THURSDAY, MARCH 20, 1884

A BIOLOGICAL LABORATORY ON THE ENGLISH COAST

ARRANGEMENTS have been made for a meeting in the rooms of the Royal Society at half-past four, on Monday, March 31, the object of which is to found a Society having for its purpose the establishment and maintenance of a well-equipped laboratory at a suitable point on the English coast, similar to, if not quite so extensive as, Dr. Dohrn's Zoological Station at Naples.

The value of such an institution to the progress of zoological science, and the simple necessity which exists for the thorough and detailed knowledge only to be gained by the constant work of a well-supported laboratory devoted to the complete exploration of a definite area of sea-bottom, if any reasonable action is to be taken in regulating and improving British sea fisheries, have been set forth at various times in these pages during the past year in connection with the conferences held at the Fisheries Exhibition.

English naturalists have at length determined to do their best to bring about the foundation of the desired laboratory. A large sum of money will be needed in order to secure a site and erect the necessary buildings, besides the provision of an annual income. The Society will be able to raise these funds and to administer them in a more satisfactory way than would be possible were the matter taken in hand by a few private individuals only. The laboratory, when once set going, together with its boats and fishermen, will be used for the purpose of carrying on investigations by any naturalists who are members of the Society, and may desire from time to time to avail themselves of its resources. Its work will therefore be chiefly carried on by volunteers, and it is quite certain that there are a very large number of thoroughly competent naturalists who are only waiting for the opportunity thus afforded. At present such men are to be found scattered here and there on our coasts, making shift to carry on observations without laboratory, boats, or any efficient appliances. Eventually it will no doubt be possible to place a qualified observer in charge of the laboratory. The laboratory will also be available for special investigations, for which a public body or other authority may have employed the services of a naturalist..

Apart from the conveniences which it can afford and the value of the moral effect of combined action even in scientific investigation—the continuous working of a number of naturalists at one spot has a most important reaction upon their work. In proportion as a particular area becomes thoroughly familiar in this way, it becomes easy to obtain special animals and plants for study which were at first regarded as rare, or were altogether unknown in the locality. The thorough and long-continued operations of such a laboratory have naturally enough the value of systematised work as compared with the casual dippings and exploratory incursions of the isolated naturalist who spends a month in one year at this place and a month in another year at another place.

Already in Scotland, on the Firth of Forth, close to Edinburgh—through the admirable energy of Mr. John Murray, the director of the *Challenger* Expedition publications—a small laboratory has been set up, and funds obtained for carrying on its work by the engagement of young naturalists to investigate special problems. The English laboratory will be erected at a point as rich as possible in respect of its marine fauna, and at the same time in proximity to important fishing grounds. No locality has yet been decided upon, but both Torquay and Weymouth have been suggested as presenting the desired combination. Everything depends on the amount of pecuniary support which the Society will be able to obtain. A great work may be done if sufficient funds are forthcoming; a smaller work will be accomplished with smaller funds, and carried on in the firm expectation of gaining increased means of activity as results are produced justifying the enterprise.

But as a matter of fact, no tentative method of procedure is needed. It is quite certain, from the experience obtained in other countries, that a properly provided observatory—with good working-rooms, large and small tanks, sea-water pump, a steam launch and well-trained fishermen and permanent staff—can turn out results which are numerous and valuable in proportion to the completeness of the arrangements and the experience of the permanent staff. France was the first country to start such marine laboratories or observatories. At present there are several in operation on the French coast—viz. at Roscoff, at Concarneau, at Villefranche, and near Cette. Italy boasts of the great international laboratory founded and carried on with wonderful perseverance and success by Dr. Dohrn at Naples. An idea of the cost of a really first-rate institution of the kind may be gathered from the fact that the palatial building in the Villa Nazionale at Naples with its fittings and fishing-boats represents a capital of 20,000*l.*, whilst the annual expenditure is over 4000*l.* Austria has such a laboratory at Trieste, maintained by the Imperial Government. Among the most successful of such laboratories have been those established on the eastern coast of the United States. That at Beaufort, directed by the Johns Hopkins University, has furnished an extraordinary amount of interesting results through the activity of Mr. Brooks and the young naturalists of the United States who make use of it. That erected by Prof. Alexander Agassiz at Newport (Maine) is no less satisfactory as an evidence of the utility of such institutions. Since the foundation of these laboratories (within the past decade) our knowledge of marine organisms has increased at an enormous rate: without them we should have gone on in the casual, uncertain way which necessarily arose from the fact that every naturalist, before the foundation of these laboratories, had to establish his own little workshop for the summer and to make a fresh start in an unexplored locality, or in one explored only by the efforts of himself alone.

The meeting on March 31 promises to be one of great influence. Prof. Huxley, P.R.S., is to preside. Prof. Flower, Prof. Moseley, Prof. Milnes Marshall, Sir Lyon Playfair, Mr. W. S. Caine, M.P. (one of the Commission on Trawling), Prof. Michael Foster, Prof. Ray Lankester, Dr. Albert Günther, Dr. W. B. Carpenter, Mr. Gwyn Jeffreys, Dr. P. L. Sclater, Mr. Frank Crisp, Sir John

Lubbock, and other gentlemen, have signified their intention of being present and supporting the resolutions which are to be submitted to the meeting.

We beg to refer those of our readers who are interested in this subject to the articles published during the past year in NATURE, and to the arguments advanced in support of the proposal to found such a laboratory, together with a sketch of the relation of zoological science to the well-being of British fisheries, in the address on the Scientific Results of the Fisheries Exhibition delivered by Prof. Ray Lankester at the conference on July 19, and published by the Exhibition Committee.

THE UNITY OF NATURE

The Unity of Nature. By the Duke of Argyll. (London: Strahan, 1884.)

THIS book is in our judgment a dreary failure. Although in the mere matter of style it is a well written popular exposition of what we may call the comfortable way of looking at things, in all matters of deeper importance it is utterly barren. Throughout its five or six hundred pages there is no single original observation in science, nor any single original thought in anything that deserves to be called philosophy. Moreover, if regarded only as an exposition, the first chapters are tedious on account of the redundant manner in which elementary science is explained, while the later chapters, in which the author's views on various philosophical questions are unfolded, display a feebleness of thought and argument which renders them even more tedious than the earlier ones. In short, the successive essays strongly remind us of a series of Scottish sermons. There is everywhere a narrow consistency in the doctrine, which is presented in a rhetorical precision of style; but the discussion never seems to get below the surface, while even surface difficulties are either unperceived or intentionally avoided. On this account the discussion itself tends to illustrate the principle of "unity" with which it is concerned; it begins, continues, and ends in a monotone. No matter how fearfully out of tune this may be with any of the notes struck by the greatest men of our time, the Duke of Argyll, like a Highland piper, is deaf to every other music, and drowns all else in the one continuous drone of his own particular instrument.

The pages of a scientific journal are not suited to an examination in any detail of the parts of the book to which these general remarks apply. We shall, therefore, proceed to examine the more purely scientific strands which are woven into the texture of the work. In this connection the chief topic which meets us is that of "Animal Instinct in Relation to the Mind of Man." Here the main question which is dealt with—that as to the mode of origin and development of instincts—appears to us most inefficiently treated. The object of the writer is to argue that the phenomena of instinct point directly to the design of a Creator, who correlates instinct with structure and environment. So far, of course, every evolutionist, who is also a theist, may go. But, in order to enforce this view, the Duke proceeds to argue that the phenomena in question are of so mysterious a nature that it is not possible to point to any causes of a proximate or physical kind which may reasonably be supposed to produce them. Now it would be easy to show—were this the place to show it—that the writer has here adopted a weak position even as an apologist; but, to consider the matter only from the side of science, surely it shows some grave want either of judgment or of consideration to make the kind of statements of which the following may be taken as fair examples :—

"I can therefore see no light in this new explanation to account for the existence of instincts which are certainly antecedent to all individual experience—the explanation, namely, that they are due to the experience of progenitors 'organised in the race.' It involves assumptions contrary to the analogies of nature, and at variance with the fundamental facts, which are the best, and indeed the only, basis of the theory of evolution. There is no probability—there is hardly any possibility—in the supposition that experience has had, in past times, some connection with instinct which it has ceased to have in the present day. . . . There was a time when animal life, and with it animal instincts, began to be. But we have no reason whatever to suppose that the nature of instinct then or since has ever been different from its nature now. On the contrary, as we have in nature examples of it in infinite variety, from the very lowest to the very highest forms of organisation, and as the same phenomena are everywhere repeated, we have the best reason to conclude that, in the past, animal instinct has ever been what we now see it to be—congenital, innate, and wholly independent of experience."

Such passages as these scarcely admit of comment, because all that can be said about them is that the writer has either never read, or has completely forgotten, the whole of the literature to which he alludes. No evolutionist has ever entertained the suicidal "supposition that experience has had, in past times, some connection with instinct which it has ceased to have in the present day;" and the conclusion that in the absence of so absurd a supposition the only alternative is to regard instinct as always having been wholly independent of experience is a conclusion which stands in direct opposition to all that constitutes "evolution" a "theory." Of course no one is bound to accept this theory; it may be rejected, or it may be left unmentioned; but it is futile to set up a nonsensical form of words, and then to call the absurdity the "theory of evolution."

And these are no mere chance expressions, which, if standing alone, might be indicative only of carelessness. The whole of the dissertation on instinct is pervaded by a similar misapprehension, or want of apprehension, of the fundamental ideas of the newer philosophy which the writer appears to suppose that he is considering. Thus, he fails to perceive that the doctrine of natural selection has any bearing upon the subject, while, with reference to the factor of what Mr. Darwin called "inherited habit," he says :—

"If the habits and powers which are now purely innate and instinctive were once less innate and more deliberate, then it will follow that the earlier faculties of animals have been higher, and that the later faculties are the lower in the scale of intelligence. This is hardly consistent with the accepted idea of evolution," &c.

Comment is needless. We shall, therefore, notice only one other point with reference to the essay on instinct, and this is the difficulty which is thus manufactured to meet the experience theory.

"Did there ever exist in any former period of the world what, so far as I know, does certainly not exist now—any animal with dispositions to enter on a new career, thought of and imagined for the first time by itself, unconnected with any organs already fitted for and appropriate to the purpose? . . . The questions raised when a young dipper, which had never before seen the water, dives and swims with perfect ease, are questions which the theory of organised experience does not even tend to solve; on the contrary, it is a theory which leaves these questions precisely where they were, except in so far as it may tend to obscure them by obvious confusions of thought."

Here one would have thought that the writer need not have gone further than the instance which he himself gives to have found evidence of the growth of an instinct by the accumulation of hereditary experience or habit, and as yet unconnected with the "organs already fitted for and appropriate to the purpose." For the dipper belongs to a non-aquatic family of birds, and therefore has no organs specially adapted to its aquatic instincts. In particular it has no webs to its feet; and therefore, so far as the structure and affinities of the bird can in themselves argue anything, they speak most distinctly in favour of the view that the species must have developed aquatic instincts while not yet having had time to develop the "appropriate organs." It would be no answer to say that this *species* does not need these organs; else why are they needed by all the *families* of birds which present the same instincts? Or, conversely, can it be said that these same organs, *i.e.* webbed feet, stand in any special correlation with the existing instincts of the upland geese, which, being terrestrial in their habits (though aquatic in their affinities), never use them for swimming or diving? Short of historical or palæontological knowledge (which in the case of instinct is of course impossible), we could have no stronger evidence of transmutation than is afforded by these two complementary cases, in one of which the absence of a structure points to the recent acquisition of the instinct, while in the other the presence of this structure points to the former existence of the instinct now obsolete. Analogous cases occur in the species of ground-parrots and tree-frogs which, while retaining their ancestral structures adapted to climbing, have nevertheless entirely lost their arboreal instincts.

Moreover, a strange want of thought is shown by the remark that, so far as the writer knows, "there certainly does not exist now any animal with dispositions to enter on a new career, thought of and imagined for the first time by itself." It is enough to quote the complete change in the instincts of nidification which has been observed to take place in the house-sparrow, and in several species of swallow, since these birds first had the opportunity of building on houses; or the more recent and perhaps more remarkable case of the mountain parrot, which has been observed to manifest a "progressive development of change in habits from the simple tastes of a honey-eater to the savageness of a tearer of flesh." Many similar instances might be given, and, as showing that they are not uncommon, I may remark that a very instructive one is published by Dr. Rae in a recent number of this journal.

So much, then, for the Duke of Argyll's views on instinct. Scarcely less unsatisfactory are his views on rudimentary organs. The explanation which he adduces to account for these structures is, not that they are remnants of organs useful in the past, but that they are prophesies of organs which, when more fully developed, are to be of use in the future. We have no space to criticise at any length this wholly untenable inversion of Mr. Darwin's teaching; but we think it will be enough to notice the singularly unfortunate instance which the Duke selects to illustrate his theory. This instance is that of the whales, and he says that Mr. Darwin's views of the rudimentary organs here to be met with "obliges us to suppose that the ancestors of the whales were once terrestrial quadrupeds, and in that case we start with the conception of hind limbs, and of the quadrupedal mammal, fully formed and perfectly developed. Whereas, if we accept the possibility of useless organs being the beginnings and rudiments of structures which are there because the germ has always within it the tendency to produce them, then we catch sight of an idea which has the double advantage of going nearer to the origin of species, and of being in harmony with the analogy of natural operations as we see them now." Is not this enough? When we remember the eloquence, as it were, with which the whole organisation of the Cetacea tells us of their having been originally, like other mammals, terrestrial, it seems that the Duke could have chosen no worse example whereby to illustrate his hypothesis.

Passing now to the long discussion of the question whether savages should be regarded as the product of evolution from lower levels of human life, or of degradation from higher levels, we may say in general terms that by adopting the latter hypothesis as applying to all savages, the Duke sets himself in opposition to the theory of evolution as a whole. Moreover, he does not appear to have reflected that the question is not one which can be investigated or decided, as it were, in the lump. It is quite likely that some savages have fallen from a higher to a lower level of savagery; it by no means follows that all savages have done the same. Further, if we were to suppose that they did, from what level of civilised or of uncivilised life are we to suppose that they all started? This hypothesis, as a general explanation of the savage state of man is, indeed, as incoherent as it is obsolete; yet it is not more so than certain other views upon the savage state to which this writer gives expression. Thus, his chief contention is that savage man shows himself to be, as it were, out of joint with the rest of Nature, or, as he expresses it, an "evident departure" from the unity or order of Nature. Perhaps it is enough to say of a doctrine which from a scientific point of view is so peculiar, that it ought to have prevented the author from styling his book "The Unity of Nature."

We have no space left to consider the only other topic that calls for consideration in these columns, viz. the essay on the Moral Sense. The whole treatment of this subject appears to us most feeble. It is also most inaccurate, as the following quotation will suffice to show:—

"It has been laid down that evolution, in its most perfect conception, would be such that the development of every creature would be compatible with the equal development of every other. In such a system it is said there would be no 'struggle for existence—no harmful competition, no mutual devouring—no death' (Herbert Spencer, 'Data of Ethics,' chap. ii. pp. 18, 19). The inspired imaginings of the Jewish prophets of some future time when the lion shall lie down with the lamb, and the ideas which have clustered round the Christian heaven, are more probably the real origin of this conception than any theory of evolution founded on the facts and laws of nature."

It is needless to say that no more ridiculous travesty than this could well be imagined, or that no such absurdity as that which professes to be formally quoted from Mr. Spencer is to be found either under the reference given or in any other part of his writings. In short, this "most perfect conception" of evolution is a pure invention, which reads almost as if it were intended to misinform the uninformed. We do not, however, suppose that such is the case. This extreme of inaccuracy we take to have been reached by the habit of drawing upon "inner consciousness," until not only the whole sense and substance of other writings are perverted, but even the most pure and delicious nonsense is seen by "the mind's eye" to occur in particular words on a particular page of some other book.

If space permitted or need required, we could point out other inaccuracies, and even still greater absurdities, both in this chapter and elsewhere; but we have doubtless already said more than enough to show that "The Unity of Nature" can scarcely be considered a successful work from a scientific point of view.

GEORGE J. ROMANES

OUR BOOK SHELF

The Electrician's Directory, with Handbook for 1884. 67 pp. (London: *Electrician* Office, 1884.)

THIS work, now in the second year of publication, contains much information of use to electric and telegraphic engineers. Amongst its contents are comprised a list of new electric companies, a list of provisional orders granted by Parliament for electric lighting, a list of the "British Cable Fleet," a list of British railways and railway officials, a fairly complete directory of the professions and trades connected with electricity; also a large amount of statistical information about different kinds of dynamo machines, electric lamps, and telegraph tariffs, much of which will doubtless be out of date in twelve months' time. There is also an obituary of electricians deceased in 1883, a table by Mr. Geipel of the cost of electric conductors as calculated by Sir W. Thomson's formula, and a set of tables by Mr. Crawley for corrections of measurements in horse-power and in watts. These two sets of tables are the only portion of the work claiming independent scientific value. We object entirely to Mr. Crawley's gratuitous remark in the prefatory paragraph of his section that the accepted system of electric units was "really foisted upon electricians by men devoted more to theoretic than to practical work." Nothing could be further from the truth than to accuse Mr. Latimer Clark, Sir Charles Bright, who originated the system, and Sir William Thomson, who did so much to perfect it, of not being practical workers. As a matter of fact, *ohms, volts, farads*, and *webers* were used by practical electricians for years before they found their way into the text-books written by the theorists.

LETTERS TO THE EDITOR

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

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On a "Magnetic Sense"

SIR WILLIAM THOMSON, in his presidential address at the Midland Institute, which is reported in NATURE for March 6 (p. 438), draws attention to the marvellous fact that hitherto we have no evidence to show that even the most powerful electromagnets can produce the slightest effect upon a living vegetable or animal body. But Sir William "thinks it possible that an exceedingly powerful magnetic effect may produce a sensation that we cannot compare with heat, or force, or any other sensation," and hence he cannot admit that the investigation of this question is completed,—for although the two eminent experimenters named by Sir W. Thomson felt nothing when they put their heads between the poles of a powerful electromagnet, it does not follow that, therefore, every member of the human race would feel nothing.

May I be permitted to point out that some slight evidence already exists in the direction sought by Sir W. Thomson? Scattered in different publications there are numerous statements made by different observers in different countries during the present century, which, if trustworthy, indicate that upon certain human organisms a powerful magnet does produce a very distinct and often profound effect. Unfortunately, with the exception of the careful and excellent observations made by Dr. W. H. Stone, who tried Charcot's experiments on a patient of his at St. Thomas's Hospital, the observations referred to are singularly wanting in precision of statement and in a due recognition of the precautions needful in order to avoid fallacious or ambiguous results from illusions of the senses.

This being the case, an attempt is being made by the Society for Psychical Research to ascertain—by direct and careful experiment, extending over a wide range of individuals—whether any trustworthy evidence really exists on behalf of a distinct magnetic sense. The sectional Committee of that Society intrusted with this and cognate work has published a preliminary report,[1] which contains a fragment of evidence pointing in the direction of the existence of a magnetic sense in certain individuals. Three persons have been found by the Committee, who, when their heads were placed near the poles of a powerful electromagnet, could tell by their sensations when the magnet was excited or not. One of these "sensitives" told the investigating Committee accurately twenty-one times running whether the current was "on" or "off" from a peculiar and unpleasant sensation he alleges that he experienced across his forehead. Every precaution that suggested itself was taken to prevent the subjects gaining any information through the ordinary channels of sensation of what was being done at the contact-breaker placed in another room. But I am sure the Committee will gratefully welcome any criticism of their procedure or suggestions for future experiment which Sir William Thomson may feel inclined to give. The honorary secretary of the Committee is Mr. W. H. Coffin, Cornwall Gardens, S.W.

Two or three months ago one of the gentlemen who appeared to have this magnetic sense was in Dublin, and I took the opportunity of repeating with care in my own laboratory the experiments previously made at the Society's rooms in London. The result satisfied me that this individual did in general experience a peculiar sensation, which he describes as unpleasant, when his head was within the field of a powerful magnet. Nevertheless the keenness of his magnetic sense, if such it be, varied considerably on different days, and sometimes he stated that he could detect little or no sensory effect. Usually the effect was felt most strongly when the forehead was in the line joining the two poles; but one day, when he was suffering from facial neuralgia, he found that his face was the most sensitive part, and complained of a sudden increase of pain whenever the magnet was excited, his face being near the poles. Sufferers from neuralgia among the students of science may therefore have a new and useful career before them, in the pursuit of which

[1] *Proceedings of the Society for Psychical Research*, Part 3. (Trübner and Co.)

their increased torture will, it is to be hoped, be vanquished by a far stronger intellectual joy.

The peculiar and unpleasant sensation which the magnet appeared to produce on the subject just referred to was described as slowly rising to a maximum in fifteen or twenty seconds after the current had been sent round the coils of the electromagnet. In like manner the effect seemed to die down slowly after the contact was broken. Unknown to the subject, the circuit was closed and opened several times, and the magnetism correspondingly evoked or dissipated, the result being that there was a fairly accurate correspondence between the physical and the psychical effect. The faint molecular crepitation which accompanies the magnetisation of iron, and can be heard when the ear is very near the magnet, is, however, very apt to mislead the imagination. To avoid this, the subject was placed at a distance where this faint sound could not be heard, and he was then requested to walk up to the electromagnet, and, judging only from his sensations, to state if the current were "on" or "off." The experiment was made twelve times successively, and he was correct in ten out of the twelve trials. He had no means of seeing or hearing the contact-breaker; of course, it is possible for a trickster, using a concealed compass-needle, to be able to impose on a careless experimenter, but care was taken, and I have not the least reason to doubt the entire *bonâ fides* of the subject of this experiment. Obviously the foregoing observation is but of little value unless corroborated by a far more extensive series of experiments, conducted with the most stringent precautions to avoid the creation of illusory effects.

I have tried experiments with large helices encircling the limbs and head, and animated by powerful currents, but have not observed any peculiar sensory effect in my own case, though I am inclined to think the headache which I have often experienced when working with a large magnet may not be altogether an accidental coincidence. Meanwhile experiments are in progress in my laboratory to ascertain, if possible, whether any sensory effect is produced upon lower organisms. I hardly anticipate any affirmative results, but it seemed worth making a systematic investigation from minute structures up to man. Sir W. Thomson's address will, I hope, stimulate other workers in this field. W. F. BARRETT

Royal College of Science, Dublin, March 11

Instinct

I WRITE one more letter on this subject, in order to observe that I do not think the only remaining difference between Mr. Lloyd Morgan and myself is so great as it may be apt to appear. In my books I have been careful to point out the peculiar disabilities under which the science of comparative psychology labours from its necessarily ejective character. But while in Mr. Morgan's view these disabilities are so great as to render any science of comparative psychology impossible, in my view they are not quite so great. I quite agree with the quotation which he gives from Prof. Huxley on the crayfish; but this does not amount to saying that no science of comparative psychology is possible. We may still, for instance, feel perfectly certain that a dog is a more intelligent animal than a crayfish, and in this we have a purely scientific proposition.

The difference, therefore, between Mr. Morgan and myself is more apparent than real, and depends upon what we mean by "a science." This is the question that must be answered before we can proceed to consider the question raised by him, viz. "Is a science of comparative psychology possible?" In my estimation the possibility of a science is furnished wherever there is material to investigate. The more vague the material, the less exact must be the science, and on this account, no doubt, comparative psychology is the least exact of all the sciences. But so long as its subject-matter admits of any investigation at all, so long, it seems to me, comparative psychology is a science.

GEORGE J. ROMANES

The Remarkable Sunsets

WITH reference to the theory that the red sunsets are due to volcanic dust in the air, I think that the following extract from a letter which has been forwarded to me is of considerable interest. The writer is Mr. Frederick Spofforth, and his letter is dated January 29, from Collaroy, 150 miles from Sydney. It will be observed that the corroboration which he gives to the theory in question is the more striking from the fact of its being so completely unconscious. GEORGE J. ROMANES

"A most peculiar sight this summer are the sunsets. The sun always goes down as red as can be, and half the night there is the same roseate hue, which lasts till past midnight. Many causes are given for it, but nearly all differ.

"Another curious thing is the enormous amount of dust—even up here, where you see nothing but trees as far as the horizon on all sides. Some days the whole landscape will be covered in dust, and where the dust comes from nobody can tell. It is always worst in the early morning."

Right-sidedness

MR. LE CONTE (NATURE, xxix. p. 452) seems rather to complicate than to simplify this question. If the right side of his body shows more dexterity than the left, surely it is his left eye that should share this excellence, if we are to suppose that this difference in dexterity depends upon any central origin. A person paralysed on the left side of the body loses sight—if sight be lost at all—in the right eye, and *vice versâ*. Further, I am right-handed, and use an eyeglass in my left eye; yet, though the right eye is the weaker, I use it for a telescope or microscope by unconscious preference. On the other hand, most persons who use a single eyeglass wear it in the right eye. I may have adopted the left for ease in adjusting the glass, so that my right hand might be free. When I am reading, if I put my hand in front of my left eye, I am conscious of some muscular alteration; if I obscure my right eye, I notice nothing but a slight diminution of the sense of light, white objects seeming less white to my right eye than to my left. And this effect is just as noticeable when I wear spectacles as when I am reading without them; so that my myopia is not the cause of the difference.

In discussing right-sidedness—whether we regard the decussation of the nerves in the medulla oblongata or not—we must not forget that prize-fighters normally strike with the left hand, using the right as a guard or to deliver the second blow; perhaps this is to gain the advantage of the greater strength of the right leg. Moreover, the habit among Western nations of writing from left to right appears to argue that right-handedness is the rule among them: but Orientals reverse the process, so that the majority of mankind must be left-handed. What do the anthropologists say to this?

Mr. Charles Reade, writing in the *Daily Telegraph* some years ago, argued that if the habitual use of the right hand led to a greater development of the left side of the brain, a further acquired use of the left hand would aid the development of the right cerebral hemisphere, and so increase the general power of the brain. But is there any evidence to show that ambidextrous people, left-handed apparently by nature, and right-handed from habit, have any general mental advantage over their fellows? I think not. HENRY T. WHARTON

39, St. George's Road, Kilburn, March 17

IN my own experience (I can with confidence only give that) I differ almost wholly from that of Mr. Joseph Le Conte, as expressed in NATURE (p. 452). In my case strength and dexterity of arm do not *in everything* go together. For instance, although strongly left-handed, I learnt to write with the right hand and shoot from the right shoulder, and could do either very indifferently indeed if attempted with the left hand or arm. I perhaps may call myself with truth a rather handy man, improved upon by living for many years in places where tradesmen were not to be had. In all connected with pencil, pen, ink, and paper, such as printing, chart-making, my left hand, although strongest, was clumsy, whereas my right showed considerable skill, as was exhibited once in rather a ludicrous manner by the Hydrographer of the Admiralty mistaking my pen-and-ink chart of some seven hundred miles of Arctic discovery for an engraving of the same. My left leg is the stronger, yet I use it in kicking and in other ways requiring dexterity; *e.g.* when very many years younger I could perform the many curious movements or steps of some of our Scottish dances with much more accuracy and ease with the left foot than with the right. I fear the subject-matter of this note may be scarcely considered a valid excuse for so much self-notice. JOHN RAE

4, Addison Gardens, March 15

Ravens in the United States

On p. 336 of Nature for February 7, Manhattan asks a question about "ravens." I do not propose to answer his question, but to state a fact. I was raised from boyhood to manhood in Tioga Co., Penn., and in my boyhood days, when the primeval forests were broken only by the recent settler's small patch scattered here and there along the valleys, the raven was as common as the crow; nor could the one ever be mistaken for the other. Before I had attained the years of manhood, however, the raven had become a *rara avis*, while the crow, on the contrary, had become vastly more abundant. The bald-eagle, and the fish-hawk, too, were then very often seen, now seldom or never. Other birds could be added to the list if desirable. The question, *why?* is not so easily disposed of as it is to state the fact. Should one be disposed to answer by saying *the rifle*, it would be pertinent to reply that the rifle was just as active against the crow, the common hen-hawk, and the crow-blackbird, as it was against the raven, the fish-hawk, and the bald-eagle; but these latter birds have all disappeared, while, in spite of the rifle, the former have increased. We must look deeper for the cause. Ira Sayles

Washington, D.C., March 3

In answer to the query of your correspondent "Manhattan," who writes from New York, under date of Jan. 11, concerning the prevalence of ravens in the United States, I would like to remark that ravens quite replace the crow in Nevada. I have never seen them here in the east. Mr. Ridgway who was with me in 1867-68 could give you much valuable information in regard to their habits and range. W. W. Bailey

Brown University, Providence, R.I. (U.S.A.), March 1

Thread-twisting

In Nature, January 31 (p. 305), I read some remarks by Prof. E. B. Tylor on a "rude method of making thread by rolling palm or grass fibre into a twist with the palm of the hand on the thigh," which Prof. Tylor regards as a "savage art" of old native tribes of Guiana, who were thigh-twisters. I have often seen shoemakers when at work prepare their threads by twisting them on the thigh with the palm of the hand. May this practice be one which has survived from a barbarous period?

Truro, March 14 J. S.

BICYCLES AND TRICYCLES IN THEORY AND IN PRACTICE [1]

When I was honoured by the invitation to give this discourse on bicycles and tricycles, I felt that many might think the subject to be trivial, altogether unworthy of the attention of reasonable or scientific people, and totally unfit to be treated seriously before so highly cultured an audience as usually assembles in this Institution. On the other hand, I felt myself that this view was entirely a mistaken one, that the subject is one of real and growing importance, one of great scientific interest, and, above all, one of the most delightful to deal with that a lecturer could wish to have suggested to him.

It is quite unnecessary for me to bring forward statistics to show how great a hold this so-called new method of locomotion has taken upon people of all classes: the streets of London, the roads and lanes in all parts of the country, testify more forcibly than any words of mine can do to what enormous numbers there are who now make use of cycles of one sort or other for pleasure or for the purposes of business.

Not only has the newly developing trade brought prosperity to towns whose manufactures were dying a natural death, but the requirements of cyclists have given rise to a series of minor industries, themselves of great importance. Riders of bicycles and tricycles come along so silently that instruments of warning have been devised. There are bells that jingle, bells that ring, whistles, bugles, and a fiendish horn which will utter anything from a gentle remonstrance to a wild, unearthly shriek. Lamps, tyres, saddles, seats, springs, &c., are made in unending variety; these form the endless subject of animated conversation in which the cyclist so frequently indulges. Cyclometers or instruments for measuring the distance run are also much used. Some show the number of revolutions made by the wheel, from which the distance can be found by a simple calculation; others indicate the distance in miles. There is on the table a home-made one of mine with a luminous face which at the end of every mile gives the rider a word of encouragement; it now indicates that a mile is nearly complete; in another turn or two you will all hear it speak.

Cyclists have a literature of their own. There are about a dozen papers wholly or largely devoted to the sport. They can even insure themselves and their machines against injury by accident in a company of their own.

The greatest and by far the most important growth is the Cyclists' Touring Club, a gigantic club to which every right-minded rider in the country belongs. This club has done more to make touring practically enjoyable than could have been thought possible when it began its labours. Railway companies have with few exceptions consented to take cycles at a fixed and reasonable rate; in almost every town in the country an agreement has been made with the leading, or at any rate a first-class, hotel, in virtue of which the touring member may be sure of meeting with courtesy and attention for himself and with clean quarters and an intelligent groom for his horse, instead of finding himself as hitherto a strange being in a strange place at the mercy of some indifferent or exorbitant landlord. In consequence of this, thousands now spend their holidays riding over and admiring the beauties of our own country instead of being dragged with a party of tourists through the streets and buildings of a foreign town. Of the delightful nature of a cycling tour I can speak from grateful experience; last autumn alone I travelled nearly 1500 miles, meeting on my way with almost every variety of beauty that the scenery of this country affords. Wherever I went I felt the beneficial influence of the C.T.C., as the touring club is called. At all the hotels—our headquarters—at which I stopped, I found the most sanguine wishes of the club amply fulfilled, our wants understood and provided for.

The C.T.C. have also done a great service in providing us with a uniform which has been proved to be as near perfection as possible. They have also designed a ladies' cycling dress, which can be seen in the library.

Though touring in the country is the perfection of our art, town riding has its advantages. I, in common with a fair number, ride daily to and from my work no matter what the weather may be: rain, snow, wind, or hail, cycling affords the pleasantest means of crossing London. Instead of waiting in draughty railway stations, of catching cold outside or being stewed inside omnibuses, or of being smoked in the Underground Railway, we, the regular cyclists, look forward to our daily ride with pleasure, for the healthy exercise, the continuous necessity of watching the traffic and avoiding ever-approaching danger, form between them a relief from mental worry or business anxiety which we alone can appreciate.

Of the dangers of the streets I have little to say: the regulation of the traffic by the police, and the consideration of drivers, though they are not in general too fond of us, make danger in the quarter from which it might be expected very remote. Our chief difficulty is due to the irregular and utterly unaccountable movements of pedestrians, whose carelessness keeps us in a continual state of anxiety.

There remains one point of the utmost importance on which I would say a few words, I refer to the effect of cycling on our general health. About a year ago there appeared in the *Lancet* an article condemning in no

[1] Lecture delivered by C. Vernon Boys, A.R.S.M., at the Royal Institution, March 7.

measured terms the evils likely to result from the development of this new craze, in which, as far as I remember, it was stated that we are now sowing the seeds of a series of new diseases, the symptoms of which will only appear possibly in years to come. I would not for a moment question the accuracy of opinion held by any professional man; whether this is or is not the case I cannot tell; however, I may mention that the only symptoms which I have so far discovered in myself are an improved appetite, increased weight, and a general robustness to which I was formerly a perfect stranger. Having, I trust, succeeded in showing that the advantages offered to riders are sufficient to account for the rapid development of cycling, that it is in fact no mere temporary craze, I shall now proceed to consider the theory and construction of the various machines at present known.

From the hobby-horse to the bone-shaker, and from the bone-shaker to the bicycle, the steps are so simple and obvious that it is quite unnecessary for me to trace them. It is also needless for me to describe the modern bicycle: every one must be familiar with it, every one must have seen the ridiculous zigzag of the beginner, and have admired the graceful gliding of an accomplished rider. Of the theory of the balance little need be said; anything supported in a mere line, in unstable equilibrium as it is called, must fall one way or the other. The machine and rider would of necessity capsize if some action of recovery were not possible. To whichever side the machine shows any inclination, to that side the rider instinctively directs it. By this means the tendency to fall to one side is balanced by the property of the rider to continue moving in a straight line, and so to go over on the other side. This action of recovery is always overdone, so that a second turn in the opposite direction must follow. Hence the extraordinary path traced by the beginner. Even with the most skilful rider, though he appears to travel in a perfectly straight line, a slightly sinuous course is essential, as the highly characteristic track left on the road indicates. If anything should happen to check this slightly serpentine motion, as, for instance, occurs when the driving-wheel drops in the groove of a tram-line, the balance at once becomes impossible, and the rider is compelled to dismount.

The extraordinary stability of the bicycle at a high speed depends largely on the gyroscopic action of the wheels. On the table is a top supported in a ring which is free to move how it pleases. So long as the top is spinning the ring is as rigid as a block; on stopping it, the freedom of the support is at once apparent.

It is a marvel to many how anything so light, how anything so delicate, can carry the weight or can travel at the speed so common without utterly collapsing. The wheels especially attract attention. In a hoop no one part can be pushed in unless some other part can go out. A bicycle wheel is a hoop in which every part is prevented from going out by the tension of the spokes. To give the wheel lateral stability, the spokes are carried not to the centre, but to the two ends of the hub, thus lying on two cones. Such a wheel is abundantly strong in its own plane: it can withstand the jars and shocks of a bad road without a groan, but once subject it to serious side strain, such as I can with ease put upon it with a jerk of my wrists, and the wheel will crumple up like an umbrella in a storm. Till this year there has been no change in the principle of construction, though in detail many improvements have been carried out and are largely adopted. By the use of hollow rims a stiffer and lighter wheel can be made; thick-ended, crossed and laced spokes are employed, and other details modified. Essentially, however, the "spider" wheel as a structure is the same as it was when first introduced. Suddenly two radical changes are presented to us. Mr. Otto, whose great work I shall describe in its proper place, has devised a wheel on a new system, in which the spokes that form the structure lie in the plane of the rim, in which position they are best able to withstand direct shocks. Such a wheel would be unstable, but requires very little to keep it true. Delicate spokes, not screwed up very tight, are therefore placed on either side, so that a side-strain is met by the whole strength of the spokes on one side, which are not as hitherto weakened by the pull of the spokes on the other. On this system much narrower wheels can be made than was possible before. The other change, due to the same inventor, is still more striking. He has found, contrary to the opinion of every one, that wheels, either of his narrow type or of the usual form, can be made and will remain true when the spokes are made elastic by being bent into a wavy or slightly spiral form. If only these wheels will stand the test of time—and I see no reason why they should not—one of the greatest discomforts and possible causes of injury from which the cyclist suffers—the vibration and jolting due to a bad road—will have been removed.

The bearings in a bicycle are perhaps more to be admired than any single part. Instead of allowing the axle to slide round in its bearings, hard steel rollers or balls are introduced, so that the parts which are pressed together roll over and do not slide upon one another. Any one who has trodden on a roller or a marble must have found in a possibly unpleasant manner the great difference between rolling and sliding friction. I can now give for the first time the result of an experiment only completed this morning, which shows the extraordinary perfection to which this class of work has attained. I have observed how much a new set of balls which I obtained direct from the well-known maker, Mr. Bown, has lost in weight in travelling 1000 miles in my machine. Every 200 miles I cleaned and weighed the balls with all the care and accuracy that the resources of a physical laboratory will permit. The set of twelve, when new, weighed 25·80400 grm. After 1000 miles, they weighed 25·80088 grm., the loss being 3·12 mgrm., which is equal to 1/20·8 grain, that is, in running 1000 miles, each ball lost 1/250 grain. This corresponds to a wear of only 1/158,000 inch off the surface. At this rate of wear—3·12 mgrm. per 1000 miles—the balls would lose only 1/34·3 of their weight in travelling as far as from here to the moon.

The twelve balls, after the first 200 miles, each weighed in grammes as follows. The loss of each in running 600 miles is appended:—

Weight in grm.	Loss in 600 miles	Weight in grm.	Loss in 600 miles
2·16605	·00050	2·14725	·00020
2·16180	·00025	2·14725	·00020
2·15550	·00035	2·14700	·00020
2·15480	·00015	2·14500	·00020
2·15000	·00015	2·14280	·00025
2·14730	·00015	2·13875	·00020

I did not weigh each ball on the first and last occasion. However, the wonderfully uniform wear in the intermediate 600 miles speaks well for the equal hardness of the balls.

The wear of the dozen during each journey of 200 miles was as follows:—

Miles	Wear in grm.
0—200	·00055
200—400	·00070
400—600	·00055
600—800	·00075
800—1000	·00062

I have given the results of these experiments at length, for I do not think that accurate and systematic observations of the kind have been made before.

We may consider, then, that the balls are practically indestructible. Knowing this, Mr. Trigwell has applied the ball-bearing to the construction of the "head" of the bicycle, not so much with the view of diminishing the friction there, but of preventing wear in a place where any shake is highly objectionable. One of his ball-heads is on the table.

The frame of the bicycle, consisting merely of the fork and backbone, is made of thin steel tube, the type of all that is light and strong. Indiarubber, besides being used for the tyres of all machines, has been worked into every part of the structure to diminish, so far as is possible, that perpetual and wearying vibration of which all bicyclists so bitterly complain. The number of improvements in every detail is so great that any attempt to enumerate them is out of the question. Suffice it to say that the modern bicycle is the perfection of all that is perfect; as a machine for racing, as a machine for hurrying over good and level roads nothing can approach it. Unfortunately, however, there is ever present danger, and danger of the most objectionable sort, for the most skilful rider knows too well that should he strike a stone of even an ordinary size he must expect to be pitched over the handles, and come with a crash to the ground. It is true that in general no harm is done, but such a fall may bring any one to a sudden and horrible end.

Many have attempted, while still retaining the advantages of the bicycle, to make these involuntary headers impossible by modifying in some way its construction. One of the earliest attempts in this direction is well named the "Extraordinary." On it the rider is placed much further behind the main wheel, but can still employ his weight to advantage, as the treadles are placed below him and are connected by levers with the cranks. In another safety bicycle a third wheel is carried in front, just above the ground, so as to resist at once any tendency to tilt forward. In another type much smaller wheels are employed, and the feet, now nearer the ground, are connected with the cranks, by levers in the "Facile," or by a hanging pedal in the "Sun and Planet." There is a bicycle with two large wheels—one in front of the other—which two can ride, which should be both safe and rapid.

By far the most curious and utterly unintelligible of all machines of the bicycle type is Mr. Burstow's "Centre-cycle." So incomprehensible did this machine seem to me that I took the trouble one afternoon last week to ride to Horsham to see it in its native place. A careful examination has convinced me that it is not only correct in its design, but that it is in many respects the most wonderful cycle at present made. There is on the table a model Plympton skate. When this is level, it runs straight; when inclined either way, it wheels around in a manner that was so familiar a few years ago. The four wheels of the Centre-cycle are a counterpart of the four wheels of the skate; when the frame leans either way, they turn in an appropriate manner, or, conversely, when they turn, the machine leans in the proper direction. It might be thought that a thing with five wheels is more nearly allied to a tricycle than to a bicycle; but this is not so, for the Centre-cycle, when ridden skilfully, has rarely more than one wheel on the ground; the leaning to one side in turning a corner (tricycles unfortunately must remain upright), and the general action is essentially that of a bicycle. The great peculiarity of this machine is the power that the rider possesses of raising or lowering any wheel he likes. Now that I have mounted it you will see that I can rest on one, three, four, or five wheels as I please. In consequence of this power of lifting the wheels, a rider can travel over an umbrella without touching it, lifting the wheels as they approach, and dropping them as they pass, after the manner of a caterpillar.

Whatever difficulty I may have had in doing justice to the bicycle, the corresponding difficulty in the case of tricycles is far greater. The number of makers and the variety of their work is so great that it would be sheer madness on my part to attempt to describe all that has been done. Those who wish to see the great variety of detail which chiefly constitutes the difference between one make and another must go to one of the exhibitions of these things which are now so common.

All I shall attempt will be an explanation of the leading principles which are involved in the design of a tricycle. For this purpose it will be necessary for me to mention occasionally some particular machine; but in justice to the hundreds to which I cannot even refer, I wish it to be understood that those named, though typical, are not of necessity better than any other.

Till a few years ago the bicycle was the only velocipede which was worthy of the name. Inventive genius and mechanical skill have given rise to a series of machines on three wheels on which any one can at once sit at ease, and which require but little skill in their management. Men who do not care to risk their necks at the giddy height of the bicyclist, ladies to whom the ordinary bicycle presents difficults which they cannot well surmount, each find in the tricycle the means of obtaining healthy and pleasant exercise, and of enjoying to a certain extent the advantages which the bicycle affords. Thanks to the perfection of the modern tricycle, cycling has become one of the most popular institutions of the day.

It is first necessary to know what combinations of three wheels will, and what will not, roll freely round a curve. The few possible arrangements determine the general forms which a tricycle can take. A wheel can only travel in its own direction; no side motion is possible without the application of considerable force, entailing strain and friction of a most injurious kind. In any combination, then, of three wheels, each must be able, in spite of the united action of the other two, to move in its own direction. There is on the table a model in which the three wheels can take every possible position. To begin with, two large ones are placed opposite to, but independent of, one another, and parallel, and a small one, parallel to the others, is mounted between them at one end. This arrangement rolls along in a straight line with perfect freedom; on twisting the plane of the third wheel it is also free to roll round a curve whether the little wheel is before or behind. If I shift the position of one of the large wheels so that, though still parallel to, it is no longer opposite, the other, then, though they can freely move in a straight line, they can by no possibility be induced to roll round a curve. It is clear, then, that two wheels that are parallel cannot be employed in a tricycle unless they are opposite one another. The only class of people who frequently appear to be familiar with this fact are nursemaids, who always tip up the front of a perambulator in turning a corner.

If one wheel is in front of and another behind a third, the combination can only roll round a curve when the front and rear wheel are turned to proportionate extents in opposite directions. The model is so arranged now; if either of the little wheels is not turned to exactly the right amount, they can no longer roll, they can only be dragged round a curve. It is not sufficient that two parallel wheels should be opposite one another, they must be able to turn at different speeds. I have now the two large wheels keyed on the same axle, so that they must of necessity turn together; this combination is ready enough to go straight, but no amount of encouragement by the steering wheel will induce it to go in any other direction.

Bearing these facts in mind, it will not be difficult to follow the development of the tricycle. It would seem impossible in the first arrangement (that with two wheels opposite one another, and a third, or steering wheel, before or behind between them) to drive both sides, for the wheels must be able to turn at different speeds; let therefore one be free to go as it pleases, if the other only is driven, we have at once a very common form of tricycle, in which one wheel drives, one steers, and one is idle.

Machines of this class have many defects. The feeble steering power, combined with their unsymmetrical driving, render them altogether untrustworthy. If any power is applied to the driver, which can only have its share of the weight upon it, it slips on the ground; if the machine is quickly stopped, owing to the small weight on the steering wheel, it is apt to swing round and upset; nevertheless, those who are content with pottering about on our wood pavement and gravel roads find this class of machine answer their purpose, and owing to their cheapness and simplicity they do not care to get a better.

The second arrangement of the model, in which riders must have recognised the Coventry Rotary, is free from most of the defects of the form just described; there is more weight on the driver, but not enough to prevent its being made to slip round; there are two steering wheels a long way apart, with plenty of weight upon them, so that the guiding power in this type of tricycle is all that can be desired.

Let me now return to the first arrangement, in which two parallel wheels are opposite one another. If by any possibility both wheels could be driven, and yet be free to go at different speeds, then there being so large a weight on the drivers they could not be made to slip; the driving being symmetrical, most of the twisting strain would be taken off the steering wheel, and still the machine would be capable of rolling round a curve with perfect freedom.

All the methods of solving the problem of double driving come under two heads, one depending on the action of a clutch and the other on differential or balance gear.

The clutch action being the simplest, I shall describe that first. In going round a corner the inner wheel must lag behind, or the outer wheel must run ahead of the other; as either wheel may be inner or outer according to the direction of the curve, each must be able to lag behind or each must be able to run ahead. If both were able to lag behind, the machine could not be driven forward, and it would be of little use; if both were able to run ahead, the machine could not be driven backwards—a matter of small importance. There is on the table a large working model, showing how a four-sided wheel is free to revolve in a ring, but is instantly seized when turned the other way, owing to a jambing action on one or more of four rollers. The four-sided wheel then can be employed to drive the ring one way but not the other. One of these "clutches" or "friction grips" is placed at each end of the crank shaft in the "Cheylesmore" tricycle, and a chain round the ring of each drives the corresponding wheel. The machine named is a rear-steerer; the clutch is also employed in some front-steerers.

The other method of double driving depends on the use of the well-known gear of three bevel wheels or of some equivalent mechanism. If the axle of the middle of the three wheels is turned round the common axle of the other two, the applied force is divided between those two wheels, yet the pair are free to move relatively. Let then the chain drive a wheel carrying the middle bevel, and let the side bevels be connected with the two drivers. Whatever happens, the power of the rider will be equally divided between them, yet the machine will be free to roll round a curve.

There are a great number of devices which are exactly equivalent to this the simplest of all, which is known as Starley's gear. There is on the table a beautiful model of the gear used in the Sparkbrook tricycle, which has been lent me by the makers of that machine, Bown's differential gear, and some others; but time will not allow me to describe them. There is one gear, however, which presents many peculiarities, which I have devised, and which may be of interest. A large working model is on the table. Between the conical edges of two wheels which are connected to the drivers lie a series of balls, outside which is a ring with sloping recesses. If the ring be turned by a chain or otherwise, the balls jamb in the recesses as the rollers do in the clutch gear. Nevertheless they are free to turn about a radial axis, and so allow the two driven cone wheels independent motion. The bursting strain on the ring and the side thrust on the cones acting on rolling balls balance one another. With this gear the rider can cause the balls to jamb one way or both ways, and so have or avoid the "free pedal" as he pleases.

In almost all good designs of front-steering tricycles the power applied to the cranks is transmitted to a differential gear by a chain. The crank and connecting rod have also been used to transmit the power, but then the clutch is necessary.

There is, however, another type of tricycle, in which the use of cranks is avoided, among which may be mentioned the "Omnicycle," the "Merlin," and that highly ingenious machine, the rowing tricycle. On the table there is the Omnicycle gear. In all these the power is applied direct to the circumference of a wheel or sector, and so dead points are avoided, which is a point in their favour when meeting with much resistance. On the other hand, the sudden starting and stopping of the feet in the two former machines and of the body in the latter make this type utterly unsuitable for obtaining anything more than a moderate speed. In the Omnicycle ingenious expanding drums are employed, so that the power may be applied with different degrees of leverage according to circumstances.

There remains one type of tricycle which, for rapid running, surpasses many: I refer to what is known as the Humber pattern. So excellent is this form in this respect that the leading manufacturers have, by turning out machines on the same lines, paid the original makers a compliment which is not altogether appreciated. This pattern departs less from the ordinary bicycle than any other; it is one, in fact, in which, instead of one, there are two great wheels, giving width to the machine, between which the power is divided by the usual differential gear.

Having spoken of the differential gear and the clutch, I had better show the comparative advantages and disadvantages of the two methods of double driving. With the differential gear the same force is always applied to each wheel, so in turning a corner the outer one, which travels furthest, has most work expended upon it (work = force × distance). In this respect the differential gear is superior. On the other hand, when one wheel meets with much resistance from mud or stones, and the other with hardly any, the latter has still half the strength of the rider spent upon it, which is clearly a mistake. With a clutch-driven machine running straight, the wheels take such a share of the rider's power as is proportional to the resistance they individually meet. When the machine is describing a curve, that is generally, only the inner wheel is driven, and the machine is for the time only a single driver, with the driver on the wrong side.

I must now describe some devices which are attracting much attention at the present time, the speed and power gears. Let us suppose there are two machines with wheels of different sizes, but in other respects alike. Then each turn will take the larger wheeled machine further than the smaller. In going up a hill the larger wheel will take its machine up a greater height than the other in one revolution, which involves more work and therefore more strength. If on the large wheel the chain pulley were increased in size, then for the same speed of the treadles it would not turn so quickly, it would not take the machine so far up the hill as before, it would in fact be equivalent to a smaller wheel, so that less strength than before would be necessary. This diminution of speed, though of great advantage when climbing a hill, is the reverse on the level, for then very rapid pedalling would be necessary to maintain even a moderate speed. To obtain the advantage of high wheels or high gearing on the level and at the same time low wheels or low gearing on the hills, some highly ingenious devices are employed. On the table is a well-known one of these, the "Crypto-dynamic," which by a simple movement changes the relative speed of wheel and treadle Time will not permit me to describe the details of this arrangement, but it contains an epicyclic gear which is or is not in action according as the rider desires power or speed. There are several other devices having the same object, some depending on an epicyclic gear in a pulley, others on the use of two chains, only one of which is active at a time. These arrangements have the further advantage of enabling the rider to disconnect the treadles from the wheels whenever he pleases.

Tricycles on which two, three, or a whole family can go out for a ride together, involve few new principles, and I shall not for this reason have a word to say about them.

There remains one machine forming a class by itself, more distinct from all others than they are from one another. It is not a bicycle in the ordinary sense of the word; it is not a tricycle, for it has only two wheels. This machine is, from a scientific and therefore from your point of view, more to be admired than any other. It is called, after its inventor, the "Otto." The Otto bicycle and the Otto gas-engine will be lasting memorials to the ingenuity of the brothers who invented them.

No machine appears so simple, but is so difficult to understand as this. Tricyclists who have been in the habit of managing any machine at once, are surprised to find in this something which is utterly beyond them. They cannot sit upon it for an instant, for so soon as they are let alone it politely turns them off. When at length, after much coaxing, they can induce it to let them remain upon it, they find it goes the way they do not want. Riding the Otto, like any other accomplishment, must be learnt. Some seem at home on it in half an hour, others take a week or more. It is not surprising that that quick perception, in which ladies have so much the advantage of men, enables them to quickly overcome the apparently insurmountable difficulties which this machine presents to the beginner.

The rider when seated is above the axle of two large equal wheels; being then apparently in unstable equilibrium, he would of necessity fall forwards or backwards if some movement of recovery were not possible. The Otto rider maintains his balance in the same way as the pedestrian. If he is too far forward, pressure on the front foot will push him back; if too backward in position, pressure on the rear foot will urge him forward. That this must be so is clear, for, whatever turning power he applies to the wheels, action and reaction being equal and opposite, they will produce an equal turning effect upon him. The steering of this machine is quite peculiar. In the ordinary way both wheels are driven by steel bands at the same speed; so long as this is the case, the Otto of necessity runs straight ahead. When the rider desires to turn, he loosens one of the bands, which causes the corresponding wheel to be free; if then he touches it with the brake or drives the other wheel on, it will lag behind, and the machine will turn. It is even possible to make one wheel go forwards and one backwards at the same time, when the machine will spin like a top within a circle a yard in diameter.

There being no third wheel the whole weight is on the drivers, the whole weight is on the steerers; the frame, which is free to swing, compels the rider to take that position which is most advantageous, making him upright when climbing a hill, and comfortably seated when on the level. Owing to a curious oscillation of the frame which occurs in hill climbing, the dead points are eliminated, so the rider need not waste his strength at a position where labour is of no avail.

Though it has been impossible for me to do more than indicate in the most imperfect manner how numerous and beautiful are the principles and devices employed in the construction of cycles, I trust I have disappointed those who were shocked and horrified that so trivial a subject should be treated seriously in this Institution.

DANGERS FROM FLIES

IN a note communicated to the *Gazzetta degli Ospitali* for August 1883, and republished in the current number of the *Archives Italiennes de Biologie* (tome iv. fasc. ii.), Dr. B. Grassi calls attention to the fact that flies are winged agents in the diffusion of infectious maladies, epidemics, and even parasitic diseases. During the summer season, when flies occur in swarms, it seems impossible to prevent them from settling on any and every object. In these countries, though sometimes troublesome, they are scarcely ever so numerous as in the warmer climates of the Continent, and even in these latter they are not often to be found such plagues as they are in Egypt; but in all these countries alike they may be seen to alight on all moist substances without distinction. It may be the expectorations of a phthisical or the ejecta of a typhoid patient that have last attracted these inquiring diptera; but, irrespective of the material they may have been investigating, their next visit may be to the moist lips or eyes of a human being. Their feet, their mouth, and the pectoral portion of their bodies will have all come in contact with the infective mass, and will all in turn be more or less cleansed of it by the moisture of the freshly visited mucous membranes. But this danger has already been known and recognised, and it seems scarcely doubtful that in Egypt ophthalmia is constantly carried to the eyes of the infant natives by such winged visitors. Dr. Grassi calls our attention to even greater danger, and this from the ejecta of the flies themselves. Every housekeeper knows how the bright surface of a mirror or the gilt moulding of a picture-frame can be covered over with the little flecks left by these flies,—no English words occur to us to translate therewith the phrase "les méfaits des mouches." The following experiences of Dr. Grassi relate to these:—At Rovellasca, between his laboratory, which is on a first floor, and his kitchen, which is on the ground floor, there lies a court-yard, with a distance between the windows of the two rooms of about ten metres. On a plate on the table of his laboratory he placed a large number of the eggs of a human parasite (Trichocephalus). After a few hours he found, on some white sheets of paper hanging in the kitchen, the well-known spots produced by the excreta of the flies, and on a microscopical examination of these spots, several eggs of the parasite were found in them. Some flies coming into the kitchen were now caught, and their intestinal tract was found quite filled with an enormous mass of fœcal matter, in which the presence of eggs of Trichocephali were detected. As it was practically impossible to keep all alimentary substances from contact with these flies, it follows that the chances of Dr. Grassi and his family being infected with Trichocephali were very great. As a matter of fact, the experiment was tried with non-segmented eggs of this worm. Another experiment was in the same direction. Dr. Grassi took the ripe segments of a *Tænia solium* (which had been in spirits of wine) and broke them up in water, so that a great number of the tapeworm's eggs remained suspended in the fluid. The flies came to the mixture, attracted by the sugar, and in about half an hour the ova of the tapeworms were to be found in their intestines and in the spots. Had these eggs been in a recent and living state, they would doubtless have been just as easily transported. To those who care to try these experiments, it is suggested that lycopod powder mixed with sugar and water is a good material, as the lycopod spores are easily detected.

It is self-evident that if the mouth-apparatus of the fly will admit of the introduction of such objects as have been above noted, that there will be no difficulty in its admitting scores of the spores of many parasitic fungi, and above all of those belonging to the Schizomycetes, the possible cause of so much disease. Already has Dr. Grassi detected in fly excrement the spores of *Oidium lactis*, and the spores of a Botrytis, this latter taken from the bodies of silkworms dead of muscardine.

There arises, of course, the question of how far the active digestion in the intestines of the flies may not destroy the vitality of germs or spores thus taken in, but it would seem probable that in many instances the larger bodies swallowed may not serve as objects for assimilation, but may be got rid of as foreign bodies, and it will be borne in mind that the flies themselves fall victims to the growth of a parasitic fungus (*Empusa muscæ*, Cohn), which is probably taken first into their own stomachs.

Dr. Grassi promises to publish the results of his experiments in fuller detail. Judging of their interest by this abstract, they will well deserve to be followed up, and though in these countries our modern sanitary arrangements do not tend to the development of such immense swarms of flies as are so constantly to be met with in Italy, still the dangers to be apprehended from them there are possibly, though in a less degree, to be encountered here, and the investigation of the fact is easy to any one possessing a fairly average microscope and the power of catching a fly. E. P. W.

EDINBURGH MARINE STATION

AT the half-yearly meeting of the Scottish Meteorological Society held on Monday last, Mr. Murray submitted a statement on the work done by the Fisheries Committee. This included preliminary reports from the Rev. A. M. Norman on the invertebrate fauna of the Scottish fresh-water lochs; Prof. Herdman's report of his researches connected with the fisheries of Loch Fyne, and similar reports from Messrs. Hoyle and Beddard from Peterhead and Eyemouth. After reading several interesting extracts from these reports, which will shortly appear in the Society's *Journal*, he then stated that the marine station at Granton would be formally opened for scientific work about the 10th of next month by Prof. Haeckel of Jena. The floating laboratory, which has been named the *Ark*, was successfully launched on Saturday last, and it has accommodation for seven biologists. The steam yacht of thirty tons, which is to be called the *Medusa*, is to be launched on the 26th inst. at Glasgow, and will be at the station ten days thereafter.

The Station will then be possessed of the three most important requisites, viz. the floating laboratory, with abundance of sea water; a steam vessel fitted with all modern appliances for sounding, dredging, and other biological and physical investigations; and lastly, a most complete library in marine biology and physics. Mr. J. T. Cunningham, B.A. Oxon., Fellow of University College, Oxford, has been appointed Naturalist in charge of the Station; Mr. Hugh Robert Mill, B.Sc., who holds a Research Fellowship in the University of Edinburgh, is to carry on physical and meteorological investigations under the superintendence of Prof. Tait; Mr. Alexander Turbyne, fisherman, Keeper; Mr. William Bell, late Royal Navy, Engineer; and it is hoped the arrangements will shortly be made that will enable a botanist and geologist to carry on systematic work at the Station. The captain of the yacht will be appointed next week.

British and foreign naturalists are invited to make use of the resources of the Station free of charge, and those who desire to do so are requested to communicate with Mr. John Murray, *Challenger* Office, Edinburgh, stating the kind of work they propose to undertake and the length of time they will probably remain. Efforts are now being made to provide living accommodation for the naturalists and others who may be working at the Station. Immediately after the meeting Mr. Murray received anonymously a donation of 100*l.* towards the further equipment of the Station. We wish every success to this undertaking, and, from the liberal spirit shown in placing at the service of scientific men the unique facilities afforded by the Station for the prosecution of inquiries of the highest practical importance, we have every confidence that the public will not be slow in seeing that the funds required for its efficient maintenance are forthcoming.

THE DEEP-SEA FISHES OF THE "TALISMAN"

AMONG the many wonderful animal forms collected during the voyage of the *Talisman* none surpass the fishes in interest. In the exhibition, now open at the Jardin des Plantes, Paris, of the various specimens collected during this voyage, the collection of fishes holds a chief place. During the cruises of the *Travailleur*, owing to the apparatus employed, the capture of a fish was a rare event, but by the employment of a kind of drag-net on board the *Talisman* the number both of species and individuals taken was quite surprising. Once, on July 29, in 16° 52′ N. lat. and 27° 50′ W. long., in one haul of the drag-net no less than 1031 fishes were taken from a depth of 450 metres. The chief surface fish noted in M. Filhol's very interesting papers, which are in course of publication in our French contemporary *La Nature* (to the editor of which journal we are indebted for the illustrations accompanying this notice), were the well-known shark (*Charcharias glaucus*), very common between the Senegal coast and the Cape de Verde Islands; its strange attendant fish, the so-called pilot fish (*Naucrates ductor*), and the very curious and odd-looking fish of the Sargassum Sea, *Antennarius marmoratus*. It is noted that not only were the pilot fishes never molested by the sharks but that they constantly swam around them, sometimes even they were seen placing themselves against the shark's sides between their pectoral fins. Many observations were made on the strange Antennarius, the colour of whose body so closely approaches to that of the alga amidst which it lives that it enables these fish to approach almost unseen, and so quite easily take their prey. It is not, however, altogether unworthy of remark that this prey, consisting for the most part of small crustacea and mollusks, is also of the same general shade of colour as the mass of the weed, so that the assuming of this uniform dull tinge of colour must mean a heightened danger to some of these forms of life.

The great interest, however, of the fish captures of the *Talisman* centres in the remarkable forms taken from the depths of the sea, which were both considerable in the number of individuals and in the newness of the forms. The question of whether certain fish inhabit certain zones of depths was closely considered, and is answered in the affirmative. These zones are of very considerable depth, varying from 600 to over 3650 metres, and in bringing up specimens from such areas of great pressure these suffer immensely through the phenomena caused by the rapid decompression of the air, the more remarkable effects being dilatation of the swim bladder, the eyes being squeezed out of their orbits, and the scales clothing the body are shed. In some cases even the fish's body has become smashed into pieces. Notwithstanding all these phenomena, the area in depth of the distribution of many of the deep-sea fish is very considerable. Thus *Alepocephalus rostratus* is met with between a depth of 868 and that of 3650 metres; *Scopelus maderensis*, between

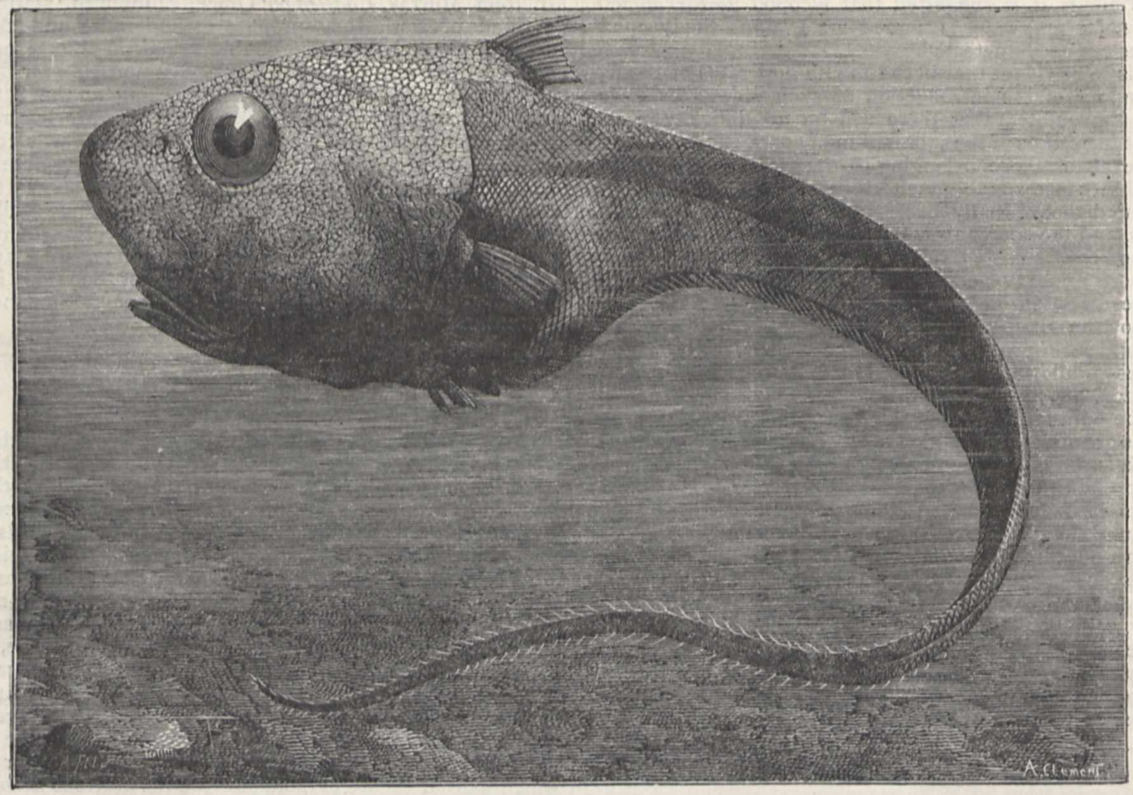

FIG. 1.—*Macrurus globiceps*, Vaill.

FIG. 2.—*Eustomias obscurus*, Vaill.

depths of 1090 and 3655 metres; *Lepioderma macrops*, between 1153 and 3655 metres; and *Macrurus affinis*, between 590 and 2220 metres. The explanation would seem to be not only that the organisation of these fishes is such as enables them to support the enormous pressures at the greater depths of the ocean, but that in the course of their movements of ascent and descent they proceed very slowly so as gradually to get accustomed to the alterations in pressure. These fishes are all flesh eaters, with well developed dental systems; the absence of light prevents the growth of marine algæ in these depths, and as a general rule all the fish found below 150 metres are of necessity predatory. These deep-sea fishes, as Dr. Günther reminds us, do not belong to any peculiar order, but are chiefly modified forms of surface types; some of these modifications being no doubt very extreme, but serving as indications not only of the struggle for existence, but also of the plasticness of the forms to adapt themselves to the extreme conditions under which they live. The most remarkable phenomena in connection with their deep-sea life is doubtless the tremendous pressure which has to be borne. No one seems to doubt but that these deep-sea forms live as active a life as surface forms, indeed their very appearance seems to indicate a swiftness and energy of movement not to be surpassed by surface swimmers; and we may believe that the abyssal pressure has a great deal to do with keeping their feebly calcareous bones and delicate muscular system compact and in a condition for effective use. The placid state of the water at these depths must also be borne in mind—no storms affect them, and the extraordinary attenuation of some organs may be directly ascribed to this phenomenon. Thus *Macrurus globiceps* (Fig. 1), which forms one of a family of deep-sea Ganoids, known as living at depths of from 600 to 2200 metres, and occurring in considerable variety and great numbers over all our oceans, is a new species, described by M. L. Vaillant as found at a depth of between 1500 and 3000 metres. Its body, globular in front, will be seen to be very greatly attenuated behind.

In some of the deep-sea fishes peculiar organs, unknown for the most part among surface fishes, are to be found; these are sometimes "more or less numerous, round, showing mother-of-pearl coloured bodies embedded in the skin"; in some fish these are to be met with on the head, or near the eyes, or along the sides and back. Dr. Günther informs us that of these strange bodies the following hypotheses are possible: (1) all these different organs are accessory eyes; (2) only those having a lens-like body in their interior are sensory, those with gland-like structure are not sensory but are phosphorescent; and (3) all are producers of light. Many serious objections can be urged against the first view. Some of the fish with immense eyes have these bodies, others without eyes want them, while as to glandular bodies being sense organs this is not yet scientifically realisable. One seems therefore justified in adopting the middle hypothesis, and though on first thought it seems strange that fish with large eyes should have accessory eyes, yet Dr. Günther's supposition may be the true one—that there are light producers behind the lenses, and that these latter may act the part of "bull's-eyes" in a lantern. This form of "light organ" might constitute a very deadly trap for prey, one moment shining it might attract the curiosity of some simple fish, then extinguished the simple fish would fall an easy prey.

Long filamentous organs are to be met with showing apparently a brilliant type of phosphorescence. Among the many curious forms of development of these tactile organs to be met with, one of the most singular is that to be seen on a fish referred by M. L. Vaillant to a new genus and species found at a depth of 2700 metres, and represented in the annexed woodcut (Fig. 2). In this form (*Eustomias obscurus*) the tactile organ takes the appearance of a long filament, which is placed underneath the lower jaw, and which ends in an inflated and rayed knob-like phosphorescent mass.

Another peculiarity now well known in deep-sea fishes is the enormous development of the mouth and stomach of these fish. In the genus Melanocetus and in Chiasmodus the capacity of the stomach is such that it can contain prey twice the size of the fish which swallowed it, and perhaps the largest gape of jaws known is that of *Eurypharynx pelecanoides*. The greatest depth at which a fish was taken during the cruise of the *Talisman* was 4255 metres; the fish was *Bythytes crassus*: but it will be remembered that during the *Challenger* Expedition a specimen of *Bathyopis ferox* was taken at a depth of 5000 metres.

We hope again to have the opportunity of referring to other of the deep-sea forms taken by the *Talisman*.

ANCIENT JAPAN[1]

THIS volume contains a literal translation of the oldest Japanese book in existence, accompanied by introductions, notes, and appendices, and is beyond doubt the most learned and remarkable work which European scholarship has yet produced from Japan. Of the many important propositions on the early history of the Japanese race established by it we shall have to speak later on; but of the work itself it may be said now that the translator claims it to be "the earliest authentic connected literary product of that large division of the human race which has been variously denominated Turanian, Scythian, and Altaïc, and it even precedes by at least a century the most ancient extant literary compositions of non-Aryan India." Indeed more than this may be said; for if the claim of Accadian to be an Altaïc language be not substantiated, not only the archaic literature of Japan (to which the *Kojiki* belongs), but also its classical literature, precedes by several centuries the earliest extant documents of any other Altaïc tongue. This alone would render the work an object of much interest, but it derives additional importance from its contents as well as from the period at which it was written. It is the earliest record of the language, customs, mythology, and history of ancient Japan, and soon after the date of its compilation, as Mr. Chamberlain points out, most of the salient features of distinctive Japanese nationality were buried under a superincumbent mass of Chinese culture; it is therefore to these "Records" and one or two other ancient works that the investigator must look if he would not be misled at every step into attributing originality to modern customs and ideas which have simply been borrowed wholesale from the neighbouring continent. It appears beyond doubt that, though the work existed in tradition for some years before that period, it was not committed to writing till the year 712 of our era, and from it a picture can be formed of the Japanese of that remote epoch. It is to the sections devoted by the translator to the manners and customs of the early Japanese and their political and social ideas that we propose to direct special attention now.

As pictured, then, in these "Records," the Japanese of the mythical era had emerged from the Stone Age and from the savage state. They were acquainted with the use of iron for weapons of the chase, such as arrows, swords, knives; but there is a curious silence about ordinary implements, such as axes and saws, though they had the fire-drill, pestle and mortar, wedge, and shuttle for weaving. The art of sailing appears to have been quite unknown, but boats for use on the inland lakes are mentioned. As would naturally be expected, the population was scattered along the seashore and on the banks

[1] *Transactions of the Asiatic Society of Japan*, vol. x. Supplement. Translation of the "Kojiki" or "Records of Ancient Matters." By Basil Hall Chamberlain. Yokohama, 1883.

of the larger rivers, while house and temple building are the subjects of frequent reference. The Japanese of the present day appear to have inherited their habits of great personal cleanliness from their early forefathers, for we read more than once of bathing, and bathing-women are said to have been specially attached to an imperial infant. Among the religious practices, too, was that of lustration. A custom of the early Japanese, which is still found existing in the island of Hachijô, off the east coast, was that of a woman before childbirth erecting with her own hands a one-roomed hut without windows, into which she was expected to retire and give birth to her child. In Hachijô formerly a woman was driven out from the village under these circumstances to a hut on the mountain side, which she was not permitted to leave under any circumstances whatever before the birth of the infant; but in later times the custom was so far relaxed that the hut was allowed to be put up within the homestead. Each sovereign on his accession, also, had a new palace erected for him; but these so-called "palaces" were nothing more than ordinary wood huts. Although cave-dwellers are referred to in the "Records," it appears that at the date to which the work refers they had quite passed away. The principal food was fish and the flesh of wild animals. Rice is mentioned in such a manner that there can be no doubt of its cultivation from immemorial antiquity; *saké*, the native rice-beer, is also referred to. In dress the mythical Japanese appear to have reached a high level, and we find many garments specialised, such as skirts, trousers, girdles, veils, and hats; while it is interesting to note that although jewellery forms no part of the attire of the modern Japanese, their ancestors adorned themselves with necklaces, bracelets, and other articles formed from stones considered precious. They appear to have had a tolerably extensive acquaintance with the animal and vegetable kingdoms, but the tea-plant was evidently not yet introduced among them. Iron, which was used from time immemorial, was the only metal they knew; and their acquaintance with colours was confined to black, blue (including green), red, white, and piebald (of horses). In the Japan of to-day the different degrees of relationship are distinguished in much the same way as in Europe, except that brothers and sisters, instead of being considered as mutually related in the same manner, are divided into two categories, elder and younger, in accordance with the Chinese usage. But the ancient Japanese had a complicated system of nomenclature, which appears to have perplexed native commentators themselves, the foundation of which was a subordination of the younger to the elder born, modified by a subordination of the females to the males. A distinction also appears to have been drawn between the chief and secondary wives, and the wife is constantly spoken of as "younger sister." It appears that consanguinity, however close, was no bar to marriage, as we hear of unions with half-sisters, step-mothers, and aunts. When the Chinese ethical code was imported, these gradually disappeared, but not, it is said, without political troubles. Exogamy did not exist, and there appear to have been no artificial impediments in the way of marriage. On death the hut of the deceased was abandoned; and there was a tradition of an earlier custom of burying alive so ne of the retainers in the neighbourhood of a royal tomb. This is the only trace of human sacrifice to be found in the records of the Japanese race, and there is also a total absence of any trace of slavery. They were unacquainted with any of the arts by which their descendants are best known; they had neither tea, fans, lacquer, or porcelain. They knew nothing of vehicles, money, or the computation of time. They were ignorant of writing, and of course had no books.

This brings us to another interesting part of the subject, viz. the antiquity claimed by native writers for their monarchy, and the reliability of their early chronology. There is no break in their history between the fabulous and the real, and the continuity of their mythology and history is a tenet of the native commentators. They hold the age of the gods to have ceased and that of their human kings to have commenced at an era corresponding with 660 B.C., and the then ruler of Japan is claimed as the first of an unbroken line of sovereigns extending down to the Mikado of to-day. All the earlier European writers on Japan have accepted 660 B.C. as the commencement of historical Japan; the Mikado himself has claimed this long descent; frequently in official publications we find this accepted as the Japanese year I.[1] In native chronologies we find the names of a series of Emperors who have reigned from that time. This antiquity, though as yesterday compared to that of the Chinese, is highly respectable if correct, but unfortunately there is nothing whatever to support it. For, in the words of Mr. Chamberlain, this era, this accession of the first emperor, "is confidently placed thirteen or fourteen centuries before the first history which records it was written, nine centuries before (at the earliest computation) the art of writing was introduced into the country, and on the sole authority of books teeming with miraculous legends." Another scholar, who made the chronology of Japan a special study, and who has published a valuable monograph on that subject, the late Mr. Bramsen, does not scruple to say that "the whole system of fictitious dates applied in the first histories of Japan is one of the greatest literary frauds ever perpetrated, from which we may infer how little trust can be placed in the early Japanese historical works." In short it appears that, for all historical purposes, Japan is a newer country than England by several centuries. Another proposition for which native scholars have always strenuously contended will also have to be abandoned. It is usual to say that early Japanese civilisation was a purely indigenous product, and that even a certain form of writing called "letters of the Divine Age" existed long before there was any contact with China. European scholars have always been doubtful about this divine alphabet, and it is now beyond doubt that they are the invention, or adaptation from Corea, of a later age; but it is also certain from these "Records" that, "at the very earliest period to which the twilight of legend stretches back, Chinese influence had already begun to make itself felt in these islands, communicating to the inhabitants both implements and ideas." It would occupy too much space here to exhibit the evidences of this. One must suffice. "Curved jewels," *magatama* as they are called, figure largely in the Japanese mythology as ornaments of the early Japanese. These are generally made of jade, or a jade-like stone, and Prof. Milne shows that no such mineral has ever been discovered in Japan. Further proofs of Chinese influence are found in the nature of the myths, the existence of the intoxicant *saké*, the language, &c. The religion of the early Japanese appears to have been merely "a bundle of miscellaneous superstitions," not an organised system. We find no body of dogmas, or code of morals, authoritatively enforced by a sacred book. The gods of their mythology were of course the object of worship; conciliatory offerings of a miscellaneous kind were made to them. Purification by water is the sacred rite of which we hear most. Trial by hot water also existed; compacts, too, resembling our oaths, were entered into with a god. Priests are mentioned, but the impression conveyed is that in early times they did not exist as a separate class. In his "History

[1] In an interview with the Japanese Minister in London, published in the *Pall Mall Gazette* of February 26. His Excellency is reported to have attributed the ardent attachment of the Japanese to his country to two facts, one that Japan has been unconquered for 2500 years, the other that for the same period it has been governed by the same dynasty. "No other State can point to such a record," said Mr. Mori, "and it is but natural that we should feel a pride in our country," &c., &c. The Minister, as will be seen, would have to deduct nearly fifteen hundred years from his major premiss before he touched the solid ground of fact.

of Civilisation," Buckle attributes some of the superstition of the inhabitants of Spain and Italy to the occurrence of earthquakes and other volcanic phenomena; but in Japan there is "no testimony to any effect produced on the imagination by the earthquakes from which the Japanese islanders suffer such constant alarms." Nor is there any tradition of a deluge, which is the more remarkable as Noah's deluge has recently been claimed as a myth of Altaïc origin. "Yet here we have the oldest of *undoubtedly* Altaïc nations without any legend of the kind." There is no such thing as star-worship, nor are there any fancies such as the imagination of other races has connected with them.

Much, very much, more might be written on this deeply interesting volume. Although more than a thousand years of Japanese history must be cut away, "the Japanese mythology is the oldest existing product of the Altaïc mind." When to this are added the facts that here we have the *ipsissima verba* (for the translation is literal) of the Japanese compiler of eleven centuries ago, that it is the first complete translation of an archaic Japanese work, that it is the first work in which an attempt is boldly made to separate Japanese history from myths, and to fix the commencement of the historical era, and that it contains abundant illustrations of the manner and ideas of this primitive race as recorded by themselves, we have said enough to attract a wide circle of students. Besides the very valuable preliminary discussions, the text is abundantly annotated by the translator, who has for this purpose made use of the works of the numerous native commentators and editors of the work.

NOTES

The gold and silver Rumford Medals have been presented by the American Academy of Arts and Sciences to Prof. Rowland of Baltimore for his researches on heat and light.

We are sure that every field-naturalist and working geologist will be grateful to Prof. Bryce for introducing into Parliament his Bill "to secure access to mountains and moorlands in Scotland." Since the substitution of deer for sheep and cattle on the Scottish moors and mountains, great restrictions have been placed on access to these favourite haunts of the lover of nature, so that in some districts the tourist and collector are faced by the trespassers' board in all directions. We have no wish whatever to infringe the rights of private property, but surely the great landed proprietors of Scotland can afford to be generous to those whose noblest game is a rare butterfly, an Alpine flower, or a chip from the rocky escarpment of a hill. Already some of the most valuable hunting grounds of science have been shut up, and in the present condition of things we may soon hear of such natural phenomena as the Parallel Roads of Glenroy being rendered inaccessible, and the traveller confined to the dusty highways. The Bill embodies every possible precaution against the abuse of the access craved, and we strongly advise the members of the many natural history societies and field clubs all over the country to use every legitimate means to obtain for it Parliamentary sanction. We need scarcely point out how greatly interested in the provisions of the Bill are all artists and the great army of tourists.

Sir J. H. Lefroy, we are glad to learn, has accepted the presidency of the Geographical Section at the Montreal meeting of the British Association.

Signor Quintino Sella, whose death on March 15 is announced, was president of the R. Accademia dei Lincei.

We regret to learn of the death of Dr. Behm, the eminent geographer of Gotha, the editor of the *Geographische Mittheilungen*, the *Geographisches Jahrbuch*, and, along with Prof. Wagner, of the well-known "Bevölkerung der Erde."

We are glad to notice the hearty manner in which the *Times* recognises the necessity for scientific education among all classes. In an excellent leading article on the Technical Institute, it maintains that the old rule-of-thumb methods will no longer suffice, and that science and organised knowledge are bound to invade industry as they have already invaded almost every branch of human endeavour.

There can be no doubt of the great scientific value of a bathymetrical survey of the Scottish lochs, about which Lord Balfour of Burleigh asked a question in the House of Lords on Tuesday, and concerning which there has been a correspondence between the Royal Society of Edinburgh and the Government. No one wishes to retard the completion of the English Survey for the purpose of this special undertaking; but this is not necessary, as, without going to any great expense, Government might easily employ other existing agencies in carrying out the work.

Sir Richard Owen was on Saturday presented with a framed and illuminated address by the Geologists' Association, on the occasion of his retirement from the post of Director-General of the Natural History Department of the British Museum. A large audience assembled in the lecture-hall at South Kensington to witness the ceremony. The address was presented by Dr. Henry Hicks, F.G.S., who said that in his retirement Sir Richard Owen would take with him the good wishes and warm interest of all who appreciated his scientific work, and his great personal kindness in communicating its results to others. Sir Richard Owen, in reply, said that, of all the recognitions which he had recently received of his years of service in the State museums, none would be more valued by him than that testimonial from his fellow workers in those walks of natural science in which he had been for over half a century more or less occupied. He would value the address amongst the rarest of his treasures, and he trusted that its contemplation would stimulate his sons and grandsons, particularly the latter, to walk in their grandfather's footsteps. He returned his grateful thanks, and wished the members and all present every happiness.

It is intended at the forthcoming celebration of the tercentenary of Edinburgh University to confer the degree of LL.D. on sixty-nine gentlemen, among whom are Prof. Cayley, Mr. Archibald Geikie, Prof. Helmholtz, Sir John Lubbock, Sir Henry Maine, and Prof. Haeckel.

At its last private sitting the Academy of Sciences of Paris debated the question of the sale of the Observatory grounds in order to find the funds required for the erection of a *succursale* in the vicinity of Paris. The matter was postponed for fifteen days, after a long and interesting discussion. The majority of the Academy is of opinion that it would be desirable to grant the credits required for the erection of a new establishment; but many members are against the sale of any parcel of ground. They contend that the present position of the Observatory must not be deteriorated under any pretence whatever. MM. Wolf and Janssen delivered addresses defending the *statu quo*.

The convention for the protection of cables has been signed in Paris by the plenipotentiaries of the following nations:—Germany, Argentine Republic, Austria-Hungary, Belgium, Brazil, Costa Rica, Denmark, San Domingo, Spain, United States of America, United States of Colombia, France, Great Britain, Greece, Guatemala, Italy, Netherlands, Persia, Portugal, Roumania, Salvador, Sweden, Norway, Turkey, and Uruguay. The protocol has been left open for acceptation by the other countries. This is the final step towards the accomplishment of the work originated at the Congress of Electricians in Paris.

THE exhibition of the submarine objects at the Museum of Paris was closed on March 15, but will be opened on a larger scale on the occasion of the session of the Délégués des Sociétés Savantes, which will take place as usual in the Easter holidays.

THE number of members of the French Alpine Club is yearly increasing, and the financial position of the Society is very prosperous indeed. The general sitting of the Paris section took place on March 10. M. Janssen delivered an address on the sun. The discourse was illustrated by projections exhibiting all the phenomena connected with the eclipse of 1883, as observed by him on Caroline Island. It is the first time these pictures have been presented to the public, and they have been very successful.

AMONGST the latest publications in the domain of electricity we notice "Das Elektrische Potential," by A. Serpieri; "Die Elektrische Kraftübertragung," by Jos. Popper; and "Die Atmosphärische Elecktricität," by Prof. Palmieri. Hartleben of Vienna is the publisher of all the works mentioned.

M. PERRIER presented on Monday to the Academy of Sciences of Paris six sheets of the map of Tunisia, which the French military geographers are executing on the scale of 1 : 100,000. The mapping of the whole country from Algeria to the Tripolitan territory will be published in a few weeks. The publication, which will contain twenty-one sheets, will be completed this year. This great work will have required only four years to accomplish. The maps are lithographed, and will be ultimately engraved.

WE have already mentioned a publication issued by the Direction of Schools at Tiflis, in which the teachers of the Caucasus have the opportunity of publishing descriptions of the interesting but little known districts where they are compelled to stay, often deprived of any intercourse with the civilised world. We have now received the third volume of this publication, which contains several valuable papers. The chief of them is the first part of an interesting memoir, by M. Lavroff, on Ossetia and Ossetians, with a map. In this first part the author describes the country, its orography and hydrography, climate, flora, and fauna, leaving the purely ethnographical part for a second memoir. M. Gadovsky contributes valuable notes on the newly-annexed province of Kars: its geography, population, tenure of land, and the occupations of the inhabitants. The second part of this volume is devoted to the rich folk-lore of the Cossacks, Tartars, and Circassians, in which the ethnographer will find rich materials.

IN the "Untersuchungen aus dem botanischen Institut zu Tübingen" F. Schwarz discusses the structure and functions of the root-hairs of flowering plants. He finds that in maize the surface of a hairy root is 5·5 times greater than that of a root not covered with hairs; in the pea 12·4 times greater. The intimate contact of the root-hairs with particles of soil is effected by the conversion into mucilage of the outermost layer of the wall of the hair; the inner layer of the membrane is stained blue, the outer layer yellowish brown by zinc chloriodide. The greatest development of root-hairs accompanies the greatest energy of growth of the root. A medium degree of moisture is most favourable for their formation; with plants growing in water they are often altogether suppressed. Nutation promotes their production, especially at the point of curvature. Contact with dry solid bodies has no effect on their production. They are always formed in acropetal succession. They have not in most cases the same form in the same species, being considerably affected by conditions of growth. In many plants the root-hairs are branched.

THE annual prize of 25,000 francs, instituted by the King of the Belgians, will for 1885 be granted to the author of the best work on the means of popularising the study of geography and developing it in the different educational establishments. Foreigners may compete equally with Belgians. The works of the competitors must be sent to the Minister of the Interior before January 1, 1885.

WE understand that Messrs. Sanderson and Co. are about to issue a small volume on tall-chimney climbing and lightning-rod testing.

CAPT. A. E. BARLOW, Commander of the P. and O. steamer *Paramatta*, writes as follows to the *Times*:—"An unusual phenomenon was observed during the recent voyage of the P. and O. steamer *Paramatta* to Sydney, New South Wales, which may be of interest to some of your readers. On December 11 and the following day, about lat. 10° S. and long. 92° E., the surface of the sea was covered with lava and pumice, some being as fine as sawdust and of a yellowish colour, but several patches of large extent were passed through with masses of pumice from the size of a cocoanut to that of a hogshead; this extended over 5° of latitude, and probably much more of longitude, as the densest patches all ran in an easterly and westerly direction. The largest specimen of pumice which I picked up was about ten inches in diameter, and appeared only to have been a few days in the water, as there was no deposit on it. This would lead to the conclusion that a submarine upheaval must have taken place long after the great eruption of Krakatoa, in the Strait of Sunda, our nearest approach to which was over 800 miles. On the homeward voyage on February 1 the same phenomenon was observed, but in a much less degree, in lat. 4° S., long. 88° E., showing that the mass had drifted to the west-north-west about 500 miles in six weeks."

UNDER the title of "New Commercial Plants and Drugs, No. 7," Mr. Thos. Christy has recently issued a continuation of his notes on useful plants which come before him in the course of commerce. The demand for economic plants of every description has of late years considerably increased amongst planters not only in our own colonies but also in other parts of the world in consequence of the general desire for the greater dissemination of staple articles of cultivation that are acknowledged sources of revenue, and also the introduction of new staples where from long cultivation or the ravages of disease the older and better known plants have ceased to be remunerative. The circulation amongst planters and colonists generally of such books as this is calculated to do a great deal of good even if it were only to let them know of the existence and properties of certain plants, for while there are many that have a knowledge of useful plants, there are also others who are content to go on growing the same crops that they have always been accustomed to, and though we may not expect full details of the uses of the plants enumerated, nor botanical descriptions of the plants themselves, sufficient is given in all cases to put the reader on the right track for further information. In some of the subjects, however, very voluminous abstracts are given from some of the best journals in which special articles have appeared. It will suffice to say that the present number of "New Commercial Plants and Drugs" contains very interesting articles on the Cacao (*Theobroma cacao*) and its preparation, the Siam benzoin tree, pepper and nutmeg cultivation, Liberian coffee, and numerous other economic plants of very varied uses.

THE Eleventh Annual Report of the National Health Society shows that the Society has carried on its work during the past year in a most practical manner. Hundreds of lectures on sanitary subjects have been delivered, not only all over the poorer parts of London, but in provincial towns, to large audiences of working men and women, classes of girls, district visitors, and others engaged in work amongst the poor. The Society is much encouraged by the practical results of the lectures on keeping the house healthy, rearing of infants, prevention of the spread of infectious diseases, preparation of food and kindred subjects, knowledge of which is so much needed in our crowded neighbourhoods. The questions of poisonous dyes in domestic fabrics, of smoke abatement, of dust collection, and the prevention of cholera have been investigated and reported upon by special committees appointed for the purpose. The Health Exhibition held by the Society last June was commented upon, and the Secretary stated that more than 100 members had joined the Society during the past year.

THE great interest manifested in the International Health Exhibition is shown by the fact that application has been made, by British exhibitors alone, for space five times as great as that actually at the disposal of the Executive Council. Information has recently been received that the French Government has appointed a Commission; and Italy—owing in a great measure to the individual exertions of a member of the Executive Council—will, it is hoped, take an active part. A portion of the Educational Section of the Exhibition will be located in the Central Institute of the City of London Technical Guilds, the handsome building in course of erection in the Exhibition Road, which has been kindly placed at the disposal of the Executive Council. The Royal Albert Hall with its musical attractions will now form an integral part of the Exhibition; and the Aquarium, a popular feature of the late Fisheries Exhibition, will continue as an important part of the Health Exhibition. In the Dress Section the most popular exhibit will probably prove to be a series which is being prepared illustrative of English dress of all ranks of life, from the time of the Conquest to George IV. An International Congress on Education will be held, and conferences and lectures will conduce to the elucidation of the subjects of the Exhibition. It is also proposed to have a library and reading-room in connection with the Exhibition, which will be open to all visitors, under proper regulations, while the Exhibition is open. The library will consist of books on various subjects comprised in the classification of the Exhibition, both English and foreign. Application has been addressed to foreign and colonial Governments, asking them for copies of reports and statistics on sanitary and educational matters, and a circular is being sent out to authors and publishers requesting them to contribute works of a similar character.

AT a meeting of the Society of Telegraph Engineers held on the 13th inst., a short paper, "Notes on a Train Lighting Experiment," was read by Mr. W. H. Massey of Twyford, who strongly advocated the use of a small engine and dynamo-machine placed on each locomotive for working incandescent lamps, by means of which railway carriages would be much better lighted than at present for less than is paid for gas. An interesting discussion took place, and the meeting was adjourned to the 27th inst., when Mr. Massey is expected to reply.

THE March number of *Petermann's Mittheilungen* contains a letter from Dr. Junker dated Sami, $6\frac{1}{2}°$ N. lat., 25° E. long., December 8, 1882, in which he gives a brief statement of the results of his journeys in the Upper Welle and Bomokandi, with notes on the various tribes that inhabit the region. Dr. Junker did some further exploring work to the south-west of his station in 1883; but his numerous cases of collections have been lost in a fire which consumed the building where they were stored.

THE additions to the Zoological Society's Gardens during the past week include a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, presented by Mrs. Dundas; a Leopard (*Felis pardus*) from Africa, presented by Mr. S. Cresswell; a West Indian Rail (*Aramides cayennensis*) from South America, presented by Mrs. Edward Hairby; a Kestrel (*Tinnunculus alaudarius*), British, presented by Mr. F. E. Baum; a Common Viper (*Vipera berus*), British, presented by Mr. W. H. B. Pain; two Mute Swans (*Cygnus olor*), European, four Redshanks (*Totanus calidris*), British, purchased.

PHYSICAL NOTES

PROF. J. H. POYNTING has published in the *Proceedings of the Birmingham Philosophical Society* a note on a method of calculating the velocity of propagation of waves of longitudinal and transverse disturbances by the rate of transfer of energy. The paper discusses the two cases by the method originally propounded by Lord Rayleigh.

WE have received from Madame Plateau copies of three posthumous memoirs by her late husband, the lamented Prof. J. Plateau. Their titles are: "Quelques Expériences sur les lames liquides minces (deuxième note)"; "Sur l'Observations des Mouvements très rapides"; and "Bibliographie analytique des Principaux Phénomènes subjectifs de la Vision." The first of these *brochures* relates to the preservation of the glyceric fluid, to certain appearances in very thin films, and on the constitution of foam. In the second the writer contrasts four methods: the rotating mirror, the stroboscopic method, the intermittent illumination by electric sparks, and the process of multiple instantaneous photography. The third is a supplement for the years 1880–1882 to the well-known bibliography compiled by the deceased *savant*.

WE have also received the first instalment of vol. i. of the *Bulletin de la Société Internationale des Électriciens* (January 1, 1884), containing the laws of the new society, a list of founders, and one of the members already enrolled, now numbering about 1200, of whom only a few are Englishmen.

PROF. QUINCKE has lately read before the Berlin Academy a paper on the measurement of magnetic forces by hydrostatic pressure. He has examined the magnetic inductive capacity, or, as he calls it, the "di-magnetic constant" of a number of liquids, by observing their rise in an open-air manometer when subjected to a field of powerful, but known, intensity, the observed change of pressure being proportional to the square of the intensity of the field and to the difference between the magnetic inductive capacity of the substance and that of the air. A number of tables are given, with copious numerical data. The di-magnetic constant of such liquids as ether, alcohol, turpentine, nitric acid, bisulphide of carbon, glycerine, water, &c., showed small negative values; whilst the values were positive, and in many cases much more considerable for solutions of chloride of iron, chloride of manganese, sulphate of nickel, and of cobalt, and for solutions of magnetic salts in general.

A SLIGHT mistake occurred in a note on p. 276, in which Bunsen's estimation that in three years 5·135 cubic centimetres of carbonic dioxide was absorbed by a square metre of glass was stated as the absorption of one square centimetre.

THE CHEMICAL WORK OF WÖHLER[1]

IT seems fitting that these walls, which have vibrated in sympathy with that brilliant eulogy of Liebig which Prof. Hofmann pronounced some nine years ago should hear something of him whose life-long association with Liebig has exercised an undying influence on the development of scientific thought. The names of Frederick Wöhler and Justus Liebig will be linked together throughout all time. The work which they did in common makes an epoch in the history of chemistry. No truer indication of the singular strength and beauty of their relations could be given than is contained in a letter from Liebig to Wöhler, written on the last day of the year 1871. "I cannot let the year pass away," writes Liebig to Wöhler, "without giving thee one more sign of my existence, and again expressing my heartfelt wishes for thy welfare and the welfare of those that are dear to thee. We shall not for long be able to send each other New-Years' greetings, yet, when we are dead and mouldering, the ties which have united us in life will still hold us together in the memory of men as a not too frequent example of faithful workers who, without envy or jealousy, have zealously laboured in the same field, linked together in the closest friendship."

[1] A lecture delivered at the Royal Institution on Friday evening, February 15, 1884, by Prof. Thorpe, F.R.S.

And yet, bound as they were in the ties of a friendship the purity and warmth of which were but characteristic of the men, and although each influenced the other's walk and work in life to a degree which it is almost impossible to gauge, such was the strength of their individuality and such the force of their genius that, without a doubt, either would have been a great figure in the history of science if the other had not existed.

The conditions under which minds of the highest type arise and develop have on more than one occasion engaged the attention of this audience. Although there were circumstances in Wöhler's surroundings which in early life may have influenced the bent of his mind, it is not easy to see whence sprang that passionate love of nature which was so strikingly exhibited in the man. His father, August Anton Wöhler, was formerly an equerry in the service of the Elector William II. of Hesse; he afterwards came to live at Frankfort, and became a leading citizen of that town. His wise liberality and public spirit are commemorated in the Wöhler Foundation and Wöhler School, institutions known to every Frankforter. His mother was connected by marriage with the minister of Eschersheim, a village near Frankfort, and it was in the minister's house that Frederick Wöhler first saw the light, on July 31, 1800. Even in early youth his passion for experimenting and collecting manifested itself, to the neglect not unfrequently of the lessons of the gymnasium; indeed it would appear that during his school career Wöhler was not characterised by either special diligence or knowledge. The bent of his mind towards natural science was directed by Dr. Buch, a retired physician who had devoted himself to the study of chemistry and physics; and it was in the kitchen of his patron's house that he prepared the then newly-discovered element selenium, of which an account was afterwards sent by Dr. Buch to Gilbert's *Annalen*, with Wöhler's name at the head of it. The elder Wöhler appears to have been a man of considerable artistic feeling, and under his direction the son was taught sketching and otherwise educated in that perception of natural beauty which comes out so strikingly in his after life; and he was encouraged to make himself familiar with the literature which the genius of Schiller and Goethe has ennobled. He had, moreover, to thank his father for that love of physical exercise and passion for outdoor life which reacted so beneficially upon his development, and contributed so largely to the uniformly good health which he enjoyed to within a few days of his death. Mainly, it would seem, because his father had been there before him, Wöhler, in his twentieth year, entered the University of Marburg. It was his own and the family's wish that he should study medicine, and he accordingly put his name down for the lectures of Bünger on Anatomy, Gerling on Physics and Mathematics, and Wenderoth on Botany. He found time also to attend Ullmann's classes on Mineralogy; and although he declined to hear Wurzer's lectures on Chemistry, he by no means neglected that science. He transformed his living-room into a laboratory, and to the great, and perhaps not undeserved, disgust of his landlady, occupied himself with the preparation and study of the properties of prussic acid, thiocyanic acid, and other cyanogen compounds. He discovered at that time, without knowing that Sir Humphry Davy had anticipated him, the beautifully crystalline but intensely poisonous iodide of cyanogen; and in the little paper on cyanogen compounds which his good friend Dr. Buch communicated to Gilbert's *Annalen* for him we have the first description of the remarkable behaviour of mercuric thiocyanate on heating, which has astonished and amused us in the so-called "Pharaoh's Serpent."

Wöhler, attracted by the fame of Leopold Gmelin, left Marburg for Heidelberg. His main idea was to hear the lectures of that distinguished man, but Gmelin declared this to be unnecessary and a waste of time. Wöhler in fact never attended any systematic lectures on chemistry; he had access, however, to the old cloisters which at that time constituted the Heidelberg laboratory, and there began the work on cyanic acid which some four or five years later was destined to culminate in the great discovery of the synthesis of urea. His association, at this time, with Tiedemann, who was engaged in physiological chemical investigation with Gmelin, had also considerable influence in determining the direction of much of his future work, whilst its immediate effect was the publication in Tiedemann's *Zeitschrift für Physiologie* of the results of an inquiry into the transformation experienced by various substances, organic and inorganic, in their passage through the organism. In 1823 Wöhler obtained his degree, when, on Gmelin's advice, he determined to follow his master's example, and abandon medicine for chemistry. At that time the great Swedish chemist Berzelius was at the summit of his fame: his masterly analytical skill, no less than his labours towards the development of chemical theory, had made him supreme among the chemists of Europe, and to Stockholm therefore, Wöhler, acting on the advice of Gmelin, determined to go. He was warmly welcomed by Berzelius, on whom his communications to Gilbert's *Annalen* had made a favourable impression, and with the offer of a place in the private laboratory of the illustrious Swede, Wöhler set out for the Scandinavian capital. Of his experiences with Berzelius his pupil has left us a delightful account. It is valuable not only as a charming character-sketch of the great teacher, but also from the side-light it throws upon the nature and disposition of Wöhler and himself. It is interesting, too, as an account of the mode in which Berzelius worked and taught, and as showing how the typical laboratory of that time contrasted with the temples which have since been reared by the disciples of Hermes.

"With a beating heart," says Wöhler, "I stood before Berzelius's door and rang the bell. It was opened by a well-clad, portly, vigorous-looking man. It was Berzelius himself. . . . . As he led me into his laboratory I was as in a dream, doubting if I could really be in the classical place which was the object of my aspirations. . . . I was at that time the only one in the laboratory: before me were Mitscherlich and Heinrich and Gustav Rose: after me came Magnus. The laboratory consisted of two ordinary rooms furnished in the simplest possible way; there were no furnaces nor draught places; neither gas nor water service. In one of the rooms were two common deal tables; on one of these worked Berzelius, the other was intended for me. On the walls were a few cupboards for the reagents; in the middle was a mercury trough, whilst the glass-blower's lamp stood on the hearth. In addition was a sink, consisting of an earthenware cistern and tap, standing over a wooden tub, where the despotic Anna, the cook, had daily to clean the apparatus. In the other room were the balances, and some cupboards containing instruments: close to was a small workshop fitted with a lathe. In the neighbouring kitchen, in which Anna prepared the meals, was a small but seldom-used furnace and the never-cool sand-bath."

Wöhler's first exercises were in mineral analysis, in order that he might become acquainted with Berzelius's special methods and manipulative procedure. At that time he prepared, among other products, some new compounds of tungsten, notably the beautifully crystallised monoxychloride and the tungsten sodium-bronze ($Na_2W_3O_9$), which some twenty-five years later was introduced into the arts as a bronze powder. It was, however, with his investigation on cyanic acid that both he and Berzelius were mainly interested. In Berzelius's opinion the existence of this body was of importance from the light it seemed to him to throw upon the validity of the new chlorine theory. "I was surprised," says Wöhler, "to hear him, the hitherto steadfast upholder of the old notion, now always talk of *chlorine* instead of oxidised hydrochloric acid. Once, when Anna, in cleaning some vessel, remarked that it smelt strongly of oxymuriatic acid, Berzelius said, 'Hearest thou, Anna, thou must no longer speak of oxidised muriatic acid; thou must call it *chlorine*: that is better.'" With what feelings would Davy have listened to that colloquy between the Swedish philosopher and his factotum! Chlorine was discovered by Berzelius's illustrious countryman, Scheele, but its true nature was first demonstrated in the laboratory of the Royal Institution.

A couple of months were now spent in travel with Berzelius, in company with the two Brongniarts, Alexander the geologist and Adolph the botanist, during which they explored the greater portion of the geologically interesting parts of Southern Sweden and Norway, and collected rich stores of those wonderful minerals for which Scandinavia is famous. Scandinavia is no less famous for salmon and trout, and it was on his return from a fishing expedition in Norway that the travellers met with Davy, who, as readers of "Salmonia" know, handled his rod with great zest and zeal. Wöhler, who as a boy had learned the story from his friend Dr. Buch, of the isolation of the alkaline metals by Davy, and who, aided by his little sister, whose business it was to blow the bellows, had toiled, not unsuccessfully, to make potassium in the kitchen fire, was presented to the famous chemist.

On the return to Stockholm, Wöhler took leave of Berzelius and prepared to return to Germany. Of his association with this great man Wöhler had ever the kindliest memories. Although the outcome of much of his subsequent work, or at least much of that which he did in concert with Liebig, might be said to bring him in occasional conflict with Berzelius's cherished convictions on points of chemical theory, the master and pupil remained to the end bound together in the warmest friendship. Scarcely a month passed without an exchange of letters. Those from Berzelius were religiously preserved by Wöhler, who, after his master's death in 1848, presented them, to the extent of some hundreds, to the Swedish Academy of Sciences. We are told that in the later letters the "trauliche Du" appears in place of the more formal "Sie," and that "*Totus et tantus tuus*" is a not unfrequent signature.

Wöhler's gratitude and almost filial reverence are seen in the circumstance that even in the full tide of his vigour, and when time was doubly precious to him, he continued to charge himself with the yearly translation of Berzelius's *Jahresbericht* into German. It is easy to trace the influence of Wöhler's contact with Berzelius in his after work. To begin with, the men had much in common: their sympathies were as catholic as science itself, and they ranged at will over every department of chemical knowledge. Wöhler attacked the composition of a mineral with as much ardour as he did the preparation of an organic compound: to him the problems of physiological chemistry were not more important than the isolation of a rare earth or the perfection of some analytical method. The artificial barriers and fancied lines of demarcation in the science seemed to have no existence for Wöhler: indeed, it was the crowning triumph of his work to break down such barriers almost at a stroke, and to demonstrate the irrationality of these attempts to draw distinctions regardless of differences. The history of chemistry is indeed like that of the nation which has done so much to advance it: its unity to-day is as complete as that of Germany itself.

Wöhler was now to embark on his academic career, and under the advice of Gmelin and Tiedemann he prepared to settle in Heidelberg as *privat docent*. But to Heidelberg he was not destined to go. His work had already been gauged by such men as Leopold von Buch, Poggendorff, and Mitscherlich, and these, without his knowledge, had strongly recommended his election to the vacant teachership of chemistry in the newly-founded Trade School in Berlin. Berzelius advised him to accept the post, and to Berlin accordingly Wöhler went in 1825. He was now in possession of a laboratory which he could call his own, and he had to justify that possession by the use which he made of it. One of the problems which he now attacked was the isolation of *aluminium*, a metallic radicle more abundant and more widely diffused than any other of the fifty bodies we are accustomed to designate as metals. He succeeded in obtaining the body by the method which, nearly twenty years after, was worked out on a manufacturing scale by Sainte-Claire Deville. Deville caused the first bar of the metal thus procured to be struck as a medal, with the image of Napoleon III. on the one side, and the name Wöhler with the date 1827 on the other, and some time after the Emperor simultaneously designated the two chemists officers of the Legion of Honour.

But of the twenty-two memoirs and papers which Poggendorff's *Annalen* exhibits as the outcome of Wöhler's activity and power of work during his six years' stay in Berlin, that on the artificial formation of urea is by far the most important. No single chemical discovery of this century has exercised so great an influence on the development of scientific thought, and the words with which Wöhler closes his account of the molecular transformation of ammonium cyanate—a body of purely inorganic origin—into urea—a substance which of all that might be named is the most characteristic of the action of the so-called vital force—are full of meaning: "This unexpected result," he says, "is a remarkable fact, in so far as it presents an example of the artificial formation of an organic body, and indeed one of animal origin, out of inorganic materials." "The synthesis of urea," says Prof. Hofmann in his account of Wöhler's life-work, "was an epoch-making discovery in the real sense of that word. With it was opened out a new domain of investigation upon which the chemist instantly seized. The present generation, which is constantly gathering such rich harvests from the territory won for it by Wöhler, can only with difficulty transport itself back to that remote period in which the creation of an organic compound within the body of a plant or an animal appeared to be conditioned in some mysterious way by the vital force, and they can hardly realise the impresssion which the building up of urea from its elements then made upon men's minds. And yet it cannot be said that chemists were unprepared for this discovery. Men were long ago in the habit of perceiving that bodies of mineral origin were but the types of those met with in the animal and vegetable organism—in both classes there were the same differences in states of aggregation, the same mutual transformations, the same crystalline forms, the same constancy in combining relations, the same conjunction of the elements according to the weights of their atoms or in multiples of these, in both classes the appearance of the same species of compounds. But all attempts to build up organic compounds from their elements, as this for a large number of mineral substances had already been done, had hitherto been futile. The chemists of that period had nevertheless the presentiment that even this barrier must fall, and one can conceive the feeling of joy with which the gospel of a new unified chemistry was hailed by the intellect of that time. With the revolution thus effected in the ideas of men, science was directed into new paths and unto new goals. Who does not know with what zeal these paths have been trodden, and how many of these goals have been reached!"

But if at this time Wöhler made a great discovery for the world, he also, at about the same time, made a great discovery for himself: he discovered Liebig. The manner in which the two men were brought together is worth mentioning, for it would seem almost as if the hand of destiny was in it. At about the time that Wöhler was at Stockholm thinking and working on cyanic acid, Liebig was at Paris engaged with Gay Lussac on the study of the metallic compounds of an acid which, on account of their formidable explosive properties, has received the not inappropriate name of fulminic acid. Liebig, with rare skill and courage, had determined the composition of that acid, and had been rewarded by the honour of a waltz with Gay Lussac, it being the habit of that distinguished philosopher, as he explained to the astonished young German doctor, to express his ecstasy on the occasion of a new discovery in the poetry of motion. But the most extraordinary result of that investigation was to show that the terribly explosive fulminic acid and the innocuous cyanic acid were of identical composition. The idea that bodies could exist of identical ultimate composition—that is composed of the same elements united in the same proportion and yet possess essentially different properties, in other words be absolutely dissimilar things, was new to science; Berzelius, the great chemical lawgiver of his time, scouted the notion as absurd; to him it was impossible to conceive that identity in elementary composition should not result in identity of properties. And yet, later on, Berzelius was forced to realise the fact by the discovery by his pupil Wöhler of the molecular transformation of ammonium cyanate into urea, and to coin for us the word *isomerism*, by which that fact is denoted.

It was thus from the singular circumstance that Wöhler and Liebig were at the outset of their career engaged upon the elucidation of the nature of two bodies of identical composition, but of dissimilar origin, dissimilar relations, and very different properties, that they were brought into juxtaposition. They desired to know each other: they met in the house of a mutual friend at Frankfort, and the names of Liebig and Wöhler became henceforth linked together for all time.

The origin of that partnership, so fruitful in consequences for science, may be seen in the following characteristic letter:—

"FREDERICK WÖHLER TO JUSTUS LIEBIG

"*Sacrow, near Potsdam, June* 8, 1829

"DEAR PROFESSOR,—The content of your last letter to Poggendorff has been communicated to me by him, and I am glad that it affords me an opportunity of resuming the correspondence which we began last winter. It must surely be some wicked demon that again and again imperceptibly brings us into collision with our work, and tries to make the chemical public believe that we purposely seek as opponents these apples of discord. But I think he is not going to succeed. If you are so minded, we might, for the humour of it, undertake some chemical work together, in order that the result might be made known under our joint names. Of course, you would work in Giessen, and I in Berlin, after we were agreed upon the plan, and had communicated with each other from time to time as to its progress. I leave the choice of subject entirely to you.

"I am very glad that you have also determined the identity of pyrouric acid, and cyanic [cyanuric] acids. L. Gmelin would say: 'God be thanked, there is one acid the less!' . . .

"Yours, "WÖHLER"

Liebig acceded to the proposition at once, and suggested some problem on the chemical nature of nitrogen; this Wöhler found himself unable to undertake, as it involved the use of chlorine, to the action of which he was at all times extremely susceptible. On the other hand, he proposed to Liebig that they should continue in common a research on mellitic acid, which he himself had begun. Their joint investigation on this body made its appearance within the following year.

It would be quite impossible within the limits of an hour to attempt to give you anything approaching to a complete analysis of Wöhler's work. In all, he was the author of 275 memoirs and papers, and of these fifteen were published in concert with Liebig. I must therefore confine my selection from this vast amount of material to those papers which are of paramount importance from the influence which they have exerted on chemical theory or on the development of the chemical arts.

Very shortly after the publication of the work on mellitic acid Wöhler proposed to Liebig a joint investigation on cyanuric acid, in the course of which he observed the extraordinary transformation of that acid into cyanic acid, and the reconversion of the cyanic acid into cyanuric acid—one of the most remarkable instances of molecular rearrangement known to the chemist. The work progressed little for some months, owing to the demands made by Berzelius's *Jahresbericht* on Wöhler's time. "Wirf die Schreiberei zum Teufel," wrote Liebig, "und gehe in das Laboratorium, wohin Du gehörst." It was that functionary, doubtless, who in due time carried off the writing to his master, the printer. Wöhler went back to his laboratory, and in a few weeks the two investigators had obtained the clue to the puzzle. Liebig wrote to Wöhler: "Now that I have received your experiments the whole thing is cleared up, and with what satisfaction for us! The matter is now decided: the cyanic aid of Serullas is identical with that from urea. . . . Ich bin ganz närrisch vor Freude, dass unser Kindlein nun fehlerlos in die Welt gesetzt wird, ohne Buckel oder Klumpfuss."

[It had been suggested to attack the fulminic acid again.] "The fulminic acid we will allow to remain undisturbed. Like you, I have vowed to have nothing more to do with this stuff. Some time back I wanted, in connection with our work, to decompose some fulminating silver by means of ammonium sulphide; at the moment the first drop fell into the dish the mass exploded under my nose. I was thrown backwards, and was deaf for a fortnight, and became almost blind."

The work on cyanic acid appeared in Poggendorff's *Annalen* during the last month of 1830, and Wöhler was able to send the "Kindlein" "im neuen Kleide," as he says, with a New Year's greeting to his friend. Liebig had suggested fresh work, but at the moment Wöhler was in no humour to attack anything organic. The Swedish chemist, Sefström, had just announced the existence of a new element in the slag of certain iron ores, and this very substance had slipped through Wöhler's fingers unperceived. "I was an ass," he wrote to his friend, "not to have detected it two years ago in the lead ore from Zimapan in Mexico. I was busy with its analysis, and had found something strange in it, when I was laid up for some months in consequence of breathing hydrofluoric acid, and so the matter was allowed to rest. Meanwhile Berzelius sends me word of its discovery by Sefström in Swedish bar iron and in slag. It is very like chromium, and just as remarkable. Moreover, it is the same metal that Del Rio found in the Mexican lead ore, and called erythronium: Descotils, however, had declared this ore to be lead chromate."

Wöhler, no doubt, found a ready sympathiser in Liebig, to whom, not many years before, a similar experience had happened. We all know the story of the young chemist whose unscientific use of the imagination cost him the discovery of the element *bromine*. Wöhler had sent some of the substance to Stockholm, and Berzelius wrote as follows:—

"Jacob Berzelius to Frederick Wöhler

"*Stockholm, January* 22, 1831

"As to the small quantity of the body marked ? I will relate the following story:—'In the far north there lived in the olden time the goddess Vanadis, beautiful and gracious. One day there came a knock at her door. The goddess was in no hurry, and thought "They can knock again"; but there came no further knock, for he who knocked had passed on. The goddess, wondering who it could be that cared so little to be let in, ran to the window and recognised the departing one. "Ah!" said she to herself, "it is that lazy fellow, Wöhler! He richly deserves his name, since he cares so little to come in." Some days after, some one else knocked, repeatedly and loud. The goddess opened the door herself; it was Sefström who entered, and, as a consequence of their meeting, vanadium came to light.' Your specimen with the ? is, in fact, vanadium oxide.

"But he that has found the mode of artificially forming an organic body can well renounce the discovery of a new metal; indeed, one might have discovered ten unknown elements without as much skill as attaches to the masterly work which you and Liebig have carried out together and just communicated to the scientific world."

In 1831 Wöhler was called from Berlin to Cassel, and for some little time he was wholly engaged in the planning and erection of his new laboratory at the Gewerbe-Schule in that town. In the spring of the following year he was again ready for a new research; and this time it was to be the finest piece of work that the two investigators jointly engaged in. It was, in fact, to be the classical research on bitter almond oil. On May 16, 1832, Wöhler wrote to Liebig:—"Ich sehne mich nach einer ernsten Arbeit, sollten wir nicht die Confusion mit dem Bittermandelöl in's Reine bringen? Aber woher Material?" It must have been a *förscherblick* amounting to inspiration which led Wöhler to take up this subject; but neither he nor Liebig could have been wholly conscious of the consequences which were to follow from their work. To-day oil of bitter almonds is made artificially in Germany by the hundredweight: at that time the investigators could only obtain it in small quantities from Paris. They had indeed to thank Pelouze for the material with which they worked. Wöhler made this his greatest research under the cloud of a great sorrow: after barely two years of married life he lost his wife. Liebig, in the tenderest manner, brought him over to Giessen, and sought to win him from his grief and the sense of his loneliness by his company and the wholesome distraction of their joint work made side by side.

On August 30, 1832, Wöhler wrote to Liebig from Cassel:—

"I am here back again in my darkened solitude. I do not know how I shall thank you for the affection with which you received me and kept me by you for so long. How happy was I that we could work together face to face.

"I send you with this the memoir on bitter almond oil. The writing has taken me longer than I anticipated. I want you to read through the whole with the greatest care, and to notice particularly the numbers and formulæ. What does not please you, alter at once. I have often felt that there was something not quite right, without being able to find what was right."

I shall not attempt to dwell upon the outcome of this great work. The investigation on the radicle of benzoic acid will ever remain one of the greatest achievements in the history of organic chemistry: the work was indeed epoch-making in the far-reaching nature of its consequences. It was full of facts and rich in the promise of new material; a veritable mine from which subsequent workers like Cannizzaro, Fehling, Piria, Stas, and Hlasiwetz have dug rich treasure. The immediate effect of the paper was to establish the doctrine of organic radicles by demonstrating the existence of groups of bodies which had their analogues and prototypes in inorganic chemistry. The concluding words of the memoir strike, in fact, the keynote of the whole investigation. "In once more reviewing and connecting together the relations described in this memoir," so wrote Liebig and Wöhler, "we find that they may be grouped round a common nucleus which preserves intact its nature and composition in its associations with other bodies. This stability has induced us to regard this nucleus as a kind of compound element, and to propose for it the special name of 'benzoyl.'"

A significant feature in the memoir was that each of the substances described and correlated was the type of a distinct group of bodies, some of which were known, but of which the analogies and relations were unthought of; others of these bodies were yet to be discovered, a matter of little difficulty when the modes of their origin had been indicated. The effect of this memoir on the chemical world was instantaneous. Berzelius was delighted. "The facts put forward by you," he wrote to Wöhler and Liebig, "give rise to such considerations that they may well be regarded as the dawn of a new day in vegetal chemistry. On this account I would propose that this first-discovered radicle composed of more than two elements should be named *proin* (from πρωΐ, the beginning of day) or *orthrin* (ὄρθρος, daybreak), terms from which names like *proic acid, orthric acid, proic chloride, orthric chloride*, &c., could be readily derived."

Wöhler remained in Cassel for nearly five years. In the autumn of 1835 died Strömeyer, Profes or of Chemistry in the University of Göttingen. Opinions were divided as to his successor; the choice lay between Liebig and Wöhler. Eventually Wöhler was selected, and entered on his work at Göttingen in the early part of 1836. He was succeeded at Cassel by Bunsen, who was at that time *privat docent* in Göttingen. In the October of that year Wöhler was again ready for fresh work. He writes to Liebig: "I am like a hen which has laid an egg and straightway sets up a great cackling. I have this morning found how oil of bitter almonds containing prussic acid may be obtained from amygdalin, and would propose that we jointly undertake the further investigation of the matter, as it is intimately related to the benzoyl research, and it would seem strange if either of us should work alone again in this field, denn es lässt sich gar nicht absehen wie weit es sich erstreckt, und ich glaube es ist gewiss fruchtbar, wenn es mit Deinem Mist gedüngt wird. . . ." In a couple of days afterwards Wöhler was ready with the fundamental facts for the research and had sketched out its plan. He writes:—

"I have just made a most remarkable discovery in relation to the amygdalin. Since it appeared that bitter almond oil might be obtained from amygdalin, it occurred to me that the one might be converted into the other by simply distilling almonds with water by an action similar to that of a ferment upon sugar, the change in this case being due in all probability to the albumen in the almonds. And this idea seems to be completely established. The facts are as follows:

"1. Amygdalin, dissolved in water and digested with a bruised sweet almond, begins almost immediately to smell of bitter almond oil, which after a time may be distilled off in such quantity that it would appear that the amygdalin was wholly transformed into it.

"2. A filtered emulsion of sweet almonds produces the same effect.

"3. A boiled emulsion of sweet almonds, in which, therefore, the albumen is coagulated, affords not the smallest trace of oil with amygdalin.

"4. Bruised sweet almonds, covered with alcohol, and freed therefrom by pressure, transform, as before, amygdalin into bitter almond oil.

"5. Bruised peas, or the albumen they contain, give no oil with amygdalin.

"There are three points, therefore, to be ascertained—

"*a*. What is the substance in bitter or sweet almonds which, in contact with amygdalin and water, forms bitter almond oil?

"*b*. Is the action by double decomposition or catalytic, like that of a ferment?

"*c*. What is the other product which in all probability is formed in addition to the oil and prussic acid?"

The merest tyro in organic chemistry to-day is familiar with the broad features of this investigation, and knows the answers which Liebig was able to give to his friend's interrogatories. The third substance Liebig discovered to be sugar. Under the influence of a nitrogenised ferment, termed by Liebig and Wöhler *emulsin*, amygdalin, in presence of water, is decomposed into benzaldehyde (bitter almond oil), prussic acid, and sugar (glucose), thus:—

$$\underset{\text{Amygdalin}}{C_{20}H_{27}NO_{11}} + \underset{\text{Water}}{2H_2O} = \underset{\text{Benzaldehyde}}{C_7H_6O} + \underset{\text{Prussic acid}}{CNH} + \underset{\text{Glucose}}{2C_6H_{12}O_6}.$$

It simply remains to explain why this reaction only occurs when the almonds are bruised and digested with water. Both the emulsin and the amygdalin exist together in the almonds, but are contained in separate cells, and are only brought into contact by the rupture of the cell-walls and the solvent action of the water. Amygdalin was the prototype of a large and important group of substances classed together as the glucosides.

At the instigation of Wöhler, the friends again returned to the question of the chemical nature of uric acid, and the memoir which they eventually published on the subject is of the profoundest interest, not only to the chemist, but also to the physiologist. Uric acid, originally discovered by Scheele, was shown in 1815, by William Prout, then a boy of nineteen, to be the main constituent of the solid excreta of reptiles; other chemists had succeeded in obtaining various derivatives from it, indeed Prout himself had prepared from it the so-called purpuric acid, a substance which years after as murexide obtained a transitory importance in the arts as a colouring matter. But nothing was known concerning the constitution of the body or of its relations to its derivatives until Wöhler and Liebig attacked the problem. The extraordinary mutability of uric acid, which had baffled and deceived previous investigators, was to Wöhler and Liebig the very clue to the labyrinth leading to a veritable treasure-house, and the wonderful insight and rare analytical skill of these two great men were never more clearly indicated than in the way in which they trod this intricate maze. No fewer than fifteen new bodies were added to the list of chemical compounds, and these were correlated with the same masterly lucidity that was so strikingly exhibited in the memoir on the radicle of benzoic acid. Some of the greatest triumphs of modern chemistry are seen in the synthesis of organic bodies. That organic chemistry was about to advance along this line was clearly foreseen by Wöhler and Liebig. In opening their account of this the last great work they did in common, they say:—"From this research, the philosophy of chemistry will draw the conclusion that the ultimate synthetical formation in our laboratories of all organic bodies, in so far as they are not organised (in so weit sie nicht mehr dem Organismus angehören), may be regarded as not only probable but as certain. Sugar, salicin, morphin will be artificially obtained. As yet we know nothing of the way by which this result is to be attained, inasmuch as the proximate materials for forming these bodies are unknown; but we shall come to know them."

Henceforth the friends worked but little in common. Liebig's energies were spent in other directions, and Wöhler turned his attention to inorganic chemistry. Time allows only the very briefest mention to be made of his more important discoveries in this department of the science. We have first his isolation of crystalline boron, and the preparation of the compounds of boron with aluminium and nitrogen, work done in concert with Sainte-Claire Deville. The readiness with which boron unites with nitrogen, and the mode in which the compound may be decomposed, led Wöhler to a conception of the origin of boric acid and borax in the volcanic waters in which they are frequently found. In collaboration with Buff he discovered the spontaneously inflammable hydride of silicon, the analogue of marsh gas, the simplest of the hydrides of carbon, and thereby laid the foundation-stone of a superstructure, which in time to come may only be less imposing than that built up of the compounds of carbon. Many years ago Wollaston noted the presence of beautiful lustrous copper-coloured cubes in the slags from the iron blast-furnaces, which he assumed to be metallic titanium; Wöhler proved this substance to be a compound of carbon, nitrogen, and titanium, and showed how it might be obtained. Of all the elements known to the chemist up to the period of Wöhler's cessation from work, it may be safely averred that there was not one but had passed through his hands in some form or other. Now he was busy with chromium, then with cerium, next with uranium and the platinum metals; titanium, tantalum, thorium, thallium, tungsten—all came in for some share of his attention. Of the minerals and meteorites he analysed, the number is legion; indeed, as Prof. Hofmann says, whoever sent him a piece of meteoric iron gained his heart. His restless activity was a source of continual wonder to his friends. "How happy art thou in thy work!" wrote Liebig on one occasion; "thou art like the man in the Indian fable, who when he laughed dropped rose from his mouth."

The names of Liebig and Wöhler are now so closely intertwined in the history of chemistry that it is hardly possible to avoid comparing the men. Such a comparison has already been drawn by one who of all others is most fitted to draw it. "Liebig," says Dr. Hofmann, "fiery and impetuous, seizing a new thought with enthusiasm, and giving to it the reins of his fancy, tenacious of his convictions, but open to the recognition of error, sincerely grateful, indeed, when made conscious of it,—Wöhler, calm and deliberate, entering upon a fresh problem after full reflection, guarding himself against each rash conclusion, and only after the most rigorous testing, by which every chance of error seemed to be excluded, giving expression to his opinion —but both following the path of inquiry in their several ways, and both animated by the same intense love of truth! Liebig, irritable and quick to take offence, hot-tempered, hardly master of his emotions, which not unfrequently found vent in bitter words, involving him in long and painful quarrels,—Wöhler, unimpassioned, meeting even the most malignant provocation with an immovable equanimity, disarming the bitterest opponent by the sobriety of his speech, a firm enemy to strife and contention—and yet both men penetrated by the same unswerving sense of rectitude! Can we marvel that between two such natures, so differently ordered, and yet so complementary, there should ripen a friendship which both should reckon as the greatest gain of their lives?"

Who can fully gauge the influence of such a nature as Wöhler's? How it was exerted on Liebig is indicated in the following letter:—

"FREDERICK WÖHLER TO JUSTUS LIEBIG

"*Göttingen, March* 9, 1843

"To make war against Marchand, or, indeed, against anybody else, brings no contentment with it, and is of little use to science. . . . Imagine that it is the year 1900, when we are both dissolved into carbonic acid, water, and ammonia, and our ashes, it may be, are part of the bones of some dog which has despoiled our graves—who cares then whether we have lived in peace or anger; who thinks then of thy polemics, of the sacrifice of thy health and rest for science?—Nobody. But thy good ideas, the new facts which thou hast discovered, these, sifted from all that is immaterial, will be known and remembered to all time. But how comes it that I should advise the lion to eat sugar!"

It was thus in philosophic contentment, happy in his work, in his home life, and in his friendships, that Wöhler lived out his fourscore years and two. He made Göttingen famous as a school of chemistry; at the time of the one-and-twentieth year of his connection with the university it was found that upwards of 8000 students had listened to his lectures or worked in his laboratory. He was a man whom the world has delighted to honour; and there was hardly an academy of science or a learned society which has not in some way or other recognised his services to science. He was made a Foreign Member of the Royal Society in 1854, a Corresponding Member of the Berlin Academy in 1855, Foreign Associate of the Institute of France in 1864, and in 1872 he received the Copley Medal from the Royal Society. On September 23, 1882—

"He gave his honours to the world again,
His blessed part to heaven, and slept in peace."

METEORIC DUST

SIR WILLIAM THOMSON has sent us the following communication for publication:—

"*Portkil, Kilcreggan, March* 13, 1884

"DEAR SIR WILLIAM THOMSON,—Herewith I inclose some of the meteoric dust collected on a cotton filter, and both ignited at a red heat. The change of colour is interesting.

"On Saturday, March 1, the snow lay 5½ inches deep at 8 a.m., pure and white. At 9.15 a.m., when I next noticed it, it was sooty looking, the blackish appearance penetrating half an inch only. The sky was clear and calm, any tendency to movement of the air being from the south-east.

"I carefully measured a superficial foot on an outlying field sloping to the south-west at a spot bisected by the 200-foot line of the Ordnance Survey, and collected the snow into two bowls of white delft, half into each. After evaporating the snow water, thoroughly drying the residue, I collected and weighed it, that from one giving 1⅞ grains, and the other 2⅛ grains, or 4 grains to the square foot exactly.

"I can personally vouch for the dust being all over the Roseneath peninsula, as I trudged through the snow to Coulport on Loch Long, and found it the same all the way north, also on the top of the Gallow-hill (414 feet). I have since seen those who noticed it at Garelochhead, so that on this peninsula alone, taking 4 grains as an average, there has fallen over 100 tons.

"From hearsay it appears to have been noticed from Kippen on the north to Largs on the south, and from Hamilton on the east to Dunoon on the west, or over an area (in round numbers) of 810 square miles, and admitting the former estimate, we have the astonishing aggregate of say 5760 tons! A weighty gift to Mother Earth, surely of some value.

"I should mention that every crack, scratch, or depression in the glaze of the bowls was filled with the finely divided matter; it was impossible, therefore, to collect it *all* for weighing, consequently 4 grains per square foot is under rather than over the probable average. The observer at Kippen, too, mentions that the snow was permeated there for one inch by the sooty appearance.

"On Monday (March 3), after snow had fallen to the depth of an additional 8 inches, I watched for a recurrence of the phenomenon, and on the sky clearing about midnight I fetched in a dish that I had left outside and found a little had fallen in small flakes; these had melted their way through the snow, leaving little tunnels about the size of crow-quills. The quantity, however, was exceedingly small. Tendency to movement of the air as before from the south-east. Barometer had risen from 29·4 at 2 p.m. to 29·6, steady at midnight, thermometer 42°. On Saturday previous barometer stood at 30·05 (90 feet above sea-level, aneroid), thermometer 44°, 12 noon. The dust I left with you previously contains a little organic matter (grassy fibre), though what I had under the microscope appeared entirely metallic.

"The snow had melted a good deal before I recognised the importance of obtaining a fair sample. My children, however, had rolled a huge snowball down the slope, at the top of which the cottage stands, and this had increased as it rolled until it was something like 6 feet in diameter, and so formed a mine from which to collect the dust. There is still some of the black water in process of evaporation; should you require it more of the dust is at your service.

"One of the older inhabitants remembers a similar occurrence here in 1828 on the 20th or 22nd of March, when the snow, he says, fell in black or sooty flakes.

"Perhaps it is well to mention that the goats suffered somewhat from influenza on Sunday and Monday, and that I myself had a sharp attack followed by severe headache for a day, caused probably by inhaling a minute quantity of the dust snuff fashion. It might have been from something else, only the coincidence is suggestive of caution.

"I am, yours faithfully,
"LEWIS P. MUIRHEAD

"Professor Sir William Thomson, Glasgow University"

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Boards for Medicine, Physics and Chemistry, and Biology and Geology, after joint deliberation, have recommended an important change in the appointments of Natural Science Examiners. It has been a regulation of the Natural Science Tripos that all answers shall be looked over by two examiners out of the eight, but it has become increasingly difficult to find examiners with the requisite extent of knowledge. Thus it practically happens that each examiner is sole examiner in a single subject, and the places of candidates are often practically dependent on the judgment of a single examiner to an extent unknown in the other Triposes. It is now recommended that two examiners shall be appointed in each subject of Natural Science, to undertake all the University Examinations in that subject, and thus the Natural Sciences Tripos, the Special Examinations for the ordinary B.A., and portions of the M.B. Examinations, will be brought into one system. The examiners should never both be changed at the same time. The payments recommended are—for each examiner in Physics and in Chemistry, 50*l.*; in Botany, Zoology, Human Anatomy, and Physiology, 40*l.*; in Geology, 20*l.*; and Mineralogy, 10*l.*

SCIENTIFIC SERIALS

Journal of the Franklin Institute, No. 697, January.—W. Dennis Marks, note on the losses per horse-power by condensation of steam in pipes and cylinders of engines.—De Volson Wood, the cheapest point of cut-off.—Prof. R. H. Thurston, the theory of turbines. This is the conclusion of a very valuable mathematical paper given in a very full abstract.—B. N. Clark, water-line defence and gun-shields for cruisers.—W. Dennis Marks, economy of compound engines.—Prof. E. J. Houston, the Delany synchronous-multiplex system of telegraphy. This invention is founded on La Cour's phonic wheel, and bids fair to supersede harmonic multiple telegraphs.

Annalen der Physik und Chemie, xxi. January.—O. Fröhlich, measurements of sun-heat. Describes amongst other matters a new pyrheliometer with a special thermopile arrangement.—A. W. Velten, the specific heat of water. The results confirm Regnault's values.—E. Pirani, on galvanic polarisation. The values are estimated by a compensation method.—W. Hittorf, on electric conductivity of gases (first part).—A. Oberbeck, on electric oscillations and on phenomena of polarisation caused thereby.—A. Toepler, on the estimation of horizontal magnetic intensity by use of the balance.—W. von Bezold, a simple experiment on the connection between the temperature of an incandescent wire and the composition of the light emitted by it. A platinum wire is stretched horizontally through the tip of a Bunsen burner and examined in a spectroscope with horizontal slit.—E. Ketteler, reply to Herr Voigt's criticisms.

No. 2, February.—S. Czapski, on the thermal variation of the electromotive force of galvanic batteries, and its relation to their free energy.—J. Kollert, on the properties of flame in their electrical relation. Confirms the previous measurements of Elster and Geitel.—F. Fuchs, on a compensation-method for estimation of the resistance of unpolarisable elements. A modification of Poggendorff's well-known method.—E. Budde, on the theory of thermo-electric forces.—H. Lorberg, on electrostriction. A discussion of Quincke's results.—B. Weinstein, on the calculation of the potential of coils. A mathematical paper.—A. von Waltenhofen, on an instructive experiment which may be made with asymmetrical thermopiles. On passing an independent current through the thermopile certain non-reversible phenomena of polarisation are observed arising from the asymmetry of the junctions that are heated.—C. Christiansen, on the emission of heat from uneven surfaces.—A. Tschirch, researches on chlorophyll and some of its derivatives.—W. Holtz, a lecture experiment in proof of the law that the velocity of rotation increases as the rotating masses approach the axis.

Journal de Physique, tome iii. No. 2, February.—G. Lippmann, physical definition and determination of absolute temperatures. This is the first part of a communication in which the author seeks to find stricter thermodynamic definitions of temperature. He attributes to Carnot the discovery of the scale of absolute temperature!—D. Gernez, researches on the duration of the solidification of sulphur, and on a new variety of sulphur. The crystallisation in octahedra takes from 25 to 100 times as long as the crystallisation in rhombic prisms. The new crystalline kind obtained by M. Gernez is in the form of very elongated prisms of a nacreous texture. They are produced by rubbing the side of the test-tube containing the surfused sulphur with the end of a platinum wire or glass rod. When these crystals are introduced into surfused sulphur, they determine a growth of similar crystals throughout the mass; and the formation is much more rapid than that of either of the previously known forms.—E. Mathieu, suspension of a liquid by a capillary vertical tube.—E. Mathieu, modification of the pressure of a liquid by capillary forces.

Rendiconti del R. Istituto Lombardo, Milan, January 24.—Biographical memoir of Emilio Cornalia (1824–1883), by Prof. Leopoldo Maggi.—Necrological notice of the late Camillo Hajech.—*Résumé* of the meteorological observations made at the Brera Observatory, Milan, during the year 1883, by E. Pini.—Some applications of Cournat's principle of least effort to the equilibrium of linked systems (theoretical mechanics), by Prof. G. Bardelli.—Meteorological observations made at the Brera Observatory during the month of January, 1884.

SOCIETIES AND ACADEMIES

London

Mathematical Society, March 13.—Prof. Henrici, F.R.S., president, and subsequently Mr. S. Roberts, F.R.S., vice-president, in the chair.—The Rev. A. C. E. Blomfield, Messrs. J. Chevallier, E. H. Hayes, R. S. Heath, and Prof. J. Larmor were elected Members.—Mr. Tucker read a paper by Prof. M. J. M. Hill on the closed funicular polygons belonging to a system of coplanar forces having a single resultant; and communicated a paper by Prof. J. Larmor, on the direct application of the principle of least action to dynamical analogues.—Mr. J. W. L. Glaisher, F.R.S., read a paper on the square of Euler's series.—Mr. J. J. Walker, F.R.S., communicated a note by Mr. J. Griffiths, further results from a theory of transformation of elliptic functions.—Mr. S. Roberts, F.R.S., read a note concerning the Pellian equation.

Physical Society, March 8.—Prof. Guthrie, president, in the chair.—Lord Rayleigh read a paper on the electro-chemical equivalent of silver. The determination was made by a method described to the last meeting of the British Association at Southampton, which consists in using two fixed coils and a movable coil suspended between these from one end of a balanced beam. These coils are in circuit with the current and voltameter. The current is reversed in the fixed coils at intervals of five minutes, and the weight required to bring the balance even is noted. The calculation of the effect by this method is independent of the precise measurement of the coils. Two or more silver voltameters were in circuit, nitrate of silver being the solution used. Careful precautions of various kinds were taken, and the result was that unit C.G.S. current deposits $1 \cdot 118 \times 10^{-2}$. It follows that 1 ampere will deposit 4·025 grm. of silver per hour.—Lord Rayleigh also read a paper on the absolute electromotive force of Clark's cell. Experiments made at the Cavendish Laboratory gave the electromotive force of this cell as 1·453 volts. The accepted value is 1·457 volts. If the B.A. unit (as Lord Rayleigh believes) is about ·9867 of a true ohm, the result, 1·453, becomes 1·434 volts.—Lord Rayleigh also mentioned that he had been making experiments on the rotation of the plane of polarised light in bisulphide of carbon, and obtained a result agreeing more nearly with Gordon's than with Becquerel's.—Profs. Guthrie and Ayrton spoke on the papers, the former eliciting the reply that electro-corrosion was less satisfactory than electro-deposition for determining the equivalent; and the latter that silver was better than copper for accurate results in the voltameter.—Mr. Shelford Bidwell, M.A., read a paper on some experiments illustrating an explanation of Hall's phenomenon. By these experiments Mr. Bidwell sought to explain Hall's effect through a combination of mechanical stress and the well-known Peltier effect on the thin metal plate which is placed between the poles of the magnet. He repeated many of the experiments, and showed how he had obtained the same results as Hall, except in the case of aluminium, which he found to be + like iron, whereas Hall made it −. Mr. Bidwell reversed the effect by cutting two slits in the strip of metal, thereby altering the stress on it. Righi's effect was also explained on the same grounds. Mr. Walter Browne said that difference in the quality of the aluminium might explain the anomaly with this metal. Prof. Perry criticised the explanation of the slitted plate, and Prof. G. C. Foster suggested that results in absolute measure should be obtained.

Edinburgh

Royal Society, February 18.—Sheriff Forbes Irvine, vice-president, in the chair.—Prof. Tait read a paper on radiation, in which he called attention to Stewart's papers of 1858 as containing, so far as it has yet been developed, the theory of exchanges. Yet, in the most recent authoritative treatise on the subject, the name of Stewart is not even once mentioned. The basis of the whole theory is Carnot's principle, and therefore no demonstration can be considered absolutely rigorous. Thus it is probable that as there are very hot particles in a gas at ordinary temperatures, so there may be feeble radiation of high wavelengths from a black body at ordinary temperatures.—Mr. Saug read a paper on the need for decimal subdivisions in astronomy, trigonometry, and navigation, in which he pointed out the inconvenience of the sexagesimal system, and estimated it as doubling the labour of calculation. The decimal division of the second, used throughout the *Nautical Almanac*, was appealed to as evidence of the need for a change. The paper was accompanied by a number of tables suited to the decimal division of the quadrant, or useful therefor.—Prof. Ewing communicated a paper by A. Tanakadaté on an electromagnetic declinometer.—Prof. Tait showed that when one polygon has its corners at the middle points of the sides of another, the condition that the first, second, or *n*th derived polygon shall be similar to the original, involves a singular equation in quaternion differences.—Prof. Tait also made some remarks on the basis of the theory of vortex atoms, pointing out that there is not necessarily any direct action between vortices in a perfect fluid; the present theory, which indicates such action, being based upon the assumed continuity of motion throughout the fluid.

Paris

Academy of Sciences, March 10.—M. Rolland in the chair.—The election of M. G. Darboux was announced, as successor to the late M. Puiseux in the Section of Geometry.—On the forms presented by the nucleus of the Pons-Brooks comet on January 13 and 19 (one illustration), by M. Faye. The author rejects the explanation of these remarkable forms proposed by Bessel, who attributed to the nucleus a polarity like that of the magnetic forces. In virtue of this polarity the nucleus and anterior nuclear emission are supposed to oscillate in presence of the sun like the needle of a compass in presence of a magnet. But M. Faye sees in these changes nothing but the effect of a rotatory motion powerfully affected by solar attraction. Under these conditions the rotation may acquire irregular pendulant vibrations without having recourse to the intervention of polar forces.—Explosive gaseous mixtures; calculation of their temperatures and specific heat at the moment of explosion (continued), by MM. Berthelot and Vieille. Tables of the results of these experiments are appended for the oxyhydric and oxycarbonic mixtures, for cyanogen, and the carburets of hydrogen.—Note on a letter of the astronomer Méchain in connection with the completion of the triangulation of Spain and the extension of the meridian to the Balearic Isles, by M. J. Lefort.—On a differential equation of the third order, by M. E. Goursat.—On the decomposition of polynomes which admit only of primary divisors of a determined form, by M. Lefébure.—On the remarkable variation of the nucleus of the Pons Brooks comet (one illustration, by M. Ch. Trépied.—On the barometric oscillations produced by the Krakatoa eruption, by M. P. Tacchini.—On the crepuscular and auroral lights observed at Morges, in Switzerland, during the winter of 1883-84, by M. Ch. Dufour.—On a method for measuring the coefficient of cubic expansion of solid substances in the form of minute particles, by M. J. Thoulet. To determine the coefficients of these bodies the author employs a solution of iodide of mercury in iodide of potassium. The extreme delicacy of the process is shown by its application to quartz, which yields a coefficient of 0·0000357 compared with M. Fizeau's 0·0003619.—On the action of two consecutive parts of the same electric current, by M. A. Buguet.—On the spectrum of absorption of water; preliminary studies connected with the spectral analysis of the rays transmitted through a more or less dense layer of water, by MM. J. L. Soret and Ed. Sarasin.—Action of electric effluvia on oxygen and nitrogen in the presence of chlorine, by MM. P. Hautefeuille and J. Chappuis.—Observations on the formula of some sal ammoniacs, by M. R. Engel.—Observation relative to a note of M. Calmels on the poison of Batrachians, by MM. A. Gautier and Étard.—On the Malpighian vessels of the Lepidoptera, by M. Cholodkovsky.—On an aberrant form of the phylum Sporozoa, by M. J. Kunstler.—On the presence of manganese in the white cipoline marbles of Carrara, Paros, and the Pyrenees; geological deductions, by M. Dieulafait.

BERLIN

Physical Society, February 22.—Prof. Landolt produced a cylinder of solid carbonic acid he had prepared about an hour before the sitting, and described the mode of its formation. From a Natterer compressing vessel a stream of liquid carbonic acid was made to penetrate into a conical cloth bag. The bag speedily got filled with a loose snow of carbonic acid, which was then, by means of a stamper, hammered together in a cylindrical vessel into a solid cylinder. Compact carbonic acid cylinders of this kind could be touched gently with the hand, and possessed the hardness of chalk, which, too, they resembled in appearance, and on account of their brittleness did not readily admit of being cut with a knife. The specific gravity of solid hammered carbonic acid was found to be 1·2.—Prof. Schwalbe showed on a beech twig the ice-swellings he had described at the last sitting. These were produced in a moderately freezing mixture, their formation failing in a strongly freezing mixture. A twig which by way of experiment had been completely dried entirely lost the capability it previously possessed in a high degree of forming ice protuberances.—Prof. Erdmann related an observation he had made some time ago, and had since very frequently repeated. In a perfectly dark room he was able only by indirect vision to perceive an object which reflected light very faintly, while, on endeavouring to look at it fixedly, the object completely disappeared. This phenomenon he observed only in the evening in going to bed, after he had been working for a considerable time in a brightly illuminated room. On the other hand, when he awoke in the night he perceived the faintly lucent object quite as well by direct as by indirect vision. He was of opinion that this phenomenon was connected with the lassitude of the middle parts of the retina, while Prof. von Helmholtz explained it by the inferior sensitiveness to light of the yellow spot in comparison with its surroundings.—Dr. Kœnig reported at length the experiments which in common with Dr. Dietrici he had instituted with a view to determining the colour-sensitiveness of normal eyes. Exhibiting the apparatus he had made use of, Dr. Kœnig explained its construction and the procedure he had followed in the experiment. Towards one angle of a prism was directed an observing telescope, which, instead of an eye-piece, had a diaphragm provided with a slit, on which the spectrum fell, so that it was possible to observe sections of any degree of minuteness whatsoever. Towards each of the two other angles of the prism was placed a collimator, which in the focus of its lens had a slit for the entrance of the light, which was polarised by means of a Nicol prism. Behind the slit was a double refracting prism, by varying the position of which in the collimator the slit-image could be doubled at pleasure. Through the slit of the objective were seen close beside each other the spectrum of the light which had passed through one collimator, and the spectrum of light which had passed through the other. While one collimator was now kept fixed, the other, by means of micrometers, was displaced till the point was reached at which the observer found the colours in both spectra alike. The wave-lengths in both spectral stripes were then measured, and their difference was the standard of colour-sensitiveness in the single regions. For each wave-length fifty readings were in this way made by each of the two observers, and the mean difference calculated of the wave-lengths in the two spectral ranges, which were perceived to be equal. These experiments extended from the wave-length of 640 millionths of a millimetre to the wave-length of 430, and were made from each 10 millionths of a millimetre, each particular spot being examined under two different intensities of light. From the results of these measurements it was established that the colour-sensitiveness of normal eyes ranged from more than 1 to about 0·2 millionths of a millimetre. The difference of the D-lines in the solar spectrum amounted to 0·6 millionths of a millimetre. Altogether three maxima of sensitiveness were found. The first maximum appeared with the wave-length of 570 near the D-line. A second greater maximum approached the F-line with a wave-length of about 490 to 470. Finally, a third smaller maximum was found with a wave-length of 450 to 440. The place of the maximum changed with the intensity in such a manner that, the greater the intensity was, the more the maximum shifted towards the more refrangible part of the spectrum. Beyond the wave-lengths of 640 and 430 these experiments could not be carried out, because, at the red end especially, no differences of colour, but only differences of brightness, were perceived. From the colour-sensibility thus found, it was calculated that within the range of the normal spectrum the healthy eye was able to perceive about 300 differences of colour. Dr. Kœnig hoped to be able to set forth on a future occasion further experiments in conjunction with the measurements here communicated, and the consequences resulting therefrom in respect of the theory of the perception of colour.

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