THURSDAY, OCTOBER 25, 1877

SCIENTIFIC WORTHIES

XI.—SIR JOSEPH DALTON HOOKER

SIR JOSEPH DALTON HOOKER,[1] Director of the Royal Gardens, Kew, and President of the Royal Society—the second and only surviving son of the late Sir William Jackson Hooker, who first made the name illustrious in botany—was born at Halesworth, in Suffolk, on June 30, 1817. He was educated at the University of Glasgow, where the father was Regius Professor of Botany from 1820 to 1840, and where the son took the degree of M.D. in 1839. He has since been affiliated to the great English Universities, receiving in the same year the honorary degrees of D.C.L. at Oxford, and of LL.D. at Cambridge, and subsequently that of LL.D. at his own University of Glasgow. Immediately on completing his medical studies, namely in 1839, he was commissioned Assistant-Surgeon in the Royal Navy, and appointed botanist of the antarctic voyage of exploration by the *Erebus* and *Terror*, under the command of Captain, afterwards Sir James Clark Ross. This celebrated expedition, leaving England in the autumn of 1839, and touching at Madeira, Teneriffe, Cape Verde Islands, St. Helena, and the Cape of Good Hope, entered, in the spring of 1840, upon its special work of antarctic exploration, which, including visits to Kerguelen Island, New Zealand, Australia, Fuegia, and the Falkland Islands, occupied the ensuing three years, years of severe labour and much hardship, but of opportunities such as probably never before fell to the lot of a young naturalist, or were ever turned to better account.

The botanical fruits of this expedition are mainly garnered in the "Flora Antarctica," the "Flora Novæ Zelandiæ," and the "Flora Tasmanica," six quarto volumes, the last of which appeared in the year 1860. These do not contain mere reports of explorations, with descriptions of whatever was novel or of peculiar interest, but are systematically elaborated and complete floras, in which all that had been gathered from every source is incorporated. The excellent analyses of the plants, and in particular those of the cryptogams, are for the most part from the author's own drawings, many of them made during the progress of the voyage.

During the preparation of the first of this series of floras, Sir J. D. (then Dr.) Hooker, being then attached to the Geological Survey of Great Britain, brought out several important papers upon points in fossil botany. They need not be here particularly enumerated, but they attracted much attention, and evinced remarkable aptitude for dealing with a difficult class of questions.

In the interval between the publication of the second volume of the "Flora Antarctica," in 1847, and the first volume of the "Flora Novæ Zelandiæ," in 1853, Sir J. D. Hooker, aided by Government, but mainly on his private resources accomplished his botanical mission to India. A general account of this undertaking is given in his "Himalayan Journals; or, Notes of a Naturalist in Bengal, the Sikkim and Nepal Himalayas, the Khasia Mountains," &c., in two volumes, 8vo., 1854.

His journey occupied three and a half years, and was not without hardship and adventure. After spending two seasons in exploring geographically and botanically the loftiest Himalayan mountains and valleys, unaccompanied by any European, he was, when on the Tibetan frontier, joined by his friend, Dr. Campbell, the political agent resident in a neighbouring British province; this led to the capture of both by the Rajah of Sikkim, and they were imprisoned for some weeks, during which time they were treated with great indignity and their lives were threatened. During his journey in Sikkim he made a survey of the whole country and the bordering districts of Nepal, from the plains of India to Tibet; this was published by the Trigonometrical Survey Office of Calcutta, and is still the standard map of the country. It is a curious fact that though nearly thirty years have elapsed since Sir J. D. Hooker was in the country, many of the trans-Himalayan passes which he then discovered and measured have not since been visited by any other traveller.

Of the scientific results, the first fruits were given to the world in various papers communicated to the Asiatic Society of Bengal; these were followed by the "Rhododendrons of the Sikkim Himalayas," a folio, with splendid illustrations from the author's own pencil; and in a volume of the "Flora Indica," by himself and his friend and schoolmate, Dr. Thomas Thomson. A series of papers upon certain groups, or discussing special points, followed later; and at length the systematic elaboration of Indian botany has been hopefully renewed, and the "Flora of British India," upon the model of the "British Colonial Floras," has reached the second volume. Although "assisted by other botanists," some of the ablest of these died before their contributions were completed, and a large part of the labour and responsibility has devolved upon Sir J. D. Hooker.

In 1869 Sir J. D. Hooker held the presidency of the British Association at Norwich. In his address he advocated the recognition of the Darwinian hypothesis as the best means of advancing the study of the natural sciences, and dwelt at length on the subject of provincial museums, showing by what means they can be rendered both instructive to the general public and be adapted to the needs of scientific men.

On an earlier occasion, at the meeting of the Association at Nottingham in 1866, he was selected to give one of the two public lectures that form part of the programme at these annual meetings. Choosing "Insular Floras" for his subject he gave the results of his own wide personal experience in a discourse which attracted much attention on account of the soundness of the views he advocated and the originality of the illustrations by which he supported them.

Of special memoirs, such as test a botanist's capabilities, four of Sir J. D. Hooker's are particularly noteworthy. These are, in the order of publication; first, the essay "On the Structure and Affinities of Balanophoreæ," a peculiar and puzzling group of phænogamous root-parasites, here for the first time well investigated upon sufficient material, admirably illustrated, and their affinities acutely discerned. The second is a shorter paper "On

[1] Since these lines were written her Majesty has signified her appreciation of Dr. Hooker's services, especially those in relation to the Indian Empire, by conferring upon him the honour of Knight Commander of the Star of India.—A. G.

the Origin and Development of the Pitchers of Nepenthes," prefacing an account, with striking illustrations, of some new Bornean species, and bringing out the conclusion that the pitcher is a modification of a gland at the apex of the midrib of a leaf. The functions and mode of action of these pitchers became the subject of a later investigation and the theme of his address to the Section of Zoology and Botany, over which he presided, at the meeting of the British Association at Belfast, in 1874. This forms one of the earlier contributions to our new knowledge of carnivorous plants. Thirdly, the "Outlines of the Distribution of Arctic Plants," cognate with which is the elaborate "Introductory Essay to the Flora Tasmanica," published earlier in the same year, 1860. These two papers embody the results of long and wide study of the geographical distribution, systematic association, and various degrees of relationship of existing species, in regard to their probable history and origin. Having been prepared before Mr. Darwin's "Origin of Species" appeared, they are among the earliest and most notable contributions to this part of our science. They are endeavours to test the practical value in systematic botany of now familiar theoretical considerations or hypotheses, the influence of which was felt and the importance dimly divined, in advance of their full development by Mr. Darwin. Fourthly, a rare opportunity was well improved when that most extraordinary of plants, *Welwitschia mirabilis*, of Western Tropical Africa, was placed in Sir J. D. Hooker's hands for study. Later investigations of completer materials may have since cleared up points which were left doubtful, and may have definitely answered questions which were placed on the way to settlement by being suggestively raised. Still the splendid memoir on "Welwitschia, a New Genus of Gnetaceæ," stands unrivalled among botanical monographs of the kind for perfection of illustration, elucidation of structure, and insight into affinities.

Turning next to labours which came in great part by inheritance, we need only refer to the thirteen volumes of the *Botanical Magazine* which follow the thirty-seven edited by Sir William Hooker, and to the two volumes in continuation of the less popular, but botanically important, "Icones Plantarum." Then, after the exhaustion of Sir William Hooker's "British Flora" in its eighth edition, Sir J. D. Hooker replaced it by his own compendious "Student's Flora of the British Islands, 1870," which is now passing to a revised edition. Those who have made the attempt well know how the conscientious preparation of such a work tasks the best powers of a botanist. Upon the presentation, in an English dress, of Le Maout and Decaisne's "Traité Général de Botanique," in a translation by the late Mrs. Hooker, the orders were re-arranged and annotated by Sir J. D. Hooker, and a chapter on the principles of classification and a synopsis of natural groups added. Then, least in size, but not in usefulness nor in difficulty of execution, comes the "Primer of Botany," for the use of young beginners. As far as possible, elementary treatises should be written by masters in science, and Sir J. D. Hooker has contributed his fair share.

We come, at length, to the "Genera Plantarum ad examplaria imprimis in Herbariis Kewensibus servata definita." In this, one of the most arduous and, as we judge, most important botanical works of our time, Sir J. D. Hooker is associated with the veteran Bentham, who has the enviable advantage of being able to devote all his time to botanical investigation, undistracted by professorial or administrative cares, and who brings to the work the largest experience, the surest judgment, and the most indomitable industry—gifts and accomplishments rarely thus associated. As two of the three compact volumes of the "Genera Plantarum" are already published, and the third is in progress, let us hope that we may all ere long see and rejoice together over the completion of a work which marks an epoch in systematic botany. Compilations and digests we may have, as we have had; and supplements and new editions of the present work may naturally and easily be provided; but, as its only real predecessors are the "Genera Plantarum" of Linnæus (1737–64) and of Jussieu (1789)—to which we may add that of Endlicher (1836–40) the latter a wonderful monument of literary labour and bibliographical ability, directed by a fair amount of botanical knowledge—so we may expect that a long generation will pass before an undertaking like the present will be again attempted and carried through.

If this cursory reference to the publications of one who is still, as we fondly hope, only in mid-career, were extended into details and specifications, it would still be far from giving a full idea of the extent and value of Sir J. D. Hooker's scientific services. While his colleague, to whom reference has been made, supplies a notable instance of what may be accomplished by one who (without declining a reasonable share of public duty) has been mainly free from engrossing administrative cares, the life of Sir J. D. Hooker, like that of his father, has fallen in the common lot of scientific men. Or rather, in both the Hookers, unusual gifts and energies have entailed more than ordinary cares and responsibilities. To develop and to sustain and extend such noble and invaluable establishments as those at Kew Gardens was a duty not to be declined, however engrossing. It has been performed in such wise as to win, along with national applause, the gratitude of the scientific world. Throughout his travels and voyages his energies were directed by his father to the advancement of Kew as a centre of scientific botany, and as a means of transmitting to all parts of the world plants useful to mankind. In 1855 he was appointed Assistant-Director, and since the death of his venerated father, in 1865, the burden of maintaining the Gardens at the high condition they had attained through the father's exertions has fallen on the son.

Botanists all over the world count this devotion to Kew Gardens high among Sir J. D. Hooker's scientific services. They admired and cheered the courageous and indomitable spirit with which he resisted and thwarted the attempt of a whilom official guardian to lower the character and diminish the scientific value of this most useful establishment. They rejoice, likewise to see the presidential chair at the Royal Society occupied for the second time by a botanist and explorer. They concede the paramount claims of public duty, yet not without a shade of jealousy and regret; for administration is time-consuming and endless, while Hookers and their like are few, and botanical work on every side is pressing.

A critical exposition and estimate of the work which Sir

J. D. Hooker has already accomplished, and by which his high scientific position has been earned, must needs be either too technical or too long for a sketch like this. Moreover, the Atlantic is no longer what it once was, when a judgment wafted across it either way was invested with somewhat of the character of the verdict of posterity. And the close relations for forty years of the present writer with the Hookers, father and son, disqualify him for the office of judge. Let that duty devolve upon our successors.

The knowledge and experience of most of our eminent botanists have been gained, and their work mainly done, in the herbarium and botanic garden. No living botanist that we know of has shared Sir J. D. Hooker's opportunities of studying in place the living vegetation of so many parts of the world; these include, besides those already mentioned as places visited during his Antarctic voyage, the southern shores of Europe, North Africa, Palestine, and India to the Chinese borders. When we have welcomed him to New England, as we hope to do before these lines are in print, and when he has traversed our continent from the Atlantic to the Pacific, it may confidently be affirmed that he has seen far more βοτάνη than ever fell to the lot of any other of his craft.

Sir J. D. Hooker was elected President of the Royal Society in 1873, an office which he still holds.

May, 1877 ASA GRAY

NOTES ON THE BOTANY OF THE ROCKY MOUNTAINS

[The contemplated visit to the United States of America alluded to in the preceding article has now been accomplished, and Sir Joseph Hooker has favoured us with the notes of his journey following.]

IN company with Dr. Asa Gray, Professor of Botany of Harvard University, Cambridge, U.S., I availed myself of an oft-repeated invitation to us both from Dr. Hayden, the distinguished chief of the Topographical and Geological Survey of the United States Territories, to join the Survey in Colorado and Utah; this we did with the view of instituting a comparison between the floras of these central and elevated territories and those of other parts of the continent, and thus obtaining some insight into the origin and distribution of the North American flora. In order to comprehend the importance of Colorado and Utah as the basis for such investigations, I should state that they occupy a very central position in the continent, and include a section of the Rocky Mountains about 300 miles long and about as broad, namely, from N. lat. 37° to 41°, and from W. long. 105° to 112°.

The mountain region thus limited consists of extensive and often level floored valleys, sometimes many miles broad, and elevated 4,000 to 5,000 feet above the sea, called "parks" in local topography, which are interposed between innumerable rocky mountain ridges of very various geological age and formation, which often reach 12,000 feet, and sometimes 14,000 feet elevation, the maximum being under 14,500.

Those of the so-called parks which are watered by rivers that flow to the east are continuous with the prairies that lie along the eastern flanks of the Rocky Mountains; those watered by rivers that flow to the west are continuous with the so-called desert or salt regions that lie along the western flanks of the range; but the divides between the head waters of the streams that flow either way are often low, and the botanical features of the east and west may hence meet and mix in one park.

Such a section of the Rocky Mountains must hence contain representatives of three very distinct American floras, each characteristic of immense areas of the continent. There are two temperate and two cold or mountain floras, viz.: (1) a prairie flora derived from the eastward; (2) a so-called desert and saline flora derived from the west; (3) a sub-alpine; and (4) an alpine flora; the two latter of widely different origin, and in one sense proper to the Rocky Mountain ranges.

The principal American regions with which the comparison will have first to be instituted are four. Two of these are in a broad sense humid; one, that of the Atlantic coast, and which extends thence west to the Mississippi river, including the forested shores of that river's western affluents; the other that of the Pacific side, from the Sierra Nevada to the western ocean: and two inland, that of the northern part of the continent extending to the Polar regions, and that of the southern part extending through New Mexico to the Cordillera of Mexico proper.

The first and second (Atlantic plus Mississippi and the Pacific) regions are traversed by meridional chains of mountains approximately parallel to the Rocky Mountains; namely, on the Atlantic side by the various systems often included under the general term Appalachian, which extend from Maine to Georgia, and on the Pacific side by the Sierra Nevada, which bounds California on the east. The third and fourth of the regions present a continuation of the Rocky Mountains of Colorado and Utah, flanked for a certain distance by an eastern prairie flora extending from the British possessions to Texas, and a western desert or saline flora, extending from the Snake River to Arizona and Mexico. Thus the Colorado and Utah floras might be expected to contain representatives of all the various vegetations of North America except the small tropical region of Florida, which is confined to the extreme south-east of the Continent.

The most singular botanical feature of North America is unquestionably the marked contrast between its two humid floras, namely, those of the Atlantic plus Mississippi, and the Pacific one; this has been ably illustrated and discussed by Dr. Gray in various communications to the American Academy of Sciences, and elsewhere, and he has further largely traced the peculiarities of each to their source, thus laying the foundations for all future researches into the botanical geography of North America; but the relations of the dry intermediate region either to these or to the floras of other countries had not been similarly treated, and this we hope that we have now materials for discussing.

Our course and direction in America was directly westward to Colorado, where we followed the eastern flanks of the Rocky Mountains for about 300 miles, that is from Denver in the north, to near the borders of New Mexico, ascending the highest northern and southern peaks, and visiting several intermediate parks and valleys, watered by tributaries of the Arkansas, Platte, Colorado, and Rio Grande. From Denver we proceeded north to Cheyenne in Wyoming, and thence westward by the Central Pacific Railway, across the range to Ogden, and the Great Salt Lake in Utah, which lies on the base of the Wahsatch Mountains, themselves the western escarpment of the Rocky Mountains proper in that latitude. After ascending these we proceeded westward by rail through Utah, to Nevada, thus crossing the great dry region that intervenes between the Rocky Mountains and the Sierra Nevada, which is variously known as the Desert, Salt, or Sink region of North America, in accordance with the prevailing features of its several parts. It is elevated 3,000 to 4,000 feet, and traversed by numerous short meridional mountain-ridges, often reaching 8,000 feet, and rarely 10,000 feet elevation; unlike the Rocky Mountains or over the Sierra Nevada, these present no forest-clad slopes, or even a sub-Alpine flora.

From Reno, at the western base of the Sierra Nevada, we proceeded south by Carson City, flanking the Sierra for some sixty miles to Silver Mountain, when we struck westwards, ascending the Sierra, which was crossed obliquely into the Pacific slope. There we visited three groves of the 'Big Trees' (*Sequoia gigantea*) at the headwaters of Stanislaus and Tuolome Rivers, and the singular Yosemite Valley, whence we descended into the great valley of California, and made for San Francisco.

From the latter place we made excursions first to the old Spanish settlement of Monterey, which is classical ground for the botanist, as being the scene of Menzies' labours during the voyage of our countryman, Capt. Vancouver, in 1798 (whose surveys are held in the highest estimation by Prof. Davidson and the officers of the Coast Survey of the United States), whom he accompanied as botanist. Then we went northwards along the coast range to Russian River to visit the forests of Red-wood (*Sequoia sempervirens*), the only living congener of the Big Trees, and almost their rival in bulk and stature. Then to Sacramento, and up the valley of that name for 150 miles to Mount Shasta, a noble forest-clad volcanic cone about 14,400 feet in elevation. Returning thence to Sacramento we took the Union Pacific Railway eastwards, and from the highest station visited Mount Stanford, on the crest of the Sierra Nevada, and Lake Taho, which occupies a basin in the mountains at about 7,000 feet elevation, and with which we finished our western journeyings.

In California the Coniferæ were a principal study, with a view of unravelling their tangled synonymy and tracing the variations and distribution of these ill-understood trees, which attain their maximum development in number of species and in stature on the Pacific slope of the American continent.

The net result of our joint investigation and of Dr. Gray's previous intimate knowledge of the elements of the American flora is, that the vegetation of the middle latitudes of the continent resolves itself into three principal meridional floras, incomparably more diverse than those presented by any similar meridians in the old world, being, in fact, as far as the trees, shrubs, and many genera of herbaceous plants are concerned, absolutely distinct. These are the two humid and the dry intermediate regions above indicated.

Each of these, again, is subdivisible into three, as follows:—

1. The Atlantic slope plus Mississippi region, subdivisible into (α) an Atlantic, (β) a Mississippi valley, and (γ) an interposed mountain region with a temperate and sub-alpine flora.

2. The Pacific slope, subdivisible into (α) a very humid cool forest-clad coast range; (β) the great hot, drier Californian valley formed by the San Juan river flowing to the north, and the Sacramento river flowing to the south, both into the Bay of San Francisco; and (γ) the Sierra Nevada flora, temperate, sub-alpine, and alpine.

3. The Rocky Mountain region (in its widest sense extending from the Mississippi beyond its forest region to the Sierra Nevada), subdivisible into (α) a prairie flora; (β) a desert or saline flora; (γ) a Rocky Mountain proper flora, temperate, sub-alpine, and alpine.

As above stated, the difference between the floras of the first and second of these regions, is specifically, and to a great extent generically absolute; not a pine or oak, maple, elm, plane, or birch of Eastern America extends to Western, and genera of thirty to fifty species are confined to each. The Rocky Mountain region again, though abundantly distinct from both, has a few elements of the eastern region and still more of the western.

Many interesting facts connected with the origin and distribution of American plants and the introduction of various types into the three regions, presented themselves to our observation or our minds during our wanderings; many of these are suggestive of comparative study with the admirable results of Heer's and Lesquereux's investigations into the pliocene and miocene plants of the north temperate and frigid zones, and which had already engaged Dr. Gray's attention, as may be found in his various publications. No less interesting are the traces of the influence of a glacial and a warmer period in directing the course of migration of Arctic forms southward, and Mexican forms northward in the continent, and of the effects of the great body of water that occupied the whole saline region during (as it would appear) a glacial period.

Lastly, curious information was obtained respecting the ages of not only the big trees of California, but of equally aged pines and junipers, which are proofs of that duration of existing conditions of climate for which evidence has hitherto been sought rather amongst fossil than amongst living organisms.

I need hardly add that the part I played in the above sketched journey was wholly subordinate to Dr. Gray's, who had previously visited both the Rocky Mountains and California, though not with the same object. But for his unflinching determination that nothing should escape my notice which his knowledge and observant powers could supply, and Dr. Hayden's active co-operation, my own labours would have been of little avail.

Moreover, throughout the expedition we experienced great hospitality, and enjoyed unusual facilities, not only from the staff of the Geological Survey, but from the railway authorities, who franked us across the continent, and on all the branch lines which we traversed.

J. D. HOOKER

SHARPE'S CATALOGUE OF BIRDS

Catalogue of the Birds in the British Museum. Vol. I., Catalogue of the Accipitres, or Diurnal Birds of Prey. By R. Bowdler Sharpe. 1874.—Vol. II., Catalogue of the Striges, or Nocturnal Birds of Prey. By the same Author. 1875.—Vol. III., Catalogue of the Passeriformes, or Perching Birds. By the same Author. 1877. (London: Printed by order of the Trustees.)

IF the visitor to the British Museum will pause at the foot of the staircase leading up to the Paleontological Gallery and look carefully into the obscurity in the right-hand corner he will perceive a door with a brass plate on one side of it. On entering this door and descending (with care) a flight of darkened steps, he will find himself in the cellar, which has for many years constituted the workshop of our national zoologists. Two small studies partitioned off to the left are assigned to the keeper of the department and his first assistant. The remaining naturalists are herded together in one apartment commonly called the "Insect-room," along with artists, messengers, and servants. Into this room is shown everybody who has business in the Zoological Department of the British Museum, whether he comes as a student to examine the collections, or as a tradesman to settle an account. Amid the perpetual interruptions thus caused, our national zoologist has to pursue his work. Some of the specimens are here, some in the galleries overhead, and some are stored away in cellars at a still lower depth than that in which he sits at work. The library attached to the department contains merely some of the most obvious books of reference, all others have to be obtained on loan from the great national depository of books in the centre of the building. No lights are allowed, and when the fogs of winter set in, the obscurity is such that it is difficult to see any object requiring minute examination.

Under these circumstances, which we trust to see materially altered when the zoological collections are removed to their new home in South Kensington, it is more than creditable to our zoologists that they should have turned out the large amount of scientific work that has issued from their department of the British Museum during the past thirty years. The zoological catalogues of the British Museum are well-known to every worker in natural history; they are not mere catalogues, but in many cases able and exhaustive monographs of the groups of which they treat. Projected and commenced by the late Dr. Gray they have been energetically carried on under the rule of the present head of the zoological department. Dr. Günther is, moreover, himself the author of one of the most important of the series "the Catalogue of Fishes," completed in 1870, in eight volumes, which is now the standard work of reference in icthyology.

Hardly less important as regards the sister science of Ornithology, if brought to so successful a conclusion, will be the "Catalogue of Birds," of which three volumes are now before us. With youth and energy on his side Mr. Sharpe may look forward to do much, but it must be confessed that if he intends to handle the whole subject himself he has an arduous task before him. The number of recognised species of the class Aves cannot now be reckoned at less than from ten to twelve thousand. There are many more workers in ornithology than in icthyology, the literature is still more widely scattered in different magazines and periodicals, and the collections to be consulted, both public and private, are much more numerous. There is also no generally recognised system of classification to follow—such as that of the illustrious Johannes Müller—our great master in the classification of fishes. All this makes the work of a general descriptive catalogue of birds one of almost herculean labour, which only length of time and great devotion to the subject can hope to accomplish.

Let us now see what progress Mr. Sharpe has already made towards the completion of the task. In 1874 he issued the first volume of the catalogue containing the Accipitres, or diurnal birds of prey; in 1875 the second, in which the Striges, or nocturnal birds of prey, were treated of. We have now the third volume before us, in which the great order usually called "Passeres," but here denominated "Passeriformes," is commenced.

Taking this as an average volume, we find about 350 species comprised in it. Unless, therefore, the present rate of progress is materially accelerated, it does not require much calculation to show that forty or fifty years must elapse before a single worker can complete the task. Even if the volumes were henceforth published annually, and 500 species on the average comprised in each volume, upwards of twenty years would be necessary to bring the work to a conclusion, and looking to the present rapid advances being made in our knowledge of birds, the older volumes would be out of date long before the last were ready for publication. But if the present style of work is adhered to, it would seem that our last supposition is one that is hardly likely to be accomplished.

The question remains, whether any alterations can be suggested that will reduce the task to one of more reasonable proportions. It must be recollected that Mr. Sharpe's "Catalogue" is not merely a catalogue of specimens in the National Museum, but approaches in several respects more nearly to a monographic essay on each group treated of. Taking, for example, the present volume, which contains the Coliomorphæ, or Crow-like Passeres, we find, on page 4, a "synopsis" of the five families into which the Coliomorphæ are divided, next a definition of the two sub-families of the Corvidæ, the first of these families. Then follows a "key" to the thirty-nine genera of Corvidæ; these genera are taken in order, and a "key" to the species is placed at the head of each genus Under the head of each species the synonymy is very fully given; then a lengthened description of each species in its various plumages, a general account of its "habitat," and finally a list of specimens of it in the British Museum, with a notice of the locality and mode of origin of each specimen. To prepare all this it is manifest that an elaborate study of each group must be undertaken, and such a study cannot be executed without much time and attention. The result will be that we shall have, when the task is brought to a conclusion, a general work, which must become the standard book of reference for all naturalists who are engaged on the class of birds.

While the plan of work pursued by Mr. Sharpe has our full approbation as a whole, there are several points of detail in which we think improvements might be made.

The descriptions of the species are, especially in the first volume, by far too long. Short diagnoses would be of much greater assistance to naturalists using the work for the determination of species. It may be said, no doubt, that such diagnoses *are* given in the tabular keys of the species under the head of each genus. This is true to a certain extent; at the same time these keys only refer to *one* point of difference between allied species, and if a student misses the particular key selected by Mr. Sharpe he will find it very difficult to open the lock. If instead of the lengthened description, a short diagnosis were given and remarks on the points of difference between the species in question and its nearest allies were appended, the result would be at least of equal, if not of greater value, to the working ornithologist, and at the same time the work would be materially shortened. Nothing serves so clearly to explain the character of an unknown object as reference to an object well known and a specification of the points of difference. Such particulars, now universally added by naturalists to their characters of species, are rarely to be found in Mr. Sharpe's catalogue.

One other point we will mention—that we trust Mr. Sharpe will consider seriously in the preparation of future volumes. The golden rule of priority and the canons of the Stricklandian Code of Nomenclature are now generally accepted by all zoologists. But like many other things that are excellent, "priority" may be pushed to a ridiculous extent, and some writers seem determined to disgust people with it if they can. The chief object of the rule of priority is the attainment of a uniform nomenclature. Unless, therefore, there is a stringent necessity in obedience to its rule to alter a generally-recognised name, it is only defeating the object in view to propose such a change. But we regret to observe that Mr. Sharpe not unfrequently strains the laws of priority in order to alter well-known and universally adopted titles, both of genera and species. For example, Linnæus called the well-known kestrel *Falco tinnunculus, i.e.*, the "Bell Falcon." In 1807 the French naturalist, Vieillot, made the group of kestrels into a genus, for which he proposed to use the name *Tinnunculus*—and this practice has been generally followed by all ornithologists who have considered the group of kestrels as worthy of generic rank. But Mr. Sharpe now wishes to reject the generic name *Tinnunculus* in favour of a subsequently-given appellation, upon the ground, we suppose, that the species first given in Vieillot's list of *Tinnunculi* is not a true kestrel. This may be the case, but it cannot be doubted that when Vieillot founded his genus, *Tinnunculus*, the species most in his eye was the *Falco tinnunculus*, and that that species should be taken as the true type of his genus.[1] Again, a well-known South American bird of prey is universally known as *Spizaëtes ornatus*. Mr. Sharpe would have it termed *Spizaetus mauduyti*, solely because, although both the names were published in the same work at the same date, the latter is given four pages before the former. We cannot believe that even the great authority of the British Museum will induce naturalists to recognise such grounds for the displacement of familiar names.

[1] Mr. Sharpe gives, as the name to be adopted for the Common Kestrel, *Circhneis tinnuncula*—apparently under the idea that *Tinnunculus* is an adjective.

These, however, are but minor defects in a work that is generally well arranged and well executed. Further than this, we have only to remark that Mr. Sharpe might have done better to adopt some one general classification in its entirety rather than to attempt to amalgamate several hardly-to-be-reconciled systems into one of his own. But whatever his classification may result in, there can be no question of the influence the "Catalogue of Birds" will have on the progress of ornithological science, and we heartily wish the author health and strength to terminate his labours at an earlier period than that which we have assigned to them.

THE ALPS

Die Naturkräfte in der Alpen, oder physikalische Geographie des Alpengebirges, von Dr. Friedrich Pfaff, O. Professor in der Universität, Erlangen. (München: Oldenbourg, 1877.)

THIS is a thoroughly unsatisfactory book. The title is attractive, and in spite of all that has been written about the Alps of late years, a treatise such as is here promised is very much wanted. Such a work if taken in hand by a master of physical science capable of grasping together the varied phenomena and exhibiting vividly their mutual bearings and relations, would be of engrossing interest, and could scarcely fail to throw new light upon many obscure questions of science. Failing this, there is room for a work in which the results of recent exploration and scientific observation should be carefully collected, intelligently arranged, and clearly set forth. Such a book might not attract many of those who have no personal experience of the Alps, but would be welcomed by thousands who have keen recollections of enjoyment among the great mountains, and would fain learn something of the nature and laws of the giant forces within whose sphere they have moved. Along with the primary, though no way common, qualifications of accuracy and clearness, the writer of such a work should have such a firm hold of physical principles as should enable him to mark distinctly the limits of the territory conquered by modern science, and distinguish the conclusions which are definitely established from those that are more or less imperfectly proved, or merely to be ranked as conjectural explanations.

In none of these respects does Prof. Pfaff show himself competent for the work he has undertaken. Except in regard to glacier controversies, where his reading is a little more extended, he appears very ill acquainted with what has been written about the Alps in England, Italy, and France during the last quarter of a century; he is strangely inaccurate, especially as to names and numbers, and often sadly deficient in clearness; and finally, there is a complete want of definiteness in his statements as to the more interesting of the disputed questions discussed in his work.

A disproportionate share, more than a third of the volume, is taken up with what may be called the statistics of the Alps—their division into groups, numerical statements as to the heights of peaks, the dimensions of valleys and lakes, the volume of mountain ridges, and various other orographic elements, some of which do not admit of accurate numerical statement, and others are of trivial importance. It is not too much to say that when the author is unable to copy from a clear-headed and accurate writer, such as von Sonklar, this part of his work is frequently a mere muddle.

In attempting to subdivide the Alps into separate groups it is possible to apply one or other of two guiding principles. You may look either to the configuration of the surface and make the deep valleys and low passes that occur here and there throughout the chain the boundaries between the different groups, or you may attend mainly to geological structure, and form groups in each of which a central mass of crystalline rock is surrounded by a girdle of sedimentary strata, but in so doing must often disregard the actual features of the country. M. Pfaff has alternately adopted the geological grouping of Studer and the orographic arrangement of von Sonklar, with the natural result that the same mountains and valleys are in some cases included in two different groups while others are left utterly unprovided for, there being no one group in which they can be placed. For some groups the author has attempted to assign limits and specify the higher summits, but considering the ill success of these attempts, he has, perhaps wisely, refrained in other cases, and for eleven groups he has given names without attempting to define their limits. Among the dominant peaks we find the two ancient impostors Mont Ollan and Mont Iséran, whose existence has long since been disproved by the active members of our Alpine Club, and, stranger still, we are told to look for the latter in an utterly new direction—in the range west of the Col de Bonhomme "between the Isère and the Rhone"—where the discovery of a mountain over 13,000 feet high would undoubtedly make a lively sensation among the natives. A list of the most important peaks in the Alps is given, in which are enumerated fourteen summits in the mass of Mont Blanc, and twelve of those of Monte Rosa, while the crowning peaks of other important groups such as the Grand Paradis and Piz Bernina, each well above 13,000 feet, are altogether omitted. Heights copied at random from various authorities are hopelessly irreconcilable. In one page the Gross Glockner is 495 English feet higher than the Ortler Spitz; a few pages later the tables are turned, and the Ortler surpasses his rival by 285 feet. What with errors in the spelling of names, confusion of standards—Paris feet being quoted in one line, Vienna feet in the next, and in the third some other foot differing from both—and arithmetical blunders or misprints, this portion of the book is simply bewildering.

Under the head of meteorology the author discusses at much length the laws connecting the decrease of temperature of the air with increasing elevation above the earth's surface. He gives the now-antiquated formula of S. von Waltershausen as in accordance with observation, and then professes to give the results of what, with characteristic accuracy, he describes as the "observations of Glaisher and Coxwell, who, on September 5 1862, in London, rose to the astonishing height of 26,800 feet" (28,565 English feet). The reader who may turn to the report of Messrs. Glaisher and Coxwell's famous ascent from Wolverhampton will fail to find anything in the least resembling the results here given, these being in truth very incorrectly calculated from the average results of all the ascents made in 1862. No reference is made to the bearing of these or other recent investigations on a matter so interesting to Alpine travellers as the measurement of heights by means of the barometer, nor does Prof. Pfaff seem to be acquainted with the various memoirs by Count St. Robert, of Turin, wherein the whole subject is discussed in a masterly manner.

Fully one-third of the volume is devoted to the glaciers wherever it would appear that the writer has made observations on his own account. In this branch of his subject he is moderately well informed, and no doubt has done his best to steer cautiously through the rocks and shoals of personal controversy with which the history of this department of scientific inquiry is unfortunately surrounded, while at the same time he fails to mark accurately the positive contributions of each inquirer to the present sum of our knowledge.

He gravely discusses the dilatation theory of glacier motion, and comes to the conclusion that "dilatation cannot be considered the only cause of the progressive motion of glaciers," and soon after remarks that the gravitation theory has now a majority of adherents; while it would be difficult to name a single competent authority who during the last twenty years has admitted that dilatation has any share whatever in producing the phenomena. He has doubtless read the writings of Agassiz, and Forbes, and Tyndall, but he shows himself unable to grasp the full force of the reasoning of the two latter writers, and in more than one instance has failed to understand them. With regard to the vexed question of the origin of the veined structure of glacier ice, Prof. Pfaff is especially unsatisfactory. He attributes the first notice of it to M. Guyot, though many previous travellers had like him observed it, but failed to discern, as Forbes first did, its significance and importance; and he further on confounds the *dirt bands* of Forbes with the superficial appearance of the veined structure. Especially imperfect and indefinite is the version here given of Tyndall's explanation of the origin of this structure; no reader would be likely to appreciate from these pages either the cogency of the arguments in favour of that explanation, or the difficulties which yet remain to be completely removed.

To those who are used to look for accurate knowledge and scrupulous care in German scientific works, it is disappointing to find that volumes designed for popular instruction in that country should be so deficient in all the requisites for imparting knowledge to unscientific readers, as this and some others which have lately appeared.

OUR BOOK SHELF

Kryptogamien Flora von Schlesien. Erster Band. (Breslau: J. U. Kern's Verlag, 1876.)

THIS flora is dedicated to the president of the "Schlesischen Gesellschaft für vaterländische Cultur," Prof. Goeppert, on the fiftieth anniversary of his receiving his doctor's degree. The whole has been ably edited by Dr. Ferdinand Cohn. It is proposed to continue the flora in two more volumes, one devoted to the Algæ and Lichens, and the third to the Fungi, but two years more must elapse before the completion of the entire work. The first volume includes two parts, the first containing the Vascular Cryptogams and Mosses, the second the Liverworts and Characeæ, with an appendix to the species of Mosses and Hepaticæ, and a copious index. The vascular cryptogams are described by Stenzel, and include twenty-one genera, fifty-three species, and ten sub-species. A history of the discovery of Silesian Pteridophyta is prefixed, and an interesting account of their distribution. Thus the species found on serpentine, limestone, and other rocks, are noted, as well as the hypsometrical distribution. Four regions of elevation are distinguished: 1, from 55 metres to 150 m.; 2, from 150 m. to 500 m.; 3, from 500 m. to 1,100 m.; and 4, from 1,100 m. to 1,500 m. The arrangement of some of the species and sub-species is not quite in accordance with our English ideas. Thus *Woodsia hyperborea*, Koch, is separated into two sub-species: 1, *arvonica*, With.; and 2, *rufidula*, Sw.; equal to *hyperborea* R., Br. and *ilvensis* R., Br. respectively. *Cystopteris montana* of British botanists is *C. sudetica*, Al. Braun and Milde. Then *A. dilatatum*, *spinulosum*, and *cristatum*, are all placed as sub-species of *Aspidium spinulosum*, Sw., and *A. aculeatum lobatum*, and *angulare* are made sub-species of *A. aculeatum*, Döll.

The Mosses and Liverworts are described by Limpricht, and occupy the greater part of the volume, there being 106 genera and 464 species of Mosses, and 39 genera and 132 species of Hepaticæ. A few additional species are added in the Appendix, bringing up the Mosses to 492 species and the Liverworts to 155. The same arrangement is here followed as to history and distribution as in the case of the vascular cryptogams. The descriptions seem excellent, and the information given very full and complete, the characters of the orders and families being given in great detail.

The Characeæ have been described by Prof. Alexander Braun. Probably this was one of the last important works from his prolific pen. All must deplore his recent loss. His vast knowledge, the importance of his contributions to botany, and his genial kindly manner, the readiness he always showed in assisting his students, are well known. To know him was to love him, and we esteem it a high privilege to have been one of his students. The Characeæ are not very numerous, three genera and fourteen species being enumerated; but in the hands of Prof. Braun it becomes a most valuable memoir on the whole group, while the species likely to be found in Silesia are all pointed out. The synonymy must be very confused, as Braun notices that *Chara flexilis*, Waller, includes three or four species of *Nitella*, three of *Tolypella*, and one *Chara*, *C. gracilis* of Sprengel is a still greater monster, as it includes five species of *Nitella*, one *Lychnothamnus*, and three species of *Chara*.

W. R. McNab

The Countries of the World, being a Popular Description of the Various Continents, Islands, Rivers, Seas, and Peoples of the Globe. By Robert Brown, M.A., Ph.D., &c. Vol. I. (London: Cassell, Petter, and Galpin. No date.)

THIS is certainly an attractive book; the wealth of illustrations renders it so. While we recognise some of the illustrations as having done service elsewhere, many of tehm are new, well-executed, and afford a good idea of the scenery, products, and people of the regions they are meant to illustrate. This volume treats of the Arctic regions and North America, contains a great amount of miscellaneous information, and is written in a rambling easy-going style. It is essentially a popular work, but might have been made valuable even to the geographical student had some of the pictures been dispensed with and a number of regional maps substituted similar to those which are so important a feature in Reclus' "Géographie Universelle," with which masterly and exhaustive work, however, it would be unfair to compare it. We have no doubt Dr. Brown's work will afford pleasure and prove instructive to many readers.

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 ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

HAVING been prevented from attending the recent meeting of the British Association by the necessity of devoting my entire vacation to mental and bodily renovation after the sad family losses I had sustained, I have only become aware within the last few days that my article in the April number of the *Nineteenth Century*, entitled "The Radiometer and its Lessons," had been there spoken of by Prof. G. Carey Foster, in his address as President of Section A, as showing an "unmistakable tendency, *either intentionally or unintentionally*, to depreciate Mr. Crookes's merits, and to make it appear that he had put a wrong interpretation upon his own results," which statement is said by your reporter to have "elicited great applause."

Of Mr. Crookes's own reply in the July number of the same periodical, entitled "More Lessons from the Radiometer," I took no notice; partly because my mind was at the time fully occupied by sad cares and urgent duties, and partly because I thought that his assertions (1) that he had not theorised on the subject at all, (2) that he had not attributed the rotation of the radiometer to the direct impetus of light, and (3) that he had never claimed the discovery of a new force or a new mode of force, were so well known in the scientific world to be inconsistent with fact, that I need not trouble myself to refute them.

Prof. Carey Foster, however, speaking with authority as President of the Physical section of the British Association, has given it as his judicial opinion that what I have written on this subject shows an unmistakable tendency to depreciate Mr. Crookes's merits, and to misrepresent his opinions; and he has further "unmistakably" suggested (as it appears to me) that this *may* have been done with deliberate intention, instead of being done in good faith under the influence of an unintentional bias. As it is impossible for me to allow such an imputation from such a quarter to pass unnoticed, I might fairly challenge Prof. Carey Foster to justify language which I must presume him to have used with all due consideration of its obvious meaning, and of *his* and *my* relative positions. But as he explicitly disavows the more serious part of this imputation, I have now only to ask to be allowed to show, in the columns of the journal which has not only recorded the accusation, but has pointedly directed attention to it,—*first*, that I have not, *even unintentionally*, "depreciated Mr. Crookes's merits" as the inventor of the Radiometer; and *secondly*, that Mr. Crookes really did in the first instance put that "wrong interpretation upon his own results" which I attributed to him. Had Prof. Carey Foster complied with the request I privately made him, that he should specify the passages which (in his opinion) justify his charge, I should have been able to reply to it much more briefly. But by declining thus to particularise, he obliges me to traverse the whole ground covered by his general accusation.

That I was not influenced, when writing on the Radiometer, by any *animus* arising from my personal antagonism to Mr. Crookes on another subject, will appear, I think, from the following extracts from the two lectures which I delivered at the London Institution (by special request) on Mesmerism, Spiritualism, &c., before Christmas, and which were published in *Fraser's Magazine* at the commencement of the present year:—

"The recent history of Mr. Crookes's most admirable invention, the Radiometer, is pregnant with lessons on this point. When this was first exhibited to the admiring gaze of the large body of scientific men assembled at the *soirée* of the Royal Society, there was probably no one who was not ready to believe with its inventor that the driving-round of its vanes was effected by the direct mechanical aid of that mode of Radiant Force which we call Light; and the eminent Physicists in whose judgment the greatest confidence was placed, seemed to have no doubt that this mechanical agency was something outside Optics properly so called, and was, in fact, if not a new Force in nature, a new *modus operandi* of a force previously known under another form. There was here, then, a perfect readiness to admit a novelty which seemed so unmistakably demonstrated, though transcending all previous experience. But after some little time the question was raised, whether the effect was not really due to an intermediate action of that *mode* of Radiant Force which we call *heat*, upon the attenuated vapour of which it was impossible entirely to get rid; and the result of a most careful and elaborate experimental inquiry, in which nature has been put to the question in every conceivable mode, has been to make it (I believe) almost if not quite certain, that the first view was incorrect, and that Heat is the real moving power, acting under peculiar conditions, but in no new mode."—*Lectures on Mesmerism and Spiritualism*, p. 8.

"I hold the warning given by the history of this inquiry, in regard to the duty of the scientific man to exhaust every possible mode of accounting for new and strange phenomena, before attributing it to any previously unknown agency, to be one of the most valuable lessons afforded by Mr. Crookes's discoveries.

"Now I maintain that it requires exactly the same kind of specially trained ability to elicit the truth in regard to the phenomena we are now considering, as has been exerted in the researches made by the instrumentality of the Spectroscope and the Radiometer. And I cannot but believe that if Mr. Crookes had been prepared by a special training in the bodily and mental constitution, abnormal as well as normal, of the Human instruments of the Spiritualistic inquiries, and had devoted to them the ability, skill, perseverance, and freedom from prepossession, which he has shown in his Physical investigations, he would have arrived at conclusions more akin to those of the great body of scientific men whom I believe to share my own convictions on this subject."—*Op. cit.*, p. 70.

No one, I think, can fail to see that in speaking of Mr. Crookes's "most admirable invention," and in giving him the fullest credit for the "ability, skill, perseverance, and freedom from prepossession," with which he had carried on his investigations in regard to it, I eulogised him as warmly as if I had never come into collision with him. It must also be apparent to any reader of these lectures, that I did not impute to him any blame for having originally fallen into an error shared at the time by the "eminent Physicists in whose judgment the greatest confidence was placed;" and that my reason for bringing forwards the subject was to enforce the lesson, that "no new principle of action has any claim to scientific acceptance, save after an exhaustive inquiry as to the extent to which the phenomena can be accounted for, either certainly or probably, by agencies already known."

Circumstances to which I shall presently advert having made me feel it desirable that this "lesson" should be yet more fully and emphatically set forth, I applied myself to a careful reperusal of Mr. Crookes's papers in the *Proceedings* of the Royal Society, with the most earnest desire to present a true history of the whole inquiry; and I availed myself of the opportunity kindly afforded me by the editor of the *Nineteenth Century*, to place before the public what I believed to be a fair statement of the case, with the lessons it conveyed.

Commencing with a description of the phenomena presented by the Radiometer when it was first exhibited by Mr. Crookes at the *soirée* of the Royal Society, I thus continued:—

"It is scarcely surprising, then, that a general impression should at once have prevailed that a capital discovery had been made—that of the *direct mechanical action of light;* which, though not indicating the existence of a new force in nature, showed that the most universally diffused of all forces, next to gravitation, has a *mode of action* which was previously not merely unknown, but altogether unsuspected. And this impression was not confined to those who had only a general acquaintance with Physical Optics; for it was shared by the greatest masters of that department of science, who had followed the course of the experimental researches on which Mr. Crookes had been for some time engaged, and of which this discovery was the culmination."—*Nineteenth Century*, April, 1877, p. 243.

I then went on to give, from Mr. Crookes's papers, a history of the investigations which had led him up to the Radiometer; and showed (p. 249) that at that stage of the inquiry, the argument for the *directness of radiant repulsion*, deducible from what was then supposed to be a fact—the increase of the rapidity of the rotation in proportion to the perfection of the vacuum—"seemed alike valid and cogent."

I next sketched the history of the opposite view originally propounded by Prof. Osborne Reynolds, supported by Dr. Schuster's experiment, and finally established by Mr. Crookes's own later researches, which have culminated in the doctrine of "heat reaction" now generally accepted. In reference to Mr. Crookes's own part in these subsequent inquiries, I say later on (p. 254), that "no sooner was adequate ground shown for calling in question his interpretation of the phenomena, and a *vera causa* found in an agency already known, than Mr. Crookes evinced the spirit of the true philosopher in varying his experiments in every conceivable mode, so as to test the validity of his original interpretation." And again in the next page I speak of his "carrying out this beautiful inquiry in a manner and spirit worthy of all admiration."—What higher praise *could* be given to a scientific investigator?

Having brought the history to its conclusion, I thus proceed:—

"Before adverting to the lessons which this remarkable history seems to me to convey, I would point out that this change of interpretation of the facts discovered by Mr. Crookes, does not in the least diminish either the interest of the facts themselves or the merit of his discovery. Nor is the value of his Radiometer in any degree lowered by the demonstration that it does not (as Mr. Crookes at first supposed) afford an absolute mechanical measure of radiant energy under any of its aspects. What (according to present views) it really does measure, is the amount of 'heat reaction' producible in gaseous atmospheres of different kinds and of different degrees of attenuation. And such a precise method of measurement appears more likely than any other mode of investigation, to furnish a test of that kinetic theory of gases, the recent development of which by Prof. Clerk-Maxwell is regarded by competent judges as constituting (if it should receive such verification) the most important advance ever made in molecular physics. Most deservedly, therefore, did Mr. Crookes receive from the Royal Society the award of one of its chief distinctions." (*Loc. cit.*, p. 251.)

To this I may add that I personally congratulated Mr. Crookes most cordially on that occasion, and expressed to him the deep interest with which I had followed his researches throughout. And though I had next to show that Mr. Crookes has another side to his mind, which makes Mr. Crookes the "spiritualist" almost a different person from Mr. Crookes the "physicist," I carefully guarded what I had to say on this point in the following words:—

"I would not be thought for one moment to disparage Mr. Crookes's merits as the inventor of the Radiometer, by now bringing into contrast with the admirable series of scientific investigations which led up to that invention, what I cannot but regard as his thoroughly unscientific course in relation to another doctrine of which he has put himself prominently forward as the champion."

I cannot but surmise that Prof. Carey Foster must have read my paper rather carelessly, and have applied to Mr. Crookes, the inventor of the Radiometer, the depreciatory remarks I felt called upon to make in regard to Mr. Crookes, the supporter of a system, a large proportion of which even Mr. D. D. Home has recently denounced as "a seething mass of folly and imposture."[1] If Prof. Carey Foster knew as much as I do of the mischief which *this* Mr. Crookes has done, especially in the United States, on the one hand to his own reputation and to that of British science,[2] and on the other to public morality, by the facility with which he has lent himself to the support of frauds as wicked as those by which fortune-tellers delude ignorant and credulous servant-girls, he would not wonder that I should feel called upon to show that the high scientific ability of Mr. Crookes, the Physicist, neither prevents him from believing in his own day-dreams, nor renders him a match for the cunning of the clever female cheats who play upon his Spiritualistic "prepossessions."

I now pass to the second part of my defence; and shall show that for "making it appear that Mr. Crookes had put a wrong interpretation upon his own results," I can adduce adequate justification from his own published statements.

Of the "repulsion accompanying radiation" shown in his early experiments by the swinging-round of the pith bar, Mr. Crookes said, in 1874 (*Phil. Mag.*, vol. xlviii., p. 94), "*My own impression is that it is directly due to the impact of the waves*

[1] See his "Lights and Shadows of Spiritualism," containing an unsparing exposure of its "delusions," its "absurdities," and its "trickeries."

[2] On the strength of a private letter from Mr. Crookes, which has been published (in *fac simile*) in the American newspapers, a certain Mrs. or Miss Eva Fay announced her "spiritualistic" performances as "endorsed by Prof. Crookes and other Fellows of the Royal Society." The particulars of the complete public exposure of this woman's disgraceful frauds, showing that Mr. Crookes's *scientific tests* are no more worthy of trust than the late Prof. Hare's *experimental demonstration of the immortality of the soul*, will appear in the forthcoming number of *Fraser's Magazine*.

upon the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation."

In a paper subsequently communicated to the Royal Society (*Proceedings*, March 12, 1875), Mr. Crookes characterised the explanation of the "repulsion from radiation" offered by Prof. Osborne Reynolds, as one which "*it is impossible to conceive*," the phenomenon taking place in a *chemical* vacuum. At the same time he stated that he was unprepared to offer any other explanation, and that "he should avoid giving any theory on the subject until a sufficient number of facts have been accumulated."

After bringing out the Radiometer, however, he reverted (as it seemed to me) to his previous "impression;" the whole phraseology of his papers of January 5 and February 5, 1876, appearing at the time, not only to myself, but to every one of the eminent scientific friends with whom I conversed on the subject, to indicate that he *then* considered the rotation as *directly due to the impact of the waves upon the surface of the moving mass*. Nor have I ever imputed it to him as a matter of blame that he took this view of it; on the other hand I have stated over and over again that this seemed the general impression of the distinguished Physicists to whom we "outsiders" looked for guidance in the matter. Anyone who remembers what took place at the Meeting of the Royal Society at which Mr. Crookes's paper was read, will, I feel sure, bear out this statement.

I shall now specify more explicitly the grounds on which I attributed to Mr. Crookes, no longer as an "impression," but as a definite "interpretation" of his facts, that the rotation of the Radiometer is due to the *direct impact of the waves*, and chiefly (I never said exclusively) to those of the *luminous* waves; and further attributed to him a claim to the discovery of a "new force" or "new mode of force."

This *key-note* seems to me to be most distinctly struck in the following passage:—After pointing out that "there is no real difference between Heat and Light, all we can take account of [I presume he means physically, not physiologically] being difference of wave-length," he thus continues: "Take, for instance, a ray of definite refrangibility in the red. Falling on a Thermometer it shows the action of *Heat;* on a Thermopile it produces an *electric current;*[1] to the Eye it appears as *light and colour;* on a Photographic plate it causes *chemical action;* and on the suspended pith *it causes motion*."

Now (1) this motion being elsewhere spoken of as due to *the impetus given by a ray of light*, (2) a set of experiments being made to determine *the mechanical values of the different colours of the spectrum*, (3) an observation being recorded on *the weight of sunlight* (without the least intimation that he was "speaking figuratively," as Mr. Crookes says that he did to his audience at the Royal Institution), (4) the term *Light-mill*[2] being used by himself as a synonym for "Radiometer," and (5) no hint whatever being given of the dependence of the result (as argued by Prof. Osborne Reynolds) on a "heat-reaction" through the residual vapour, I still hold myself fully justified in attributing to Mr. Crookes the doctrine of *the direct mechanical action of light;* and I call on Prof. Carey Foster to prove—not that Mr. Crookes himself did not hold that doctrine—but (which is a very different thing) that I am not justified by Mr. Crookes's own language in attributing it to him.

That Mr. Crookes considered such action a "new force" or a "new mode of force," plainly appears from my previous citation; in which he ranks Motion as a mode of Radiant action *additional* to Light, Heat, and Actinism, differing as much from either of them as they differ from each other. If it does *not* mean this, what does it mean?

So, if Mr. Crookes has *not* changed his mind as to the interpretation of his facts, I ask (1) why he now repudiates as inappropriate the term *Light-mill* adopted (if not originally given) by himself? and (2) why does he now admit that dependence of the movements upon the presence of residual gas, which he originally affirmed to be *impossible to conceive?*

I have carefully confined myself to the *main issues* of this question. Prof. G. Carey Foster will doubtless be able to pick out *points of detail* in my article, as to which fault may be found by a severe critic. But I venture to think that I have said enough to prove that what I said on the subject was written under the honest conviction that I had adequate ground for my statements; and that I shall at any rate be absolved from the imputation of having ill-naturedly referred to the history of the Radiometer for the purpose of putting Mr. Crookes in the wrong; the "lesson" with which I concluded the article being as follows:—

"The lesson which this curious contrast [the 'duality' of Mr. Crookes's mental constitution, which I speak of as having plenty of parallels in past times, to say nothing of the present] seems to me most strongly to enforce, is that of the importance of training and disciplining the *whole* mind during the period of its development, of cultivating scientific habits of thought (by which I mean nothing more than strict reasoning based on exact observation) in regard to *every* subject, and of not allowing ourselves to become 'possessed' by any ideas or class of ideas, that the common sense of educated mankind pronounces to be irrational. I would not for a moment uphold that test as an infallible one; but it ought to be sufficiently regarded, to make us question the conclusions which depend solely upon our own or others' subjectivity, and to withhold us from affirming the existence of new agencies in Nature, until she has been questioned in every conceivable way, and every other possibility has been exhausted." (*Op. cit.*, p. 256.)

October 10 WILLIAM B. CARPENTER

I HEAR from Dr. Carpenter that he is sending to you, for publication in the next number of NATURE, a statement intended as a refutation of an opinion which I ventured to express, in my address to the Mathematical and Physical Section of the British Association at Plymouth, respecting an article on the "Radiometer," contributed by him to the *Nineteenth Century* for April. As Dr. Carpenter appears to have interpreted that expression of opinion in a sense different from that which it was intended to convey, I shall be much obliged if you will afford me space in your next issue for a few words of explanation.

The words which I used in referring to his article were these: "An eminent and accomplished scientific man had published, within the last few months, an account of the discovery of the radiometer, the unmistakable tendency of which was, either intentionally or unintentionally, to depreciate Mr. Crookes's merits, and to make it appear that he had put a wrong interpretation upon his own results." The word *depreciate*, which occurs here, is, I am aware, susceptible of various shades of meaning, and perhaps it would have been better if I had guarded myself against the possibility of misconception that lurks in it. What I meant was that Dr. Carpenter's account of Mr. Crookes's researches was likely to make his readers form a lower estimate of their scientific value than, in my opinion, they deserved; but whether or not it was *intended* to have this effect I did not undertake to say. I did not mean, and had not the smallest idea of suggesting, that Dr. Carpenter had been guilty of intentional and conscious unfairness towards Mr. Crookes. I should have thought it entirely unnecessary to disavow this latter interpretation of my words, and indeed should have considered it an insult to Dr. Carpenter's reputation to suppose that anyone would understand them in this sense, had he not himself (in a recent correspondence with me) endeavoured to fix this meaning on them in spite of my repeated assurances that it was not intended.

I do not wish to say anything on this occasion in support of the opinion which I have admitted that I did intend to express, but I shall ask you to allow me to do so in a future number of NATURE. G. CAREY FOSTER

University College, London, October 14

Mr. Wallace and Reichenbach's Odyle

As Mr. Wallace has attempted (presumably with Mr. Crookes's editorial concurrence) to rehabilitate, in the July number of the *Quarterly Journal of Science*, the Odyle-doctrine of Baron Reichenbach, I think it well to state that I yesterday availed myself of an opportunity of personally asking my friend

[1] Having never heard of any physical philosopher from Seebeck to Sir William Thomson, who looked at the *electric current* generated in the Thermopile as anything else than an effect of the *heating* (whether by *conduction* or by *radiation*) of the two metals of which it was composed, I was greatly surprised at finding it ranked by Mr. Crookes as one of the *immediate* modes of Radiant action; and I called attention in my "Radiometer" paper to what I supposed to be his mistake on this point. It may be that in my ignorance of the newest developments of thermo-electric theory (my knowledge of it not being later than 1872, "Everett's translation of Deschanel," p. 652), I have here unintentionally "depreciated Mr. Crookes's merits;" and I shall be quite ready to recant and apologise for my mistake, if Prof. G. C. Foster will show that it is Dr. Carpenter, not Mr. Crookes, who is here in the wrong.

[2] It is impossible not to see, in the use of this term, a suggestion that the vanes are driven round by the direct mechanical impetus of Light upon them, in the same way as the sails of a Wind-mill are driven round by the direct impetus of the Wind.

Prof. Hoffmann, of Berlin, whether that doctrine any longer finds support among scientific men in Germany. *His reply was a most emphatic negative*; the doctrine, he said, being one which no man of science with whom he is acquainted would think worthy of the slightest attention. Yet in Mr. Wallace's judgment (*query* in Mr. Crookes's also?) the unanimous verdict of the scientific world of Germany, to say nothing of England, is a prejudiced one; only Mr. W. and his spiritualistic allies appreciating correctly the real force of the evidence originally advanced by Reichenbach, and confirmed by those trustworthy (?) authorities, Drs. Ashburner and Gregory.

In thus setting his own judgment on a question which lies altogether outside the scientific domain which he has made his own, against the unanimous verdict of the eminent physicists and physiologists who have carefully "tried" the Od-force and "found it wanting," and in rebuking myself and those who think with me for our incredulity, does not Mr. Wallace put himself somewhat in the attitude of his old opponent, John Hampden, who thinks everybody either a fool or a knave who maintains the earth to be round? WILLIAM B. CARPENTER

October 22

Potential Energy

WITH reference to the views of "John O'Toole" on the subject of energy perhaps you will allow me to say how one of the class to which "poor Publius" belongs has conceived the matter of terminology with satisfaction to himself.

1. Energy being unanimously defined by "the doctors" to be "capacity for doing work," and also energy conveying in its derivation the notion of activity, this term is properly applicable only to the bodies of material systems the motions of which are contemplated. Hence all energy is in its nature kinetic—the very term kinetic is logically included in the term energy.

2. When a material system is in motion it actually possesses, *ipso facto*, a capability of doing work, that is to say, it has actual energy.

3. When in any configuration of the system we contemplate as possible the action of causes which will alter its motions and give it a second configuration, the excess of the energy which it would possess in this second configuration over the energy which it possesses in the first is properly called its potential energy in the first configuration.

4. The assertion that in any configuration the sum of the energies, actual and potential, of a material system is constant, is what Kant would call an analytical proposition, or what "X." (quoting Herschel) calls "only a truism after all." But I further remark—

5. That this truism is not the principle of the conservation of energy, but that this principle is a true "synthetical proposition" which some fairly regard as an almost immediate deduction from Newton's third law, and which others regard as proved by often repeated and much varied experiment; and hence that "X.'s" statement of this great principle in the form—"The sum of the actual and potential energies *of the universe* is a constant quantity," (the italics are mine) is not its proper definition.

6. That, leaving the consideration of bodies, and referring to forces, the term to be employed instead of energy is work, and that the term analogous to the "potential energy of bodies" is the "potential work of forces," this latter being the amount of work which they are capable of doing in displacing their points of application from their actual configuration to any fixed chosen one.

7. That by the expenditure of a fixed amount of work on any material system the same amount of actual energy (whose type is $\frac{1}{2} m v^2$) is under all circumstances produced, and that, through whatever forms this actual energy is made to pass, if the whole of it is always utilised, it will finally be reconvertible into the same original amount of work, this being the principle of the conservation of energy.

8. That instead of the statement in 5, we must substitute the synthetical proposition that "the sum of the actual energy of the bodies in the universe and the *potential work of its internal forces* is a constant quantity," and the same is true of every material system which is regarded as complete in itself; or in other words, wherever and however a given quantity of potential work is lost by the forces of the system, this always appears in the shape of a fixed quantity of actual energy, in the form which we call heat, or in some other.

Hence we have energy, actual and potential, of bodies; and work, actual and potential, of forces.

A few remarks in conclusion. "J. M." has very happily illustrated the propriety of the expression potential energy, as, in strict consequence of the definition of energy, a potential capacity of doing work; and if in his illustration the "power of purchasing" is considered with reference to a further object, there may be not merely a "double remotion from" what we may regard as "tangibility," but a remotion of a higher multiple order. "W. G." has well explained that it is only in consequence of the fixedness of the earth that the potential energy of the system of the earth and stone is by the "doctors" located in the stone. Finally, I can hardly conceive how "X.," who has devoted so much attention to the literature of this subject, can have fallen into such a grievous error with regard to the clock.

Royal Indian Engineering College, Cooper's Hill G. M. MINCHIN

YOUR "Potential Energy" corespondents will find three letters on the "Conservation of Energy" in the *Engineer* for January 12 and 19 and February 2 which may interest them. The writer "Φ Π" assumes that all the phenomena of force are explained by the theory that only matter and motion exist, and that what we call potential "energy" is only "quantity and motion," which motion is indestructible but diffusible. Z.

London, October 20

Origin of Contagious Diseases

I HAVE been much struck by the following passage in Dr. Richardson's address, NATURE (vol. xvi. p. 481):—

"(*c*) That as regards the organic poisons themselves and their physical properties, the great type of them all is represented by the poison of any venomous snake. . . . It is the type of all the poisons which produce disease."

Now has it been really proved, by experiment, that the poison of snakes produces the effects characterising the contagia? viz., "(*o*) . . . Each particle of any of these poisons brought into contact either with the blood of the living animal or with certain secretions of the living animal, possesses the property of turning the albuminous part of that same blood or that same secretion into substance like itself. . . ."

In other words, if an animal is suffering from snake poison does its blood or any of its secretions acquire the power of transmitting the disease, *i.e.*, the effects of a snake's bite, to another individual, as is the case with an animal affected with carbuncle, glanders, hydrophobia, &c., &c.?

Unless this question has been decided in the affirmative it would appear rather difficult to uphold the sentence (*c*) as quoted above. D. W.

Freiburg in Brisgau, G. J., October 14

[Dr. Richardson informs us that D. W. does not properly understand his argument. Dr. Richardson does not suppose that the person or animal poisoned from a poisonous snake is, in turn, poisonous, although that may be the fact. He merely uses the illustration that as a poisonous snake secretes a poison so an infectious person is for the time secreting a poison.]

I SEE by your issue of October 4, that Dr. Richardson has honoured me by mentioning my name and placing me as the first, in modern times, to advocate the hypothesis that living germs are the exciting agents of epidemic and infectious diseases. But he says further, "I protest, I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive." It may be so, but I look in vain through the whole story he narrates in his lecture to find a rational substitute for it, and it appears to me desirable at the present juncture that the principles of the germ theory, as I have interpreted them, should stand side by side with Dr. Richardson's "glandular theory." It is now nearly thirty years since I endeavoured to find some common root or cause for those diseases which we find in plants, animals, and man, and which are communicable among the individuals of each order in nature; also, in some instances, from one order to another. During that thirty years every step in scientific research and medical experience as far as my inquiries have carrried me, has tended to confirm the views I put forward in my original "Essay" and in subsequent papers read before the Epidemiological Society. Notably the latest advocates of a germ theory are two of our most eminent men, the one a leader in science, the other a leading physician. I need hardly say I allude to Prof. Tyndall and Sir Thomas Watson; surely these gentlemen cannot be charged with committing themselves to an hypothesis "*the most distant from the phenomena it attempts to explain.*"

Now if it can be shown that the germs of disease are subject to the same laws as other living things and exhibit similar phenomena, and further, that without the inference that they are endowed with vital properties, it is impossible to unravel the most striking character which they present to us for consideration, viz., the fact that they reproduce their kind, then I think there is more reason for following up, in all its intricacies, the germ theory, than to start with an *assumed catalysis, molecular motion, and a glandular matrix*, as suggested by Dr. Richardson.

Starting, then, from the indisputable fact that the *materias morbi* of every communicable disease reproduces its kind, I have considered this a primary law, and have tabulated other laws which are associated with living beings by which it will, I think, be found that there is a parallelism of a kind to attract and rivet attention, especially, too, when many otherwise inexplicable circumstances bend to this hypothesis.

Primary Law of Reproduction, by which all living things reproduce their kind.

SECONDARY LAWS.

Objective Laws.	*Subjective Laws.*
1. The diffusion or dispersion of germs.	1. Seasons of activity.
2. Their static existence.	2. Climatic influence.
3. Limited duration of active existence.	3. Relation to latitude.
4. Period of development, maturity, and decay.	4. Subjection to physical forces.
5. Intermittent reproduction.	5. Influence of locality.

Without amplifying this subject, which would carry me far beyond the limits of an ordinary communication, I will only add that though the above tabulation is very imperfect, there is quite sufficient for any one who will follow out the ideas conveyed by it to trace the intimate relation that exists between living beings and the germs of disease. I would refer finally to the fact that many diseases in men and animals have yielded up living germs as their cause, chiefly, I may add, skin diseases it is true; but *aphtha*,[1] closely associated with diphtheria, is, I think, acknowledged by all unprejudiced persons to have its origin in an unmistakable and demonstrable germ. JOHN GROVE

The Zoological Relations of Madagascar and Africa

WITHOUT entering into the details of this very difficult question I wish to be allowed to state some of the general reasons which have led me to a different conclusion from Dr. Hartlaub,[2] and also to point out where he has not quoted my opinions with perfect accuracy. Instead of saying that "the fauna of Madagascar is manifestly of African origin," my actual statement is as follows:—"We have the extraordinary fauna of Madagascar to account for, with its evident main derivation from Africa, yet wanting all the larger and higher African forms; its resemblances to Malaya and to South America; and its wonderful assemblage of altogether peculiar types" ("Geog. Dist. of Animals," vol. i. p. 286). My reasons for believing in the "*main derivation*" of the fauna from Africa can only be understood by considering the theory, now generally admitted, of the origin of the fauna of Africa itself. All the higher mammalia are believed to have entered it from the northern continent during the middle or latter part of the tertiary period, and the occurrence of *Psittacus* and of forms supposed to be allied to plantain-eaters and to *Leptosomus* in the miocene of France, render it probable that many of the peculiar groups of African birds had their origin in the old Palæarctic region. Now Madagascar presents many cases of special affinity with South Africa, especially in insects, land-shells, and plants; and if we suppose it to have formed part of a South African land before the irruption of the higher mammals and birds from the north, we shall I think account for many of its peculiarities. Such facts as its possessing *Potamochærus* and the recently extinct *Hippopotamus*, while it has thirteen or fourteen peculiarly African genera of birds against four or five that are peculiarly Oriental; of its having many African genera of lizards and tortoises; of its butterflies being decidedly African; of its numerous African genera of Carabidæ, Lucanidæ, and Lamiidæ; while the specially Oriental affinities of its mammals, reptiles, and insects are hardly if at all more pronounced than the South American affinities of the same groups,—all seem to me to warrant the general conclusion that the "main derivation" of the Madagascar fauna is from Africa.

Dr. Hartlaub speaks of my "attempted parallel between Madagascar and Africa, and the Antilles and South America" in such a way that his readers must think I had dwelt upon this parallel in some detail as being special and peculiar. The fact is, however, that I have always referred to it in a very general way. At p. 75 vol. i. I say: "The peculiarities it (the Malagasy sub-region) exhibits, beings of exactly the same kind as those presented by the Antilles, by New Zealand, and even by Celebes and Ceylon, but in a much greater degree." And again, at p. 272, vol. i., I speak of it as "bearing a similar relation to Africa as the Antilles to Tropical America, or New Zealand to Australia, but possessing a much richer fauna than either of these, and in some respects a more remarkable one even than New Zealand." This *general* comparison with the *two* other great insular sub-regions is, I think, justifiable, notwithstanding great differences of detail. There is in all a rich and highly peculiar fauna, a great poverty of mammalia, and a total absence of many large families of birds characterising the adjacent continent, together with special points of resemblance to distant continents or to remote geological periods.

It seems to me that such a problem as this cannot well be solved by means of a group which, like birds, do not require an actual land-connection in order to reach a given country; and, if *all* land animals are taken into account, the evidence does not appear to warrant the supposition of a recent land-connection of Madagascar with India or Malaya. At a very remote epoch such a connection *may* have taken place, but if we are to give any weight to the general facts of distribution as opposed to those presented by birds only, the union of Madagascar with South Africa is more recent and has had more influence on the character of the Malagasy fauna. The numerous and very remarkable points of affinity between Madagascar and South America in almost every group except birds, are not alluded to by Dr. Hartlaub, yet they would equally well support the notion of a former union of those two countries independently of Africa. It seems, however, more consonant with our general knowledge of distribution to consider these as cases of survival of ancient and once wide-spread types in suitable areas; and this is a principle that must never be lost sight of in attempting to solve the problems presented by such anomalous countries as Madagascar. ALFRED R. WALLACE

Selective Discrimination in Insects

YOUR correspondent S.B., in his letter NATURE of yesterday's date, must be referring to some short abstract only of my lecture on flowers and insects. I quite agree with him that odour is very important in attracting insects, and dwelt upon it in my lecture, as well as in my little book on "Flowers and Insects." A striking illustration is afforded by night flowers, which often become peculiarly odoriferous towards evening, as has been already pointed out by various observers.

S.B. attributes, I think, too little importance to the colouring of flowers, but his letter shows him to be a careful observer, and I hope he will continue to devote his attention to the subject.

He would find H. Müller's "Blumen und Insekten" a mine of most interesting and accurate observation.

London, October 19 JOHN LUBBOCK

Protective Colouring in Birds

WITH reference to the statement in my "Naturalist in Nicaragua," p. 196, that the macaw "fears no foe," &c., the well-known geologist, Prof. Gabb, sends me the following information:—"I willingly comply with your request to repeat the statement about the *Kukong pung* or macaw hawk of Costa Rica. Not having your book by me now I cannot refer to page nor quote your statement exactly. But as I recall it, you speak of the great red and blue macaw as being so well defended as to need no protective colouring, and that no hawk dares attack it. In this you are mistaken. Not only have I seen on several occasions heaps of the unmistakable feathers of the bird in the woods, left in the manner that all woodsmen recognise as hawk's work, but I have the statements of various Indians, not in collusion, confirming each other, and finally I have had the bird pointed out to me (I am not sure but that it may occur in the collection I sent to the Smithsonian). It is a fair-sized hawk of dark

[1] See *Medical Times*, 1851, vol. ii. p. 95.
[2] NATURE, vol. xvi. p. 498, and the *Ibis* for July, 1847, p. 334.

colour. It always attacks its prey on the wing, swooping down and disabling it when least able to use its effective weapon. It is well known to the Indians, and its specific name among them indicates its habits—*Kukong* (macaw) *pung* (hawk)—in the same manner as the eagle is called *sar pung* or monkey hawk."

There can be no doubt therefore that the macaw is not so free from molestation in Costa Rica as I supposed it to be in Nicaragua. Whilst the statement respecting its immunity from attack will need modification, the argument I founded upon it may still hold good. Birds on the wing could not evade the keen sight of a hawk by any protective colouring, and if when at rest the macaw did not need concealment, natural selection would not work to tone down the colours that sexual selection tended to make more pronounced.

It will gratify all naturalists to learn that some of the results of Prof. Gabb's long and critical study of the miocene molluscan fauna of Santo Domingo and Costa Rica and its relation to the existing species of the Atlantic and Pacific Oceans, will shortly be ready for publication. Much light will be thrown by them on the interesting question of the time of the latest connection of the two oceans through the strait that once separated the northern from the southern continent. THOMAS BELT

The Cedars, Ealing

"On the Question of Free-Will"

I SHOULD like to call the attention of your readers to what appears an important matter in connection with the above subject, which has attracted considerable attention of late, and which has also its physical bearings. In a recent lecture by Prof. Tyndall, the aspect of compensation and punishment for actions was treated of in connection with the question of free-will, and I think that it cannot but have struck many that the conclusions arrived at as regards this special point were less satisfactory or complete than the otherwise able reasoning of the lecture. What I should like to submit is that this special point is entirely independent of any question of free-will.

The argument is that if the will be *not* free, then reward for a good action, or punishment for a bad action cannot be *deserved*; but are merely *expedient*. I submit that the contrary holds true, quite independently whether the will be free or not. For it seems to me that the great point (that has apparently not been taken into account) is that the expectancy of the reward enters in as an element *to determine the will*. If there were no reward in prospect, the action would not be done. It must therefore be an error to argue that because the will is not free, therefore the reward is not deserved. To withhold the reward would be to reverse the conditions under which the action was willed.

In the same way as regards punishment. A person (say) for his own benefit appropriates to himself a sum of money. The person in appropriating the money contemplates the possible punishment, or takes this eventuality into consideration as an element in determining his will. If, therefore, the punishment were withheld, it would (quite independent of the question of free-will) be an injustice, because the person would derive a benefit without any compensating disadvantage. So in the same way in the previous case of the reward, he would (if the reward were withheld) undergo inconvenience without any compensating advantage. Thus I submit that rewards for good actions and punishments for bad actions have nothing to do with the question of free-will, for these in any case enter as elements *in determining the will*. Therefore punishment for an offence (like reward for a good action) is not merely an *expedient* thing, but in accordance with reason and justice.

Is not the question of free-will in itself rather a quibble? A man's will is dependent on his reason, or *will* may be said to be a special act of reason. Reason, it will be generally admitted, depends on brain structure. Else what are our brains for? Hence *will* depends on brain structure. Can it be said that on that account *will* is not free? For a man to be dominated (if conceivable) by a will independent of his brain structure, he would surely be a slave; for surely brain structure enters into the determination of a man's identity. So long as *will* is subject to brain structure, it is subject to reason, [for brain structure is the mechanism of reason (or, at least, a mechanism *necessary* to reason). To have a will not subject to brain structure would be, therefore, to have a will not subject to reason (or a will that runs wild). Can any greater slavery be imagined than to be dominated by an independent will not subject to reason? I say, therefore, that *because* the will is subject to brain structure, *therefore it is free*; this, therefore, in direct opposition to the opposite party who hold that, for the will to be free, it must not be subject to anything, *i.e.* must run wild independently of the controlling mechanism of brain structure.

The most powerful argument *against* anything is perhaps the argument of an exceedingly competent reasoner *in favour of* a wrong cause. Thus the portion of Sir John Herschel's lecture on "The Origin of Force," in which he supports independent free-will (so termed), constitutes the most powerful argument against it; as, in order to support his conclusions, he is obliged to assume the *creation* of (a small amount of) energy; or, to support independent free-will, he has to touch upon the perfection of the principle of the conservation of energy. It is a known fact that a man, however able, may not be an equally competent reasoner on *all* points. It may be observed that those persons who would maintain an independent free-will would thereby entirely ignore the beautiful mechanism of the brain, and suppose it useless. *Will* subject to brain structure (*i.e.* to the mechanism of reason) *is* surely free, for the emancipation of the will from reason would be anarchy or slavery. If, therefore, we admit that under no conceivable circumstances would we have the will otherwise than subject to reason, then even if we could conceive the will emancipated from brain structure, the will (if consistent with reason) would still be *the same* as when subject to brain structure; for brain structure, being the mechanism of reason, determines the *will*, and makes it consistent with reason. Therefore I contend that the question of free-will is a quibble, or the will subject to and determined by brain structure (the mechanism of reason) *is* perfectly free.

The subject is a difficult one, and I may, no doubt, have said some things that admit of improvement, but I should be glad to have in any way contributed to throw a true light on this interesting question. P. Q.

London, October 16

Early Observations of the Solar Corona

THE "Astronomical Column" in NATURE, vol. xvi. p. 255, has drawn attention to an observation of the solar corona by Clavius during the total eclipse of 1605. This is, however, by no means the earliest known case in which the corona was remarked. Plutarch already had alluded to the faint light round the eclipsed sun, but the first eclipse, during which the corona appears to have made a strong impression on the observer, seems to have been that of August 31, 1030. On this day a fierce battle took place at Sticklestad, in Norway, between the Christian king Olaf (afterwards the national saint) and his heathen subjects. During the battle the sun was totally eclipsed, and a reddish light appeared round it. Before the eclipse of 1842 had made astronomers familiar with the corona and protuberances, Hansteen had suggested that it might be the zodiacal light which caused the red light in 1030.

Observatory, Birr Castle, Ireland J. L. E. DREYER

Sense Perception of Electricity

IN the very interesting address of Prof. C. von Nägeli at Munich, on "The Limits of Natural Knowledge," of which a first portion is printed by you (NATURE, vol. xvi. p. 531), in illustration of his argument that there may be many forces in nature which we have not the requisite senses to perceive, he instances electricity as an universal element which might well have escaped our cognisance but for its occasional concentrations and disturbances making vivid appeal to two senses that we have —in lightning and thunder. The illustration is an apt and telling one, but is it worth while to note that though we have no sense differentiated to perceive electricity as the eye receives the light-wave and the ear the sound-wave of the circumambient ether (an organ, by the way, which would be useless to us unless we had also the power of self-insulation on the approach of this danger), we *have* a very general physical perception of electrical changes? The remark, for instance, is very common, "I thought it felt like thunder;" and in some this consciousness is quite abnormal. I knew personally one gentleman to whom this sensitiveness was such a constant source of *malaise* that he was medically advised to wear a fine silk vest as an insulator. In his case the success of the experiment was so marked that, according to his own statement, it "made life another thing." It would be interesting to know whether such a peculiarity was transmitted. HENRY CECIL

Bregner, Bournemouth, October 22

The Future of our British Flora

I MUCH fear that the botanists of 1977, when roaming in search of specimens, will find it much more difficult to collect a varied herbarium, and that the poets of that period will be unable to obtain gratification from the sight of many interesting and beautiful plants which are already fast becoming rare and in a short period will likely be extinct with us.

Indeed the very curiosity to learn something more about botany, the teaching of which is now included as a "special subject" in our elementary schools, laudable as it is, has a tendency to hurry forward the extinction of rare forms, the collecting and recognising of which is almost a passion with beginners and much preferred to the slower and more instructive work of becoming thoroughly acquainted with what is common or widely diffused.

A few reminiscences culled from my experience will make plain what I mean. No one who has paid attention to the flora around a great and fast-increasing city such as Glasgow but must be struck with the extinction, in twenty or thirty years, of almost everything that used to be rare in the country district around. Showy flowers, such as the great mullein and the foxglove, are the first to suffer. They are like the large game in a newly-colonised country. In vain you look for them on the wayside of any well-macadamised road. They possess the fatal gift of beauty, and are either rooted out to perish miserably in a smoky garden, or destroyed from mere wantonness. A few days ago I visited the romantic ruins of Crookston Castle, about four miles from Glasgow, but I looked in vain for the common arum which, being rare in Scotland, made a visit to the ruins still more interesting when it flourished there healthily about twenty years ago.

In one single spot in Renfrewshire did I know of the existence of a few plants of the lizard orchis, but on making to see them after a long absence, I learned that the very last plant had been rooted and taken off this season by a schoolmaster eager to teach his pupils botany. Indeed, when an Edinburgh professor's class was about to visit a well-reputed floral locality this summer, I went to the farmer upon whose grounds the bird's-nest orchis grew, and implored him that, when the living wave passed by, he was not to betray to any student that alone in a wide district he was the possessor of so rare a treasure, and I believe my salutary warning prevented it from being all dug out.

In my own parish the wallrue spleenwort is represented by a single plant, and on the walls of a castle in a neighbouring district it grows no further down than human hands can reach. Even such common ferns as the royal osmund, the green spleenwort, and the parsley fern are completely lost to some districts within my recollection, and many of your readers could give other instances to the same point.

Will the flora of the future then tend to a dull uniformity so that only a limited number of widely-diffused species shall carpet the earth? To neutralise this tendency we have no doubt many escaped garden plants which in process of time get established on our waysides and unoccupied spaces. I have even known the case of the successful introduction of wild flowers formerly unknown to a district, and in my own parish I was pleasantly surprised to find, well-established in one locality, the field allium, the seeds of which had been sown by a friend, who obtained it in Sussex, many years ago. J. SHAW

Tynron, Dumfriesshire

The Towering of Wounded Birds

IT is still supposed by some sportsmen and gamekeepers that the towering of a wounded bird is caused by an injury to its head. In some instances this may possibly be the case, if the lesion of the brain be not so severe as to cause instant death; more generally, however, towering seems to be the result of internal hæmorrhage and asphyxia. In the unconscious condition produced by the circulation of venous blood the bird rises as it continues to fly, and at last falls dead.

The following is a good example of this mode of death; it occurred a few days ago in the case of a partridge. I made a careful examination of the bird, and found a condition that confirmed the explanation that towering is due, not to injury to the head, but to internal hæmorrhage and its consequences:—

On October 2 a partridge (it was an old male bird) was fired at at about forty yards. The bird flew about 450 yards after being hit, then towered high, and fell; it was picked up at about five poles, quite dead.

I examined it early next day; the feathers were all carefully removed. There was a slight wound in the neck; a pellet of No. 2 shot had perforated the skin, but had not penetrated or injured the subjacent tissues. There was a wound in the right pectoral muscle; the pellet had penetrated very slightly, and lay under the integument.

Two pellets had penetrated the abdominal cavity, one through the abdominal wall, the other through the bone. Both had passed forwards; one had slightly wounded, but not perforated, the stomach, and had then passed through the right lung; a large vessel had been divided. There was much blood clot in the cavity, and both lungs were congested and also collapsed.

On examining the trachea it was found that there were three blood clots—one near the bifurcation, a second about an inch above it, which firmly plugged the tube; a third, smaller, near the larynx. There was a small blood clot in the mouth and œsophagus—swallowed blood.

The heart and liver were uninjured, the brain was carefully examined; there was no injury; it was quite healthy and normal, except that the surface of the cerebellum had some slightly congested vessels. The spinal cord was uninjured; death caused by asphyxia and hæmorrhage. As the subject has been previously discussed, this case may be of sufficient interest for insertion in NATURE.

The pursuit of sport gave an opportunity of elucidating a point of some physiological interest that might not have been permitted to that of science. J. FAYRER

Meteors

ON Tuesday evening, October 2, at 8·59 P.M., whilst watching for shooting stars, I saw a fine meteor. At first scarcely brighter than a first magnitude star, it suddenly increased to the apparent size of Venus when about three parts of its path had been traversed, and then it appeared to explode with remarkable brilliancy. The motion was rather slow, and just in the place where its maximum was attained, it left a short luminous streak that I could trace as a faint nebulous patch on the sky for about three and a half minutes, drifting some five degrees away from the place it first occupied, and gradually dying out until I finally lost it amongst the small stars of Cassiopeia. It had moved from R.A. 346°, Dec. 57° N. to R.A. 352°, Dec. 54° N. The position of the meteor's course as I observed it was from the star β Cephei to the direction of (and below) α Andromedæ.

On the following night, October 3, 8·38 P.M., another large meteor was observed here, falling with a very swift, short path a few degrees to the right of the Pointers in Ursa Major. It must have been as bright as Venus, for it gave a strong flash in a very foggy condition of the atmosphere. There was a bright streak left in its track for about fifteen seconds.

A third meteor, far brighter than either of the two preceding, was recorded on Monday, October 8, at 11·50 P.M., and estimated to be twice as brilliant as Venus. Its path was nearly vertical, close to the eastern horizon, and probably directed from a radiant near θ Tauri, at R.A. 77°, Dec. 31° N. The sky was brightly illuminated with its intense lustre at the moment of appearance. There was a short streak visible for three seconds, and this, as in the two previous cases, served accurately to indicate the direction of its path.

The following were the exact courses of these large meteors and of a few others seen recently by me:—

Date, 1877.	Time.	Mag.	Began R.A. Dec.	Ended. R.A. Dec.	Path.	Notes.
			° °	° °	°	
Sept. 15	12 21	♃	56± 0	55−6	6	Streak 4 sec., rapid.
,, ,,	15 30	♃	139+78	282+87	15	Streak, rapid.
,, 16	14 42	♃	73+5	60−5	16	Streak, rapid.
Oct. 2	8 59	♀	325+67	354+48	24	Streak 3½ min., slowish.
,, 2	9 46	♃	203+40	198+36	6	Slow.
,, 3	8 38	♀	155+64	152+55	10	Streak 15 sec., rapid.
,, 7	9 14	♃	310+60	319+55½	6½	Slow streak, near radiant.
,, 8	11 50	2×♀	109+17	116+12	8	Streak 3 sec., very swift.
,, 8	13 35	♃	46+50	346+32	47	Streak 25°, not rapid.
,, 15	16 25	♃	222+83	263+76	10	Streak 3°, very rapid.
,, 16	13 18	♃	105+39	116+50	13	Streak 5° 3 sec., rapid
,, 17	16 28	♃	150+54	186+56	20	Streak, rapid.
,, 18	16 38	♃	26+43	1+41	19	Streak, rapid.

The fine weather prevailing this month and during part of September allowed me to maintain long watches for shooting stars as follows:—

September	4	watched during	$4\frac{1}{2}$	hours and saw		37	meteors.
,,	5	,, ,,	$3\frac{1}{4}$	,,	,,	33	,,
,,	7	,, ,,	4	,,	,,	38	,,
,,	15	,, ,,	$4\frac{1}{4}$	,,	,,	58	,,
,,	16	,, ,,	$4\frac{3}{4}$	,,	,,	59	,,
October	2	,, ,,	$4\frac{1}{4}$	,,	,,	37	,,
,,	3	,, ,,	$4\frac{1}{2}$	,,	,,	35	,,
,,	4	,, ,,	5	,,	,,	55	,,
,,	5	,, ,,	$4\frac{1}{4}$	,,	,,	31	,,
,,	8	,, ,,	10	,,	,,	105	,,
,,	16	,, ,,	5	,,	,,	83	,,
,,	17	,, ,,	$3\frac{1}{4}$	,,	,,	70	,,

Giving an aggregate watch of 57 hours and 641 meteors visible for September 4–October 17; but this merely relates to a portion of the work, for I have only included in this list those nights when I watched for long periods together.

From these numerous observations I was enabled to deduce many radiant points, and have selected a few of the most important:—

No.	Date.	R.A. Dec.		No. of ↓'s.		Max. dates.
		° °				
1.	September	61 + 36	...	15 meteors	...	Sept. 7 and 15
2.	Sept. and Oct.	85 + 54	...	26 ,,	...	Sept. 5 and Oct. 5
3.	Sept. and Oct.	109 + 38	...	20 ,,	...	October 8
4.	Sept. and Oct.	220 + 78	...	18 ,,	...	September 15
5.	Sept. and Oct.	60 + 85	...	16 ,,	...	October 2-8
6.	September	87 + 34	...	10 ,,	...	September 16
7.	Sept. and Oct.	103 + 12	...	27 ,,	...	October 8
8.	October	133 + 79	...	22 ,,	...	October 3-4
9.	October	310 + 77	...	17 ,,	...	October 3-4
10.	October	225 + 52	...	10 ,,	...	October 2
11.	October	133 + 21	...	18 ,,	...	October 15
12.	October 15-20	92 + 15	...	57 ,,	...	October 18

The last position is that of the well-known October shower, the *Orionids*. Several of the above radiants are probably new, and it is noteworthy that No. 8 agrees very closely with the radiant and date (R.A. 134°, 77° Dec. N., October 7 +) of Comet II., 1825, as calculated by Prof. A. S. Herschel.

Ashleydown, Bristol, October 22 W. F. DENNING

A METEOR of unusual brilliancy was seen by the passengers in the train from Exeter to Bristol, about 6·15 P.M. yesterday. The train was at the time about two or three miles south of Weston Junction. As nearly as I could judge, the meteor made its first appearance at an altitude of about 35°, and 4° or 5° south of west, and moved rapidly towards the horizon almost in a vertical line. The colour was a greenish white, and the train lasted about fifteen seconds. JOHN L. MCKENZIE

Independent College, Taunton, October 20

LAST Friday evening (October 19) we in Aberystwith saw a very beautiful meteor. It was ten minutes past six in the evening, when as I was walking along the shore and looking seaward (west), I saw the meteor rapidly descending as a pear-shaped body of red, yellow, and purple light, increasing much in brightness till it reached about twenty feet, as it looked, from the sea surface, when it suddenly and completely disappeared. Its track seemed a part of it—a tail to it—being at first a pale golden light continuous with the body of the meteor below, extending vertically up and ending rather abruptly above.

This narrow band of light lived complete for a short time, but after one minute most of the track had become a white or slightly grey fleecy cloud about a foot broad and three yards long, as it appeared, only its central part remaining bright as a golden nucleus to the cloud.

By two minutes atmospheric currents had bent the vertical cloud into an arc, the extremities turned to the north with the bright nucleus still distinct. Gradually the nucleus disappeared, but the cloud was still visible for a quarter of an hour, when the increasing dusk of the evening helped to obscure it.

Its course appeared exceedingly rapid, and the brightness was such that a passer-by who did not see the meteor itself, said that the place was lit up "like lightning."

Weather dull and wet, but Friday evening was fine.

University College of Wales, WALTER KEEPING
Aberystwith, October 20

Curious Phenomenon during the Late Gale

THE following may perhaps be of interest to your readers. At about 6·50 P.M. on the night of Sunday the 14th inst., I was walking in a south-easterly direction through the village of Lower Tooting, when I suddenly saw fall from the sky what looked like a huge ball of green fire. What struck me especially was its size, its vivid colour, and also the strange noiselessness of its fall. It seemed to come from a part of the sky somewhere near where Jupiter was then visible, and to fall not a hundred yards from me. This, I fear, is the most accurate information I can give. It took me so completely by surprise that I rubbed my eyes and wondered whether I had not been dreaming, a supposition which seemed to be supported by the indifference displayed by the numerous passers-by in the face of so extraordinary a phenomenon. Indeed I should hardly have thought seriously of the matter again had I not heard of a letter in the *Tim s* last Monday, describing a very similar phenomenon observed at Brixton some twenty minutes later on the same evening. This and other reports of a like nature, which seemed to imply that the atmosphere was in a somewhat unusual condition, before and during last week's storm, led me to think it worth while to lay before you, and if it so pleases you, before the readers of NATURE, what I at any rate have a strong conviction that I saw on the night in question. G. A. M.

Wine-Coloured Ivy

THE question has been discussed of late whether the ancient Greeks had an acute and true sense of colour. I remember once to have seen the remark that Sophocles shows his want of colour-sense by speaking of wine-coloured ivy. Now this really shows how true his perception of colour was. I inclose two ivy leaves which I have gathered to-day off a wall; I could have gathered plenty of the same colour, which, as you will see, is claret colour.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, October 21

OUR ASTRONOMICAL COLUMN

THE SATELLITES OF MARS.—In No. 2,161 of the *Astron. Nach.*, Prof. Asaph Hall has published his measures of both satellites from the dates of their discovery to September 16, though the observations are not completely reduced, differential refraction and the small corrections to refer the measures to the true centre of the planet or the corrections for the gibbous phase having yet to be applied. Prof. Hall intends to make a thorough discussion of the observations taken during the present opposition, and requests other astronomers to forward to him, at Washington, copies of any they may succeed in making.

Subjoined are a few positions of the inner satellite calculated from elements which represent roughly Prof. Hall's observations from August 17 to September 16, as the following selected dates will show:—

			Error in Pos.		Error in Dist.		Observed Pos.
			°		″		°
Aug.	17	...	− 1·1	...	+ 2·1	...	73
,,	26	...	+ 3·5	...	− 2·7	...	253
Sept.	1	...	− 2·0	...	− 2·5	...	250
,,	4	...	− 1·1	...	+ 1·7	...	69
,,	14	...	− 1·5	...	+ 1·2	...	67

There is perhaps a sensible ellipticity of orbit. The period adopted is 7h. 39m. 13s.

For the outer satellite the elements used for the last ephemeris in this column have been again employed; they agree closely with measures taken by Mr. Common at Ealing on October 16. It appears probable that Mr. Common saw the inner satellite about 9 P.M. on October 17, the calculated and estimated positions sufficiently according.

8h. G.M.T.			Inner Satellite.					Outer Satellite.				
			Pos. °		Dist. ″			Pos. °		Dist. ″		
Oct.	26	...	Pos. 152	...	Dist. 9			Pos. 91	...	Dist. 44		
,,	27	...	,, 91	...	,, 17			,, 213	...	,, 31		
,,	28	...	,, 69	...	,, 22			,, 256	...	,, 55		
,,	29	...	,, 37	...	,, 12			,, 324	...	,, 22		
,,	30	...	,, 306	...	,, 10			,, 64	...	,, 51		
,,	31	...	,, 263	...	,, 19			,, 103	...	,, 33		
Nov.	1	...	,, 242	...	,, 20			,, 224	...	,, 34		
,,	2	...	,, 194	...	,, 9			,, 262	...	,, 47		

THE SATURNIAN SATELLITE HYPERION.—The following positions are from Prof. Hall's elements in *Astron. Nach.*, No. 2,137. Mr. Common observed this very difficult object with his 18-inch silver-on-glass reflector on October 14, at 10h. 15m. G.M.T., when its position was 92·0° and distance 208″; the elements give 93·2° and 208″. This satellite appears to be truly an *experimentum crucis* even for our larger telescopes.

AT 8h. G.M.T.

Oct.	26	Pos.	276·6°	Dist.	222·6″	Nov.	3	Pos.	90·6°	Dist.	154·5″
,,	27	,,	277·9	,,	202·6	,,	4	,,	92·5	,,	192·2
,,	28	,,	279·6	,,	167·5	,,	5	,,	93·9	,,	215·8
,,	29	,,	282·4	,,	121·3	,,	6	,,	95·1	,,	222·4
,,	30	,,	289·4	,,	68·1	,,	7	,,	95·6	,,	209·8
,,	31	,,	340·4	,,	30·8	,,	8	,,	97·8	,,	176·3
Nov.	1	,,	77·1	,,	53·3	,,	9	,,	100·4	,,	124·2
,,	2	,,	87·2	,,	106·9	,,	10	,,	108·1	,,	58·1

THE SATELLITE OF NEPTUNE.—The ephemeris subjoined is deduced from Prof. Newcomb's tables in the appendix to the Washington Observations for 1874:—

At 11h. G.M.T.

Oct.	26	Pos.	221·5°	Dist.	16·9″	Nov.	3	Pos.	64·8°	Dist.	10·7″
,,	27	,,	194·0	,,	10·5	,,	4	,,	37·8	,,	16·9
,,	28	,,	72·0	,,	9·2	,,	5	,,	1·9	,,	8·3
,,	29	,,	40·2	,,	17·0	,,	6	,,	241·9	,,	11·4
,,	30	,,	10·6	,,	9·8	,,	7	,,	216·5	,,	16·8
,,	31	,,	248·1	,,	10·0	,,	8	,,	176·2	,,	7·6
Nov.	1	,,	219·0	,,	17·0	,,	9	,,	59·2	,,	12·2
,,	2	,,	186·6	,,	9·0	,,	10	,,	35·3	,,	16·5

THE VARIABLE NEBULA IN TAURUS (G.C. NO. 839).—Dr. Tempel gives some particulars of his examination of the neighbourhood of this object with the large Amici-telescope of the observatory at Arcetri, near Florence. Around the variable star which is close at hand (T Tauri of Prof. Schönfeld's Catalogue) a nebulous appearance was easily recognisable, but Dr. Tempel says he has remarked the same nebulous glimmer about other variable stars, amongst them in one of Goldschmidt's, which wholly disappears; in this case the glimmer is discernible before the star itself becomes visible. Near the variable star there are two small star-clusters, about which, however, there is no trace of nebulosity in a telescope that is capable of resolving them. We believe changes in the disposition of nebulosity near the variable star (which was only one minute of arc from the centre of the nebula at its discovery in October, 1852) were remarked some years since by Otto Struve with the Pulkowa refractor, but there has been no appearance of late, like that presented by the object in 1852, when it was conspicuous enough with a seven-inch aperture, which in 1863 and on several later occasions did not afford the least trace of it. The vicinity may be recommended for observation during the coming winter by those who are provided with instruments of sufficient grasp of light. Dr. Tempel has carefully delineated all the features that he has noticed with his large telescope for comparison with any other drawings that may be made by competent observers.

F. L. ALPHONS OPPENHEIM

PROF. OPPENHEIM, whose tragic fate was briefly alluded to a few weeks since, was born at Hamburg, February 14, 1833. In 1852 he graduated from the gymnasium there, and entered the University of Bonn. Here, and at Göttingen, he pursued a widely-extended course of scientific studies until 1857, when he passed the examination for Ph.D. at the last-named place. In the same year, after a short residence at the University of Heidelberg, he proceeded to London, where he carried out a number of researches in Prof. Williamson's laboratory. From here he went to Paris, where his chemical investigations were prosecuted in the laboratory of Prof. Wurtz until 1867, when he returned to his native country and entered the University of Berlin as a privat-docent. This position was soon exchanged for that of an extraordinary professor, and early in the present year he accepted a call to the chair of chemistry in the Royal Academy of Münster. Just at the entrance of a career of widely-extended usefulness, while superintending the equipment of his new laboratory, a gloom was cast upon his path by the sudden decline of his wife, an English lady, to whom he was passionately attached. Months of watching and anxiety caused a condition of the most utter mental prostration. On September 16, within two hours of his wife's death, one of the deadliest drugs known to the chemist did its swift, painless work, and he was no more.

This sudden death has caused a feeling of sadness in an unusually large circle. Prof. Oppenheim was not only held in high esteem by the scientific men of his own country, but was warmly regarded by many leading chemists in France and England, while in the columns of this journal and in the meetings of the British Association his name became familiar to a more extended class. Prof. Oppenheim's chemical investigations are characterised chiefly by their variety, thoroughness, and theoretical value. We can only allude to his researches on tellurium and its compounds, the exhaustive monograph with F. Versmann on the application of saline solutions to render textile fabrics non-inflammable, the numerous papers on allylen and propylen derivatives, the extensive studies in the turpentine group, which yielded, among other results, the theoretical composition of cymene and the ethers of pyroracemic acid. During the past few years he carried out a number of interesting researches on the derivatives of aceto-acetic ether and its homologues, the most valuable of which were the discovery of oxyuvitic acid with F. Pfaff and of propionyl-propionic ether with R. Hellon. As one of the founders of the German Chemical Society, and for many years its secretary, Prof. Oppenheim did much to contribute to the efficiency of this organisation and bring it to its present prosperity and widespread sphere of activity. Besides numerous contributions to contemporary scientific literature, he translated into German Odling's "Manual of Chemistry" and Wurtz' "History of Chemical Theories," the English edition of which, by Watts, is so well known.

Prof. Oppenheim's charming social qualities attracted to him friends in all ranks of society, and the literary and scientific celebrities of Berlin were often to be met at his table. The many foreign scientific students at Berlin who recall their hospitable welcome in his home will join with his friends in the feeling of grief over this abrupt termination to a career of such promising scientific usefulness.

T. H. N.

ELECTRIC LIGHTS FOR LIGHTHOUSES

REPORTS to the Trinity House have just been issued giving the results of some experiments made at the end of last year and the beginning of the present, by Prof. Tyndall and Mr. J. Douglass, Chief Engineer of the Trinity House, on the comparative value of various magneto-electric machines for lighthouse purposes.

The machines experimented on by Prof. Tyndall were the following:—(1) Holmes' machines, which have been already established for some years at the South Foreland; (2) Gramme's machines; (3) Two Gramme's machines coupled together; (4) Siemens' large machine; (5) Siemens' small machine.

Prof. Tyndall's observations were made on November 21 and 22 last year, from the Corporation's steamer *Galatea*, the position first chosen being not far from the Varne Light, and at a distance of 11½ miles from the lighthouses on the Foreland. Observations were subsequently made at various other distances.

In the first place, the new machines sending their currents to the Low Lighthouse were compared in succession with Holmes' machine, which produced its light in the High Lighthouse. Subsequently the new machines were pitted in pairs against each other—one of the two being in the High and the other in the Low Lighthouse. Care was taken in each instance to reverse their positions. Thus, whenever Siemens below was compared with Gramme above, the observation was immediately followed by a comparison of Siemens above with Gramme below, and so of the others. All irregularities arising from differences in the apparatus employed above and below were thus eliminated.

The following are the results of the observations on the nights of November 21 and 22:—The new machines mark a great advance, both in economy and power, as regards the application of the electric light to lighthouse purposes. Thus the machine of Holmes was found practically equalled by a single machine of Gramme, of considerably less volume and considerably smaller cost.

This discrepancy as to cost and volume was still greater in the case of the small Siemens machine, which yielded a light sensibly equal to that of Holmes'.

The single Gramme and the small Siemens machines are sensibly equal to each other, both of them producing an exceedingly fine light.

Prof. Tyndall was particularly impressed by the performance of the small machine of Siemens. Its power, in relation to its size, is surprising. The large machine of Siemens, however, greatly transcends both his small machine and the single machine of Gramme; it is sensibly equal to the two Gramme's machines coupled together, the price of the former being less than half that of the latter. The light from the large Siemens, as also that from the two coupled Grammes, is of extraordinary splendour. Siemens' and Gramme's inventions, Prof. Tyndall states, undoubtedly place at the disposal of the Elder Brethren electric lights of surpassing energy. Combining either the large machine of Siemens, the two Gramme's machines, or, if practicable, the two small machines of Siemens, with one of the group-flashing dioptric apparatus which have been recently devised by Dr. Hopkinson, a light transcending in power and individuality all other lights now existing would probably be obtained. Such a light would displace, with enormous advantage to the mariner, the two lights hitherto displayed at the Lizard. A fixed light, even should it be the electric light, at a distance is not to be distinguished from a ship-light or an ordinary shore-light near at hand.

On November 22 Prof. Tyndall visited the South Foreland, inspected the arrangement of the machines, and observed their light-producing power close at hand. In both Siemens' and Gramme's machines the induced currents are sent in a constant direction. One of the carbons is always positive, the other always negative—not alternately negative and positive as in the machine of Holmes. The positive carbon is heated more intensely and it wastes more rapidly than the negative one; its shape, moreover, is a point of some practical importance. From the positive to the negative carbon there is a transfer of particles which usually produces a crater-like hollow in the positive carbon. The concave surface of this crater is the place of most vivid incandescence, and it is easy to see that the radiation from that surface, when the positive carbon is the higher one, as it is in the arrangement at the South Foreland, would be directed to the earth. To obviate this inconvenience, the negative carbon is usually somewhat displaced, so as to cause the most vivid incandescence to occur on one side of the positive carbon. The portion of space towards which this side is turned receives from it a greatly augmented radiation. But the radiant power thus concentrated on one side is withdrawn from the other, which would be inadmissible if a whole circle had to be uniformly illuminated. In most cases, however, only a portion of the entire circle is required; and no disadvantage arises from the weakening of the landward radiation.

If no valid mechanical grounds oppose the alteration, it would, Prof. Tyndall thinks, be a decided advantage to make the lower carbon the positive one. Its upward radiation would be utilised by the upper prisms to a far greater extent than its downward radiation is now utilised by the lower ones.

Mr. Douglass in his Report describes a series of experiments made during the first four months of the present year, the results obtained by him being essentially the same as those obtained by Prof. Tyndall. As in the November experiments, the various machines were pitted against each other. Messrs. Siemens' small-sized, or No. 58 machine had proved so satisfactory that they were asked to furnish a second one for the trials.

For the photometric measurement of the light the flame of the Trinity House 6-wick lamp, when consuming colza oil, was adopted as the standard. This lamp was placed at a distance of 100 feet from the electric lamp, and the measurements were taken by a Bunsen photometer. The 6-wick lamp was maintained, as nearly as practicable, at its intensity of 722 standard candles, and this intensity was checked from time to time by candle measurements taken with a separate Sugg photometer.

Mr. Douglass refers in some detail to the greater consumption in the top-carbon of the Gramme and Siemens machines than in the other machines. A portion of the light is thus prevented from being fully utilised in the extreme upper prisms of a dioptric apparatus by the edge of the crater thus formed. In order to avoid this loss, and obtain the maximum of light from the carbon, they are usually so placed in the lamp that the axis of the bottom carbon is nearly in the same vertical plane as the front of the top carbon. This arrangement has the effect of sending a condensed beam of light of maximum intensity in one direction, and moreover the light is much steadier than with any other arrangement of the carbon points, in consequence of the current through the upper carbon being held steadily at the front edge. Mr. Douglass found with this arrangement of the carbons, and assuming the intensity of the light with the carbons having their axis in the same vertical line to be represented by 100, the intensity of the light in four directions in azimuth, say, E. W. N. and S., will be nearly as follows:—E. 287, N. and S. 116, W. 38. The mean intensity is thus as 139 to 100; but Mr. Douglas thinks that for lighthouse purposes a mean of E. (or front), N. and S. may be taken, giving a mean intensity of 173 to 100.

Mr. Douglas describes various experiments made to test the rival machines, and in an appendix the tabulated results of observations of the Siemens and Gramme machines are arranged. A series of experiments on January 18 on the power of light of the machines resulted as follows:—

	Mean condensed light. Standard candles.	Mean diffused light Standard candles.
1 Holmes M. E. machine	... 1,494	... 1,494
2 ,, ,,	... 2,721	... 2,721
1 Alliance ,,	... 1,953	... 1,953
1 Gramme machine	... 5,333	... 3,215
2 ,, ,,	... 9,126	... 5,501

Next day measurements were taken of the light produced by the Siemens No. 1 and No. 58 machines. The light produced by the latter machine was tested against the light produced by the Gramme machine, and the light produced by one Holmes machine against that produced by No. 58 Siemens machine, the lamps being 100 feet apart. The results were as follows:—

With 1 Gramme *versus* No. 51 Siemens, the relative intensity was found to be as 100 to 100·6. With 1 Holmes *versus* No. 58 Siemens, the relative intensity was found to be as 100 to 384. The last two experiments were checked by exchanging the conducting wires and lamps.

On January 20 the lights produced by the machines were tested against each other as follows, viz. :—1 Gramme *versus* No. 58 Siemens, 1 Holmes *versus* No. 1. Siemens, 1 Holmes *versus* 2 Grammes. An experiment was also made for determining the relative intensity of the light and horse-power absorbed by the Siemens No. 58 machine when running at half and full speed. With the machine running at half speed the light was found to be so unsteady that it could not be correctly measured.

The relative intensities of the light produced by the machines were as follows, viz. :—

1 Gramme *versus*	No. 58 Siemens	as 100 to	116
1 Holmes ,,	No. 1 Siemens	as 100 to	557
1 ,, ,,	2 Grammes	as 100 to	663

On a subsequent day comparative trials were made of the two small machines of Messrs. Siemens, numbered respectively 58 and 68, when the intensity of the light was found to be as 100 for 58 to 109·5 for 68, being 9·5 per cent. in favour of the latter machine.

A trial was made of the two small Siemens machines, Nos. 58 and 68, working singly, and also together in parallel circuit. The intensities were found to be as follows, viz. :—

No. 58 Siemens machine	4,446
,, 68 ,, ,, ...	65,63
For the two machines	11,009
Nos. 58 and 68 coupled together	13,179

There was thus shown to be a superiority in the intensity of the light produced by the two machines coupled together over that produced by the two machines when working singly, as 11,009 to 13,179, or as 100 to 119·7, being 19·7 per cent. more light with the two machines coupled together.

Experiments were also made for determining the relative intensities of the diffused beam of light with the carbons in the same vertical line, and of the condensed beam of light with the axis of the bottom carbon nearly in the same vertical plane as the front edge of the top carbon; also the intensities of the side and rear light. With the latter arrangement of the carbons the intensities were as follow, viz. :—

SIEMENS MACHINE, NO. 68.

	Intensity. Standard in candles.
1. Carbons with axis in same vertical line ...	2,021
2. Axis of bottom carbon in same vertical plane as front edge of top carbon. Front beam	5,804
3. Same arrangement of carbons. Side beam, 90° from No. 2	2,346
4. Same arrangement of carbons. Back beam, 180° from No. 2	772

Messrs. Siemens having submitted for trial with their machines a conducting cable of larger dimensions than the South Foreland cables, and of the length required between the engine-room and the High Lighthouse, Mr. Douglass made some experiments with it in connection with each machine. The cable was 1,400 feet in length, and composed of 19 copper wires of No. 16 B.W. gauge well insulated. The cable was cut into two equal lengths of 700 feet each, and arranged in two coils in the engine-room. The currents from the Nos. 58 and 68 Siemens machines, separately and collectively, were sent through it to the electric lamp, which was also placed in the engine-room, and at a distance of 100 feet from the 6-wick oil test lamp. The short current to the lamp was made through 22 feet of the small cable of Messrs. Siemens, composed of seven copper wires of No. 13 B.W. gauge. The loss of light with the machines was found to be as follows, viz. :—

	Per cent. of the whole light.
No. 58 machine	24
No. 68 ,, ...	23
Nos. 58 and 68 coupled	35

The experiment previously referred to with the Siemens machine No. 58 showed a loss of light of about 43·8 per cent. with the current sent through 700 feet of the small lighthouse conducting cable. There would therefore appear to be a reduction in this loss of 43·8 less 24 = 19·8 per cent. by adopting the larger cable.

The results of these interesting and carefully-conducted experiments are entirely in favour of the small Siemens machine, which both Dr. Tyndall and Mr. Douglass recommend for adoption at the Lizard.

THE MOVEMENTS OF A SUBMERGED AQUATIC PLANT[1]

FOR a long time the researches of Dutrochet and Payer, taken up and continued by Duchartre, Sachs, and others, have familiarised botanists with the movements of torsion or of flexion presented by certain plants. Notwithstanding these conscientious researches this question is still one of the most mysterious problems in vegetable physiology. I propose to draw the attention of biologists to a fact of the same kind, which I believe new, and which is connected with the phenomena observed in phanerogamous aquatic plants, living entirely submerged. It relates to a well-known aquatic plant, *Ceratophyllum demersum*, which must be included among the number of those which, in certain of their parts, and at certain periods, spontaneously execute regular movements subject in their range to a well-marked periodicity.

It is known that the Ceratophyllum grows in the still water of ponds, and that its slender, branching, floating stems bear whorled leaves. Their ordinary position in stagnant waters is vertical, or nearly so. It is in the upper part of these stems (of those at least whose whorls are separated by about one or two centimetres) that these movements show themselves. They consist in the regular bending and straightening of the axis or of the branches, combined with a torsion more or less pronounced.

Taking the axis at its maximum of erection, it is seen to bend regularly, and with the peculiarities I shall indicate immediately, to curve more and more for about six hours, when it reaches its maximum of flexion; then straightening itself more gently, in twelve hours it resumes its original position; it next bends in the direction opposite to its first flexion, and in four hours it attains its maximum of inverse deviation, resuming its first position in four hours more.

Thus a young branch is vertical at 6 A.M., at its maximum of inclination at midday, perfectly straight again at midnight, inclined at the maximum towards the south at 4 A.M., vertical again at 8 A.M, at its maximum of inclination to the north at 2 P.M., quite erect at 2 A.M., inclined at the maximum to the south at 6 A.M., vertical at 10 A.M., and so on.

The total duration of an evolution will thus be about twenty-six hours. These oscillations, although nearly equal in duration, do not present at all ages of the plant the same extent nor the same amplitude. At first not well marked, but involving the entire axis, they become more and more pronounced with the age of the branch; then the lower internodes become successively immobile, and the terminal ones alone continue to move.

The branches of the Ceratophyllum present two different aspects. Sometimes the whorls are very close to each other, the internodes being very short; the leaves of the consecutive whorls, resting on each other, make with the stem a very acute angle and form a compact mass. Sometimes the internodes are elongated, the whorls are

[1] From an article in *La Nature*, by E. Rodier.

separate, the leaves gradually extend, forming with the axis a greater and greater angle, and some finish by turning themselves down towards the base of the branch.

It is under the last form that the plant accomplishes, in the most apparent manner, the movements to which we refer. These become more manifest when young branches grown in an aquarium assume, in consequence, a slender and weak aspect, and the leaves become almost capillary. Consequently the best manner of observing and measuring the oscillations consists in submerging a piece of the stem bearing an axillary bud, and fixing the fragment by means of a weight. The young branch then assumes a vertical position, and its movements soon make themselves apparent. It is then easy to see that the movement of flexion is produced first in the superior internodes, and that it is propagated thence in a diminishing degree from above downwards; while, on the contrary, the movement of erection commences with the inferior part terminating with the superior, which sometimes, shortly before quite recovering itself, forms with the axis a very acute angle.

The oscillations continue very apparent during several days; ordinarily they diminish at the end of a certain time. Their amplitude decreases, and the branch becomes motionless or apparently so. But after thus remaining stationary it may resume its former variations. There are, moreover, branches, especially those which are almost horizontal, which continue motionless.

Light does not appear to have any influence on these movements. At least the suppression, the diminution, the change of colour, or the direction of the luminous rays have not had any apparent influence on them. Although I have seen the leaves participate in the movements of the axis, the modifications they have undergone may well be mechanically produced by the inflexions of the axis itself.

As to the movement of torsion, I am not able to say anything precise for want of experiments sufficiently conclusive. The movement is nevertheless very apparent. It takes place sometimes in one direction, sometimes, and much more energetically in another. By means of an index made of a thin plate of mica or glass, supported by a small glass float sufficiently weighted, the whole resting on one of the whorls, and carefully turned round by means of a pin moving over a scale, I have measured angles of tension of 35 degrees in nine hours, 120 degrees in seven hours, 450 degrees in nine hours, &c. But having only lately commenced the research, I must abstain from co-ordinating the results.

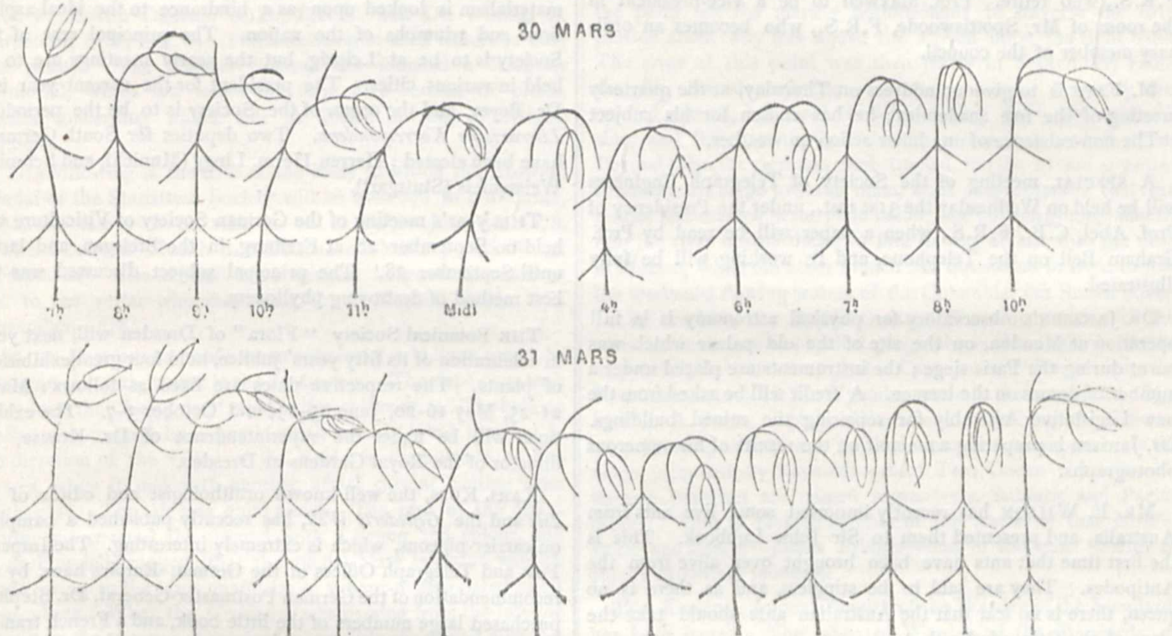


FIG. 1.—Various positions assumed by a branch of *Ceratophyllum demersum.*

Such are the general facts which I have to indicate, and in support of which I give an illustration of particular cases. The figure gives for a branch nineteen positions for March 30 and 31 and April 1. The very marked movements took place from north-east to south-west, and the entire evolution in twenty-six hours.

The priority of action of the superior internodes for the flexion, and that of the inferior internodes for the erection, is here very evident. The nutant attitude of the apex of the shoot, almost at the moment of complete erection, is also extremely striking.

To April 26 the movements continued, but they were then limited to the superior extremity of the branch. It appeared to shrink from the light, but as at the same time a branch in close proximity turned towards the light, the direction of displacement could not be so accounted for. I, nevertheless, during the following days (1) totally suppressed the light, (2) threw light upon the plant, by means of a mirror, in a direction opposite to that of the ordinary light, (3) placed a screen reaching almost to the middle of the plant, (4) placed in the path of the rays a red glass, intercepting as far as possible the other rays. The phenomena remained the same.

NOTES

THE Queen has conferred the honour of the Companionship of the Civil Division of the Order of the Bath on Prof. Frederick Augustus Abel, F.R.S., Chemist to the War Department and President of the Chemical Society, and on Major-General Charles Wright Younghusband, F.R.S., R.A., Superintendent of the Royal Gun Factories.

THE University of Cambridge proposes to confer the degree of LL.D. upon Mr. Charles Darwin.

UNDER the title "Challenger-Briefe" a collection of letters are now being published at Leipzig (Engelmann), which were written by the well-known zoologist, Dr. Rudolph von Willemoes-Suhm, of Munich, to his mother during the years 1872–5, when he was taking part in the *Challenger* Expedition, from which he was never to return. Prof. Kupffer has written a preface to these interesting documents.

WE are glad to be able to state that the fears expressed in our paragraph a fortnight ago respecting the loss of Mr. Seebohm's collections in the wreck of the *Thames* were unfounded. Mr. Seebohm has reached England with a very large collection of birds' skins and eggs, made in Northern Russia during the past summer.

THE following changes in the council of the London Mathematical Society are proposed to be made for the coming session: Prof. Clerk Maxwell, F.R.S., and Mr. Harry Hart to take the places of Prof. Clifford, F.R.S., and Rev. R. Harley, F.R.S., who retire. Prof. Maxwell to be a vice-president in the room of Mr. Spottiswoode, F.R.S., who becomes an ordinary member of the council.

M. FAYE is to give an address on Thursday, at the quarterly meeting of the five academies; he has chosen for his subject "The non-existence of any lunar action on weather."

A SPECIAL meeting of the Society of Telegraph Engineers will be held on Wednesday the 31st inst., under the Presidency of Prof. Abel, C.B., F.R.S., when a paper will be read by Prof. Graham Bell on the Telephone, and its working will be fully illustrated.

DR. JANSSEN'S observatory for physical astronomy is in full operation at Meudon, on the site of the old palace which was burnt during the Paris siege; the instruments are placed under a number of domes on the terrace. A credit will be asked from the new Legislative Assembly for repairing the ruined buildings. Dr. Janssen is preparing a memoir on the results of his numerous photographs.

MR. E. WILSON has recently imported some live ants from Australia, and presented them to Sir John Lubbock. This is the first time that ants have been brought over alive from the Antipodes. They are said to be stingless, and as there is no queen, there is no fear that the Australian ants should take the place of the Colorado beetle!

A NUMBER of Esquimaux are expected daily at the Jardin d'Acclimatation, Paris, with their dogs, sledges, &c. They are to stay at the Gardens during winter, camping out of doors.

M. MARCHE, one of the Ogové explorers, was present at the last meeting of the Paris Geographical Society. He expressed the decided opinion that, considering all the circumstances connected with Stanley's discovery, the Ogové is merely a branch of the Congo. He met some natives on his journey who said they came from a large river where white men are to be found, which river cannot possibly be other than the Congo. The course of the stream gave indication of a south-easterly bend which supports such a hypothesis.

THE Geographical Society of Paris purchased a piece of ground for building its hotel, some time ago, and the Municipal Council of Paris has diminished by about 2,000*l.* the purchase-money which had been agreed upon. That sum is about one-fourth of the total price.

AT the Huddersfield Mechanics' Institution, on Tuesday, Mr. Forster again insisted on some of the ideas so forcibly brought out by him recently at Bradford. The gist of his address was that it will be impossible to stop at the three R's in elementary teaching; that, as in Germany, Switzerland, and America, elementary schools should be supported entirely from the taxes, and that the education should be such that tax-payers of any class might send their children to these schools in the confidence that they would get a complete and thorough education; whether they did so or not would be their own affair.

LORD HARTINGTON laid, on Wednesday last week, the foundation-stone of a public hall to be erected in memory of George Stephenson, at Chesterfield. The hall will cost 13,000*l.*, and is to be used for scientific and educational purposes.

WE would call the attention of our readers to a scheme for a Channel Islands' Museum and Institute of Pisciculture Society, the objects of which will be found described in our advertising pages. The scheme seems to us to deserve encouragement.

AT Leipzig a "German Society for Mental Work" has been founded. Its object is the promotion of literature, art, and science, and of mental culture generally, in all German lands, on the basis of a contest against materialism, inasmuch as materialism is looked upon as a hindrance to the ideal aspirations and triumphs of the nation. The principal seat of the Society is to be at Leipzig, but the yearly meetings are to be held in various cities. The president for the current year is a Dr. Beyer, and the organ of the Society is to be the periodical *Literarische Korrespondenz*. Two deputies for South Germany have been elected: Herren Herm. Lingg (Munich), and Schmidt-Weissenfels (Stuttgart).

THIS year's meeting of the German Society of Viticulture was held on September 26 at Freiburg, in the Breisgau, and lasted until September 28. The principal subject discussed was the best method of destroying phylloxera.

THE Botanical Society "Flora" of Dresden will, next year, in celebration of its fifty years' jubilee, hold four great exhibitions of plants. The respective dates are fixed as follows: March 21–25, May 16–20, June 25–29, and October 3–7. The exhibitions will be under the superintendence of Dr. Krause, the director of the Royal Gardens at Dresden.

KARL RUSS, the well-known ornithologist and editor of the *Isis* and the *Gefiederte Welt*, has recently published a pamphlet on carrier-pigeons, which is extremely interesting. The Imperial Post and Telegraph Offices of the German Empire have, by the recommendation of the German Postmaster-General, Dr. Stephan, purchased large numbers of the little book, and a French translation is now in progress.

THE "Dismal Swamp," so well known in connection with American slave-stories, the *New York Tribune* informs us, is said to be an entire misnomer. There is nothing dismal about it except its general solitude. It is described by recent visitors as a capital resort for sportsmen, game being abundant, and fish ready to rise to the fly. The most curious features of the swamp are the sweetness and wholesome character of the water, and the entire freedom of its few inhabitants from malarious diseases. This purity is ascribed to the influence of the juniper tree, which certainly colours if it does not improve the water, and possibly contributes an antiseptic property to the air.

SEVERAL bolides have been observed in France, one on October 14, at half-past 6 P.M., from Vincennes (near Paris), Havre, and Clermont-Ferrand, appearing in the vicinity of the Great Bear; another on October 16, at 8 P.M., at Chambery, travelling from east to north-west.

THE annual meeting of the Yorkshire Naturalists' Union was held on October 6 at Wakefield. The annual report showed that the past year had been one of unusual success and steady progress. Six meetings had been held—at Pontefract, Wetherby, Nostell Priory, Shipley Glen, Goole, and Copley, and the attendance had uniformly been good. The sections which had been established had quite justified by success the experiment of their formation. The number of societies in union had increased from twenty-one to twenty-five, and of members from 1,050 to 1,300. The change of name having been mentioned, the pro-proposed issue of Transactions was spoken of. In response to the council's appeal for funds, a liberal, though of course not as yet sufficient, response had been made. There are at present about 105 subscribers to the funds, which, after defraying the expenses of the meetings, will be devoted to the publication of Natural History Transactions for the county. The financial statement showed a balance available for purposes of publication of over 20*l*. Mr. Henry Clifton Sorby, F.R.S., of Sheffield, was unanimously elected president.

THE Committee of the Chester Society o Natural Science announces that in connection with the Gilchrist Educational Trust, six Popular Science Lectures will be given in the Town Hall, Chester, on successive Tuesday evenings, commencing Tuesday, November 6, by Prof. W. C. Williamson, F.R.S., Prof. P. Martin Duncan, M.D., F.R.S., and Dr. William B. Carpenter, C.B., F.R.S. The admission to each lecture is only one penny. The Chester Society has founded a Kingsley Memorial, which has taken the form of a number of prizes, details concerning which will be found in our advertising columns.

THE following is the title of the essay to which the Howard Medal of the Statistical Society will be awarded in November, 1878, the essays to be sent in on or before June 30, 1878, "The Effects of Health and Disease on Military and Naval Operations." The council have decided to grant the sum of 20*l*. to the writer who may gain the "Howard Medal" in November, 1878.

M. DE FONVIELLE sends us an account of a balloon ascent he made on the 18th instant in the *Hydrogene*. The departure took place from the Champ de Mars at half-past three P.M. The balloon at first was pushed by a gentle north-easterly wind blowing in the direction of the valley of the Seine to an altitude of 600 metres, when it met with another aërial current coming from north-west. The air was quite moist. The rays of the disappearing sun covered the ground with a strong red colour, and M. de Fonvielle suggests that the planet Mars takes its reddish shade owing to the large quantities of vapour disseminated in its atmosphere. When the *Hydrogene* was passing from the inferior current to the superior it received a sensible shock and vibrated like a pendulum for some time. From 1,200 metres to 1,600 he found a wind blowing from the north-east like the first met after starting. The air was cold and dry. The temperature, when ascending, was 13° Cent. in the shade and at 1,600 meters he found 3° Cent. under 0. When landing at a quarter-past six the thermometer gave 10° Cent. on the ground at Bonelle, about forty kilometers from the Champ de Mars. During about twenty minutes, having descended in the vicinity of land not above 300 metres, the balloon met a south-westerly wind which was prevailing in the valleys of Bievre. That current of air was obviously enough a modification of the principal wind in consequence of the hilly character of the district. Such local winds observed by meteorologists can lead to no practical conclusions at all. If we consult the readings taken at the stations of the International Service on October 18 at six P.M., we find almost no two stations having the same wind in the Parisian district. The only wind worthy of note was that shown by the direction of the clouds, and of which no account was taken. M. de Fonvielle states he never met in his many aërial journeys with circumstances so strongly in favour of the introduction of cloud observations in weather prognostications. The principles advocated by Buys Ballot, and practised by Norwegian observers, received a strong confirmation.

ON the map of Major W. F. Raynolds, embracing that portion of the Missouri River country traversed by himself in 1859 and 1860, there was first recorded the peculiar hydrographical feature known as the "Two-Ocean Water." Its position is there indicated roughly by means of dotted lines, according to the account given by Bridger, the guide of the party. In the report of the expedition Raynolds remarks that "having seen this phenomeon on a small scale in the highlands of Maine, where rivulet discharges a portion of its waters into the Atlantic and the remainder into the St. Lawrence, I am prepared to concede that Bridger's 'Two-Ocean River' *may* be a verity." Dr. Hayden, after a careful reconnaissance of the region, reported that such a phenomenon was at least doubtful, at the same time suggesting that the "low ridge in the great water divide of the continent has doubtless given rise to the story of the Two-Ocean River, and such a stream has found its way to most of our printed maps. The expedition of Capt. W. A. Jones, in 1873, ascended the valley of the Upper Yellow-stone for twenty-five or thirty miles, the trail of the party left the marshy bottom-lands to traverse the drier portion about fifty feet above the stream upon the right bank. The river at this point was then (early in September) rather sluggish, the slope being somewhat gradual. Presently they crossed a small, but rapid rivulet, coursing down the mountain side, and falling abruptly into the valley just beneath them. Beyond them the view was unobstructed, but the stream appeared to ascend the slope towards them, until they observed that the rivulet had divided in the plain below, one portion gliding silently into the river behind them, to find its way at last into the Gulf of Mexico, while the other branch had descended in front to join the westward flowing waters of the Columbia, *via* Snake River, finally reaching the Pacific Ocean. The true position of this remarkable feature of physical geography is clearly shown on the map which illustrates the report of Capt. Jones ("Report upon the Reconnaissance of N.W. Wyoming made in the summer of 1873, by Wm. A. Jones, Captain of Engineers. Washington: Government Printing Office, 1874"). The mountain stream now bears the name proposed by Raynolds—the "Two Ocean Creek"—and its two branches are named respectively, Atlantic and Pacific creeks. Thus is verified another of the stories of that faithful guide and hunter—James Bridger—one of the most worthy of Rocky Mountain pioneers.

THE Eleventh Annual Report of the Warden of the Standards has been issued. The office of Warden of the Standards, on the retirement of the late Warden, Mr. Chisholm, was associated with that of the Permanent Secretary of the Board of Trade, so that the Report is signed by "T. H. Farrer," as Warden of the Standards. The practical duties of the department are discharged by Mr. H. J. Chaney. Among the subjects referred to in the Report is the rude and antiquated method of teaching weights and measures in our schools, which has been referred to in previous papers of the department. It would appear, from present arithmetic books that the student may still be taught the particulars of weights or measures which can be of no possible use to him in after-life. Another interesting point referred to is the merits of "short-arm" and "long-arm" balances. To meet the requirements of modern science a balance is needed by which the weights of bodies may be determined most accurately and quickly. Prof. Mendeléef, of St. Petersburg, and Herr Bunge, Mechanicus, Hamburg, have shown that it is possible to weigh quickly and accurately by the use of a balance whose beam has much shorter arms than those now in general use. A practical test of the relative merits of a beam with long arms and or one with short arms has been made in the department. By this test, as well as from the mathematical consideration of the question, it has been ascertained that whilst the probable error of a weighing made with the short-arm balance is slightly greater than the probable error of a weighing made with the long-arm balance, yet a weighing by the long-arm balance occupies twice as much time as one made with the short-arm balance. Consequently great economy of time is obtained by the use of a short-arm balance. Prof. Barff's process for preventing the corrosion of iron appeared to the department to be likely to prevent the oxidation of Standard weights made of iron. Prof. Barff has undertaken to submit some specimens of iron weights to this process, and it is intended to place these weights in the hands of some local inspector of weights and measures so that it may be ascertained whether iron Standard weights thus protected could safely be used in place of the expensive bronze or brass Standard weights at present used. The Report refers to several other points connected with the working of the department, one of importance to the general public being its operations in connection with the testing of gas-meters.

MR. A. RINGWOOD, of Adelaide, South Australia, publishes a plan by which one observer may measure the height of the clouds. The observer is to note the altitude and azimuth of the cloud, the azimuth of the cloud's shadow, and the spot in the surrounding country where the shadow falls; from this last observation by means of a map he can find the distance of the shadow from him. From these elements, together with the altitude and azimuth of the sun, a variety of expressions for the height of the cloud can be deduced. The method is equivalent to taking observations at each end of a base whose length is the distance of the shadow from the observer, the observer at the shadow end of the base being the sun. The difficulty of recognising the shadows of individual clouds and the comparatively short time in each day during which it would be possible to do so, joined with the fact that the higher clouds, such as cirrus and cirrocumulus, hardly ever cast defined shadows, must prevent the method from becoming generally useful; but still there is this to be said for it, that if in an observation the base, *i.e.*, the distance of the cloud's shadow from the observer, is long, a good measure of the height of the cloud may be got with comparatively rough observations of the other elements.

THE singing of mice is a phenomenon which was recently affirmed by Dr. Berdier in a letter to *La Nature*. A distinguished herpetologist, M. Lataste, suggested that he may have made confusion with the singing of a raniform batrachian, the *Bombinator igneus*, but Dr. Berdier said there was no marshy ground near the room in which he had heard it, and he stuck to his assertion. His observation has been confirmed at a recent meeting of the French Société d'Acclimatation, by M. Brierre, who stated that he, with several others, had heard mice sing at Saint-Michel-sur-l'Heron (in Vendée), in 1851–1853. The singing (which was at first attributed to reptiles) came from an old cupboard bought in a market-place, and concealing mice. It was about sunset that the sounds generally commenced. M. Brierre soaped the joints and the wood so that he might open the cupboard suddenly without noise. He did the latter one evening soon after the sounds had commenced, and succeeded in observing, for about a minute, the movements of the throat of a mouse, which emitted a song like that of a wren, the snout being elongated and held up in the air, as a dog does when he howls. He seized the animal with his hand and called others to see it, but it got off. The singing was resumed the same night and those following. M. Brierre is unable to attribute the singing of the mice (as Dr. Berdier does) to imitation of that of canaries, for he had no birds in the house, nor had the previous proprietor of the cupboard any.

IN describing some recent falls of meteoric stones in America, Mr. Lawrence Smith has pointed out (*Comptes Rendus*) that in the last eighteen years there have been, in the United States, twelve falls of meteorites which have been collected; and he notes the remarkable fact that eight of the meteorites, representing more than 1,000 kilogrammes of matter, have fallen in the region of the Western Prairies, and on a surface which does not exceed one-eighth of the extent of the United States. This cannot evidently be attributed to there being a dense population and numerous observers (a consideration sometimes urged). Still more striking is the circumstance that in the last sixty years there have been twenty falls of meteorites observed in the United States, of which ten, or the half, have fallen in this same region; and, moreover, these falls have brought about 1,200 kilogrammes of mineral substance, a quantity twenty times greater than that of the ten other falls recorded as having occurred outside of this region.

IT has long been known that the photographic image of a luminous object is dilated at the expense of the dark parts or the field itself. This has been merely attributed to a gradual advance of the chemical action (without further attempt at explanation). The phenomenon has recently been studied by M. Angot (*Journal de Physique*). He finds that the dimension of the images increases with the intensity of the light, with the duration of exposure, with the sensibility of the plate, with diminution of the aperture of the objective, and that it is greater when the plate has not previously been impressed by diffuse light than when it has. M. Angot discards the hypothesis of a mysterious advance of chemical actions, and shows how the phenomena are accounted for by the ordinary theories of optics. This variation of the diameter of images is inevitable in practice; to render it very small the operator should satisfy himself that the objective used is aplanetic, *i.e.*, free, as far as possible, from aberrations of sphericity and refrangibility. He has only then to take account of variations due to diffraction, which may be attenuated by using objectives of large aperture. It is by using an objective without sensible aberration and of fifteen inches aperture that Mr. Rutherford has succeeded in obtaining his magnificent photographs of the moon.

SOME interesting experiments on the photo-electricity of fluorspar have recently been described by M. Hankel to the Saxon Academy of Sciences. His attention was drawn to the phenomena in studying the thermo-electric properties of crystals. The new effects were found much more intense than those got by heating the crystal, or by friction of its surface with a brush; moreover, they were of contrary sign, and so must be attributed to an action proper to light. The principal results of experiment are these: The centre of a crystalline face presents, after exposure of about an hour to sunlight, a strong negative tension, while the tension towards the sides is much less, and even most frequently positive. An exposure of the crystal to the temperature of 95° for several hours produces, during cooling, the smallest positive tensions at all points of the crystal. Experiments made by filtering light through coloured glasses, a layer of water, a solution of alum or of sulphate of quinine, showed that the chemical rays are much the most active. Lastly, a too strong concentration of light on the crystalline face removes all sensibility to the ulterior action of light. To give an idea of the degree of tension observed in such experiments, M. Hankel states that a brass plate 95 mm. in diameter, connected with a zinc element, copper, and uninsulated water, gave a deflection (in an electrometer composed of a gold leaf hung from an insulated brass rod between two insulated plates of brass) of 1°·2 on approaching the centre point as nearly as possible, and about 0°·6 on approaching the edge. The deflections obtained by exposition to light reached 21°, and even 26° in the centre of surface of a crystal electrified by light.

AS supplemental to the article on the blue gum-tree, at p. 443 of NATURE, vol. xvi., the following notes from a report on the culture of *Eucalyptus* in Algeria by Consul-General Playfair will, no doubt, be interesting. With regard to rapidity of growth, it seems that the first trees ever planted in Algeria were sown in 1862, and upon being measured in 1874, that is at twelve years of age, gave a circumference at one metre from the ground of 1·52 metres, another of eleven years growth gave 1·42, and another, planted in fresh alluvial soil, nine years of age, gave 1·57 metres—about six inches more, Col. Playfair says, than he could embrace with both arms. To the question of the sanitary effects of *Eucalyptus* a good deal of attention has been directed. An inquiry was instituted by the Society of Physical and Natural Sciences at Algiers under the presidency of Dr. Bertherand, and the result was that from thirty localities reports were received, all of which speak favourably of the *Eucalyptus* as a fever preventive. On the banks of Lake Fetzara, near Bône, 60,000 young trees of *Eucalyptus globulus* were planted in 1869. At the present time they have attained a height of from 7 to 8 metres each, and have, it is said produced a very marked effect on the locality. Such was the feverish condition of this district on the annual fall of the water and the denudation of its banks that the director of the Jardin d'Essai, who went to examine the condition of the plants, was immediately seized with a violent fever which lasted twenty days. This gentleman, however, now reports that the miasmatic influences which affected him so strongly then have disappeared, and the mosquitoes which rendered the place uninhabitable have disappeared with them. At the great iron mines of Mokta et Hadid it was formerly impossible for the workmen to remain there during the summer; those who attempted to do so died, and the Company was obliged to take the labourers to the mines by train every morning, and to carry them back to Bône at night, a distance of 33 kilometres each way. From 1868 to 1870 the Company planted more than 100,000 *Eucalyptus* trees, and now the workmen are able to live all the year through on the scene of their labour. Consul Playfair advances the following reasons as accounting for the causes of the improvement in climate from planting *Eucalyptus* trees:—"In some places," he says," the trees destroyed miasma by utilising the moisture of the soil in which they were planted, and thus draining marshes; the emanations from their leaves also may have produced a salutary effect. They contain a large quantity of essential oil very similar to turpentine, which they emit in great quantities, especially when stirred by the wind, and this acts, it is supposed, as a febrifuge." We are further told that considerable numbers of *Eucalyptus* have been planted all along the railway from Algiers to Oran. Where this line passes through the Metidja the trees have grown most successfully, but in the Chelif they have proved almost an entire failure. This, however, may have been due to their receiving no attention whatever after being planted.

WE have received reprints of two papers by Dr. C. Le Neve Foster, one "On Some Tin-Lodes in the St. Agnes District," and the other "On a Deposit of Tin at Park of Mines."

WE have received an interesting little publication, by Mr. Edwin Lees, F.L.S., reprinted from the *Transactions* of the Malvern Naturalists' Field Club, on "The Forest and Chase of Malvern, its Ancient and Present State," with notices and illustrations of the most remarkable old trees remaining within its confines.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. Richard Turner; an Azara's Fox (*Canis azaræ*), two Brazilian Cariamas (*Cariama cristata*), a crested Screamer (*Chauna chavaria*) from the Argentine Republic, two Crab-eating Raccoons (*Procyon cancrivorus*) from South Brazil, a Yarrell's Curassow (*Crax carunculata*), a Sclater's Curassow (*Crax sclateri*), a Globulose Curassow (*Crax globulosa*), a Garden's Night Heron (*Nycticorax gardeni*) from Brazil, four Silky Cow Birds (*Molothrus bonariensis*), a Banded Cotinga (*Cotinga cincta*), a Yellow-footed Thrush (*Turdus flavipes*) from Bahia, deposited; an Ostrich (*Struthio camelus*) from Africa, received in exchange.

THE LIMITS OF NATURAL KNOWLEDGE[1]

II.

HAVING considered the capacity of the subject and the accessibility of the object, we must now turn our attention to the copula, *i.e.*, the demands which we make of knowledge.

As all conceptions which we form of nature are exclusively the results of sensual perception, our knowledge cannot go further than to compare the phenomena we have observed, and judge them with reference to one another. If any phenomenon of a special nature occurred only once, if, for instance, we were the only existing organisms our insight would be extremely limited, because all our knowledge of the human organism we have essentially obtained from its connection with all other organic beings. The comparison of many phenomena gives us a unit or a standard by which we can measure and determine each single one. We therefore obtain just as many measures as there are properties in nature which we can perceive by our senses or which can be inferred from sensual perceptions by our judgment. As these measures are deduced from finite facts they have only a relative value, and our knowledge remains finite for the same reason.

We therefore understand a phenomenon, we know its value with regard to other phenomena if we can *measure*, *count*, or *weigh* it. We have a clear idea of the size of the lowest fungus, of which we must place some 2,000,000 or 3,000,000 individuals side by side to complete the length of a metre, of the size of an elephant, of the earth, of our solar system, the radius of which is somewhere about 3,000,000,000 miles. We have a clear idea of the time in which a ray of light carries to our eye the writing of a book which we read, and which is about the $\frac{1}{8000000000}$th part of a second—of the life-time of the lowest fungus which in a plant-box or in the human body is replaced by a new generation after only twenty minutes—of the life-time of an oak which may be several thousand years, and of the 500,000,000 years which have passed since the generation of organisms upon our earth.

Natural bodies are composed of parts; the value of their internal structure, of their organisation, is exactly determined by the quantity, nature, and arrangement of these parts. They therefore give us the measure by which we judge the compound whole, and with which we measure its organisation as it were. The morphological or descriptive natural sciences by these measurements obtain their scientific data. Chemistry, which at the present time is still an eminently morphological science and which investigates the formation of compounds from elementary atoms, and mineralogy, which presupposes the uniform arrangement of molecules, have arrived at a state of great perfection. The common measure for organisms is the cell, and further on the organ; the common measure for the systematic unities of organic nature (for varieties, species, genera) we find in individuals and generations.

We are enabled not only to compare the different objects and measure them by one another, but in as far as it changes, we may also compare a system, a unit (*einheitliche*) group of things of similar nature, with itself and measure it by itself. The knowledge of the change is complete if the later stage is proved to be the necessary consequence of the earlier one, or the latter to be the necessary predecessor of the later one, if one can be constructed from the other, if therefore both stages can be brought into the relation of cause and effect to one another.

In the elementary domains of the material this causal relation is the mechanical necessity, which for two successive stages demands the equal sum of motion in a certain direction (or living force) and of potential energy. Among the sciences which apply here astronomy ranks first; next in efficiency are several physical sciences, particularly the mechanical theories of heat and optics. Physiology, or the physics of the organic world, tries to penetrate into a far more difficult and more complicated domain, by following the footmarks of her older sister.

In the higher domains of the material we cannot for our causal knowledge uphold the demand for this mechanical necessity. Indeed this is perhaps impossible in the case of all structure.

[1] Address delivered at the Munich meeting of the German Association, by Prof. C. von Nägeli, of Munich. (The author, in a note to the German original, remarks that this lecture had to replace another in the programme, which had been promised by Prof. Tschermak, of Vienna. At the eleventh hour Prof. Tschermak announced his inability to attend the Munich meeting, and the author was requested by the secretaries to fill the gap thus occasioned. The address therefore, the author states, bears the stamp of its hasty origin, as it was written during a journey in the Alps, when there was neither sufficient leisure nor opportunity for careful and elaborate work.) Continued from p. 535.

We shall probably never succeed in explaining definitely why the origin of a chemical compound and of a crystal must be the necessary result of known forces and motions of elementary atoms and molecules. This will be the case still less with the formation of cells, with the growth of organisms, with the propagation and inheritance of peculiarities. And yet we may, even in these domains, speak of causal knowledge with some show of right, only the elements which constitute this knowledge are not simple forces and motions, but very complicated combinations of these, which are not analysed further. Our causal knowledge will arrive at perfection when we succeed in predicting future events with the same certainty and exactness as astronomers do. Now we already find certain indications of this in the chemistry of compounds and in organic morphology, since it is possible to make deductions from certain stages of development of an organism with regard to earlier or later stages of the same. And a time will arrive when the organic laws of the still youthful history of development of the individual and of the still younger history of development of the species will have been more investigated, and when we need no longer presuppose ontogenetic and phylogenetic necessity as a matter of course, but when we will also be able to understand the cause of this necessity.

The objection will perhaps be raised that causal knowledge certainly consists of our understanding the necessity, as in the case of mechanics, but that this does not apply in domains where we must start from uninvestigated compound objects. The mechanics of the heavens is based upon general gravitation and centrifugal force, and both are simple forces acting in a straight line. But both are hypotheses, which rest upon our experience and of the reason of which we are ignorant. Astronomy reveals to us the necessity of astronomical phenomena only under the supposition of facts we have experienced—not the necessity in itself. If we were to demand that to our knowledge the "why?" should be clear, there would not even be any astronomical nor yet any physical knowledge. In the organic domains causal knowledge is entitled to the same importance as physical knowledge is in the inorganic field. By experience we know a system of forces and motions, for example, the cell. We ascertain certain general facts relating to this system (in the same way as with gravitation and centrifugal force in the heavens), and we use these facts for further deductions. Our insight into the necessity of some process of growth consists in our recognising this process as a necessary consequence of those facts.

Our knowledge of natural things therefore rests upon our being able to measure them, either by themselves or by one another. Another method of observation leads us to the same result. We understand and master something perfectly, if we create it ourselves, because in this case we see its cause. The only thing in the domain of knowledge, which, based upon our sensual perceptions, we can accomplish, is mathematics. The tenor of this formal science is perfectly clear to us, because, indeed, it is the product of our own mind. We can therefore also understand real things with certainty, as far as we find mathematical ideas, number, magnitude, and everything which mathematics deduces from these, realised in them. Natural knowledge therefore consists in our applying mathematical methods to natural phenomena; to understand a natural event means nothing else as it were, than to repeat it in thought, to reproduce it in our mind.

While designating natural knowledge as *mathematical* and at the same time as *relative*, which judges things according to a measure deduced from themselves, I depart considerably from the views of my predecessor, Prof. Du Bois Reymond. He considers it to be a condition of natural knowledge, that we should succeed in reducing the changes in the material world to motions of atoms caused by their central forces which are independent of time, or in other words, in resolving natural phenomena into the mechanics of atoms.

While Du Bois Reymond thus starts from the undeniable fact that a compound can only be known from its parts, yet he stops not at the finite and real parts, but continues the division down to the real *unities*, which are unthinkable, and thus he marks out the conditions for impossible *absolute* knowledge. But as we do not crave divine but only human knowledge, we may not ask more of the latter than that in each finite sphere it should advance as far as mathematical understanding; and the saying of Kant, that in each special natural science we can find only as much *real* science as we can find mathematics in it, is after all still quite correct.

If Du Bois Reymond wishes to continue the analysis of matter down to atoms with simple central forces, he carries a favourite method of modern physics and physiology to extremes, and if he shows that this way of proceeding does not lead to understanding, he destroys the claims of exclusive adherence to the domain of science, which the employers of this method sometimes raise. If physics and physical physiology go back to supposed atoms, material points, elements of volume which we imagine to be infinitely small, then this hypothesis is justified inasmuch as the real chemical molecules are so small that we may, without error of calculation, consider space to be continuously filled with matter. For instance, for a molecule of albumen, consisting of numerous atoms of carbon, hydrogen, oxygen, and nitrogen, we may substitute a mass differential of this compound. At all events it is useful to make this hypothesis, as it must be seen how far a conception of this kind can be treated mathematically, and as from the result we may draw conclusions backwards with regard to the composition of matter.

But we must beware of the opinion which is frequently associated with this method, that it alone is natural science and that knowledge can only be gained by employing it. In this case we would have to confine our desire to understand nature to a single domain, and we would lose others which are capable of safe confirmation. Natural knowledge need not necessarily begin with hypothetical and the smallest unknown things. It begins wherever matter has shaped itself to unities of the same order, which may be compared to and measured by one another, and wherever such unities combine to form compound unities of a higher order, and yield a measure for their comparison with one another. Natural knowledge may begin at every age from the organisation or composition of matter; at the atom of chemical elements, which forms the chemical compounds; at the molecule of the compounds, which composes the crystal; at the crystalline granule, which composes the cell and its parts; at the cell, which builds up the organism; at the organism or individual, which becomes the element of the formation of species. Each natural scientific discipline has its justification essentially in itself.

Our knowledge of nature is therefore always a mathematical one, and consists either in simple measurement, as in the morphological and descriptive natural sciences, or in causal measurement, as in the physical and physiological sciences. By means of mathematics, however, by weight, measure, or number, we can only understand relative or quantitative differences. Actual qualities, absolutely different properties, escape our understanding, since we possess no measure for them. We cannot conceive really qualitative differences, because qualities cannot be compared. This is an important fact for our attempts to understand nature. Its consequences are, that if within nature there are domains which are qualitively or absolutely different, scientific knowledge is only possible separately within each single one of them, and that no connecting bridge leads from one domain into another. But another consequence is that, as far as we can investigate nature continuously, as far as our measuring knowledge advances without gaps, and especially as far as we understand one phenomenon through another, or can prove it to have arisen from the other, that absolute differences, chasms which cannot be filled, do not exist at all in nature.

I have tried to determine the capacity of the Ego, the accessibility of nature, and the essence of human understanding. It is easy now to fix the limits of natural knowledge.

We can know only what our senses acquaint us with, and this is limited in time and space to an infinitesimal domain, and perhaps only to a part of the natural phenomena occurring in this domain, on account of a deficient development of our organs of sense. Of that with which we are acquainted at all, we can only know the finite, the changeable and perishable, only what is relative and differs by degrees, because we can only apply mathematical ideas to natural things, and can judge the latter only by the measures we have gained from themselves. Of all that is endless or eternal, of all that is stable or constant, of all absolute differences we have no conception. We have a perfect idea of an hour, a metre, a kilogramme, but we have no idea of time, space, matter and force, motion and rest, cause and effect.

The extent and limit of our possible natural knowledge we may shortly and exactly state thus:—*We can only know the finite, but we can know all the finite which comes within reach of our sensual perception.*

If we are clearly conscious of this limitation of our knowledge we free natural observation from many difficulties and errors, which consist, on the one hand, in the attempt to investigate not only the really finite, but a mixture of the finite and the eternal, which is uninvestigable; and, on the other hand, in our not following the finite strictly and incessantly, but stopping here and there in the midst of it and changing it for the eternal.

It would lead me far indeed, if I were to consider the consequences singly, which have arisen from the want of a correct method based upon principles. The most remarkable ones, which at the same time claim a general interest, are the opinions, that finite nature is divided into two radically different domains, and particularly that there is an insuperable limit between inorganic and organic, or between material and spiritual nature. I will speak only of the latter opinion.

The antagonists of an intimate connection between material and immaterial nature draw the line of separation in different places. In the opinion of some, living nature generally (or "life-endowed" (*beseelte*) nature, inasmuch as life is also ascribed to plants) represents something absolutely special, while others admit this only for the animal world endowed with sensation, and yet others only for the spiritually conscious human race; new immaterial or eternal principles are said to apply to the higher grades. Du Bois Reymond holds the second of these views; he says that in the first trace of pleasure which was felt by one of the simplest beings in the beginning of animal life upon our earth, an insuperable limit was marked, while upwards from this to the most elevated mental activity, and downwards from the vital force of the organic to the simple physical force he nowhere finds another limit.

It is difficult for the naturalist to oppose the supposition of immaterial principles, which are said to arise suddenly here and there in nature, as it places itself at once upon a stand-point which floats in the air outside of natural science, and cannot, therefore, be attacked directly and contradicted by him. Natural science can only show that this supposition is superfluous, because everything can be explained in a natural way, and also improbable, because otherwise a contradiction is introduced into finite nature which gainsays the whole of our experience, and offends our mental desire to find causal relations everywhere.

Experience shows that from the clearest consciousness of the thinker downwards, through the more imperfect consciousness of the child, to the unconsciousness of the embryo, and to the insensibility of the human ovum, or through the more imperfect consciousness of undeveloped human races and of higher animals to the unconsciousness of lower animals, and of sensitive plants, and to the insensibility of all other plants, there exists a continuous gradation without definable limit, and that the same gradation continues from the life of the animal ovum and the vegetable cell downwards through organised elementary and more or less lifeless forms (parts of the cell) to crystals and chemical molecules.

But the conclusion we draw by analogy is this:—Just as all organisms consist of and have been formed of matter, which occurs in inorganic nature, so the forces, which are inherent in matter, have of course entered into the formations as well. If matter combines with other matter, then their forces unite to some total result, and this represents the new property of the resulting body; this property is of course only relative. Thus vermilion is mercury + oxygen − heat; sugar is carbon + hydrogen + oxygen − heat. And thus life and feeling are new relative properties which albumen molecules obtain under certain circumstances. Accordingly, experience shows that spiritual life is everywhere connected in the most intimate manner with natural life, that the one influences the other and cannot exist without the other. It is necessary, therefore, as everywhere in nature forces and motions are united only with material particles, that the spiritual forces and motions also appertain to matter, in other words, that they are composed of the general forces and motions of nature and are connected with them as cause and effect. No naturalist can avoid the conception of a causal connection of this nature, unless he becomes unfaithful, consciously or unconsciously, to his first principle. The problem is, therefore, to understand how the forces of inorganic matter combine in matter which forms into organisms, so that their result represents life, sensation, and consciousness. The solution of this problem is yet very remote; but it is possible. We may give sufficient indications for each single point.

Permit me to speak more minutely of one of these points; I mean the one in which my predecessor sees a limit to natural knowledge. This is all the more tempting since for the rest Du Bois Reymond places himself upon the basis of the causal principle, if indeed not in words quite so direct, yet quite as determined and unconditional; and since if this *one* gap were filled, no other would exist for his point of view. To him the whole world-history, even the whole system of the universe, is the consequence of the mechanics of atoms. There is no action of the mind, which could not be calculated from the forces and the motions of matter, if it were possible to know these. The material occurrences which are connected with the solution of an arithmetical problem, with the pleasure of musical sensation, with the intellectual pleasure over a scientific discovery, are products of cerebral mechanics. The mind can indeed be looked upon as the secretion of the substance of the brain, in the same way as gall is the secretion of the liver, as Karl Vogt, and previously Cabanis, have said.

Du Bois Reymond declares all this to be intelligible in principle; but, he says, we learn to know only the conditions of mental life, but not how from these conditions mental life results. Sensation and consciousness doubtless accompany the material processes in the brain by necessity, but they stand outside of the causal law and remain eternal enigmas to us.

It is not uninteresting to follow Du Bois Reymond's view, which I have just stated and which he details and illustrates with various examples, into its consequences, and to consider clearly its general result. We then arrive at this:—The finite mind, as it has developed itself through the animal world up to man, is a double one; on the one side the acting, inventing, unconscious, *material* mind, which puts the muscles into motion and determines the world's history; this is nothing else but the mechanics of atoms, and is subject to the causal law; and on the other side the inactive, contemplative, remembering, fancying, conscious, *immaterial* mind, which feels pleasure and pain, love and hate; this one lies outside of the mechanics of matter and cares nothing for cause and effect.

Generally both sides of mental life are collectively called mind. Du Bois Reymond exclusively designates the latter as mind, and if the separation existed in the way described this would certainly be the truly unintelligible secretion of the material mind, or of the atoms of the brain; it would not be anything but the useless ornament of this material mind, its infallibly following, unreal shadow. Because it stands outside of the chain of cause and effect, it is powerless and without influence upon actions; without it the world's history would have run exactly the same course as it did. Also without consciousness mathematical formulas would have been invented, written down, taught, and applied, telegraphs and steam engines would have been constructed; also without consciousness theological and philosophical discussions would have been held, printed, read, and their authors burnt at the stake; also without conscious memory lessons would have been learnt by heart in the schools and examinations held; also without musical sentiment music would have been composed, repeated at rehearsals, performed and listened to with all external signs of pleasure or disapprobation; also without poetical or artistic sentiment poets, painters, and sculptors would have produced their works, and these would have been admired and criticised. Therefore *without* a conscious and perceived mental life, we should have thought, done, and spoken everything, but only mechanically, and not otherwise than a very artistically-invented dead automaton would think, act, and speak.

We cannot deny the sublimity of this conception of the universe; the impression it makes upon the naturalist must be all the greater, because it proceeds consequentially everywhere and does not offend any natural scientific principle; as to the immaterial and the unintelligible it assigns a domain, which lies outside of the connection of natural and real things. For this reason also this conception cannot be discussed from a natural scientific point of view. And yet to the naturalist certain objections present themselves.

Can we imagine that so many occurrences, which most evidently resulted from sensation and consciousness, have some other sensationless and unconscious origin? Can we imagine that sensation and consciousness are so entirely useless, and while everywhere utility (*zweckmässigkeit*) is so eminently prominent in organic nature, that so useless and superfluous a phenomenon should occur just where we expect the greatest utility? Can we imagine that the causal principle, which governs the whole of nature, fails us just at the most important part? Can we imagine that organised matter accidentally and without cause acquires a property (sensation and consciousness), and loses it again accidentally and without effect, because in the ovum and in the embryo the conscious and perceived mental life would not be present, it would arise gradually, it would be lost in sleep every night, obtained again more or less completely in the waking state, and annihilated for ever in death?

The conscience of the naturalist is little satisfied by this new dualism, although he cannot directly contradict it. It is true that this dualism is infinitely different from the ordinary dualism, since it assigns the exclusive power to the forces of nature, and to the mind only an inactive, empty dignity, and thus hinders in no way the strictly causal or materialistic conception of all material occurrences, also of those which bring about mental life. But nevertheless we would wish for a solution which corresponds more to our experiences and to our theoretical conceptions. And I believe that this solution lies very near if we extend our judgment of the phenomena in organic nature to those of inorganic nature as well.

It is quite correct in Du Bois Reymond to say that we can only know the material conditions of mental life, but that how this results from those conditions remains a secret to us for ever. But it would be an error to suppose that we generally understand the origin of natural life from its causes. In all purely material phenomena we find the same barrier as in the mental ones. We know by experience that in the inorganic world the cause is lost in the effect, but we cannot understand the nature of the transfer. We know by experience that a stone thrown up into the air falls to the ground, and we say that this happens because the earth attracts it; but this attraction is for us incomprehensible.

What we do know is, that two bodies which are apart act upon one another in such a way that, if there is no obstacle, they approach one another until they touch. In what, however, this action consists, how it adduces the mutual motion, is for us just as unintelligible, and will remain just as eternal an enigma, as the origin of sensation and consciousness from material causes. With all material, physical, and chemical phenomena, we find the same. A body charged with positive electricity, and another one charged with negative electricity, move towards each other; two bodies similarly electrified repel one another. If we say that in the former case attraction, and in the second repulsion takes place, then these are only short expressions which comprise whole series of similar phenomena, but give no explanations. But we accustom ourselves to such expressions; little by little we use them so frequently and easily, that we believe we really understand the phenomena they designate. And that is why the view is generally held, that nature in her simpler inorganic phenomena offers no difficulties to our conception, whereas in reality the difficulties are everywhere the same in principle.

The objection will perhaps be raised, that the two sides of the question are not quite so equal as I say; that with purely material phenomena the relation between two material particles, which causes their motion, is indeed incomprehensible; that with mental phenomena this incomprehensible relation between the material particles is also given; but that something else, something new is added, namely, the mental action which accompanies the material phenomenon. But this objection, if indeed we raise it, would be unfounded; we should have overlooked that the two sides into which we should divide the mental phenomenon are equally present with the purely material phenomenon, only that they are not separately conceived here but in one, namely, the sensation and the reaction which this sensation causes.

This fact, that the simplest inorganic phenomena are quite as inaccessible in their origin as the most complicated occurrences in the human brain, constructs the bridge which may lead us to a monistic (*einheitliche*) conception of nature. Let us start from what we know—and in this case it is the complicated mental phenomenon—in order to obtain from it a conception of what we still ignoran of.

We know mental life only from our subjective experiences; we know that we draw conclusions, that we remember, that we feel pleasure and pain. That similar but undeveloped phenomena occur with children and higher animals, we conclude from their actions and from their somatic manifestations, which we interpret as the expression of emotion and sensation. Actual proofs that even the lower animals still possess sensation, which is only different in degree from the conscious sensation of man, we have only in their movements consequent upon some irritation, and in the important circumstance that these movements upon irritation in the ascending animal classes pass through all gradations upwards to the most complicated phenomena in the human brain. From these irritation-movements of the lowest animals we imperceptibly get to those of the unicellular plants and of the sensitive plants, and thence to the phenomena of the apparently insensible plants, which cannot be distinguished from the phenomena of inorganic nature. Between the irritation-movements of plants and animals, however, and the apparently insensible inorganic movements, there is no other difference but this, that in the case of irritation a powerful cause acts upon numberless material particles arranged in a similar manner, and thus produces a movement of place or sensation which becomes perceptible to our senses, while when this perceptible movement is wanting, the cause of the molecular movements, which take place in several directions, is not called an irritation.

In the higher animal world sensation is distinctly present in the movements consequent upon irritation. We must therefore credit the lower animals with it as well, and we have no reason to deny it in the case of plants and inorganic bodies. Sensation causes us to feel pleasure or displeasure. Generally speaking the feeling of pleasure arises when our natural inclinations are gratified, and the feeling of pain when this gratification is denied. As all material phenomena are composed of the motions of molecules and elementary atoms, pleasure and pain must have their original seat in these particles; they must be caused by the manner in which these infinitesimal particles are able to respond to the attracting or repelling forces which act upon them. Sensation, therefore, is a property of the albumen molecules; and if we grant it in the case of albumen molecules we must grant it likewise in the case of the molecules of all other substances.

Let us now consider the relation of two molecules of different chemical elements (for instance that of a hydrogen molecule and one of oxygen), which are at a minute distance from each other. Each of them, according to the present notions of chemistry, consists of two not further divisible, but yet decidedly compound atoms. By means of its composition the atom has different properties and forces, and therefore acts differently (attracting or repelling) upon other atoms. The two molecules in question experience or feel their mutual presence in a different manner; they act upon each other with different attractive or repulsive power.

Let us examine what happens in the case of a certain attraction, for instance, in that of a chemical one Three possibilities exist: either the molecules follow their inclination and approach one another, or they are condemned to rest through other forces which are equal to the attraction, or they move away from one another, the forces counteracting their inclination gaining the upper hand. The same three possibilities are given for a certain repulsion, for instance, through heat; the two molecules follow their natural inclination and move away from each other, or they remain at the same distance, or they are pushed towards one another by other causes, their inclination being overcome.

Now if the molecules possess anything which is ever so distantly related to sensation, and we cannot doubt it, since each one feels the presence, the certain condition, the peculiar forces of the other, and, accordingly, has the inclination to move and, under circumstances, really begins to move, becomes alive as it were, moreover, since such molecules are the elements which cause pleasure and pain; if therefore the molecules feel something which is related to sensation, then this must be pleasure if they can respond to attraction and repulsion, *i.e.*, follow their inclination or disinclination; it must be displeasure if they are forced to execute some opposite movement, and it must be neither pleasure nor displeasure if they remain at rest.

As the molecules act upon each other with several unequal attractive and repulsive forces, some of their inclinations, whenever they are in motion, are always gratified, while others are offended. But these different sensations are necessarily unequal with regard to condition and intensity, according as they are caused by the general attraction of gravitation, by the general repulsion of heat and of elasticity, by electric and magnetic attraction and repulsion, or by chemical affinity. The simplest organisms which we know, if I may use this expression, the molecules of chemical elements, are therefore simultaneously influenced by several qualitatively and quantitatively different sensations, which conglomerate to a total sensation of pleasure or pain.

At the lowest and simplest stage of material organisation which we know, we therefore find on the whole the same phenomenon as we do at the highest stage, where it appears as conscious sensation. The difference is only one of gradation; at the highest stage the influences have only become so much more vivid in consequence of a vast accumulation of different material particles, and much more compound and intricate on account of the complicated organisation.

If we look upon mental life in its most general signification as the immaterial expression of the material phenomena, as the mediation between cause and effect, then we find it everywhere in nature. Mental force is the capacity of material particles to act upon each other. The mental phenomenon is the performance of this action, which consists in motion, therefore in a change of position, of the material particles and the forces inherent in them, and by this leads directly to a new mental occurrence. Thus the same mental chain encircles all material phenomena.

The human mind is nothing else but the highest development upon our earth of the mental phenomena which move and animate nature everywhere. But it is not the product of secretion of the cerebral substance; as such it would be without further influence upon the brain, just as the secreted gall is of no further signification for the liver. On the contrary, sensation and consciousness have their firm seat in the brain, with which they are indissolubly united, and in which, by their intervention, new conceptions are formed and converted into actions. Just as the stone would not fall down if it did not feel the presence of the earth, so the trampled worm would not wriggle if it had no sensation, and the brain would not act reasonably if it had no consciousness.

This conception satisfies our causal demands entirely. For the naturalist it is a logical necessity to admit only differences of degree in finite nature. In the same way as there is a common measure for everything in space as well as for everything in time, so there must be a common measure for all mental phenomena. In the same way as there are gradations in material nature from the most simple to the most complex, so there must be similar gradations in mental nature, which is parallel to the former. It is true that in atoms and molecules we do not yet find pleasure and pain or love and hate pronounced with decision, but yet we find the first germs, as it were the original beginnings, of these feelings, and it would be the task of a comparative psychology to follow consciousness through unconscious sensation down to the insensible action of material particles.

But the domain of the mind offers far greater difficulties to our investigation than the material domain, because we can only use our subjective perceptions as immediate experience, and because we do not possess a special organ of sense which enables us to make objective observations of other bodies. The observation with our senses, which are organised for quite different objects, acquaints us only in a roundabout way and in a very defective manner with the mental occurrences in other beings, and our judgment of them is all the more uncertain the further we depart in nature from the human species itself. It will therefore, perhaps, never be possible to find the measure for the mental phenomena really, to determine it, and to raise comparative psychology to the rank of a natural science.

Natural knowledge remains limited to what is finite, the naturalist must therefore confine himself strictly to the finite only. The demand, which is often addressed to him, that he should have a more philosophical mind, that he should criticise in a philosophical manner, because it is impossible to avoid metaphysical speculation entirely, only shows how difficult it is to separate two absolutely different domains, which have once been mixed up only to produce general confusion. The power of education and habit also was, up to the most recent period, an obstacle in the way of a complete and radical separation of these two domains, and yet it is certain, and we know by experience, that every metaphysical addition turns natural science and natural investigation into a turbid and muddy alloy.

Natural science must be exact; it must rigidly avoid everything which oversteps the limit of the finite and the intelligible, and which is transcendental; it must proceed in a strictly materialistic manner, because its sole object is finite, force-endowed matter; and it must not forget that this true materialism is an empirical and not a philosophical one, and that it is bounded by the same limits as those of the domain upon which it moves.

I do not wish to say by this that the naturalist is not allowed to philosophise, that he is forbidden to move in ideal and transcendental domains. But he ceases to be a naturalist, and the only thing, which from his vocation is perhaps of advantage to him, is that he keeps both domains strictly apart; that he knows how to treat the one as the pure domain of investigation and knowledge, and the other, while he frees it from everything that is finite, as the hidden domain of presentiment.

To the human mind, to our desire of investigation and knowledge, the whole sensually-perceptible world is open. We penetrate into the greatest distances by means of the telescope and calculation, and into the smallest spaces by means of the microscope and combination. We investigate the most complex and complicated organism, which belongs to ourselves, in the most varied directions. We recognise the forces and laws governing nature, and through this we subject the whole inorganic and organic world, as far as we can reach it. If man reviews the triumphs in the domains of science and power which have been obtained up to the present, and thinks of the still greater future conquests, then he may with pride feel himself lord of the world.

But what is this world, over which the human mind reigns? Not even a grain of sand in the eternity of space, not even a second in the eternity of time, and only an outwork of the true essence of the universe. Because even of the infinitesimal world, which is accessible to us, we only know what is changeable and perishable. All that is eternal and stable, the *how* and the *why* of the universe, remains for ever incomprehensible to the human mind, and if it tries to overstep the limit of finiteness it can only puff itself up to a ridiculously-adorned idol, or desecrate the eternal and the divine by human disfiguration. Even the matured mind, which would have arrived at complete natural scientific insight, and would wish to free the divine of everything finite and perishable, could, in its restriction, make of divinity only a constitutional phantom-king, who, according to the words of a statesman recently deceased, would "reign, but not govern.' In the finite world the eternal natural forces rule unalterably, and we recognise their effects in the laws of motion and change. Whether and how they are the tenor and expression of a conscious eternal design is past our comprehension.

If my predecessor, Prof. Du Bois Reymond, ended his address with the crushing words, *Ignoramus, et ignorabimus*, then I close mine with the conditional but more consolatory utterance that we do not merely know, but really understand the fruits of our investigations, and that our knowledge bears in itself the germ of an almost infinite growth, without, however, approaching omniscience by the smallest step. If we practise reasonable resignation, if, as finite and perishable human beings, as we are, we are satisfied with human insight, instead of claiming divine knowledge, then we may say with full confidence—

"We know, and we shall know!"

ON THE SOLAR ECLIPSE OF AGATHOCLES

B.C. 310 (15th August).[1]

THE mean motion of the moon round the earth was formerly assumed to be constant, until Halley showed that it has been gradually increasing by a small amount during the last few thousand years. Halley made this discovery by the study of ancient solar eclipses, which were found always to occur to the *eastward* of their calculated places:—this indicates a slower mean motion of the moon in former times, as may be thus shown—a spectator in the northern hemisphere looking at a solar eclipse will face the south, having the west on his right hand, and the east on his left hand; and he will see the moon cross the sun's disc from right to left. When we calculate backwards to an old eclipse (attributing to the moon her present mean motion), we are, in fact, unwinding, from left to right, the path she has described since the eclipse happened, and by this unwinding process we find that we always place the moon to the *right* (*west*) of the place where she was actually when the eclipse occurred. Thus, all the ancient eclipses being observed at places to the *eastward* (*left*) of their calculated places of observation, we learn that the moon's mean motion was formerly slower than it now is. The coefficient of the moon's mean motion, found by Halley, from ancient eclipses, was

$$10{\cdot}2 \times n^2,$$

where n is the number of centuries.

The acceleration of the moon's mean motion was first explained by Laplace, who showed that the mean central disturbing force of the sun, by which the moon's gravity towards the earth is diminished, depends not only on the sun's mean distance, but on the eccentricity of the earth's orbit. This eccentricity has been diminishing for many ages, while the mean distance remains unaltered. In consequence of this, the sun's mean disturbing force is diminishing, and, consequently, the attraction of the moon towards the earth has been increasing, and with it, of course, the mean motion of the moon has been also increasing. The calculations of Laplace, confirmed and extended by Damoiseau

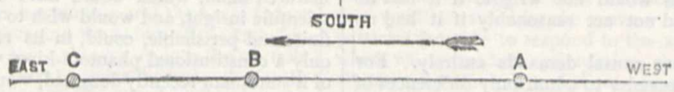
[1] Paper read before the Mathematical Section of the British Association, Plymouth, 1877, by Rev. Dr. Samuel Haughton, F.R.S. (Trin. Coll. Dublin.)

and Plana, gave a coefficient for the moon's mean motion agreeing with that found from observation by Halley.

This satisfactory agreement between theory and observation remained unchanged until 1853, when Adams announced[1] that he had found a deficiency in Laplace's calculation, arising from the fact that Laplace had considered the radial disturbing force only, and had neglected the tangential disturbing force.

When the fuller computation is made, it is found that the coefficient of Halley's expression is reduced from 10·2 to 6·11, leaving 4·09 not accounted for.

Adams' calculations were verified by Delaunay, who found them quite correct, and who had the merit of suggesting the explanation of the 4·09, which form a *residual phenomenon*. According to Delaunay, this uncompensated portion of Halley's coefficient is to be explained by the retardation of the earth's angular velocity, and consequent increase in the length of the day, caused by the residual tidal current setting constantly from east to west. This residual current, although excessively small, is a *vera causa* always acting, and must, in due course of time, produce a sensible effect in lengthening the day. It is easy to show that the effect of the lengthening of the day upon ancient solar eclipses acts in the same direction as the acceleration of the moon's mean motion, viz., it throws the place of observation to the eastward (left) of the calculated place; for the earth moves from right to left, in the same direction as the moon, and as its rotation in that direction, from the period of the eclipse, has been *greater* than that assumed in our calculation from the *present* rotation of the earth, it follows that, at the time of the eclipse, all places on the earth's surface must have been absolutely, with reference to a meridian fixed in space, to the westward (right) of their present positions. According to this view, therefore, the displacement eastwards of the places of observation of ancient eclipses, when compared with the calculated places of observation, is the sum of two displacements—one caused by not allowing for the *acceleration of the moon*, and the other caused by not allowing for the *retardation of the earth*.

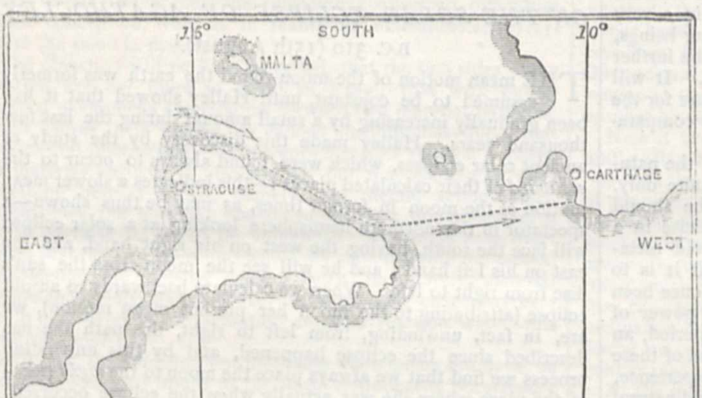


Thus, if B represent the true position of the eclipse in space, its calculated place will be A, to the west of B, the interval AB being due to the neglect of the acceleration of the moon's mean motion (with coefficient = 6·11) in the calculation; and the point exactly below B, on the earth's surface, will have moved on to C, to the east of B, in consequence of the neglect of the retardation of the earth's rotation in the calculation.

Let us illustrate the case by one of the most famous solar eclipses on record, that of Agathocles, on August 15, 310 B.C. The accompanying outline map represents the course taken by the expedition of Agathocles from Syracuse to Carthage.[2]

This eclipse is recorded by Diodorus Siculus, and has been always considered one of the most important in support of Halley's coefficient, 10·2 seconds.

It has recently, however, been called in question by a high authority; for at the meeting of the American Association for the Advancement of Science (1877), "Prof. Simon Newcomb presented a communication on the secular acceleration of the moon, and its increasing deviation from uniformity through many years. He reviewed the existing theory on the subject; the calculation of Laplace according with Halley's estimate of the acceleration as about 10½ seconds of time, to be multiplied by the square of the centuries for a given period; also the Adams theory, which reduces the explanation of Laplace to 6 seconds, leaving more than 4 seconds to be otherwise accounted for. In ascribing the surplus acceleration to diminished rotation of the earth, we are dealing with a subject where the evidence should be carefully weighed. Much dependence seemed to be placed on the record of ancient eclipses. Prof. Newcomb considered these eclipses separately. *The most promising of the Greek solar eclipses was that of Agathocles, tyrant of Syracuse, occurring at the commencement of his voyage to attack Carthage. But we do not know on which side of Sicily he sailed: according to whether he was on one or the other side of the coast, the difference of time for that eclipse may be calculated as justifying the 10 seconds or the 6 seconds acceleration of the moon.* The eclipse known as that of Thales has a record still more open to criticism, because it came to its historian by hearsay, and probably through two or three generations after the lapse of a hundred years. It seems curious that if Thales predicted the year (by an estimate of lunar periods) he did not also predict the day. Each of the ancient solar eclipses yielded similar elements of doubt, on careful examination. From the records of lunar eclipses, if all uncertain features be weeded out, the old estimate of acceleration will be reduced one-half. The Arabian records of lunar eclipses were published at Leyden in the early part of this century. The work is very rare. Altitudes of sun and moon are constantly given in it. Calculations from these eclipses give the smaller estimate of acceleration. From all the data he has been able to study, Prof. Newcomb concludes that the whole amount of acceleration is about 8·4 seconds. He hopes to make further estimates from modern records, having had the good fortune to pick up in Paris carefully compiled data of occultations going back to 1680."



Let us compare this statement of Prof. Newcomb with the original account of Diodorus Siculus. Agathocles was blockaded in Syracuse by the Carthaginian fleet, and the town was in danger of starvation; under these circumstances he formed and carried out the daring project of breaking the blockade, and undertaking an expedition by sea against Carthage itself; which he successfully accomplished. Diodorus says: "But Agathocles, thus overtaken and surrounded, hit upon an unexpected chance of escape when night came on; and on the following day there came to pass so great an eclipse of the sun that night appeared universally, the stars being seen in every direction; wherefore the people of Agathocles, believing that the Divinity foreshadowed some evil to happen them, were in still greater anxiety of mind than before. When they had voyaged for six days and as many nights, at the dawn of day the fleet of the Carthaginians appeared unexpectedly, not far off. . . . But when Africa came

[1] *Proceedings* of the Royal Society, vol. vi. p. 321.

[2] The places passed in order by the expedition of Agathocles along the Sicilian coast are described in the fine lines of Virgil:—

Sicanio prætenta sinu jacet insula contra
Plemmyrium undosum: nomen dixere priores
Ortygiam. Alpheum fama est huc Elidis amnem
Occultas egisse vias subter mare: qui nunc
Ore, Arethusa, tuo Siculis confunditur undis.
Jussi numina magna loci veneramur: et inde
Exsupero præpingue solum stagnantis Helori,
Hinc altas cautes projectaque saxa Pachyni
Radimus, et fatis nunquam concessa moveri
Adparet Camarina procul campique Geloi,
Inmanisque Gela fluvii cognomine dicta,
Arduus inde Acragas ostentat maxima longe
Mœnia magnanimum quondam generator equorum,
Teque datis linquo ventis, palmosa Selinus;
Et vada dura lego saxis Lilybeia cæcis,
Hinc Drepani me portus et inlætabilis ora
Adcipit.—ÆN., Lib. III., 692–708.

in sight, an incredible exhortation to the rowers and rivalry took place. The ships of the barbarians indeed went faster, because for a length of time they had been accustomed to the handling of the oars; but the ships of the Greeks preceded them by a small interval; and, having finished their voyage as quickly as possible, they immediately sprang upon the strand like wrestlers; and, indeed, the leading ships of the Carthaginians attacked the aftermost ships of Agathocles, having come within range of missiles."—DIOD. SIC., lib. xx., ch. 5, 6.

'Ο δ' 'Αγαθοκλῆς περικατάληπτος ἤδη γενόμενος, ἐπιλαβούσης τῆς νυκτὸς, ἀνελπίστου σωτηρίας ἔτυχε. Τῇ δ'ὑστεραίᾳ τηλικαύτην ἔκλειψιν ἡλίου συνέβη γενέσθαι, ὥστε ὁλοχερῶς φαινῆναι νύκτα, θεωρωμένων τῶν ἀστέρων πανταχοῦ. . . . 'Εξ δ'ἡμέρας καὶ τὰς ἴσας νύκτας ἀυτῶν πλευσάντων, ἀποφαινούσης τῆς ἕω ἕω, παραδόξως ὁ στόλος τῶν Καρχηδονίων οὐκ ἄποθεν ὢν ἑωράθη.—DIOD. SIC., Lib. xx., ch. 5, 6.

From this narrative it can be clearly shown that Prof. Newcomb is mistaken when he says that "*we do not know on which side of Sicily he sailed.*" It is quite certain that the eclipse occurred before the expedition had weathered the promontory of Pachynus, or had made any sensible westing in their voyage.

The total distance, on a coasting voyage from Syracuse to Carthage, is 350 English miles, and the distance from Syracuse to Cape Pachynus is forty miles. Now, the whole time of the voyage was six days and as many nights, together with a portion of a night at Syracuse, and a portion of a day near Carthage (the stone quarries). Allowing six hours each to these, we have:—

	Hours.
Part of night of outset at Syracuse	6
Six days and as many nights	144
Part of day near Carthage before landing at the "Stone Quarries"	6
	156

This is the minimum time allowable from the narrative, and any longer time allowed will strengthen my argument. The rate of rowing during the voyage was, therefore,

$$\frac{350}{156} = 2{\cdot}25 \text{ miles per hour.}$$

At this rate of rowing it would require 17h. 48m. to reach Cape Pachynus, a distance of forty miles; so that if the expedition sailed at midnight, it would have been off Pachynus, to the eastward, at 5 P.M., which is the time assigned by Petavius for the middle of the eclipse (Syracusan time). It is, therefore, perfectly clear that if the expedition had got so far to the westward as to allow of the coefficient (6·11), the eclipse must be thrown into the wrong day, which is inadmissible.

If Delaunay is to be trusted, the expedition must have gone out of the Mediterranean into the Atlantic before the coefficient 6·11 could be verified. He says:—"Nous avons dit que la durée du jour augmentait d'une seconde dans l'espace de 100,000 ans. Mais cela se produit progressivement, de telle manière que ces augmentations successives des jours s'ajoutent, et au bout d'un grand nombre de jours, font un total appréciable. Si on remonte à une époque, de 2400 ans époque à peu près, à laquelle on rapporte les éclipses historiques dont on a parlé, on voit que l'observation de l'une de ces éclipses a dû être faite $1^{h}\frac{3}{4}$ plus tôt que si le ralentissement du mouvement de rotation de la terre n'avait pas existé.

"La variation relative aux anciennes éclipses va donc jusqu'à $1^{h}\frac{3}{4}$. Ainsi une éclipse a été observée à un certain moment $1^{h}\frac{3}{4}$ plus tôt qu'elle ne l'aurait été sans le ralentissement.

"Prenons les trois éclipses principales rapportées par l'histoire. Celle de Thalès, arrivée 585 ans avant J.—C., a été vue en Asie Mineure; sans le ralentissement du mouvement de rotation de la terre, on l'aurait vue dans l'ile de Sardaigne.

"Celle de Darisse (557 ans avant J.—C.) a été observée en Perse; on l'aurait vue dans la régence de Tripoli, sans le ralentissement.

"Enfin, celle d'Agathocle (310 ans avant J.—C.), signalée près de Syracuse, aurait dû se montrer près de Cadix."[1]

[1] "Cadix," pp. 18, 19.

STRIDULATING ORGANS IN SCORPIONS

AT the September meeting of the London Entomological Society, Mr. J. Wood-Mason announced the discovery of stridulating organs in scorpions. While recently working at the anatomy of a species allied to *S. afer*, he had met with structures which, from his familiarity with the analogous ones in other arthropods, crustaceans as well as insects, he had at once without hesitation determined to be sound-producing apparatus—even before he had found that sounds could be produced by them artificially by rubbing the parts together or accidentally in the mere handling of alcoholic specimens. He had, however, been enabled to place the matter beyond all doubt, for while at Bombay waiting for the steamer, he had obtained, by a happy chance, from some Hindustani conjurors, two large living scorpions belonging to another species of the same type; these, when fixed face to face on a light metal table and goaded into fury, at once commenced to beat the air with their palps and simultaneously to emit sounds which were most distinctly audible not only to himself, but also to the bystanders, above the clatter made by the animals in their efforts to get free, and which resembled the noise produced by continuously scraping a piece of silk woven fabric, or, better still, a stiff tooth-brush with one's finger-nails. The species—a gigantic one from the Upper Godaveri district—in which he had first observed stridulating organs, had these organs more highly developed than in the one experimented upon at Bombay, and must stridulate far more loudly, for by artificially rubbing the parts together in a dead alcoholic specimen he could produce a sound almost as loud as, and very closely similar to, that made by briskly and continuously drawing the tip of the index-finger backwards and forwards in a direction transverse to its coarse ridges, over the ends of the teeth of a very fine-toothed comb. The apparatus, which, as in the *Mygale*, is developed on each side of the body, was situated—the *scraper* upon the flat outer face of the basal joint of the palp-fingers; the *rasp* on the equally flat and produced inner face of the corresponding joint of the first pair of legs. On separating these appendages from one another, a slightly raised and well-defined large oval area of lighter coloration than the surrounding chitine was to be seen at the very base of the basal joint of each; these areæ constituted respectively the *scraper* and the *rasp*; the former was tolerably thickly but regularly beset with stout, conical, sharp spinules curved like a tiger's canine, only more towards the points, some of which terminate in a long limp hair; the latter crowdedly studded with minute tubercles shaped like the tops of mushrooms. He had met with no stridulating organs in this position in any scorpions besides *S. afer* and its allies; but in searching for them in other groups he had come to the conclusion that the very peculiar armature of the trenchant edges of the palp-fingers in all the *Androctonoidæ*, and in some at any rate of the *Pandinoidæ* (no *Telegonoidæ* or *Vejovoidæ* had yet been examined), was nothing but a modification for the same purpose, for the movable finger of this pair of appendages when in the closest relation of apposition to its immovable fellow could most easily be made to grate upon it from side to side so as to produce a most distinct crepitating sound; but when separated from it ever so little appeared to be incapable of the slightest lateral movement. It was his intention on his return to India to endeavour to determine this question, as well as many others relative to the species in which the presence of sound-producing apparatus had now been demonstrated by careful observation and experiment upon living animals.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Mr. Thomas Whittaker, from the Royal College of Science, Dublin, has been elected to a Natural Science Scholarship at Exeter College.

At Jesus College the following elections to Welsh scholarships have been made:—In mathematics, Mr. David Davies, from the College, Llandovery; in science, Mr. William Williams, from Dolgelly Grammar School.

The Commissioners commenced their sittings at the Clarendon Hotel on Monday. The proceedings of the Commissioners were of a formal character, but Tuesday, it was understood, they would proceed to take evidence.

CAMBRIDGE.—The master and seniors of Gonville and Caius College will proceed in December to elect a prælector in chemistry, in succession to the late Mr. Richard Apjohn. The duties of the prælector will be to take charge of the college laboratory, to prosecute original research, to instruct in chemistry the members of the college, and such members of the university as the master and seniors may from time to time direct.

LONDON.—The London Ladies' Educational Association opened its tenth session at University College for day lectures on Monday. Most of the evening lectures—intended chiefly for governesses and candidates for public examinations open to women—began a fortnight ago. In the past session of 1876–77 there was a decrease in the number of students as compared with the preceding session, in which the number had been greatly in excess of any previous year. There was, however, but a very slight diminution last year in the total amount of fees received, which rose considerably above the former level, the students, as a rule, showing a desire to avail themselves of a regular course of study by attending a larger number of classes. Moreover, the number presenting themselves for examination showed a very marked increase. The range of subjects offered to ladies in the coming session is fully as wide as in any preceding one, and comprises the language and literature of England, France, Germany, and Italy; Greek and mathematics, in elementary and advanced classes; physiology and hygiene; physics; English history, in two classes, intended as a preparation for the Cambridge higher local examinations for women; English Constitutional History; and history of Grecian literature and art; to which will be added, next term, an additional course of Modern History, and a course of lectures on Architecture. Besides these classes, which are for ladies only, ladies are admitted as regular students to the following classes in the college:—Anglo-Saxon, Higher Senior Mathematics, Philosophy of Mind and Logic, Political Economy, Jurisprudence, and Roman Law. Ladies are also permitted to receive practical instruction in the physical laboratory of the college, whilst the Fine Art Department has from its commencement always been open to them.

MANCHESTER.—The Dalton Chemical Scholarship has been awarded to Mr. C. F. Cross. Mr. Cross presented an original investigation upon "Normal primary heptyl alcohol, and its derivatives."

The Dalton Mathematical Scholarship has been awarded to Mr. E. T. Littlewood.

Prof. Boyd Dawkins, M.A., F.R.S., began on the 22nd inst. a course of six museum lectures on "Man's place in the tertiary period." These lectures are open to the public at a small nominal fee.

The session of the evening classes was opened on the 15th inst. by an address on "The Great Masters since Handel and Bach, with especial reference to the form of their compositions." The address was delivered by Mr. Hecht, Lecturer on Music, and was illustrated by selections from the masters' works, performed on the pianoforte.

LEEDS.—On Tuesday the foundation stone of the new Yorkshire College was laid by the Archbishop of York on the site of the Beechgrove estate, at Little Woodhouse, near Leeds, in the presence of a large gathering of friends and subscribers to the undertaking. Dr. Heaton, the chairman to the council of the College, delivered a short statement in the nature of a history of the college from its initiation and its establishment down to the present time. With these details our readers are already familiar. The cost of the site has been 13,000*l.*

BRISTOL.—On Saturday next, October 27, the Dean of Westminster will give a public address to the students of University College, and those interested in its success. The introductory lectures have been duly delivered, and in most cases attracted large audiences, and were well reported in local papers.

ABERDEEN.—*Apropos* of a recent correspondence in the *Times*, the following fact is of some interest:—

At a recent meeting of the Aberdeen University Court a letter was read from Sir Louis Mallet, Secretary to Lord Salisbury, asking what special provision the University would make for superintending the conduct of students selected for the Indian Civil Service during their two years of probation. In reply, the court adopted a motion, in which they resolved respectfully to inform the Secretary of State for India that the University could not undertake to institute any separate or severer system of oversight or discipline for one class of students than for another, and that the present system had been found in practice to be perfectly effectual in securing the steadiness, moral training, and good behaviour of the students.

A memorial from the University Council, asking the court to take steps to institute evening lectureships in science and art, was referred to the Senatus for a report.

DUBLIN.—Prof. Emerson Reynolds will commence a course of lectures on General and Medical Chemistry on every Tuesday, Thursday, and Saturday from November 1 to March 31 following. The first course of Practical Chemistry will comprise laboratory instruction in Qualitative Analysis (including Spectrum Analysis), commencing in Michaelmas Term; Volumetric and Simple Gravimetric Analysis, commencing with Hilary Term; Organic Preparations and Analysis, commencing with Trinity Term. The second, or advanced, course of Practical Chemistry will comprise instruction in the higher branches of Experimental and Analytical Chemistry, and in Methods of Research.

Prof. Macalister, M.D., will commence a course of lectures on Zoology in November, to be continued through each term until the end of June.

Prof. E. P. Wright, M.D., will commence a course of lectures on the Morphology of the Cells and Tissues of Plants, and one on the Natural History of Algæ and Fungi in November.

CORK.—Prof. J. Reay Greene has resigned the Professorship of Zoology and Botany in the Queen's College, Cork, retiring on a pension. There is, however, no vacancy, as Prof. Harkness will lecture the students on these subjects.

GALWAY.—Prof. Cleland, M.D., F.R.S., has resigned the Professorship of Anatomy in the Queen's College, Galway, having been elected to the valuable Professorship of Anatomy in Glasgow College, vacant by the resignation of Prof. Allen Thomson. The vacancy in the Queen's College, Galway, will be filled up by H.E. the Lord-Lieutenant of Ireland on October 27.

LANCASTER.—A very fine set of new buildings for the Lancaster Royal Grammar School was opened on September 24. The buildings include a well-fitted laboratory, erected at the expense of W. Bradshaw, Esq., placed at some distance from the main building. It is a pleasant, well-lighted room, thirty feet by twenty feet. The whole school is taught physics, and every boy will pass through a course of chemistry at about the fourth form stage. We hope the authorities will feel encouraged soon to introduce other branches of science as a regular part of the curriculum.

AMSTERDAM.—A university has been opened at Amsterdam.

SCIENTIFIC SERIALS

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. x. fasc. xv.—New contribution to the efficacy of the elastic ligature in surgery, by M. Scarenzio.—Gleanings in the Zoological Museum of Pavia, by M. Pavesi.—Note by M. Curioni on the contents of his work, "Applied Geology of the Lombardy Province."—On microphytes, which produce certain diseases in plants, by M. Cattaneo.—Physico-chemical researches on the different allotropic states of hydrogen, by M. Tommasi.

Journal de Physique, September, 1877.—From this number we note the following papers: On the application of a new apparatus for the determination of visual astigmatism, by M. Javal.—On the spectrum of the electric spark in a compressed gas, by A. Cazin. The author made two series of experiments, one in which he simply observed the spectrum directly by means of the spectroscope, and the other in which he photographed the spectra and thus obtained more accurate results.—Experimental determination of the principal elements of an optical system, by A. Cornu.—On the currents produced by a liquid passing through a tube, by E. Edlund.—On the spectra of chemical compounds, by P. Moser.—On the modes of crystallisation of water and the causes of the various aspects of ice, by Raoul Pictet.—On the influence of light on the electrical resistance of metals, by R. Boernstein.

Zeitschrift für wissenschaftliche Zoologie, vol. 29, part 2 (July).—H. Reichenbach, on the early development of the fresh-water crayfish, 75 pp. 3 plates.—H. Ludwig, on Rhopalodina (class echinodermata).—O. Bütschli, on the process of division of cartilage-cells; on the development of *Paludina vivipara*, in relation to Bobretzky's and Lankester's recent papers; on the development of *Neritina fluviatilis*, and on the segmentation process and formation of the blastoderm in *Nephelis vulgaris*.

Part 3 (September).—Prof. A. Wrzesniowski (Warsaw), Contributions to the natural history of the infusoria, 57 pages, 3 plates, containing descriptions of many new species, and discussions on *Oxytricha*, *Epistylis flavicans*, *Ophrydium versatile*, &c. —Marie von Chauvin, on the power of adaptation of the larvæ of *Salamandra atra*.—Ernst Zeller, on the reproduction of opalina (parasitic on batrachians), 2 plates.—W. Kurz, studies on the lernæopods, 3 plates.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, October 3.—Prof. J. O. Westwood, M.A., F.L.S., president, in the chair.—Mr. W. L. Distant exhibited a specimen of the ravages of *Dermestes vulpinus* in a cargo of dried hides from China. On the arrival of the cargo in this country it was found to be swarming with the insect in all stages.—Mr. McLachlan also exhibited a piece of wood which had formed part of a case containing hides from Shanghai and which was riddled with borings of the larvæ of the same insect. The president remarked that his attention had been directed some years ago to the depredations of this larva in a cargo of cork.—Prof. Westwood exhibited a drawing of the pupa of a species of *Anabolia* which swam about in water like a *Notonecta* and was remarkable for using its middle legs as swimming apparatus. Prof. Westwood also made remarks upon the homology of the mouth organs in the pupæ of Trichoptera and suggested that the mandibles of the pupæ (which are aborted in imago) are for the purpose of eating their way out of the cases in which they undergo their transformation.—The president next exhibited a small lepidopterous insect from Lake Nyassa with a pupa case of a species of *Tachina* from which it was supposed to have been bred.—Prof. Westwood next called the attention of the Society to the remarkable lepidopterous larva attached to the homopterous larva which had been handed to him by Mr. Wood-Mason at the last meeting and stated his belief that the relation of the Lepidopteron to the Homopteron was probably one of true parasitism, the former (*Epipyrops*) feeding on the wax secreted by the latter. Mr. Wood-Mason stated that he was inclined to consider the *Epipyrops* larva as a messmate of the Homopteron having attached itself to the latter for the sake of being carried about to its food-plant and having covered itself with the waxy secretion for the purpose of rendering itself inconspicuous to its foes.—Prof. Westwood then exhibited a moth from Brazil which had been bred from a caterpillar found among the hairs of some animal.—The president finally read a note from Albert Müller announcing the formation of an entomological station at Basle.—Mr. Meldola announced that the Longicorn beetle received from Birkenhead and exhibited at the last meeting had been identified by Mr. C. O. Waterhouse as *Monohammus titillatus*, Fab., a species inhabiting the United States. Mr. Meldola also exhibited a collection of Lepidoptera formed by him in 1875 in Ceylon and the Nicobar Islands.—Mr. H. Goss exhibited a series of *Lycæna Arion* taken in the Cotswolds which were remarkable on account of the small size of some of the specimens, about one-third being below the average size.—Mr. McLachlan read a paper on *Notiothauma Reedi*, a remarkable new genus and species of Neuroptera from Chili pertaining to the family *Panorpidæ*.—A paper was communicated by Mr. A. G. Butler on the Lepidoptera of the family *Lithosiidæ* in the collection of the British Museum.

WELLINGTON

Philosophical Society, July 21.—Mr. W. T. L. Travers, F.L.S., president, in the chair.—The hon. Mr. Mantell read a paper by Mr. J. C. Crawford, F.G.S., on gold found in the rocks of the Tararua and Rimutaka ranges in the province of Wellington. Mr. Crawford had forwarded a specimen to Melbourne, and Mr. J. Chapman, the assayer to the bank of Victoria, had reported that the specimen was composed of sulphate of iron, and gold at the rate of 1 oz. per ton. The hon. Mr. Mantell said he would like some explanation regarding the presence of sulphate of iron. Dr. Hector stated that there must have been some mistake, probably iron bi-sulphide was meant. He reminded the society that a great deal had been done in prospecting the country referred to by Mr. Crawford, and that in 1869 he (Dr. Hector) had communicated to the society the results obtained. Eighteen analyses had been made of quartz specimens from reefs in the district; of these only six had proved auriferous, varying from mere traces up to 13 dwts. per ton of gold, the richest being from Wainuiomata, the same locality from which Mr. Crawford's specimen had come. In his former communication he had warned prospectors against the solid quartz reefs which traverse the sandstones and slate, as the gold at Makara and Terawiti appears to occur in jointed sandstones, chiefly as dendritic films.—Capt. Edwin, R.N., communicated a notice by Mr. J. F. Marten, of Russell, Bay of Islands, regarding the occurrence of the tidal wave which took place on May 11 last. Mr. Travers said he had observed this occurrence in Wellington harbour, and that he believed Dr. Hector had taken observations of the rise and fall. Some years ago a similar wave was observed in New Zealand, after which we had news of an earthquake in America, and no doubt the wave on May 11 last was due to a like cause.—Dr. Hector reported that tidal disturbance on May 11 had been observed on every part of the New Zealand coast, and also in Australia in the same manner, but not so intensely as the waves of August, 1868. The origin of the waves on that occasion was clearly traced to a great volcanic disturbance near the west coast of South America, and in this instance a violent convulsion has also been reported from that quarter as having occurred on May 10. We have not the full particulars yet, but if this date is correct the wave felt on our coast must have been due to a still earlier shock, perhaps in some other place, as it was first noticed at 5 A.M. on the 11th, corresponding to 1 P.M. of the 10th on the South American coast. From this date must be subtracted about seventeen hours for the time of transmission of the wave across the Pacific Ocean, which would require that the shock should have taken place about 8 A.M. on the 9th. This tends to confirm the belief that there is a periodicity in earthquakes, and that they occur independently at distant localities at nearly the same time. He observed that a writer in the last received number of NATURE notices this coincidence in reporting a sharp earthquake at Comrie, in Scotland, on May 11. At Napier, where the engineer of the harbour works, Mr. Weber, makes exact observations, the tides were disturbed from the 11th to the 19th. The position of Napier renders it peculiarly sensitive to oceanic oscillations. Thus on May 1 the highest sea ever experienced in Napier washed over the shingle spit and damaged the rails in front of the Court-house. This phenomenon was only local, and attributed to a long continuance of south-east wind. He called attention to a recent paper by Mr. Russell, the Government Astronomer at Sydney (*Journ.* Ast. Soc. N.S.W., 1876, p. 37), which states that the slightest earth shocks felt in New Zealand are nearly always recorded on the tide gauges in Sydney and Newcastle, and are most unaccountably coincident with abnormal readings of one of the thermometers in the Observatory. If we had well-placed tide-gauges on the New Zealand coasts it is probable the most interesting results would be obtained. Every addition to the observed facts bearing on this subject would be valuable. The investigation of earthquakes would be similar to that of the influence of sun-spots recently examined by Prof. Balfour Stewart, in so far that the release of prodigious latent energies might depend on very obscure and trivial exciting causes. Mr. Carruthers said he did not consider it necessary to suppose that seventeen hours must elapse before a tidal wave due to the same cause as the South American earthquake would reach New Zealand. He did not think the earthquake caused wave, but that both were due to the same cause. He thought earthquakes were locally intensified exhibitions of a great deep-seated movement of the floor of the ocean, and that if the floor were not in movement an earthquake, however violent, would be unable to propagate a wave for such distances as from America to New Zealand. The intensified action which so often shows itself in this part of South America he thought was due largely to the great bend made in the line of elevation of the Andes at this point, which had the effect of converting a deep-seated movement of the earth's crust into a violent crushing of the surface. Dr. Hector explained that the period of seventeen hours for the transmission of a wave across the Pacific Ocean was derived from observation in 1868, when the commotion of the sea extended not only to New Zealand and Australia, but to Japan, Sandwich Islands, and the Cape of Good Hope. He agreed that earthquakes were widespread phenomena locally intensified, but it is the strong local convulsion that originates the oceanic waves. Such waves could not keep pace with a tremor propagated through the solid floor of the ocean, which travels at six times greater speed and generates what is termed the *forced wave*. The ocean wave once generated would take its own time. Dr. Newman did not think we had yet sufficient data to decide on the subject. The depth of the ocean should be considered. He could not agree with Mr. Carruthers that earthquakes extended over so large an extent of the ocean bed. The president said that the works of Darwin, Humboldt, and Mallett on this subject, would be found interesting. He agreed with Dr. Hector that we must look outside our globe for the prime causes of such disturbances, such as sun-spots, influence of the moon, &c.—Before the close of the meeting, Dr. Hector drew attention to several exhibits on the table, more especially to an albino of the New Zealand crow (*Glaucopis wilsoni*), and to a Tui (*Prosthemadera tui*, Zel.) with brown plumage. A fine series of crustacea from the Californian coast, and a selection of the more interesting fossils obtained during the past year by the Geological Department, were also exhibited and explained.

Paris

Academy of Sciences, October 15.—M. Peligot in the chair.—The following papers were read:—On some applications of elliptic functions, by M. Hermite.—On the movements of the apsides of the satellites of Saturn and on the determination of the mass of the ring, by M. Tisserand.—On the non-transparence of incandescent iron and platinum, by M. Govi. He denies the assertion that iron raised to a red or white heat becomes transparent.—Employment of lime-water to fix fatty acids of feed-water of boilers in engines provided with surface-condensers, by M. Hetel. The fatty matters become fixed in insoluble combinations, so that the water reaching the boiler is neutral or even slightly alkaline, containing only a calcareous soap and free glycerine, which is inoffensive and non-adherent to the boiler.—On the ravages produced in the vines of Narbonnais by the disease of anthracnose, by M. Porte.—On the employment of colza and rape, sown in vineyards to preserve the vine from frost, by MM. Serrès and Rérat. These seeds are sown in October or November, and by May, when the frosts are most to be feared, the plants have grown to more than a metre in height, giving good protection. When the frost is fairly gone the rape or colza is cut and the vines then grow with more vigour.—Reply to a former note by M. Stephan on the discovery of the planet 174, by M. Watson.—On cases of reduction of Abelian functions to elliptic functions, by M. Hermite.—Formation of allylene at the expense of bromocitrapyrotartaric anhydride, by M. Bourgoin.—On dibromomethylcarbylamine, by M. Tcherniak.—Researches on the physical constitution of the blood corpuscle, by M. Bechamp. He has succeeded in demonstrating the separate existence of an enveloping membrane by nourishing the corpuscles in a solution of fecula. The membrane is thus made both more resistant to the action of water, and more visible, while retaining its osmotic properties. The effects obtained in thus treating the blood of duck, &c., are minutely described.—On the organic *débris* contained in the quartz and silex of Roannais, by M. Renault.—Researches on vegetable glycogenesis, by M. Jodin. *Inter alia*, the constant presence of certain sugars in all champignons, proves the independence of the glycogenic function, and the chlorophyllian function. Researches are desirable on the influences which cause the saccharimetric quantity in leaves to vary, and the nature of the relation between these variations and the exercise of the chlorophyllian function, &c.—Researches on fatty bodies introduced fraudulently into butter, by M. Husson.—Relation between barometric variations and the sun's declination, by M. Poëy. Low pressures follow exactly the course of the sun, while high pressures follow an opposite course. The observations were made at the Observatory of Havana.

Geneva

Society of Physics and Natural History, July 5.—M. Loret continued the account of his researches on the polarisation of quartz, carried on in conjunction with M. Ed. Sarasin. A first series of measurements of the solar light with a spectroscope having a fluorescent eye-piece, had been extended as far as the Fraunhofer line R. In the new series, and in order to carry further their observations on the ultra-violet part of the spectrum, they operated upon the light proceeding from a strong induction discharge issuing between two points of cadmium. They determined the rotatory power of the quartz for eight of the principal lines of the ultra-violet spectrum of that metal, for which M. Mascart has given the wave-lengths, and for two lines situated still farther beyond. Their experiments show that in these limits, much more extended, the law of Boltzmann is confirmed in a very satisfactory manner. It appears, however, necessary to add a third term to his formula.—Prof. Schift communicated the results of his experiments on the contractions presented by the diaphragm after its nerves have been cut.—M. H. Fol described observations made by him on the origin of the follicle which surrounds the egg of ascidians. It has been wrongly supposed that the cellules of the follicles form part of the strorna of the ovary. The cellules originate in the interior of the young eggs, at the surface of the germinative vesicle, and traverse the whole thickness of the vitellus to reach the surface of the ovule and detach themselves from it. This example of so singular a mode of formation of the follicular cellules is unique, so far as known, in the animal kingdom.

August 2.—Major Ed. Pictet presented the hydrographic chart of the Lake of Geneva, in its south-west part, from its line Coppet-Hermamé to the exit of the Rhône. The form of the bed of the lake is defined by equidistant curves of five metres, vertical distance. The work will be published as the first development of an old investigation by Sir H. de la Beche.—Prof. Colladon has investigated new cases of lightning-stroke, which has confirmed his former conclusions on the effects of lightning upon trees. One of the determining causes of a stroke of lightning on a tree is the neighbourhood of a spring or of a subterranean sheet of water. That cause acts more powerfully than the relative height of different tides. He shows that most of the metallic wires used as lightning conductors are too slender; he calculates that they ought to have a section of at least 500 square millimetres.

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