of animal life in Europe from the Pliocene to the present day. This old diluvial theory runs more or less throughout the first part of the book, and leads the author very frequently astray. There are also minor points which require correction. The little globular sponges, with a hole in the middle, washed out of the chalk, and found in the gravel at Amiens, which are supposed by Dr. Rigollot to have formed necklaces, are figured as "fossil shells." If the author will refer to D'Orbigny he will find that it is figured as *Coscinopora globularis*. That it is a protozoon and not a shell there can be no doubt. It is indeed hard to understand how the mistake could have occurred, for the arrangement of M. Figuier's woodcuts is the same as that given by Sir Charles Lyell ("Antiquity of Man," p. 119) by whom it is rightly described. Again, why does the editor allow M. Figuier to call Professor Huxley "Hippocrates Huxley," Professor Vogt, "Galen Vogt," and Sir Charles Lyell "Celsus Lyell," because the first says that the Engis skull might have belonged to a philosopher, the second, that it belonged to a degraded savage, and the third offers no opinion whatever? It is a piece of wit too subtle for ordinary minds. The latter is certainly not a Doctor, nor has he written, like Celsus, on agriculture, rhetoric, or military affairs. The only possible point of resemblance between the three couples is that each was at the head of his own particular profession in his own time.

The portion of the book relating to the Neolithic Bronze and Iron ages is much better than the earlier part, but it contains little or nothing of importance that has not been published by Sir John Lubbock. We have done our duty by calling attention to the leading blunders. The book is the work of a remarkably skilful compiler, and is written altogether for effect. W. B. D.

OUR BOOK SHELF

An Elementary Course of Plane Geometry and Mensuration. By Richard Wormell, M.A., B.Sc. Second Edition, revised and enlarged. Fcap. 8vo., pp. 276. (London: T. Murby.)

THIS book is the work of a reformer, not so much of geometry, as of the mode of presenting it to the young. Sciences begin in practical applications, and tend by a universal law to become more and more abstract; and the doctrine of the reforming school is that whatever the science may have developed into, it is necessary in teaching it to go back to practical applications, and to seek for a sure foundation for abstract notions in the familiar experience of common objects. Teachers need to be incessantly reminded of this necessity. In teaching physics or chemistry or botany it is perhaps admitted, though not always obeyed; but in mathematics it is generally not admitted; and when admitted, it is rarely followed out to its logical consequences. Geometry, arithmetic, algebra, must alike be presented first in their applications; and then alone, in most cases, can definition and soundness be given. In most cases, we say, because where mathematical talents of a moderately high order exist, as in the generality of mathematical teachers, this necessity is not felt. And for this reason mathematical teachers who are not also observant of mental phrases, may be slow to believe what has just been pointed out as a necessity in their art. Many of them, we suspect, have a secret sympathy with the mathematician who proposed the health of "The prime numbers, the only branch of mathematics that has not been defiled by contact with the concrete."

The aim of Mr. Wormell's book is to teach scientific geometry, the logical dependence of truths on one another being shown, and to make the teaching sound by giving familiar illustrations of all the conceptions involved, and applications of the result attained. The book is interspersed with examples, geometrical and arithmetical. Among the subjects mentioned—we turn to the index, as giving a good idea of the book—are star polygons, axis of symmetry, graduation, land-surveying, spirals and volutes, transmission of motion by cogs, &c. Considering what the aim of the writer seems to have been, the illustrations and applied part of the book have been well done. It is generally clear, moreover, and accurate in style, and is interesting. In form it is adapted for a school book; and for certain schools it may be a success. But it fails as a scientific geometry intended to replace Euclid, in three respects; want of clear statement of axioms and exhibition of the science as rigidly deductive; the avoidance of the difficulty of incommensurables; and a degree of undefinable inelegance of treatment throughout. The book is too long, and is too nearly scientific, to use as an introduction to Euclid or to any of its modern substitutes; and, though it would replace them with advantage if the mathematical education of the student were to end with the reading of this book, it is not easy to see how the student proceeding to higher mathematics could do so without previously mastering another more complete geometry. It seems to me, therefore, a successful contribution to "technical education," and a valuable and suggestive attempt, but not altogether a successful one, to teach scientific geometry on true principles. The book is well adapted for middle-class schools. It is scarcely worth while to notice minor faults, either of the printer, which are very few, or of style. But it is really to be regretted that a degree should have been defined thus:—"Suppose we have a circumference sufficiently large to be divided easily into 360 equal parts, each part is called a degree." A degree is an angle, and this conception ought to be prominently brought forward.

J. M. WILSON

THE *Sitzungs-Berichte* of the Isis Natural History Society of Dresden for the first three months of the present year contains, as usual, a great number of short communications of more or less interest, and among them a few longer notices. Of the latter we may mention a note on the "Occurrence of Precious Stones in Saxon Switzerland," by M. A. Stelzner; a paper on "Diatomaceæ," by Dr. Eulenstein; a notice by Count Pourtalès on the "American Deep Sea Explorations;" a paper on the "Course of the Boomerang," by Professor Schneider; and, especially, a paper (illustrated) by Dr. Geinitz, on some "Fossil Fruits from the Zechstein and Coal Measures." The society has established a section for prehistoric archæology, the first meeting of which is here recorded; its proceedings consisted chiefly in the delivery of a long opening address by the President, Captain Oscar Schuster.

WHETHER the inhabitants of Rhenish Prussia and Westphalia are at the present moment devoting much of their attention to Natural History may fairly be doubted, but hitherto they have shown great activity in this department, and the publications of their Natural History Society generally contain much valuable matter. We have lately received the volume of their transactions for 1869 (*Verhandlungen des Naturhistorischen Vereins*, vol. xxvi.), in which we find several important papers. Kaltenbach contributes the continuation of his valuable "Natural History of the German Phytophagous Insects," consisting of an alphabetical list of the principal plants growing in Germany, either in a wild or cultivated state, with an account of the insects feeding upon each of them. Dr. C. Schlüter gives descriptions of numerous fossil Echinodermata from North Germany, with good illustrations on three large plates; whilst from Mr. F. Winter we have a contribution to the knowledge of the cryptogamic flora of the Saar district, now the scene of military operations. Another important botanical paper is a contribution to the flora of the Rhine by Dr. P. Wirtgen. In the section of the work denominated the "Correspondenzblatt" we find a note by Dr. Mohr on the "Theory of Coal," and in the "Proceedings of the Natural History and Medical Society of the Lower Rhine," a great number of notices upon scientific subjects of all sorts. This latter part is published separately for the present year. We have received the first number for 1870, including the proceedings during January and February.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

School Natural History Societies

IN your notice of the various School Natural History Societies, you have omitted one, which has been established for more than a year, and is now in a very flourishing state. I allude to the Clifton College Scientific Society. Unfortunately, no one is here to give me any statistics, but I understand that it has seventy members, with about twenty more waiting for admission. Mr. Percival has built a museum at a cost of 1,500*l.*, which will, I believe, be opened next September. T. B. PRESTON

4, Lanesfield Villas, Durdham Down, Bristol
August 1

IN the leading article of NATURE for July 28 (which I have just received), you name some of the chief public schools in which Natural History Societies exist. I am sorry you have omitted to mention Cheltenham College as among that number, and conclude the omission arose from ignorance of the fact. Perhaps it may be interesting to some readers of NATURE to hear that last March, a society consisting of members of that College, was founded under the presidency of the principal (Mr. Jex-Blake) to whom the college is much indebted for the encouragement of Natural Science as a part of the general education of the place.

The Society numbers about fifty members, the department chiefly worked at present being botany, at which several of the boys are becoming tolerably proficient, with a small sprinkling of devotees to geology and zoology.

Our difficulty lies in making meetings sufficiently attractive. Original papers, containing original observations, are scarcely to be expected from the boys themselves till they have been educated to observe; and in our case the number of masters who take an active part in the society is too few to keep interest alive. I cannot, however, doubt, in spite of such difficulties, which have been felt (and in some instances overcome) by others as well as ourselves, that these societies are sowing good seed of which it is not too much to hope the world and science will reap the fruit by-and-by. L. C.

Boscastle, Cornwall, Aug. 10

Our Dublin Correspondent and the Parturition of the Kangaroo

ON my safe delivery, after a good deal of labour, from the perils of war, and on my arrival in London from Germany, I found your letter, enclosing a copy of Dr. J. Barker's communication as printed in your issue of the 14th ult. Dr. Barker has, apparently, no fault to find with my report, which, as a matter of necessity, could not be otherwise than imperfect. But he somewhat loftily criticises the writer of the comments on my report, who, in spite of the facetious title given to him by Dr. Barker, I believe to be a gentleman of considerable merit, and one whose comments on my correspondence appear to be always most just. Dr. Barker is right when he states that the late Earl of Derby's father did not observe the facts about the Kangaroo which he records; these were observed by the keeper of his collection, but they were placed on record by the Earl and hence the mistake. Dr. Barker seems annoyed that he should be made to appear as if he adopted the views, the absolute nonsense, of the writer whose paper he permitted to be read. Those who know Dr. Barker know what absolute nonsense it would be to believe him capable of adopting them. Yet, ought he not, as chairman, to have repudiated and refuted them? Would it not have been well if he had given the members of the learned societies, on the occasion in question, the information which he now offers to the readers of NATURE, and instead of telling them "that the actual passage of the fœtal kangaroo from the uterus to the pouch was not yet proved," he had told them that the fact of there being such an actual passage had long since been proved; though how the actual transit, whether with the help of the mother's paws or lips, takes place, is still regarded as a matter for further observation; and so, instead of appearing to justify the reading of such a paper as the one referred to, he would from the extent of his knowledge on the subject, have reflected credit on his position, and on the societies to which he belongs, and have made, at least, an effort to advance the science he is so zealous for.

August 13 YOUR DUBLIN CORRESPONDENT

The Horse-Chestnut

I SAW to-day in the last number of NATURE, a letter on the meaning of the word horse-chestnut. As I do not see NATURE regularly, I do not know whether any of your correspondents have called your attention to the similar use of Ἵππος in composition. In case this should have escaped them, I send the following extract from Liddell and Scott:—

"ἵππος, vi., in Compos.; it expressed anything *large* or *coarse*, as in our *horse-chestnut*, *horse-laugh*; v. ἱππό-κρημνος,-μάραθρον, -σέλινον, -τυφια, -πορνος; cf. βου-."

Long Ditton, Kingston, Aug. 5 M. W. MOGGRIDGE

The Rotundity of the Earth

WE have seen the statement signed "Parallax," at page 236 of No. 38 of NATURE, and shall be obliged if you will afford us an opportunity of briefly saying in reply, that when we tried the "flag experiment," the person calling himself "Parallax" was not present.

The experiment was conducted in his absence, as he did not come at the time appointed. He did not come at all that we know of; we did not see him.

J. NEWBEGIN
C. W. MILLARD
W. H. DAKIN

Norwich, August 10

Cuckow's Eggs

A SHORT time ago I addressed you on the subject of Cuckow's eggs, giving you some experiences of my own. I now have much pleasure in forwarding to you a portion of a letter on the same subject from an esteemed and observant correspondent, Mrs. Barber, of Highlands, near Graham's Town, to whom I communicated the substance of my letter to you. Mrs. Barber's name is well known in the botanical world as a most accurate scientific observer; of her ornithological acuteness my work on the Birds of South Africa amply testifies, and you may place full confidence in the statements she has made in this communication.

Cape Town, June 1870 ED. LAYARD

"Your remarks on the eggs of the cuckow tribe are very interesting. I confess that I am a believer in natural selection, and Darwinian in my opinions, but nevertheless in this matter I do not see the necessity for the intervention of natural selection; however, I hope you will bear in mind that I am speaking only with regard to the cuckows of my own country (South Africa), and as far as my observation extends, the eggs of these birds bear no resemblance to those of the birds upon which they are parasitic.

"Many of the different species of the cuckows of this country lay white eggs; the whole of those included in the genus *Chalcites* produce white eggs, the birds upon which they are parasitic are the various species of *Fringillidæ*, they do not, however, confine themselves entirely to this tribe.

"I have frequently seen the eggs of the 'Dedric' (*Chalcites auratus*) and the 'Metje' (*C. klaasii*) in the nest of the Cape canary (*Fringilla canicollis*) and the 'Streep Koppie' (*Fringillaria vittata*), where they were conspicuous not only for their pure white unspotted appearance, but for their size also, which is nearly twice that of the Cape canary, and considerably larger than the eggs of the 'Streep Koppie.'

"I have also found the egg of the 'Dedric' in the nest of the green Sun-bird (*Nectarinia famosa*), where it was also much larger than the grey speckled eggs of the sun-bird, and likewise dissimilar from its pure white colour.

"The egg of *Cuculus solitarius* is of a dark mahogany brown, and this egg I have seen in the nest of the wood robin (*Bessonorius phœnicurus*), when its difference was obvious both in size and colour, my son (F. H. Barber) found one of these dark brown eggs in the nest of the Cape canary! and despite its great dissimilarity compared to the small white speckled eggs of that bird, the work of incubation was quietly going on.

"The birds upon which the 'Honey Guides' are parasitic are *Laimodon leucomelas* (vel *L. unidentatus*). I have frequently seen them at the nests of these birds, where great conflicts occasionally take place between the *Indicators* and *Laimodons*, the latter being fully aware that the 'Honey Guide' is an intruder, the egg of the *Laimodon* is speckled, that of the *Indicator* white.

"The 'October bird' (*Oxylophus edolius*) deposits her white eggs in the nest of the large woodpecker; my brother (Bertram Bowker) once met with *three* of the young of this cuckow in the nest of that bird; it is not a common occurrence, I believe, that so many eggs should have been deposited in a single nest; the large woodpecker is, however, equal in size to the 'October bird;' when the birds upon which they are parasitic are smaller, the cuckow deposits but one egg, as the food and space required will in that case be only sufficient for a single individual.

"In the nest of the green sun-bird (*Nectarinia famosa*) I once observed a young 'Dedric,' which nearly filled the nest. It was not quite full fledged, and its frequent calls for food induced the sun-birds (both male and female) to exert themselves to the utmost, and in fact they had to work hard to satisfy the cravings of this greedy intruder; however they did it with a good will, and apparently without any suspicion that they were being imposed upon. Birds in general have no suspicion on this score, they suspect no trickery, and are therefore willing to incubate any kind of egg, provided it is not too large to fill up the nest. I think I told you how I had occasionally changed the eggs of various species of birds from one nest to another, making fearful confusion in consequence, yet the owners of the nests never suspected that anything was wrong, but proceeded quietly with their work. With regard to eggs, the discriminating power of birds is very obtuse, in fact they have none at all, and therefore in this case the agency of natural selection would not come into play; it would not be required. In nature there is no waste, no failure, no useless expenditure of time and ingenuity, every arrangement is sufficiently perfect to work out its own end without being overstrained or overwrought.—M. E. BARBER."

Special Modification of Colour in the Cushat

IN reading the chapter on "Mimicry," in Mr. Wallace's valuable collection of essays lately published, I was struck by a remark there made in regard to the special modification of the colour of the wood-pigeon. It is stated (p. 53), on the authority of Mr. Lester, that "the wood-dove, when perched amongst the branches of its favourite fir, is scarcely discernible, whereas, were it among some lighter foliage, the blue and purple tints in its plumage would far sooner betray it." This description may be accurate enough in regard to *Columba œnas*, but our experience is against its application to *Columba palumbus*. It was a common pastime of our boyhood to stalk the cushats in a mixed wood of the usual Scotch trees, and while familiar enough with their habit of making their nests in the spruce, unquestionably their favourite perches were on beeches and other hard-wood trees. Even after surmounting the somewhat delicate task of approaching the roosting-place of a cushat, it was no easy matter to detect the bird, except by its note, so closely did its general colours blend with the smooth, lichen-covered boughs of the beech, even where no leaves intercepted the view. The bird appears to build its nest especially in the spruce, not because its general colour agrees therewith (which it does not), but because the thick nature of the foliage and branches gives it, the eggs and young, sufficient privacy. Under all other circumstances it prefers to perch on the beech and other hard-wood trees, where its colours so adapt it for concealment. Of course the casual alighting on the pinnacles of the spruce during the breeding season is of little moment in the present question.

W. C. MCINTOSH

Colour Blindness

ALTHOUGH I have no intention of discussing the theory of colour-blindness propounded by Mr. Monck in NATURE of July 28, it may not be inopportune, while the subject is under the notice of your readers, to call their attention to a peculiarity with respect to the perception of colour, of which I have been able to discover no instance.

Some years ago I was sitting in a chapel opposite to a stained glass window, a portion of which (towards my left) was hidden from me by a pillar, and I observed that, as I moved my head to the right, the window flashed out into brilliancy where it had appeared dull before, while the contrary effect was produced as I moved my head to the left. On examining the conditions of the phenomenon carefully I found that it was due to the fact (which I had not the least suspected before) that my right eye is distinctly less sensitive to colour than my left. This I have since verified in various ways, though the difference is not very easily perceived unless the colours are brilliant, as in stained glass, bright coloured flowers, many of Turner's pictures, &c. The difference consists in this, that all colours appear less bright, or, as I should say, *greyer*, when seen with the right eye, and the more delicate gradations of colour cease to be perceived, while in many cases of even strongly contrasted colours, I should find it difficult to distinguish them with certainty with the right eye, especially if I had not previously seen them with the other eye. I have found too that the central part of the retina of my right eye is more defective as to the perception of colour than the lateral portions, since in looking at an extended surface of a single bright colour, scarlet for instance, a kind of shadow appears to come over any part of the surface to which I direct the eye.

With respect to my absolute power of perception of colour, I believe that though I cannot be said to be colour-blind, my eyes are less sensitive to colour than the average of those who have equally good general sight. For instance, scarlet and green do not appear to present to me the same degree of contrast that they do to most persons with whom I have made the comparison. Close at hand the contrast is sufficiently vivid, but a scarlet uniform seen at some distance in a green field would not attract my attention by contrast of colour, though I could make out the difference under a favourable light when my attention had been called to it; so also the scarlet berries of the mountain ash would at a little distance attract my notice rather by their form than by their colour, especially if seen against the sky or a bright object. Again, I can never pronounce with certainty as to the colour of distant bright lights; the colours of the lights, for instance, used for railway signals, though distinctly enough perceived by me when close at hand, puzzle me much when seen at a distance, while I am quite incapable of assigning with certainty a colour to a star or a meteor.

I should add that my ordinary power of vision is good; though here my right eye has a slight, but unmistakeable, advantage as to distinctness over my left. Hence, in looking at a brilliantly-coloured picture I have found that I could appreciate the drawing best with my right eye, the colour with my left, while in using both eyes each appears to remedy the defect of the other.

I think that the facts which I have here stated cannot fail to be of interest to those who are inclined to theorise on the nature of colour-blindness; but apart from all theory it would be satisfactory if the statement of my case should induce others to examine their own perceptions of colour with each eye separately, and in the event of their observing anything confirmatory of, or contrasting with, my observations, to send an account of them to NATURE. I think it quite possible that such cases may not be very uncommon, since the defect is one which may easily escape the notice of the subject of it.

Harrow, August 1 ROBERT B. HAYWARD

The Source of Solar Energy

MR. GREG still misses my meaning. I do believe that meteors supply a portion of the solar energy, and I also believe that they fall in enormous quantities into the sun; what I do not believe is that the whole solar energy is derived from meteors, or that any meteors fall in a solid state upon the sun (whose surface is also certainly not solid, even if any part of his mass be).

Mr. Greg's reasoning only proves what I have already pointed out, that none of the meteor systems our earth encounters can supply a meteoric downfall on the sun. This is, however, so obvious as to need no enforcing.

The reasoning by which I show that enormous quantities of meteors must fall upon the sun is wholly untouched by Mr. Greg's arguments, and is, so far as I can see, simply incontrovertible.

Surely Mr. Greg is not in earnest in saying that there would be a loss of solar energy if a large mass of iron fell on the sun before it was quite melted (any conceivable mass would, by the way, be vaporised), *because the sun would have to melt the portion which remained solid.* That solar energy would be consumed in the process is true enough; but if Mr. Greg supposes that the total solar energy would be diminished, he altogether misapprehends the whole subject he is dealing with. If the action of the olar energy in changing the condition of matter forming (as the imagined meteorite would) part of the sun's substance had to be counted as loss of energy, the sun would be extinguished in a very short time indeed. Such processes involve exchange, not loss.

If the earth could be placed on the sun's surface, the action of the sun in melting and vaporising the earth, and producing the dissociation of all compound bodies in the earth's substance, would involve an enormous expenditure of energy, yet the solar energy, considered as a whole, would be recruited, even apart from the fact that the earth would serve as fuel. The absolute temperature of the sun would, I grant, be diminished in this imaginary case, though quite inappreciably, but his total heat would be increased by whatever heat exists in the earth's substance.

Apart from this, however, if the minimum velocity with which a meteor or other body can reach the sun, is such as would—if wholly applied to heating the body—completely melt it, then the size of the body makes no difference whatever in the result. The meteor might not be melted if enormously large, but in that case the balance of heat would be communicated to the sun. In reality, of course, the heat correspondining to meteoric motion near the sun is very far greater than is here implied.

But I really must apologise for bringing before your readers considerations depending on the most elementary laws of the conservation of energy. RICHARD A. PROCTOR

Müller's Physics and Meteorology

FROM Prof. Jack's Review of Müller's "Physics and Meteorology," in your issue of August 4, I infer that he is not aware that an earlier edition of that valuable book was translated into English more than than twenty years ago, and formed one of the volumes of Baillière's *Scientific Library.* M.A. Cant.

Aug. 7

Colour of Water

MR. E. R. LANKESTER, in his letter in NATURE on 21st July, does not mention what is certainly one of the most remarkable known instances of a decided colour in water, I mean the Blue Lake near the road from Frutigen to Kandersteg in Switzerland. It is very small, not a stone's throw across. I think it is fed by springs. Its blue tint is so decided as to give the idea of some colouring stuff mixed with the water—not that it can be really so.

The Lakes of Neufchâtel and Bienne are of the same light-green tint as those of Lucerne, Brientz, and Thun, although the latter are fed by glacier streams and the former are fed by the streams of the Jura, where there are no glaciers. This appears to prove that the solid matter which glacier streams contain in suspension can have nothing to do with causing the green tint of most of the Swiss lakes. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Aug. 5

Water Analysis

YOUR article entitled "Water Analysis" consists of a review of a book, a commentary on a paper, and the reviewer's opinions of the character of Mr. Chapman and myself.

I shall not trouble you with any rectification of the statements contained either in the review or in the commentary on the paper; inasmuch as both the book and the original memoirs are accessible to the readers of NATURE, and the entire subject has already been very fully discussed.

In giving his opinion on the character of the authors of the book, the reviewer "deplored that two young chemists, with such undoubted abilities as Messrs Wanklyn and Chapman possess, should have rendered themselves notorious by attacking older workers in scientific investigation."

Perhaps you will allow me to say, that in rendering ourselves notorious in this manner, we have committed no crime, and that I cannot see why it should be deplored.

I believe that a great deal of the work which these older workers have done is unsound, and have endeavoured to sweep away some of that which I believe to be unsound. In this sweeping I have been to some extent successful, successful to an uncomfortable and alarming extent, I suppose your reviewer would say. But, if the rottenness of much that passes current in science is appalling, it is surely matter for congratulation that there are young men who will undertake a crusade against it, even at the risk of incurring the disapprobation of the older men, and of suffering every wrong that the possession of place and power enables these older men to inflict.

London, Aug. 14 J. ALFRED WANKLYN

[Mr. Wanklyn omits the sentence following his quotation:—"It is, no doubt, very laudable in a young and ardent investigator, when he points out that high authorities may err, and frequently have erred, but the manner in which these gentlemen have carried out their corrections has made their matter more distasteful."—ED.]

Suckers from the Apple Tree

MOST of the orchards in the west of Herefordshire have had their herbage injured during the present season by the extraordinary profusion of suckers thrown up by the apple-trees. In many places it looks like a miniature plantation, and is a serious detriment to the pasture. In an equal degree the elms have exhibited the same tendency to throw up an infinite number of shoots, and I am curious to know whether I am right in considering it to be due to the great heat of the surface of the soil.

C. J. ROBINSON

A Natural Fernery

NEAR where I am writing, in this parish (East Woodhay) is a deep hollow lane, with high sloping banks, which are abundantly clothed with the following ferns (nomenclature and arrangement from Dr. Hooker's new "Flora"):—*Pteris aquilina*, *Lomaria spicant* (rare), *Asplenium Adiantum-nigrum*, *A. filix-fœmina*, *Scolopendrium vulgare*, *Aspidium aculeatum*, *A. angulare*, *Nephrodium filix-mas* (with several pretty barren varieties), and *Polypodium vulgare*. Although the ferns are of the most common species, yet from the sloping nature of the ground, and the intermixing of a few other plants, such as *Equisetum sylvaticum*, *Lactuca muralis* (very fine), *Digitalis purpurea*, *Campanula Trachelium*, *Hypericum pulchrum*, *Juncus glaucus* and *compressus*, with a few pretty *Rosæ* and *Rubi*, tend to make it the most charming bit of fern scenery that I have ever fallen in with.

HENRY REEKS

The Science and Art Department

IN your impression of the 4th you have touched upon a point which has of late interested me much, viz., the science teaching of the "Science and Art Department."

At the commencement of the article you say that the work done is so little known that you have ventured its history. If you will allow me a small portion of your valuable space, I will put before your readers a few facts concerning the said "Department."

The teachers in the employ of the Department have no fixed salary; payments are made upon results to those persons who have passed in the first or second class (advanced stage) in the subject or subjects in which they give instruction, or who have passed in "Honours."

Now, a teacher's certificate is by no means difficult to obtain (advanced stage), provided Honours are not tried for, and consequently there is a large number of low-class teachers in the ranks, or those who are only grounded in elements of the subject which they are certificated to teach. For instance, a pupil on entering in 1869 for the examination and taking a second class in the advanced stage is entitled to teach and earn payments on results; such a teacher may only be grounded in the elements of the subject (say chemistry), and when he is applied to for the solution of a problem governed by physical laws he is at a loss and the pupil gets no answer to his query. It is my opinion that the teachers' examinations are much too easy. I think a certificate ought not to be granted unless the candidate has shown that he is familiar with the subject he intends to teach, and also with the cognate sciences. A chemistry teacher should know, at least, light and heat, magnetism and electricity; but at present such knowledge is not required.

I am not speaking specially for chemistry; geometry I may instance. There are many teachers at present engaged in giving instruction who know not a jot of Euclid's elements, nor can they work out the simplest algebraical problem. Should not a *teacher* know the theory as well as the practice, so as to give a definite and true answer to a pupil's inquiries. Machine drawing is the same; the examination consists of mere copying, and I can safely say that there are many teachers of mechanical drawing who would not be able to answer a single question of the paper set by the Society of Arts' examiner.

Science-teaching is daily becoming more and more appreciated, in some districts, however, only but very slowly; and I think it is only right that those teachers who do take an interest in science should endeavour to keep up the standard of the teacher. I should like to see every teacher obliged to pass in Honours—then the chemist would not pass without a thorough knowledge of physics, neither would the teacher of plane and solid geometry escape so easily, nor the pseudo-machinist obtain a certificate in a subject he was not competent to teach. But we must look at science as now taught from another point of view. It is for the encouragement of science among artisans (designated in the *Science Directory* as "the industrial classes") that the payments are made to teachers, the teachers not being able to claim any sum upon those which do not *strictly* come under that denomination.

Now, in the branch of chemistry some important rules have lately been made: candidates for the advanced stage are to be taught qualitative analysis, and each is to be supplied with a 2*l*. set of apparatus, the artisan cannot afford to supply it himself; the institution, or other place where the class meets, has only just enough funds to enable it to keep its head above water—who is then to supply it? The teacher; but often he will not see his way clear, for if he supplies it, 50 per cent. only is allowed him if the candidate obtains a first class. The pecuniary result to a teacher then is—for passing a student in the first class 1*l*., and in the second a loss of 1*l*., so therefore it will be advantageous for a teacher to keep his pupils out of the advanced stage altogether. If the authorities continue to let the rule stand, I should think payments would be allowed on middle-class students, as they are the only ones who probably could provide them with the apparatus.

It is all very well to say that an artisan, if he take sufficient interest in the science, *will* provide himself with the apparatus, but it generally happens that the willing ones are those who have the least opportunity of doing what they wish.

The past session has been a trying one for all teachers, for not only have the payments been reduced, but, to make up the list of evils, the standard has been raised, and consequently fewer have passed. The current session seems to be attended by as many drawbacks, which, if not withdrawn or somewhat modified, will, in the opinion of the majority of teachers, produce injurious results.

AN "HONOURS CERTIFICATED SCIENCE TEACHER"

The Intended Engineering College

WILL you allow me a small space in NATURE to call attention to a subject which seems to me to require the serious consideration of everyone who is interested in the progress of science in this country?

In reply to a question put to him in the House of Commons by Mr. Plunket, on the 9th instant, Mr. Grant Duff is reported to have said that it is the intention of Government to establish an engineering college for the Indian service, to be entered by competitive examination. Mr. Grant Duff does not appear to have entered into any detail with regard to the instruction to be given in the intended college, but it is fair to assume that Government would not think it worth while to take the education of engineers for India into its own hands, except with the intention of giving them a thorough and systematic training, at least as complete as what is already supplied by existing institutions. In this case, the instruction to be given in the new college will embrace at least a three years' course of study devoted to pure and applied mathematics, mechanics, physics, chemistry, geology, and the principles of engineering, in addition to the actual practice in the drawing office and workshop. That is to say, the Government school of engineering will be, on this supposition, what the Government School of Mines has become, in the main, a school of pure science.

Now, in view of this probability, the question suggests itself—whether it is fair and just that institutions like University College and King's College in London, and Owens College in Manchester,* which, without Government support or help of any kind, offer precisely the kind of training which we suppose the new college is intended to impart, should have to compete with an institution supported by Government prestige and Government money. The answer to this question affects not merely the interests of private institutions, such as those which have been mentioned, but, so far as the existence of these institutions is a benefit to the general public, it affects the interests of the whole nation; for in the case of colleges which depend for all or much of their income upon the general demand for education, their efficiency and their power to supply teaching of the highest kind, cannot but be seriously interfered with, if they are to be deprived of all share in the training of a class of pupils numerous enough to induce the Government to found a separate college for them alone. The course which Government proposes to adopt would, it seems to me, be justifiable only if the existing colleges were inefficient, and it had a clear prospect of establishing a more successful institution. As I have the honour to be connected with one of the Colleges in question, I do not intend to discuss their merits further than

* The Universities of Glasgow and Edinburgh might also be referred to although they do receive some amount of aid from the public funds.

to say that the quality of the teaching in any institution must depend upon the qualifications of the teachers, and that none of the three Colleges I have named need shrink from a comparison in this respect with any Government school at present in existence.

In answer to these considerations it might perhaps be urged that it is the duty of Government to secure efficient public servants, and that they are not called upon to consider how far private institutions are benefited or injured by the means that appear necessary for this purpose. To this I reply that, even adopting for the sake of argument this point of view, it is the imperative duty of Government not to spend public funds in doing what is already well done by private effort; and that, if anything was required which is not already supplied, public money would be far more productively expended in helping existing institutions to supply the defect, than in founding a new institution which will have to be supported entirely at the national expense. The obviously proper plan is for Government to test the qualifications of candidates for the public service with any degree of strictness they may think proper, but not to burden the country with the expense of educating them.

Perhaps, however, the assumption with which I started may be entirely wrong, and the proposed Engineering College may be intended to supply only a purely professional training. If this be so, there is even less to be said for it than before. In this case it will be merely a "Technical School," attempting to impart the knowledge and experience which can only be really acquired by actual practice under a working engineer.

In any case it seems strange that Government should announce its intention of establishing an Engineering College at the very time when a Royal Commission is making "inquiry with regard to Scientific Instruction, ... and the aid thereto derived from grants voted by Parliament."

University College, London, Aug. 15 G. C. FOSTER

OUR SALAD HERBS

THERE is perhaps no country in the world so rich as England in native materials for salad-making, and none in which ignorance and prejudice have more restricted their employment. At every season of the year the peasant may cull from the field and hedge-row wholesome herbs which would impart a pleasant variety to his monotonous meal, and save his store of potatoes from premature exhaustion; and there can be no question that in hot seasons a judicious admixture of fresh green food is as salutary as it is agreeable. Much has been said lately about the advantage which the labouring man would derive from an accurate acquaintance with the various sorts of fungus, and he has been gravely told that the *Fistulina hepatica* is an admirable substitute for beef-steak, and the *Agaricus gambosus* for the equally unknown veal cutlet. But deep-rooted suspicion is not easily eradicated, and there will always be a certain amount of hazard in dealing with a class of products in which the distinctions between noxious and innocuous are not very clearly marked. There is not this difficulty with regard to salad herbs, and we conceive that the diffusion of a little knowledge as to their properties and value would be an unmixed benefit to our rural population.

The first place must be assigned, on the score of antiquity, to the sorrel plant (*Rumex acetosa*), which in some districts still preserves the name of "green sauce," assigned to it in early times when it formed almost the only dinner vegetable. Its acid is pleasant and wholesome, and more delicate in flavour than that of the wood-sorrel (*Oxalis acetosa*), which, however, is used for table purposes in France and Germany. Chervil (*Anthriscus cerefolium*) is often found in a wild state and is an admirable addition to the salad bowl; and it is unnecessary to enlarge upon the virtues of celery (*Apium graveolens*) when improved by cultivation. John Ray, writing in 1663, says that "The Italians use several herbs for sallets which are not yet, or have not been used lately, but in England, viz., *selleri*, which is nothing else but the sweet smallage; the young shoots whereof, with a little of the head of the root cut off, they eat raw with oil and pepper;" and to this we may add that the alisander (*Smyrnium olusatrum*) is no bad substitute for its better known congener. The dandelion, which in France is blanched for the purpose, affords that *amari aliquid* which the professed salad maker finds in the leaves of the endive, and the same essential ingredient may be supplied by the avens (*Geum urbanum*), the bladder campion (*Silene inflata*), and the tender shoots of the wild hop. Most people are familiar with the properties of the water cress (*Nasturtium officinale*), and the garlic hedge mustard (*Erysimum alliaria*); but it may not be generally known that the common shepherd's purse (*Capsella bursa-pastoris*) and Lady's smock (*Cardamine pratensis*) are pleasant additions, whose merits have long been recognised by our foreign neighbours. In fact there is scarcely a herb that grows which has not some culinary virtue in a French peasant's eyes. Out of the blanched shoots of the wild chicory (*Cichorium intybus*), he forms the well-known *Barbe des Capucins*, and dignifies with the title of *Salade de Chanoine* our own neglected corn salad (*Fedia olitoria*). It would be very easy to extend the dimensions of our list of native salad herbs, for there are, perhaps, some palates to which the strong flavours of the chive (*Allium schœnoprasum*) and stone-crop (*Sedum reflexum*) may commend themselves, but enough has been said to show that Nature has not dealt niggardly with us, and that only knowledge is needful to make the riches she offers available. If the British peasant can be taught to discover hidden virtues in these plants with whose outward forms he has had life-long familiarity, we do not despair of his acquiring the one secret of salad-making, viz., the judicious employment of oil so as to correct the acrid juices of the plants and yet preserve their several flavours unimpaired.

C. J. ROBINSON

TESTIMONIAL TO PROF. MORRIS, F.G.S.

THE presentation of a testimonial to Prof. Morris on July 14, was the occasion of the meeting of nearly one hundred gentlemen, occupying prominent positions in connection with geology and the allied sciences.

Sir Roderick I. Murchison, Bart., K.C.B., occupied the chair, and, in opening the business of the day, expressed the sincere gratification he experienced in having been requested by the subscribers to this Testimonial to act as Chairman on an occasion when it was sought to do honour to Prof. Morris, whom he highly esteemed as a geologist, and whom he loved as a friend. He then gave a sketch of the career of Prof. Morris as a geologist, showing that in his earlier researches he was among the first to make most valuable communications upon the structure and fossil contents of the Tertiary formation of the South and East of England; and how he next threw much new light upon various members of the Oolitic formation and the Lias, describing the fossil contents with great acumen and ability. These were followed by his *opus magnum*, the "Catalogue of all British Fossils," which had gone through two editions, and was a work which would for ever hold a high place in science, as a truthful record of the succession of all classes of known animals from the earliest traces of life to those of the youngest Tertiary formations connecting ancient with existing nature.

The address was then presented, as follows:—

To John Morris, Esq., F.G.S., Professor of Geology in University College, London.

We, the undersigned, Friends and Cultivators of Geology, taking into consideration the degree in which this science has been advanced by your long and successful labours, are desirous of offering to you a Testimonial of the high estimation in which they are held. Always working in the field, and classifying public and private collections, you have for many years been among the foremost and most diligent of the Students of

Geology and Palæontology; your careful publications have their places among those of the Masters in every geological library; and your thoughtful lectures live, and will live, in the minds of your scholars. We shall be happy if by this Testimonial you may be encouraged to persevere in a course alike honourable to yourself and advantageous to the public, and that it may be pleasant for you in future years to remember that among the names to be submitted to you are many of those who have known and shared and been instructed by your labours.

"*Forsan et hæc olim meminisse juvabit.*"

Mr. Prestwich said: I have known Prof. Morris now some 35 years, and I can truly say that a more earnest geologist, and one more ready to impart to others the great and varied information he himself possesses, there cannot exist. His aim has ever been to spread a knowledge of that science which he has cultivated with so much pleasure and so much success. Often, too, regardless of other considerations, his first object has always been the free, and, if possible, the gratuitous diffusion of geological science—possibly too much so. Still, if he has failed often to secure his reward in £ *s. d.*, he has always secured the respect, esteem, and affection of his friends, and of all with whom he has worked. With most men the prospect of a good fee would be an incitation to work, but with my good friend Morris the surest way, I believe, to get him to work was too often to say that no fee or remuneration would attach to it. His first introduction years ago was by a valued mutual friend, Mr. Lonsdale, who suggested that a work on the Tertiary Geology of the London Basin was needed, and he thought that Prof. Morris might undertake the palæontological and I the stratigraphical part. I am inclined to believe that Mr. Morris must have considered the work as likely possibly to be remunerative, as, though taken up warmly at the time, the first chapter still remains unwritten. This testimonial is, I feel, but a small earnest of our feelings towards Prof. Morris. For the opportunity thus afforded of expressing our good wishes towards our friend, we have to thank the *Mining Journal* for the publicity it has given to the matter, and for an active part in the management we are, I believe, indebted to Mr. Hearn, whilst Mr. Milnes has kindly acted as treasurer for the fund, which for Prof. Morris's sake I only wish had been doubled.

The meeting was then addressed by Mr. Hearn, and Prof. Tennant, and a letter was read from the venerable Prof. Sedgwick; and Prof. Morris made an eloquent reply, in the course of which he said: It is with deep and sincere feeling that I thank you and the members of the Geological Society, and other friends, for the handsome testimonial which I have received at your hands this day. I not only thank you for the kind manner with which you have expressed yourself, but I recall with pleasure the encouragement to pursue my labours you gave me more than 25 years since from that chair when President of the society, in 1842, and still more so when in after years your kind suggestion induced me to become a candidate for, and your strong recommendation placed me in, the position I now hold at University College, the duties of which, during the last 15 years, I have earnestly endeavoured to fulfil, so that I trust you have had no cause to regret the confidence you then placed in me.

Prof. Sedgwick's letter was as follows:—

"The infirmities of old age make it impossible for me to attend the public meeting at the rooms of the Geological Society. This is to me a great sorrow, for no one can value Prof. Morris's most laborious and most useful palæontological works at a higher price then I have done. And I honour him not merely as a man of science but also as a kind personal friend, who has for many years taken his place in the first rank of practical English geologists. Pray present to him my heartfelt congratulations, which the load of 85 years, and a great infirmity of sight and hearing, will prevent me from offering personally."

The meeting then separated.

WHEAT RUST AND BERBERRY RUST

THE theory has long been prevalent among practical agriculturists that the proximity of berberry trees produces rust in wheat. Men of science, unable to trace herein the sequence of cause and effect, long derided the idea, and placed it among the prejudices of the agricultural mind. The facts of the farmer have, however, been too strong for the science of the botanist, and experience has won the day over theory. Let us trace for a moment the history of the inquiry. The first reference to the injurious influence of the berberry on corn appears in Krunitz's Encyclopædia, published in 1774. Marshall, in 1781, speaks of the berberry having been extirpated in Norfolk for this reason, and Schöpf, in 1788, mentions the same idea as prevalent in New England. Other writers of the same period give similar testimony; and in 1806 Sir Joseph Banks writes thus in the *Annals of Botany*:—"It has long been admitted by farmers, though scarcely credited by botanists, that wheat in the neighbourhood of a berberry bush seldom escapes the blight. The village of Rollesby, in Norfolk, where berberries abound, and wheat seldom succeeds, is called by the opprobrious appellation of 'Mildew-Rollesby.' Some observing men have lately attributed this very perplexing effect to the farina (pollen) of the flowers of the berberry, which is in truth yellow, resembling in some degree the appearance of the rust, or what is presumed to be the blight in its early state. It is, however, notorious to all botanical observers that the leaves of the berberry are very subject to the attacks of a yellow parasitic fungus, larger, but otherwise much resembling the rust in corn. Is it not more than possible that the parasitic fungus of the berberry and that of wheat are of the same species, and that the seed is transferred from the berberry to the corn?" The acute suggestion thrown out by Sir Joseph Banks, at a time when so little was accurately known of the structure of fungi, was not followed out for half a century; it was reserved for the German fungologist, De Bary, within the last few years to establish the truth of his theory, and to prove the existence of the

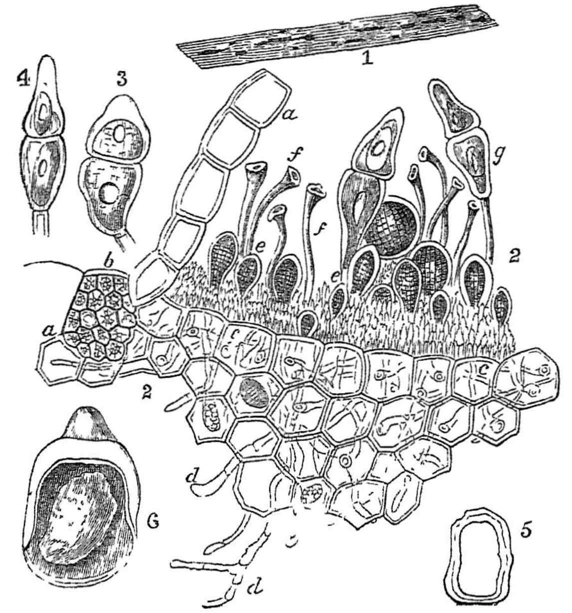

FIG. A.—*Puccinia graminis*, Pers. 1. Leaf of grass, with mildew, natural size. 2. Section of the leaf, with a patch of mildew and rust: *a*, epidermis of the leaf; *b*, bast-cell nerves; *c c*, outer layer of cells of the leaf, on which the parasite rests; *d d*, mycelium; *e e*, young and old spores of *Cæoma lineare*; *f f*, stalks from which the spores have fallen off; *g*, spores of the *Puccinia*. 3, 4. Spores. 5. Section of the wall of the lower spore-cell. 6. Longitudinal section of the upper spore-cell with the spore-nucleus. (2–6 magnified.)

phenomenon of Alternation of Generation among Fungi. The researches of Steenstrup and others have made us familiar with this remarkable phenomenon among the lower forms of animal life, but had hardly prepared us to meet with it in the vegetable kingdom. It appears probable, however, that the phenomenon is by no means uncommon here also,—affording another instance of the law that it is in their lowest forms that the animal and vegetable kingdoms approach one another most nearly,—and that whole tribes of fungi hitherto considered distinct are but different phases of one another. This remark applies especially to the two genera of minute parasitic fungi, *Æcidium* and *Puccinia*, to which the rusts in question belong, both belonging to the family *Uredineæ*. The well-known orange-red spots so common on the leaves of the berberry are produced by the *Æcidium berberidis*, while the rust of wheat and other cereal crops, but found equally on some other species of grass, as the common couch-grass or *Triticum repens*, is the *Puccinia graminis*. In the volume for 1865 of the *Monatsberichte der kön. preuss. Akademie der Wissenschaften zu Berlin* is a paper by Dr. De Bary, giving an elaborate account of his experiments on the propagation of these two fungi, in which, if his experiments are reliable, he clearly proves the correctness of Sir Joseph Banks's suggestion that they are one and the same species. The experiment was tried, with due precautions, of inoculating the leaves of the berberry with the spores of the *Puccinia*, the result being the production, not of the same fungus, but of the *Æcidium*, while the sowing of the spores of this latter fungus on the leaves of couch or wheat produced conversely the *Puccinia*. By sowing the spores of either fungus on the plant on which it was itself parasitic, he failed altogether to reproduce the same plant; and this alternation of generation may serve to account for the fact which has often been noticed, that rust is apt to appear not in successive but in alternate years on the same crop.

It is unfortunate to find that in a work bearing a considerable amount of scientific authority among agriculturists, and published in the same year, 1865, Prof. Buckman's "Science and Practice of Farm-cultivation,' the theory which thus appears to have been proved on the Continent was scouted in the following terms: "*Æcidium berberidis* is here referred to, from an opinion prevailing

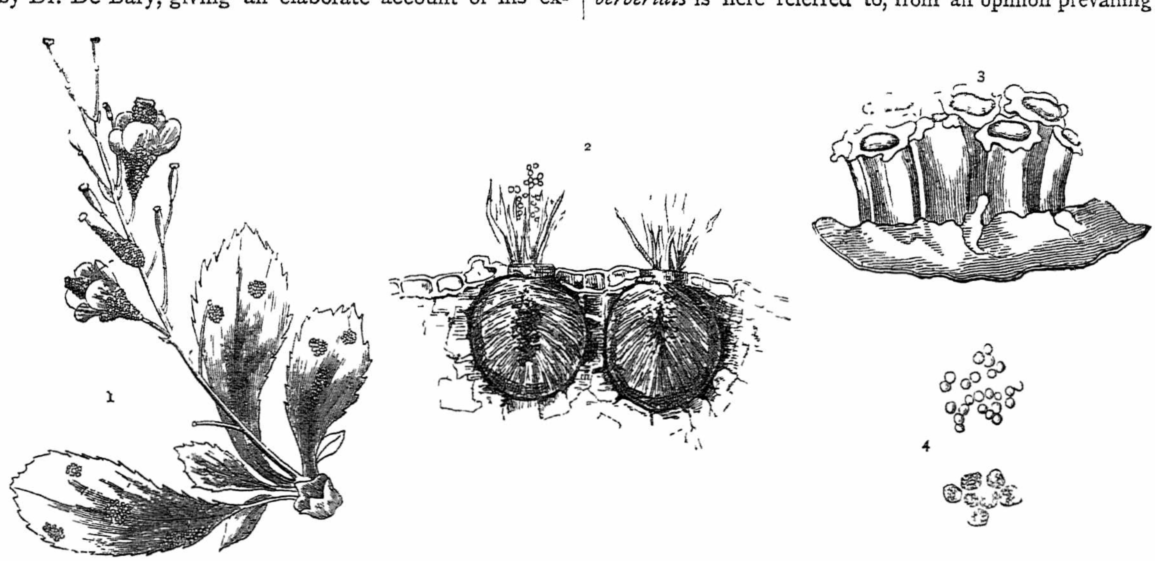

FIG. B.—*Æcidium berberidis*, Gmel. 1. Branch of berberry with spots of rust, natural size. 2. Spermagonia. 3. A group of Peridia with their orifices dentated. 4. Sporidia. (2, 3, and 4 magnified.)

that it is the cause of rust or mildew in wheat. We can no more believe that the berberry rust would produce rust in wheat than the rust of any other plant would do so. Still that wheat growing under a berberry hedge may be more blighted than in the rest of the field is quite true, and so it is with wheat growing under any kind of hedge." Mr. Buckman fails entirely to grasp the argument, which is not that wheat "growing under a berberry hedge" is attacked by rust, but when growing in the proximity of a berberry tree, say at the distance of a field's breadth. Nothing is more certain to weaken the hold of science over practical men than when men of science, in order to support their own theories, set themselves systematically to deny well-known facts. We therefore greatly regret the decision at which it is understood the council of our Royal Agricultural Society has arrived, to refuse a thorough investigation of the subject which has been urged upon them, calling in the assistance of experienced men and the most able fungologists of the day. This is not the way to command the confidence of practical farmers.

We commend to the consideration of the Royal Agricultural Society the conduct of a railway company in the south of France, described in the *Bulletin de la Société botanique de France* for January of this year, to which we have already alluded (see NATURE, vol. i., p. 516). In the commune of Genlis, department of Côte-d'-Or, a berberry hedge was not long since planted on one of the railway embankments; when immediately the crops of wheat, rye, and barley in the neighbourhood became infested with rust. The complaints of the farmers caused the appointment by the company of a commission to investigate the subject, who reported, after a full inquiry, that wherever the berberry was planted the cereals were more or less attacked by rust; where they were absent the crops were free from the disease; and that the planting of a single berberry bush was sufficient to produce the rust where it had never appeared before. The railway company's own commission held that compensation was due from the company to the farmers.

Our illustration of the *Æcidium berberidis* is taken (in part) from Greville's "Scotch Cryptogamic Flora;" that of the *Puccinia graminis* from Corda's "Icones Fungorum."

ALFRED W. BENNETT

In considering the question of the influence of the berberry on the production of rust in wheat, assuming that De Bary's observations are perfectly correct, it is necessary to consider the nature of what is commonly called "rust" in cereals. Presuming that his views are strongly confirmed by the analogous connection of *Rœstelia cancellata*, the pear blight, with the gelatinous parasite of *Juniperus sabina*, it is well to attend to the following facts:—Professor Henslow in an article on the diseases of wheat, in the Journal of the Royal Agricultural Society, proved distinctly that what is commonly called rust is merely a condition of the common mildew, and this at a time when comparatively little was known about these parasites, and when many were inclined to accept the views of Unger that they were mere abnormal developments of tissue or spontaneous growths. The observations of Tulasne and others confirmed to a certain extent Professor Henslow's view, but threw further light upon the matter by showing that many so-called *Uredos* were merely a subsidiary form of so many species of *Puccinia*. Meanwhile, though *Uredo rubigo vera* was the subsidiary form of *Puccinia graminis*, it was recognised that *Uredo linearis* is nothing more than the early stage of the *Puccinia*. Though there is some resemblance between the Uredinoid form of the *Puccinia* and the rust of the berberry, there is none between the perfect condition of the parasite. Our readers will have noticed that at the meeting of the French Academy on August 1st, M. Roze contributed some further illustrations of this interesting subject.

The great difficulty has always been that mildew is most prevalent in countries where not a berberry bush is to be found; and the same remark applies to the pear rust, which abounds where not a single plant of savine is to be seen, the parasite of the savine being comparatively of rare occurrence. I think, always assuming the fact of the connection between the two parasites, that it may be easily accounted for. It may be true that the berberry plant produces mildew; but how is this? not probably from the spores of the present year, but from those which fell to the ground the previous season. There is no doubt that these parasites penetrate into the tissues of the young germinating plants, by means not of the original spores, but of minute secondary spores which are produced on them, a circumstance which is fully proved in the case of bunt. This, then, will account for the cereal being mildewed in the neighbourhood of the berberry. But another consideration is necessary to explain the prevalence of mildew where the berberry does not exist, or where it is confined to gardens. The subsidiary spores have no doubt, equally with the *Puccinia* itself, the property of reproducing the mildew, and there are always enough of these blown about, either from previous crops or from the neighbouring grasses, especially in the fens, where every ditch is filled with reeds affected more or less with mildew; and thus the parasite may be propagated season after season without the Æcidioid form intervening, a circumstance which is not without analogy in other branches of the vegetable kingdom. I may be allowed, perhaps, to recall attention to an article on the development of bunt in the second volume of the Journal of the Horticultural Society of London, which seems entirely to have escaped notice on the Continent, where it is stated in a paper communicated by me on Jan. 18, 1847, with reference to the phenomena described, that "it is quite possible that in plants as well as in the lower animals there may be an alternation of generations." M. J. BERKELEY

NOTES

A RUMOUR is current that the Government have refused both ships and assistance to the Royal and Royal Astronomical Societies, which have been for some time organising expeditions to observe the approaching total eclipse of the sun. We can hardly believe that the Government will thus venture to brave the opinion of all men of science and culture. It would be a direct acknowledgment that the Government cares as little for a recent position for England in science and the arts of peace as it did a little time ago for her position in the arts of war. Verily we are a nation of Philistines!

OUR readers will hear with great regret that Prof. Wyville Thomson is prevented by illness from taking that share in the scientific exploration of the Mediterranean basin, now about to commence, which has conduced so greatly to the success of the previous expeditions in which he has been one of the workers.

M. OTTO STRUVE, director of the Observatory at Poulkova, M. Wild, director of the Physical Observatory at St. Petersburg, and M. Mohn, director of the Meteorological Institute of Christiania, have just arrived in Paris, for the purpose of taking part in the international conference charged with establishing a universal metre. In consequence, however, of the war, the meeting of the conference is postponed until such time as it may be summoned to meet by the Government.

AMONG the chances of war which have necessitated that Paris should be placed in a state of defence against a besieging enemy, the rasing of the Bois de Boulogne has become one of the first necessary operations. The fine collection of animals belonging to the Société Impériale d'Acclimatation will then have to share the fate of those belonging to the Zoological Society of Cologne, and be dispersed or removed till better times. It is even said that the axe is already at work.

THE Franco-German War is telling heavily on science on the Continent. In the number of the *Revue des Cours Scientifiques* for August 13th, the Editor hints at the possible suspension at an early date of the publication of his journal till the war is over.

To form an idea of the results of a general armament in Germany, it will be sufficient to learn that from the Berlin Chemical Laboratory, besides a great number of students, all the assistants, seven in number, have joined the army, partly as soldiers partly as field-apothecaries. When large masses of troops passed through Berlin, the director of the laboratory placed room for twenty soldiers at the disposal of the military authorities. University lectures have been prematurely closed. The military schools, the agricultural school, and the school of architecture had to close for want of pupils. The upper forms of the grammar schools have also sent many of their pupils into the field; one of them as many as eighteen out of forty.

THE engineering works of Messrs. Siemens and Halscke, and the ironfoundry of Borsig, are now almost exclusively occupied with the manufactory of torpedoes. It is said that great improvements have been made in these war-engines; the point chiefly kept in view being to make them moveable from the shore by means of an electro-magnetical rotatory apparatus.

THE season for Congresses Scientific and other has now fairly set in. A Medical and an Engineering Congress are now sitting in the north; we last week gave some of the arrangements for the forthcoming meeting of the British Association at Liverpool, and it is now announced that the Social Science Congress will meet from the 21st to the 28th of September, at Newcastle-on-Tyne, under the presidency of the Duke of Northumberland. The Social Science Congress has done what the British Association might also do to a certain extent with great advantage. It has stated the questions which press most for solution in the different branches of inquiry with which it deals. With two of these sections, namely, those of Education and Health, we are especially interested, and we willingly acknowledge the high importance of the questions which it is proposed to discuss. They are as follows:—Education. 1. Can better educational results in primary schools be obtained by the amalgamation of such schools? 2. By what means can a direct connection be established between the elementary and secondary schools and the Universities? 3. Is it desirable to teach science in elementary schools, and, if so, what branches of science? 1. Health. What is the best method of disposing of sewage and excreta? 2. What modifications are desirable in the existing sanitary laws and administration? 3. What legislative measures ought to be taken to prevent the adulteration of food, drink, and drugs?

THE British Medical Association will meet next year at Plymouth, under the presidency of Mr. Whipple, consulting surgeon to the Plymouth Infirmary.

WE have received the report of the Scottish Astronomer Royal to the Board of Visitors read at their visitation on the 29th of June. The report, in Mr. Piazzi Smyth's most curious style, refers to so many additional "facilitations" in the observatory that we may soon hope for some work to be done there, and to so many questions which have nothing to do with the observatory that we may possibly return to it.

AT the London Institution, Finsbury Circus, on the 10th inst. Mr. J. P. Gassiot, D.C.L., F.R.S., distributed the prizes awarded, and certificates granted to students who have passed examinations on courses of Educational Lectures delivered during the past session. In the examination connected with Prof. Guthrie's course on Elementary Physics, Edmund Strode gained the first prize, while T. Lyon, Miss Esther Greatbatch, and Miss Annie Piper obtained prizes of the second order. In the examination on Professor Bloxam's chemical lectures, A. J. Richardson obtained the first prize, Isidore Harris and Miss E. M. Hutton taking second prizes. In the examination on Prof. Bentley's botanical course, Miss Emma Ball took the first prize; and A. J. Wallis and Miss Ellen Benham second prizes. Mr. Gassiot stated that the Educational Lectures of the London Institution were commenced in the spring of 1869, by Prof. Huxley, and that the attendance of students from the leading metropolitan schools had fully justified the belief of the managers that increased facilities for scientific education were needed. In distributing the prizes, which consisted of standard scientific books, handsomely bound, Mr. Gassiot referred to the early days of some of the eminent authors and discoverers who had been connected with the London Institution. He stated that arrangements for developing the educational powers of the institution were in progress, and delighted his audience by the announcement that Professors Odling and Huxley would deliver courses of lectures early in the coming session.

WE have had from time to time to record evidences of the increased interest in science felt in the United States. We have just received the first number of the *American Scientific Monthly*, published at Iowa City, and edited by Prof. Gustavus Hinrichs. It contains articles of a popular rather than a deeply scientific character, on various branches of science, chemistry, geology, physics, physiology, &c.

THE Sais subscription fund has now been closed, the total amount received being 12,283 francs.

M. DAVAUNE states, in a recent number of *Cosmos*, that the proposal to raise a subscription on behalf of M. Niepce de St. Victor has met with a warm and hearty response.

REFERRING to the note in our issue of July 28, L'Abbé Moigno makes the following reply in *Les Mondes*:—"Le journal NATURE a émaillé de points d'exclamations notre petit article sur les silex du Sinai. Il s'étonne que nous nous soyons fait fort de prouver jusqu'à l'évidence que ces silex sont plus vieux que ceux des grottes d'Aurignac et autres. M. Louis Buchner, dont la NATURE devrait bien reproduire les propres paroles, l'a dit avant nous. Il s'étonne plus encore de notre confiance dans le progrès, et rappelle de notre appel à M. Sorby:—attendons; le spectroscope jugera entre nous. En tout cas, c'est déjà quelque chose que des silex ou couteaux en pierre, vieux de 3,350 ans."

MR. ERNEST HART has been unanimously elected Editor of the *British Medical Journal*, in the place of Mr. Jonathan Hutchinson.

THE death is announced, at the age of 75, of Baron Charles von Hügel, one of the founders of the Horticultural Society at Vienna, and a collector, on a large scale, of new Australian plants. He was the author of several botanical works, and held the office of Austrian plenipotentiary at the court of Brussels.

IT is hoped that the Cornell University will be able to form the nucleus of a museum, as Professor C. Fred. Hartt, with a professor of botany and nine students, intends exploring a portion of the valley of the Amazon and the Brazilian coast southward to Bahia, and to collect objects in natural history and geology. Professor Hartt proposes to take stereoscopic views of interest along his route.

THE Royal Academy of Sciences of Belgium offers a prize of 800 francs for an essay on the Affinities of *Lycopodiaceæ*. The essays should be written in Latin, French, or Flemish, and forwarded to M. Ad. Quetelet, the secretary of the Academy, at Brussels, before June 1, 1871.

THE practical examination of workmen and students for the Whitworth Scholarships has been fixed by Sir Joseph Whitworth to take place at his works at Manchester, on the 30th August and 1st September next.

THE *Engineer* gives a plan and section of a proposed tunnel under the Bosphorus, planned by Mr. J. Haddan, one of the chief engineers to the Turkish Government. Unlike Messrs. Bateman and Révy's proposed Channel railway, the idea is to suspend the tunnel at about thirty-five feet below the surface of the water, and fix it to the bottom by means of holding chains.

MR. GEORGE FAWCUS, of North Shields, has contrived an equilateral triangular drawing-board for isometrical drawing. An ordinary T square applied on the edges of an equilateral triangle draws tangents that meet each other at angles of 120°, and other lines drawn parallel to these radiating ones form with them angles of 60° and 120°, which are the exact angles of the apparent squares of isometrical cubes. The inventor believes that the use of this new drawing-board will make the teaching of isometrical drawing both simple and easy. The practice of isometrical drawing is strongly urged in the science and art drawing classes.

A REMEDY has been found for the "borer" that ravages Indian and Ceylon coffee plantations, by applying carbolic acid before the eggs are hatched.

THE curator of the Botanical Exchange Club has just issued his Report for the year 1869. Dr. Boswell-Syme takes the opportunity of recording all the observations which have come under his notice respecting new forms or varieties of British plants, or new localities of the rarer species.

PROF. CHANDLER, chemist to the Metropolitan Board of Health, New York, gives, in the *American Scientific Monthly*, his analysis of fifteen different kinds of fashionable hair-tonics, restoratives, &c., in all of which he finds lead varying in amount from one-ninth of a grain to sixteen grains in the ounce. He states that they owe their action to this metal, and are, consequently, highly dangerous to the health of persons using them. Lotions for the skin he found to be free from lead or other injurious metals, with the exception of one containing corrosive sublimate. The enamels examined consisted of either carbonate of lime, oxide of zinc, or carbonate of lead, suspended in water. The latter kind are highly dangerous; the two former are "as harmless as any other white dirt when plastered over the skin to close the pores and prevent its healthy action." The white powders for the skin are harmless to the same extent.

THE *Western Monthly* (Chicago), in its August number, has a very readable article on Sun Spots and their lessons, in which the author discusses the consequences of the obscuration of one part in one hundred and thirty of the sun's visible surface in the present year, and thinks that he is about to open one of the sealed volumes which contains the principles of prognostic meteorology.

THE "Proceedings of the Birmingham Natural History Society and Microscopical Society" for 1869, is another of those records of natural history researches in the provinces, of which we have so many gratifying instances, the more valuable when coming from the centre of a manufacturing district, where the thoughts of men are naturally turned in such very different channels. We have papers of various lengths on mineralogy, botany, microscopy, physiology, geology, entomology, malacology, and other branches of natural history, showing, if not much originality, much careful observant work. Appended are preliminary lists of the flowering plants, mosses, Lepidoptera, and Mollusca found within a radius of ten miles from Birmingham, a by no means inconsiderable array.

THE astronomical and meteorological observations of the National Observatory of Santiago, in Chile, have now been regularly published since 1853, chiefly by Don José Tomas Vergara. They also include the Valparaiso meteorological observations.

CINCHONA culture has now so far advanced in Madras that the Government is preparing to deal with it as an annual crop.

MR. WORTHINGTON SMITH records in the *Journal of Botany* an instance of a fatal case of poisoning by eating the root of the Water-dropwort, *Œnanthe crocata*, an umbelliferous plant common in ditches and wet places. A carter at Staplehurst, in Kent, ate some of the roots whilst at work, supposing them to be the wild parsnip; in about an hour he became unconscious and convulsed, and death occurred in another half-hour, before medical aid could be obtained. The man had fed his horse with roots of the same plant, and the animal also expired about two hours after eating them. There is no doubt that the *Œnanthe* is a virulent poison, but it seems strange that the horse, as well as the man, should not have rejected a plant of so acrid and suspicious a flavour. Several wild Umbelliferæ are among the most dangerous of British plants, and it is probable that the Greek poison κώνειον was obtained from others besides the hemlock.

SHARKS appear to have recently again made their appearance in the Gulf of Trieste, and the police have issued a notice forbidding people to bathe in the port or on the coast. For each fish destroyed in the waters between Punto Grona and the castle of Duino a reward of 300 florins (about 30*l.*) is given.

THE Indian Government has selected the Khond Hills for cinchona experiments. If they succeed the cultivation will be thrown open to private enterprise, with the view of further promoting employment and cultivation among the Khonds.

THE extent of the Wurdah coal-field in India has been confirmed, and the seams in Berar have been found to be 45 feet in total thickness.

THE total eclipse of July was observed at Constantinople by the Rev. C. Gribble, H.B.M. Chaplain, formerly of the Royal Navy, a local astronomical observer. He contributes an account to the *Levant Herald* of July 20. The dogs of Constantinople continued barking until about the middle of the eclipse.

WE have had occasion to refer to wild-beast legislation and administration, an important matter in India. A curious discussion has arisen in Bombay. Tigers having come to Salsette and killed several people, the magistrates applied to increase the reward, but the Government have refused, thinking that the report of the presence of tigers there will attract English sportsmen from Bombay.

"A CATALOGUE of Maps of the British Possessions in India and other parts of Asia," published by order of Her Majesty's Secretary of State for India in Council, is a very useful publication for those interested in our Asiatic possessions.

THE first and second quarterly publications for 1870 are issued of Auwers' and Winnecke's "Vierteljahrsschrift der Astronomischen Gesellschaft."

DR. SACHS'S "Lehrbuch der Botanik nach dem gegenwärtigen Stand der Wissenschaft" has reached a second enlarged and partly rewritten edition. It is illustrated with 453 woodcuts.

DR. KARL KARMARSCH'S "Technological Dictionary, English, French, German," of which the second edition is just issued, is a most useful compilation, containing the corresponding terms in these three languages employed in Architecture, civil, military, and naval; Civil Engineering, including bridge building, road and railway making; Mechanics, machine and engine making; Ship Building and Navigation; Metallurgy, mining and smelting; Artillery; Mathematics; Physics; Chemistry; Mineralogy; and generally in the Arts and Sciences.

DR. ENGELMANN publishes the first part of his "Results of Observations in the Leipzig Observatory," comprising those made with the meridian-circle.

M. VIDAL'S statement (says the *Photographic News*), that a sensitive plate, if exposed to light in the camera, and then placed behind the yellow glass window of a dark room, becomes attacked by the yellow rays and yields a fogged image, whereas a sensitive plate previously unexposed to light is not affected in the same manner, has been confirmed by results obtained in America, and detailed in the *Philadelphia Photographer*.

PAPERS ON IRON AND STEEL

I.—A VERY COSTLY AND VEXATIOUS FALLACY

I.

"A FRIEND of mine has been converting some common cinder pig-iron into either very fine iron or steel by a very simple process, but does not know who to apply to to learn its value. He is willing to share the profit with anyone who will help him in the matter. I have some small samples of it if you would like to see it, or can tell me who would be likely to interest themselves in the matter. From what I can make out I should think it would make good steel, for it will harden and temper now."

The above, quoted from a letter I have recently received, is a typical sample of a number of others I have had at different times, and it represents the labours of quite a multitude of patient, long-suffering, and miserably deluded investigators. The published specifications of abandoned patents make painful record of wasted money, time, and ingenuity; and suggest dark tragedies of ruined hopes, all arising from the same misunderstanding of the changes which take place in the conversion of *ordinary* pig-iron or cast-iron into *merchantable* steel.

The most humiliating feature of this delusion is that it is not the offspring of popular ignorance, is not prevalent among the beer-drinking class of iron-workers, who sign their names with a ×, but crops out among intelligent self-taught men, who have studied the chemistry of iron and steel as expounded in recognised chemical books. The costly fallacy I allude to is directly traceable to the teachings of our highest scientific authorities. As NATURE is now largely circulating among the class of self-taught and energetic men who supply this ever-recurring crop of victims, and also among those who most unwittingly and unwillingly have deceived them, there can be no better medium through which to effect the demolition of this mischievous error.

By reference to almost any text-book on chemistry, it will be found that cast-iron is described as a compound or mixture of iron and carbon; that steel is another compound or mixture of iron and carbon, but with a less proportion of carbon; and that wrought iron is nearly free from carbon. Further, we are told that the ordinary method of making steel is, first to remove all the carbon from the cast or pig-iron by making it into wrought or bar-iron, and that this bar-iron is afterwards converted into steel by causing it to take up a new dose of carbon in the cementing furnace. The natural inference of a thinking reader is, that this is a clumsy complication, especially if he knows that the process of cementation is slow and costly, that on account of the irregular diffusion of the carbon in the blistered bars, other expensive processes of shearing, tilting, casting, &c., have to follow. Why not at once produce the steel from cast-iron by a process of decarburisation which shall stop at the right point, *i.e.*, when the 3 or 4 per cent. of carbon of the cast-iron is reduced to the one or one and a half per cent. required to produce steel? By doing this, not only the cost of converting wrought-iron into steel, but also the cost of puddling to produce wrought-iron will be saved; and steel, which is but a carburet of iron intermediate between cast and wrought-iron, instead of being so much dearer than either, should be made at an intermediate cost, or cheaper than wrought-iron.

If he dips further into the literature of the subject, and reads the history of the manufacture of iron, he will find further confirmation of such reasoning, as he will learn thereby that the direct production of steel is an ancient art, and that weapons of renowned quality were made from steel thus produced.

By reference to one of the most recent and elaborate English treatises on the subject, Dr. Percy's "Metallurgy," he will find on page 778 that this is described as "the ancient method, which is still extensively practised on the Continent, especially in Styria;" and further down on the same page that "if steel be regarded simply as iron carburised in degrees intermediate between malleable and cast-iron, then it is obvious that the latter during its conversion into the former in the processes of fining and puddling, must pass through the state of steel." On page 805 of the same work he will find further confirmation of his theory in the words, "it is obvious that steel must be produced by melting malleable and cast-iron together in suitable proportions."

I might multiply quotations from this and every other work I have seen in which the chemistry of iron and steel is treated, and show by each of them that the thousand-and-one of unfortunate inventors who have struggled in vain to make steel directly from English pig-iron, have been encouraged in their delusion by the teachings of high chemical authorities.

"If steel be regarded simply as iron carburised in degrees intermediate between malleable and cast-iron," these inventors are perfectly justified in seeking some substance which at the melting heat of cast-iron shall give off a definite quantity of oxygen; and they have logical grounds for believing that by bringing such a substance in contact with the molten cast-iron, and properly regulating its quantity, they may burn out just that surplus carbon which makes all the stated difference between cast-iron and steel. As a multitude of compounds when thus heated do give off oxygen, a vast field of effort is open, and accordingly every available peroxide and decomposable oxygen salt has been administered by strange devices to the melted iron, the same obvious substances used over and over again, and the same failures continually repeated by expectant inventors ignorant of what each other have done or are doing. Gas and vapours have been blown over the surface and under the surface, and through from bottom to top of melted cast-iron, and all (including Mr. Bessemer) have failed to produce merchantable steel from ordinary English cast-iron, without first making it into malleable or wrought-iron.

The reason of this is, that the removal of the surplus carbon is only a small portion of the work which has to be done in order to convert cast-iron into steel of any commercial value. Several other substances have to be removed also; *and no process has yet been discovered by which these impurities can be removed without at the same time removing the carbon in corresponding degree.* I put this in italics because I am convinced by experience of its great practical importance; because I do not find it clearly and distinctly enunciated in any general or special treatise; and further, because I have seen so plainly that the want of clearly understanding it is the rock upon which so many unfortunate inventors have split.

These inventors have not been informed with anything like the necessary degree of distinctness, that the Styrians and others who have made, or are making, steel directly from cast-iron, have started with a very different material to that which bears the same name of cast-iron in England; the difference being sufficiently great to alter totally the conditions of the problem. The cast-iron of the Styrian steel-makers is a nearly pure carburised iron; *our* cast-iron is a carburised, silicised, phosphurised, and sulphurised iron; *their* problem in steel-making is, merely the partial decarburisation of their cast-iron; *ours* is the total desilicisation, the total dephosphurisation, and the total desulphurisation in addition to this. Now, the partial removal of carbon from iron is one of the very easiest problems in practical metallurgy, while the complete removal of silicon, phosphorus, and sulphur, is among the most difficult.

To illustrate the grossness of the fallacy which represents the difference between cast-iron and steel as merely, or "essentially," due to carbon, I may state that on looking down a tabular statement of the analyses I have recently made of thirty brands of ordinary English pig-iron (excluding hæmatite pigs), I find that seven among them contain less than 2 per cent. of carbon, or an average of 1·77 per cent. Now this is below the percentage of carbon which exists in some of the finest and most expensive samples of cast-steel. Therefore, to convert these particular brands of cast-iron into the finest steel, the carbon must neither be increased nor diminished, and if, as Dr. Percy says, the differences between steel, wrought-iron, and cast-iron, "*essentially* depend upon differences in the proportion of carbon," all these brands of pig-iron should be described as steel rather than cast-iron.

Nevertheless they are utterly worthless for any of the purposes for which steel is used, and the common result of the costly experiments of the inventors who endeavour to make steel directly from English pig-iron, is to produce a material very much like them. They usually succeed perfectly in their effort partially to decarburise the pig iron. They take out, say one half of the carbon, and with it a considerable portion of the silicon, and *some* of the phosphorus, sulphur and manganese; but to make a *perfect* steel they must take out *all* of these latter, and leave nothing but pure iron and carbon. Absolute perfection is not, of course, practically attainable in steel-making, but it is approximated in exactly the same degree as the purification of the iron from everything excepting the carbon is effected.

The most notable modern attempt to produce steel directly by the simple decarburisation of English cast-iron was that of Mr. Bessemer. His first idea was to blow air through melted cast-iron, and thereby to oxidise the carbon, and then, when a sufficient degree of decarburisation was effected, to stop the blowing. He supposed that when by this means the proportion of carbon was reduced to about one and a half per cent. the result would be useful steel. He failed entirely in this; he never succeeded in producing merchantable steel from ordinary English cast-iron by this method.

The Bessemer process, as at present conducted, consists in first oxidising simultaneously all or nearly all the carbon and silicon, and then adding to the decarburised iron a new dose of carbon, by means of a known quantity of spiegeleisen of known composition; thus reverting to the old Sheffield principle of first bringing the cast-iron to the state of wrought or decarburised iron, and then adding carbon to convert it into steel.

It is commonly represented that the failure of the early attempts at direct steel-making by the Bessemer process arose simply from the difficulty of determining the right moment at which to stop the blow, and thereby to regulate the proportion of carbon; and that the whole advantage of the spiegeleisen is the means it affords of doing this. Dr. Percy says:—"In attempting to produce steel by the methods specified by Bessemer, it has hitherto been found very difficult, if not impracticable, *at least in this country*, to ascertain with certainty when decarburisation has proceeded to the right extent, and when therefore the blast should be stopped. Accordingly the plan now adopted is to decarburise perfectly, or nearly so, and then add a given proportion of carbon in the state in which it exists in molten spiegeleisen, the precise composition of which should of course be known."* Neither in Dr. Percy's nor any other account of the Bessemer process do I find that the necessity of complete decarburisation as a means of completely separating the silicon is fairly appreciated.

If merchantable steel could be made from English pig-iron by simply stopping the blow before complete decarburisation, Mr. Bessemer would surely have produced some good steel in the course of his long and costly efforts which preceded the idea of introducing the spiegeleisen, for it must be remembered that the quantity of carbon required in steel extends over a very wide range—that steel may contain from 0·40 to 2·00 per cent. of carbon, and that steel with every degree of carburisation within this wide range is in demand in the market at good prices, provided it be free from phosphorus, silicon, &c. Nothing is practically easier than to stop the blow at such a moment as shall ensure a degree of carburisation somewhere between this wide range; and there can be no doubt that, in his early experiments, Mr. Bessemer, like other inventors of direct processes, made an abundance of iron that was duly carburised within the above-stated limits, although he failed to produce useful steel.

Dr. Percy's qualification, "at least in this country," is rather curious. He has probably learned that steel has been directly made in Sweden (though he does not mention it in his work) by the Bessemer process, and he seems to attribute this to the superior ability of the Swedish operators, enabling them "to ascertain with certainty when decarburisation has proceeded to the right extent." I differ entirely from Dr. Percy in this conclusion, being convinced that Mr. George Brown, the manager of the Bessemer Department at the Atlas Works, Sheffield, who was the first to work the Bessemer process with commercial success, is better able (on account of his much greater experience and thorough knowledge of the work) than any of the Swedish manufacturers, to determine when any required degree of decarburisation has been attained. It is not the superior skill of the Swedish operators that has enabled them to make steel directly by the Bessemer process; but the fact that they, like the Styrian workers, used a very superior charcoal-iron to start with; and that the blowing out of all the carbon was not absolutely necessary for the sufficient purification of this quality of iron.

W. MATTIEU WILLIAMS

* "Metallurgy," "Iron and Steel," p 814. The italics are my own.

ON THE NATURAL LAWS OF MUSCULAR EXERTION

THE experiments published by Mr. W. Stanley Jevons, in NATURE on the 30th June last, illustrate well two laws of muscular exertion which were established by experiments made by myself in 1862 and 1863. These laws may be thus stated:—

Law 1. The work given out by a single group of muscles, in a single contraction, is constant.

Law 2. When the same group of muscles is kept in constant action, the total work done by them until fatigue sets in, multiplied by the rate at which they are compelled to work, is constant.

Mr. Jevons' first series of experiments, in which different weights were thrown by the arm to various distances on level ground, illustrates the first law. In throwing weights in this manner, the arm, after a little practice, instinctively pitches the weight at the angle corresponding to the maximum range, and as the maximum range is proportional to the square of the velocity of projection, it may be used to replace that velocity squared, in estimating the work done by the arm.

The total work done is the same as if the weight used and the weight of the arm were concentrated at the centre of oscillation of the loaded arm, regarded as a compound pendulum.

Let us assume

w = weight held in hand;
x = weight of arm;
v = velocity of centre of oscillation.

By Law 1, the work done is constant and is represented by

$$(w + x)\, v^2 = \text{const.} \qquad (1)$$

Let

V = velocity of hand;
l = radius of oscillation;
a = length of arm.

then

$$v = V\frac{l}{a} \qquad (2)$$

It is easy to show (assuming the arm to be a uniform cylinder) that

$$\frac{l}{a} = \frac{2}{3} \cdot \frac{(3\,w + x)}{(2\,w + x)} \qquad (3)$$

By means of (2) and (3), equation (1) becomes

$$\frac{(w + x)\,(3\,w + x)^2}{(2\,w + x)^2} \times R = A\,; \qquad (4)$$

where R denotes the range (proportional to V^2) and A denotes a constant, if Law 1 be true.

Mr. Jevons' experiments give the following corresponding values of w and R.

w	R
56 lbs.	1·84 ft.
28 "	3·70 "
14 "	6·86 "
7 "	10·56 "
4 "	14·61 "
2 "	18·65 "
1 "	23·05 "
½ "	27·15 "

We are required to assign certain values to x and A, which will make equation (4) best coincide with the eight simultaneous values of w and R found by observation.

I find by trial that these values are

x = 8·1 lbs.
A = 262·2.

If we solve equation (4) for R, we find

$$R = \frac{A\,(2\,w + x)^2}{(w + x)\,(3\,w + x)^2} \qquad (5)$$

Substituting for A and x in this equation their values above given, we can obtain by calculation the distances to which the weights should be thrown, according to Law 1.

We thus obtain the following comparison between theory and observation.

w	R (observed).	R (calculated).	Difference
56 lbs.	1·84 ft.	1·90 ft.	−0·06 ft.
28 „	3·70 „	3·51 „	+0·19 „
14 „	6·86 „	6·06 „	+0·80 „
7 „	10·56 „	10·02 „	+0·54 „
4 „	14·61 „	13·90 „	+0·71 „
2 „	18·65 „	19·11 „	−0·46 „
1 „	23·05 „	23·85 „	−0·80 „
½ „	27·15 „	27·39 „	−0·24 „

The agreement here shown between observation and calculation founded on Law 1, is quite as complete as the agreement between observation and the empirical formula used by Mr. Jevons, which may be written, in the notation of the present paper, as follows :—

$$(2w + 7\cdot8)\,R = 231\cdot3. \qquad (6)$$

Mr. Jevons' third series of experiments consisted in holding various weights on the hand extended horizontally, and noting the time during which the weights could be so held. The following are the weights and times observed:—

w	t
18 lbs.	14·8 secs.
14 „	32·5 „
10 „	60·3 „
7 „	87·4 „
4 „	147·9 „
2 „	218·9 „
1 „	321·2 „

Omitting the first of these experiments I find that Law 2 satisfactorily accounts for the remaining six, and gives a constant, which is nearly identical with that obtained from my own experiments made in 1863.

When the arm is extended horizontally, if allowed to fall through an indefinitely small arc, the centre of oscillation falls like a free body under the influence of gravity, and the muscles then lift back the arm through the same arc, and this goes on continuously until the muscles are tired out.

Let us use the following notation :—

w and x are, as before, the weight held in the hand and the weight of the arm.

l = radius of oscillation ;
a = distance of centre of gravity of loaded arm from centre of shoulder joint ;
δs = small space through which the centre of oscillation falls ;
n* = number of such falls during
t = whole time required to fatigue the muscles.

The total work done by the muscles in the time t, is evidently

$$(w + x)\frac{a}{l}\,n\,\delta\,s;$$

but, $n\,\delta\,s$ varies as t, and, therefore, the total work done varies as

$$(w + x)\frac{a}{l}t.$$

The *rate* of work is evidently proportional to

$$(w + x)\frac{a}{l}$$

and since, by Law 2, the total work done before fatigue multiplied by the rate of work is constant, we obtain

$$(w + x)^2\frac{a^2}{l^2}t = \text{Const.} \qquad (7)$$

* I have ascertained the number n from acoustical observations made on the muscular *susurrus*.

And, since

$$\frac{a}{l} = \frac{3}{4}\cdot\frac{(2w + x)^2}{(w + x)(3w + x)}, \qquad (8)$$

we find, by substitution,

$$\frac{(2w + x)^4}{(3w + x)^2}t = a. \qquad (9)$$

This equation (9) is the statement of Law 2, as applied to Mr. Jevons' experiments ; and we are required to find values for x and a, which will make equation (9) best correspond with the given observations.

I find, by trial, that the following values will answer best :—

$$x = 7\cdot4 \text{ lb.}$$
$$a = 22{,}050.$$

If we solve equation (9) for t, we find

$$t = A\frac{(3w + x)^2}{(2w + x)^4}. \qquad (10)$$

From this equation, substituting the values of x and A, we obtain the following comparison of observation and theory :

w	t (observed).	t (calculated)	Difference.
14 lbs.	32·5 secs.	34·2 secs.	− 1·7 secs.
10 „	60·3 „	54·7 „	+ 5·6 „
7 „	87·4 „	84·8 „	+ 2·6 „
4 „	147·9 „	147·6 „	+ 0·3 „
2 „	218·9 „	234·4 „	−15·5 „
1 „	321·2 „	305·5 „	+15·7 „

This comparison is very satisfactory, the differences being much less than possible errors of observations. Mr. Jevons' experiments further show that the *useful effect* has a maximum corresponding to a certain weight. This weight, which gives the maximum of useful effect, may be readily calculated from Law 2.

By equation (10), the useful effect is

$$w\,t = A\,.\,\frac{w\,(3w + x)^2}{(2w + x)^4}. \qquad (11)$$

This will be a maximum, when

$$(2w + x)(9w + x) = 8\,w\,(3w + x)\,;$$

or when

$$6w^2 - 3\,x\,w - x^2 = 0\,;$$

or when

$$w = \frac{3 + \sqrt{33}}{12}x\,; \qquad (12)$$

or,

$$w = 0\cdot73\,x.$$

Substituting for x its value 7·4lb., we find for the weight that gives the maximum useful effect,

$$w = 5\cdot40\text{lb.}$$

The useful effect observed by Mr. Jevons was as follows :

w	Useful effect
18 lbs.	266
14 „	455
10 „	603
7 „	612
4 „	592
2 „	438
1 „	321

The actual maximum corresponds to 5·4lb. lying between 7lb. and 4lb.

I may observe, in conclusion, that the difference of weights x of the arm, found in the two sets of experiments is quite natural.

In the experiments in which the arm was held out horizontally, its weight, 7·4lb., is the weight of the arm below the centre of the shoulder joint.

In the experiments in which the weights are thrown by the arm, a portion of the shoulder blade is in motion, in addition to the simple arm, and the total weight becomes 8·1lb.

SAMUEL HAUGHTON

PROFESSOR ABEL'S CONTRIBUTIONS TO THE HISTORY OF EXPLOSIVE AGENTS *

THE degree of rapidity with which an explosive substance undergoes metamorphosis, as also the nature and results of that metamorphosis, are, in the greater number of instances, susceptible of several modifications by variations of the circumstances under which the conditions essential to chemical change are fulfilled. Gun-cotton furnishes an excellent illustration of the manner in which such modifications may be brought about. If a loose tuft or large mass of gun-cotton-wool be inflamed in open air by contact with, or proximity to, some source of heat, the temperature of which is about 135° C. or upwards, it flashes into flame with a rapidity which appears almost instantaneous, the change being attended by a dull explosion, and resulting in the formation of vapours and gaseous products, of which nitrogen-oxides form important constituents. If the gun-cotton be in the form of yarn, thread, woven fabric, or paper, the rapidity of its inflammation in open air is reduced in proportion to the compactness of structure or arrangement of the twisted, woven, or pulped material; and if it be converted by pressure into compact masses, solid throughout, the rate of its combustion will be still further reduced. If to a limited surface of gun-cotton, when in the form of a fine thread or of a compactly pressed mass, a source of heat is applied, the temperature of which is sufficiently high to establish the metamorphosis of the substance but not adequate to inflame the products of that change (carbonic oxide, hydrogen, &c.), the rate of burning is so greatly reduced that the gun-cotton may be said to smoulder without flame; the reason being that the products of change, which consist of gases and vapours, continue, as they escape into air, to abstract the heat developed by the burning gun-cotton so rapidly that it cannot accumulate to an extent sufficient to develop the usual combustion, with flame, of the material. For similar reasons, if gun-cotton be kindled in a rarefied atmosphere, the change developed will be slow and imperfect in proportion to the degree of rarefaction, so that, even if an incandescent wire be applied, in a highly rarefied atmosphere, to the gun-cotton, it can only be made to undergo the smouldering combustion, until the pressure is sufficiently increased by the accumulating gases to reduce very greatly the rate of abstraction, by these, of the heat necessary for the rapid combustion or explosion of the substance. If, on the contrary, the escape of the gases from burning gun-cotton be retarded, as by enclosing it in an envelope or bag of paper, or in a vessel of which the opening is loosely closed, the escape of heat is impeded until the gases developed can exert sufficient pressure to pass away freely by bursting open the envelope or aperture, and the result of the more or less brief confinement of the gases is a more rapid or violent explosion, and consequently more perfect metamorphosis of the gun-cotton. So, within obvious limits, the explosion of gun-cotton by the application of flame or any highly heated body is more perfect in proportion to the amount of resistance offered in the first instance to the escape of the gases; in other words, in proportion as the strength of the receptacle enclosing the gun-cotton, and the consequent initial pressure developed by the explosion, is increased. Hence, while gun-cotton has been found too rapid or violent in its explosive action when confined in guns, and has proved a most formidable agent of destruction if enclosed in metal shells or other strong receptacles, it has hitherto been found comparatively harmless as an explosive agent if inflamed in open air or only confined in weak receptacles. Modifications, apparently slight, of the manner in which the source of heat is applied to explosive agents, when exposed to air under circumstances in other respects uniform, suffice to modify the character of their explosions in a remarkable manner. Thus a modification of the position in which the source of heat is placed with reference to the body of a charge of gunpowder, which is only partially confined, suffices to alter altogether the character of the explosion produced.

The product of the action of nitric acid upon glycerine, which is known as nitroglycerine or glonoine, appears to be susceptible of only two varieties of decomposition. If a sufficient source of heat be applied to some portion of a mass of this liquid in open air, it will inflame and burn gradually without any explosive effect; and even when nitroglycerine is confined, the development of its explosive force by the simple application of flame or of other sources of heat, by the ordinary modes of operation, is difficult and very uncertain. But if the substance be submitted to a sudden concussion, such as is produced by a smart though not very violent blow from a hammer upon some rigid surface on which the nitroglycerine rests, the latter explodes with a sharp detonation, just as is the case with gun-cotton. Only that portion of the explosive agent detonates which is immediately between the two surfaces brought into sudden collision; the confinement of this portion between the hammer and the support, combined with the instantaneous decomposition of the portion struck, prevent any surrounding freely exposed portions from being similarly exploded by the detonation. A similar result is obtained if any explosive compound or mixture be submitted to a sufficiently sharp and violent blow, but the tendency of surrounding particles to become inflamed by the detonation is in direct proportion to the rapidity of explosive action of the substances. The practical difficulties and uncertainty which attend attempts to develop the explosive force of nitroglycerine by the agency of flame or the simple application of any highly heated body, even when the material is confined in strong receptacles (such as iron shells or firmly tamped blast-holes), appeared fatal to any useful application of the powerful explosive properties of this substance, until M. Alfred Nobel's persevering labours to utilise nitroglycerine, eventually resulted in the discovery of a method by which the explosive power of the liquid could be developed with tolerable certainty. M. Nobel first employed gunpowder as a vehicle for the application of nitroglycerine. By impregnating the grains of gunpowder with that liquid, he added considerably to the destructive force of the powder when exploded in the usual way in closed receptacles. M. Nobel's subsequent endeavours to apply nitroglycerine *per se* were based upon the belief that its explosion might be effected by raising some portion of a quantity of the liquid to the temperature necessary for its violent decomposition, whereupon an initiative explosion would be produced which would determine the explosion of any quantity of the substance.

The circumstance that nitroglycerine, or any preparation of that substance, may be violently exploded when freely exposed to air, by the explosion in contact with it of a small confined charge of gunpowder, or of a detonating substance, while other modes of explosion by the application of heat or flame, which have been described by M. Nobel, only develop explosion under special conditions, points to a decided difference between the action of the two modes of ignition, and appears to indicate that it is not simply the heat developed by the chemical change of the gunpowder or detonating powder which determines the explosion of the nitroglycerine. An experimental investigation of this subject has left no doubt on my mind that the explosion of nitroglycerine through the agency of a small detonation is due, at any rate in part, to the mechanical effect of that detonation, and that this effect may operate in exploding the nitroglycerine, quite independently of any direct action of the heat disengaged.

The readiness and certainty with which gunpowder, gun-cotton, and other explosive substances may be detonated through the agency of a blow from a hammer or a falling body, are regulated by several circumstances; they are in direct proportion to the weight of the falling body, to the height of its fall, or the force with which it is impelled downwards, to the velocity of its motion, to the mass and rigidity, or hardness, of the support or anvil upon which the body falls; to the quantity and mechanical condition of the explosive agent struck, and to the ready explosibility of the latter. Thus a sharp blow from a small hammer upon an iron surface will detonate gunpowder with very much greater certainty than the simple fall of a heavy hammer, or than a comparatively weak blow from the latter. It is very difficult, by repeated blows applied at very brief intervals, to ignite gun-cotton, if placed upon a support of wood or lead, both of which materials yield to the blow, the force set into operation by that blow being transferred through the explosive agent and absorbed in work done upon the material composing the support. If, however, the latter be of iron, which does not yield permanently to the blow of the hammer, the detonation of these substances is readily accomplished. If the quantity of explosive agent employed be so considerable as to form a thick layer between the hammer and support, the force applied appears to be to so great an extent absorbed in the motion imparted to the particles of the comprehensible mass, that its explosion is not readily accomplished; and if the material be in a loose or porous condition (as, for example, in a state of powder or of loose wool), much work has to be accomplished in moving particles of the mass through a comparatively considerable space,

* An abstract of a paper, by Prof. Abel, F.R.S., Chemist to the War Department, in the Philosophical Transactions.

and a second or even third blow is therefore required to determine its explosion.

These circumstances would appear to afford support for the belief that the detonation of an explosive material through the agency of a blow is the result of the development of heat sufficient to establish energetic chemical change, by the expenditure of force in the compression of the material, or by the friction of the particles against each other, consequent upon a motion being momentarily imparted to them. It is conceivable that, from either of these causes, sufficient heat may be accumulated with almost instantaneous rapidity, in some portion of the mass struck, to develop sudden chemical change. The circumstance that the detonation of those portions of an explosive compound (such as gun-cotton or nitroglycerine) which are immediately between the surfaces of the hammer and the support is not communicated to the surrounding portions, may be ascribed to a combination of two causes, the instantaneous nature of the explosion, and the close confinement of the portions struck at the instance of their explosion. The mechanical effect of the detonation is absorbed by the masses of metal between which it occurred, and the gases developed disperse the surrounding portions of the explosive agent, as they rush away from between the two surfaces. It is possible also to detonate gunpowder and other explosive mixtures by a blow in such a manner that only the portions immediately struck are ignited; but those substances may also be exploded, though much less violently, by a less sudden or powerful application of force, in which case they detonate much more feebly; their explosion is accompanied by a larger volume of flame, and by the ignition of those portions which surround the part struck by the hammer. The power of accomplishing the explosion or detonation of gun-cotton or nitroglycerine in open air through the agency of a detonation produced in its vicinity, would therefore appear to be correctly ascribable to the heat suddenly developed in some portion of the mass by the mechanical effect, or blow, exerted by that detonation, and would seem to be regulated by the violence and suddenness (either singly or combined) of the detonation, by the extent to which the explosive material is in a condition to oppose resistance to the force, and by the degree of sensitiveness of the substance to explosion by percussion. There are, however, several well-known facts, and some results of experiments instituted with special reference to this subject, which do not appear to be in harmony with the assumption that the detonation of nitroglycerine and gun-cotton in the manner described is simply due to the *suddenness* of the development and application of physical force.

With the view of ascertaining whether the relative power of different explosive agents to accomplish the detonation of gun-cotton appears to be in direct proportion to the relative mechanical effects of their explosion (*i.e.* to the work performed by them upon a body placed in contact with them), a series of experiments was instituted with the object of comparing this particular action of the several explosive materials. It would appear from these experiments that, when unconfined, the violence of explosion of chloride of nitrogen is less than that of the iodide, and that, if confined under water, it very considerably exceeds that of the exposed iodide, but falls very short of that exerted by unconfined silver-fulminate. It also appears that the mercuric fulminate, which is much less rapidly explosive than either of the other substances, exerts less mechanical force than any of them, if freely open to air, and if inflamed at some portion of the exposed surfaces; if ignited at the lower inner portion of the mass, where the part first inflamed is enclosed by the mass of the material itself, it exerts a destructive force little inferior to that of the chloride of nitrogen enclosed by water; but if confined in a strong envelope (*e.g.* of sheet tin), the mercuric fulminate is greater in violence of action than the unconfined silver-fulminate. These results to a great extent confirm the correctness of the view that the readiness with which the detonation of gun-cotton is accomplished is in proportion to the mechanical force exerted by the initiative detonation to which it is subjected. The force exerted by small quantities of strongly confined silver and mercuric fulminate greatly exceeds that developed by the explosion of comparatively large proportions of the iodide and chloride of nitrogen. This may be accepted as accounting, to some extent, for the fact that the detonation of gun-cotton could not be accomplished by an amount of iodide of nitrogen twenty times greater than that of fulminates required for the purpose, while ten times the quantity of the confined chloride were required to produce the result. That the quantity of mercuric fulminate required to produce detonation is reduced in proportion as means are applied to increase the violence of the force exerted by it at one time, is quite in accordance with the above view.

I venture to offer the following as being the most satisfactory explanation which occurs to me of the remarkable differences exhibited in the behaviour of different explosive agents. The vibrations produced by a particular explosion, if synchronous with those which would result from the explosion of a neighbouring substance which is in a state of high chemical tension, will, by their tendency to develop those vibrations, either determine the explosion of that substance, or at any rate greatly aid the disturbing effect of mechanical force suddenly applied, while, in the case of another explosion which produces vibrations of different character, the mechanical force applied by its agency has to operate with little or no aid; greater force or a more powerful detonation must, therefore, be applied in the latter instance, if the explosion of the same substance is to be accomplished by it.

In conclusion, it may not be out of place to refer briefly to a few illustrations of the important bearings which the new mode of developing the explosive force of gun-cotton has upon the practical uses of the material as a destructive agent. The confinement of a charge of gun-powder or gun-cotton in a blast-hole, by firmly closing up the latter with earth, powdered rock, or other compressible material (by the process known as tamping or stemming) to a depth greater than the line of least resistance opposed to the action of the charge, is essential to the success of a blasting operation; but the great rapidity of explosion, by detonation, of a charge of gun-cotton greatly reduces the value of this operation; the destructive effect of the material, when exploded in a hole which is left open, is not inferior in extent to that obtained by similarly exploding a charge confined in the usual manner. Thus the most dangerous operation in connection with blasting may be entirely dispensed with. In submarine operations, it is no longer necessary to enclose the charge of explosive agent in the strong and therefore cumbersome metal receptacles hitherto required to ensure the full development of its explosive force; the destructive action of a charge of gun-cotton, enclosed in a waterproof bag or thin glass vessel and exploded by detonation, being decidedly greater than that furnished by a corresponding charge confined in a strong iron vessel and exploded by flame. Small charges of gun-cotton simply resting upon the upper surfaces, or loosely inserted into natural cavities, of very large masses of the hardest description of rock or of iron, have broken these up as effectually as if corresponding charges had been firmly imbedded in the centre of the mass and exploded in the usual manner. Lastly, the certainty, facility, and expedition with which certain important military destructive operations may be accomplished by means of gun-cotton exploded by detonation, are not among the least important advantages which are now secured to this interesting and remarkable explosive agent.

SCIENTIFIC SERIALS

POGGENDORFF'S *Annalen der Chemie und Pharmacie*, vol. cxl. part I.—This number contains (1) the first part of an elaborate paper by E. Ketteler, "On the Influences of Ponderable Molecules on the Dispersion of Light, and on the Signification of the Constants in the Mathematical Formulæ for Dispersion" (pp. 1 to 53). This is a critical examination, based chiefly on Mascart's experimental measurements, of the formulæ by which Cauchy and others have endeavoured to connect the indices of refraction of the various kinds of light with their wave-lengths. The nature and scope of the investigation may be gathered from the four following criteria which the author gives as the tests of a satisfactory formula:—"1. A rational formula must enable us to calculate accurately from their wave-lengths the succession of the several colours and their distribution in space, for the whole measured extent of radiation, for some definite density of the dispersive medium. 2. The constants of the formula must be capable of a distinct physical interpretation, analogous to the interpretation assigned by Cristoffel to the constants in his formula. 3. When the density of the dispersive medium is altered, these constants must participate in the change of molecular constitution in some simple manner, corresponding to what has been ascertained in respect to them in the case of gaseous media. 4. Consequently, as the medium approaches the limit of rarefaction, all the indices must approach unity as their limiting value." The author finds that none of the formulæ hitherto proposed reproduce the experimental results within the limits of error of the measurements, but that this can be done by a formula which he proposes. (2.) "On the Sounds produced by Heated Tubes, and on the Vibrations of Air in Pipes of various Forms," by C. Sondhaus (pp. 53 to 76). Many experimenters must have observed the frequent production of a musical tone when a bulb has been blown at the end of a rather short and narrow glass tube, the sound beginning just as the tube with the still hot bulb is removed from the lips. This phenomenon formed the subject of an investigation by the author twenty years ago, and he now returns to it in a paper which is to be concluded in the next number of the *Annalen*. The principal result which he now publishes is that when the dimensions of bulb and tubes are properly proportioned, similar tones can be obtained with heated glass bulbs from which two open tubes proceed in opposite directions. He also gives an empirical formula which expresses approximately the pitch of the tones obtained in terms of the dimensions of the bulbs and tubes; but as this formula does not seem to be based on any physical explanation of the way in which the sounds are produced, and as it takes no account of temperature, the agreement between its results and those of observation must be considered as at least to some extent accidental. Perhaps the remainder of the paper may give further explanations on these points. (3.) "On Chromates," by C. Freese (pp. 76 to 88), to be concluded in the next part. (4.) "Thermo-chemical Investigations" (continued), by Julius Thomsen (pp. 88 to 114). This section of Professor Thomsen's researches relates to the acids of nitrogen, phosphorus, and arsenic. The thermo-chemical behaviour of these acids, when neutralised with caustic soda, appears to agree in the main with the commonly-received views of their basicity founded upon their chemical properties. (5.) "Further Researches into the Development of Electromotive Force between Liquids," by Jacob Worm-Müller (pp. 114 to 144). Among other results the author arrives at the following remarkable conclusion: "Solutions of acids and alkalis in equivalent proportions (that is such that equal volumes of the solutions neutralise each other) and of the salts formed by mixing equal volumes of these solutions, do not give rise to electric currents when connected so as to form a circuit." This paper also is to be concluded in the next number of the *Annalen*. (6.) "Researches in Electrical Dust-figures," by Wilhelm von Bezold (pp. 145 to 159). (7.) "On the Law of Formation of Kundt's Dust-figure," by Theodore Karrass (pp. 160 to 168). (8.) "On an Electrophorus-machine for Charging Batteries," by Peter Riess (pp. 168 to 172). The author describes a modification of Holtz's electrical machine, which renders it applicable for charging Leyden batteries to a high tension. (9.) "On the Measurement of the Absorption of Light by transparent media by means of the Spectroscope," by C. Vierordt (pp. 172 to 175). The author's method of measurement consists essentially in diminishing the intensity of each part of a normal spectrum, by means of smoked glasses of known absorptive power and the partial closing of the slit of the spectroscope, until it is identical with that of the light transmitted by the medium to be examined. (10.) "An Observation on the Induction-spark," by Dr. A. Weinhold (p. 176).

In the *Journal of Botany* for August, the original articles relate almost entirely to extra-English botany, with the exception of the conclusion of Mr. Worthington Smith's *Clavis Agaricinorum*, which forms an important contribution to the literature of cryptogamic botany.

In the *Proceedings of the Asiatic Society of Bengal* for June are three articles on the Andamanese, the most important of which is by Surgeon Francis Day. He estimates the number now living on the island as probably not much over 1,000, divided into several tribes, which have distinct dialects, so that members of the Little Andamans are scarcely able to understand those of the South Andamans. Their language is very deficient in words; many English and Hindustani words are now beginning to be incorporated in it; numerals are entirely absent. They are anything but prolific, and appear to be gradually dying out from excess of deaths over births. Mr. Day only saw one woman who had as many as three living children; during one year thirty-eight deaths were reported, and only fourteen births among the families living near the European settlements; few appear to live to a greater age than forty, and they are subject to a variety of diseases. We hope to return to this article again. Dr. G. von Martens contributes "Notes on some Javanese Algæ." The remaining articles in the number are philological.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 8.—Papers were read on the relation between the specific heats and the coëfficients of dilatation of any body, by M. Phillips, and on the decimal division of the quadrant, by M. A. d'Abbadie, in which he communicated two letters on the subject from M. Nadau and Prof. Airy; and MM. Jamin and Richard contributed some observations on the determination of the relation between the two specific heats of gases.—M. Jamin replied to the two notes by M. Sainte-Claire Deville on July 18th, and entered again at length into the subject of the variations of temperature produced by the mixing of two liquids.—M. Laborde contributed a note on some new experiments on Holtz's electrical machine.—M. Elie de Beaumont presented, on behalf of M. Delesse, a lithological map of the embouchure of the Seine.—A note by MM. Rabuteau and Peyré was presented by M. Ch. Robin, on the poisonous effects of the m'boundou or icaja, a poison used at the Gaboon. The poison used was extracted chiefly from the bark, a small quantity also from the root. The experiments showed that the poison is extremely rapid; but that its fatal effects can be prevented by artificial respiration; the symptoms are in some respects similar to those produced by strychnine.—A letter was read from M. Lichtenstein to M. Dumas, on a means of preventing the irruption of the *Phylloxera vastatrix* in vines not yet attacked. The proposed plan is simply by destroying carefully, from May to August, all the branches on which the winged form of the insect has made its appearance.—A short note was also presented by M. L. Laliman, on a variety of vine (of the American species *V. æstivalis*) not subject to the attacks of the *Phylloxera*.

BOOKS RECEIVED

ENGLISH.—Lectures on Art: J. Ruskin (New York: Wiley and Son).—The Laws of Verse: J. J. Sylvester (Longmans).—The Wind in his Circuits: R. H. Armit (J. D. Porter).—Matter for Materialists: T. Doubleday (Longmans).—The Book of the Roach; G. Fennell (Longmans).

FOREIGN.—(Through Williams and Norgate)—Etudes sur la maladie des vers à soie: L. Pasteur.—Streifzüge (landwirthschaftliche) in Frankreich u. Algerien im Jahre 1862-68: A Petzhold.—Leçons de Chimie, années 1868-69, Dehérain, &c.—Mineralogie der Vulcane: Dr. C. Landgrebe.—De l'enseignement supérieur en Angleterre et en Ecosse: J. Demoycot.—Zonula ciliaris: Dr. F. Merkel.—Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege: C. Reclam.—Prodromus Floræ Hispanicæ, Vols. 1 and 2: M. Willkom.—Algæ japonicæ Musæi botanici Lugduni-Batavi: W. F. R. Süringer.—Die Osteologie und Myologie von Sciurus vulgaris: C. K. Hoffman.

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ERRATA.—Page 296, first column, lines 10 and 11 from bottom, for "blosz Erscheinung" read "blose Erscheinung."—Page 308, first column, line 14, for "Phylloscerae" read "Phylloxeræ."