THURSDAY, SEPTEMBER 26, 1889.

THE TERTIARY FLORA OF AUSTRALIA.

Contributions to the Tertiary Flora of Australia. By C. von Ettingshausen. Memoirs of the Geological Survey of New South Wales. (Sydney: Charles Potter, 1888.)

THE work consists of translations from the German originals of two memoirs, published respectively in 1883 and 1886, with explanatory notes on the geology by the Survey officers. Ettingshausen's contribution consists of about 170 pages of "exact determinations of fossils" and a few pages of theoretical considerations and tabulated lists. In these we are informed that the "Tertiary floras" formed one universal flora, which spread over all lands outside the tropics, and "that in this flora all the elements of the different floras of the world are found combined" (p. 3). The "Tertiary period," from this point of view, consists of sub-periods of uniform conditions, susceptible of exact classification and correlation, with an orderly beginning and definite close. From another point of view it is a period of the world's history, so stupendous, so broken and diversified, that we can never hope to reconstruct a complete history from its imperfect records, and to marshal its secrets into exact order. Embraced in its vast folds are sediments, perhaps coæval with our chalk, beginning when much that is now land was the abyssal depths of ocean, and enduring while continents and seas slowly changed their places. Its episodes were the joining hands of widely severed lands and the parting of them again asunder into isolated tracts, until at last it saw the existing continents settling into their present form. So enormous was its duration, that its "newer" periods sufficed to raise and scarp the loftiest mountain ranges of the world; and its close to submerge and re-elevate again and again, to trim, alter, and finally cut into separate islands the insignificant portion of the globe we inhabit as Great Britain. During all which time, floras as distinctive as any of those now existing were swayed hither and thither by the changing climatic conditions accompanying the oscillations of land and water—here picking up recruits on their passage, and there deserted by the worn out; here coalescing with distinct hosts who struggled and thinned, or swelled, each other's ranks, and there dwindling in numbers as stations or habitats became submerged and broken up. As well might we try to arrive at a complete history of the feuds and migrations of Palæolithic man, as of the floras of the "Tertiary period," for at most each country can but contribute a few isolated facts regarding the floras that have passed over it. Thus, though we possess a broken record in the Isle of Wight of 3000 feet of Tertiaries, mostly deposited under conditions extremely favourable to the preservation of plants, this has, in the island, yielded an adequate idea of its forest vegetation for about 6 inches of its thickness. Nevertheless, remains of floras are repeatedly sandwiched in our Eocenes. These begin abruptly, with nothing leading up to them; and if we go to Ireland and Scotland we can supplement them with other 3000 feet of volcanic rocks with still older floras sandwiched among them, but affording no beginning to the "Tertiary formation." And our series ends abruptly, leaving an enormous gap of most critical time unrepresented between Oligocene and Pliocene, yet having revealed flora after flora as utterly distinct from each other as those of the antipodes, and with scarce any elements in common. Thus, however such conditions may have obtained in Carboniferous times, this theory of a uniform flora or fauna spreading, during the Tertiaries, over both hemispheres, from the limits of vegetation to the confines of the tropics, is altogether outside practical science, and simply leads to affinities being discovered between imperfectly preserved common types of vegetable organisms, where none such perhaps exist.

With regard to the 170 pages of "exact determinations of fossils," though no species-makers are so prolific as palæo-phytologists, our author certainly bids fair to beat the record, for *sp. nov.* is attached to as many of the fragments as the limits of the collection would well allow. The three ferns and three monocotyledons are negative, if unsatisfactory, but there would have been one less belonging "undoubtedly to the Monocotyledones," had not a stray Carboniferous specimen been included in the consignment. The single Cycad is a *sp. nov.*, bearing "a remarkable and specific relation" to a North Greenland fossil. The Coniferæ are determined on very poor material, but most are considered as at least allied to Australian forms; yet *Sequoia* is imported when the native *Athrotaxis* would better meet the case. A new genus, *Heterocladiscos*, is actually founded on some insignificant cupressineous foliage only, and another, *Pseudopinus*, is certainly curious if its supposed fruits are cones and not catkins. Of the some 150 new dicotyledons, the vast bulk would be classed as indeterminable fragments by any reasonably cautious palæontologist. The less characterized of these figure as the exotics to Australia, whilst the most satisfactory are found among the Proteaceæ and other Australian forms as *Boronia*, *Eucalyptus*, species of *Piper*, *Ceratopetalum*, &c. Many of the species are founded on single fragments, sometimes without base or tip, and unless the plates do them injustice, with scarcely any visible venation or character.

We cannot judge of the difficulties of collecting, but it certainly appears that if it is worth while to publish anything on fossil plants at the Government expense, it would be worth while to gather proper material for it. When broken specimens of leaves are obtainable, entire ones can as a rule be extracted, and when these are to hand, though exotic genera may well have flourished in Australia as in Europe in bygone ages, it will be surprising if more of them cannot be matched with plants nearer their own home.

J. STARKIE GARDNER.

OUR BOOK SHELF.

Useful Rules and Tables. By William J. M. Rankine. Seventh Edition, revised by W. J. Millar, C.E. (London: C. Griffin and Co., 1889.)

THIS is the seventh edition of a work which at the present day is almost indispensable to engineers in general. The increase and development of mathematics, year by year, and also the greater tendency towards accurate results, call for a book containing all the various rules and tables relating to those parts of mathematical and mechanical science whose application most frequently occurs in the useful arts, and especially in engineering and practical mechanics. In this volume of moderate bulk, such a work has been provided. The use of algebraical symbols has been avoided as much as possible, excepting in those cases in which the rules cannot be clearly expressed without them.

The book is divided into ten parts. The first deals with arithmetic and mensuration, including tables of cubes, squares, logarithms, a summary of the rules in trigonometry, with tables of arcs, sines, &c., concluding with the measurement of areas of surfaces, volumes of solids, and lengths of curves, &c. Part 2 treats of the measures of different nations, with tables and rules relating not only to measures of angles, time, length, surface, &c., but to those of speed, heaviness, pressure, work power, &c. Engineering geodesy, distributive forces and mechanical centres, balance and stability of structures, and strength of materials are included in the next four parts. Part 7 relates to machines in general, and gives rules for the comparison of the motions of different parts of a machine, and for the designing of teeth of wheels, speed cones, &c., with rules relating to work at uniform and variable speed. Parts 8 and 9 treat of hydraulics and heat, together with the steam-engine. The former includes rules for flow of water, prime movers, propulsion of vessels, &c.; the latter consists of tables of elasticity, volume, and specific heat of gases, factors of evaporation, with rules relating to furnaces, boilers, expenditure of heat in cylinders, efficiency of strain, &c.

Part 10, written by Andrew Jamieson, has been revised and considerably extended, and consists of electrical rules, tables, and formulæ. The information has been brought up to date as much as possible, and many points of difficulty, such as directions of currents, magnetic force and motion, are made clear by means of illustrations. Electrical engineering symbols and units of measurement, heat, and light are first given. These are followed by various forms of Wheatstone bridges, apparatus for testing electric light cables, the wire-testing batteries on the General Post Office system, tables of resistance, general data of the different submarine cables and batteries.

In the appendix there is a useful diagram of the mechanical properties of steam, showing the absolute pressure in pounds per square inch, and volumes in cubic feet per pound of dry saturated steam, and the mean absolute pressure, in decimal parts of absolute pressure of admission. A complete index adds greatly to the value of the work; and we may say that the more one looks through the pages of the book the more one is struck by the large amount of useful information collected together in these 456 pages.

Colour. By C. T. Whitmell. (Cardiff: Wm. Lewis, 1888.)

THIS book is designed for the general reader, and is, on the whole, well suited to this class of person. The principal drawback it possesses is unquestionably the want of a good index, while the division into short numbered sections of in many cases a few lines only is very inconvenient, and produces a sensation of discontinuity of subject. Some parts are excellently done, notably the illustrations given of irregular reflection by turbid media, the description of colour produced by absorption, and the part dealing with colour-blindness. In connection with this last subject much extension of our knowledge would no doubt result from systematic observation of the progressive development of colour-blindness in cases of locomotor ataxy.

It is to be regretted that the author did not supply coloured diagrams to at least some of the sections, or failing this it would be more suggestive in the diagrams of light transmitted through different specimens of coloured glass to shade the part representing the absorbed and to leave unshaded that representing the transmitted light. Some preliminary description of the optical apparatus employed would be also serviceable, *e.g.* in section 10 the reader is told the properties of a spectrum produced by a diffraction grating, no reference being made until much later, and then a very incomplete one, as to the principle involved in the formation of the spectrum.

The book is fairly up to date, containing as it does reference to Langley's bolometric observations and to König's researches on the theory of colour vision. Interesting cases are given of errors due to ignorance of scientific principles; and in view of the frequency of their occurrence—perhaps more noticeably from neglect of the effects of refraction than of the principles of colour—it is regrettable that manuals such as Church's, Rood's, or this, are not more generally read by painters.

LETTERS TO THE EDITOR.

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Sailing Flight of Large Birds over Land.

IT gave me great pleasure to see, by Lord Rayleigh's letter in NATURE of May 9 (p. 34), that the remarks made by me some months ago on this subject were not made in vain.

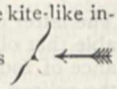
Ever since 1863, the sailing flight of large birds (which is here very common) has been a subject of observation to me, and odd notes have been sent home to the *English Mechanic* and to the Aëronautical Society (see Sixteenth Report, 1881, pp. 10-17).

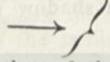
Mr. A. Baines, in NATURE of May 2 (p. 9), well describes the sustained sailing of the albatross, indicating what I take to be the *vera causa, i.e.* the rising, kite-like, when it sweeps round to meet the wind, the energy of motion being gained by descending *with it* along an incline. But this problem, as seen among the sea-birds, seems to be complicated by the possibility of lifting action due to the waves; and, in Mr. Baines's letter, by different velocities of the air near the sea surface and at elevations of 20 feet or so. Out here, these two features are not only eliminated, but we see the bird doing more work.

The sea-birds merely sustain for hours a given weight, say twenty pounds, without flapping the wings, whereas the land-birds lift this twenty pounds, in two or three hours, to a height of 1 or 2 miles vertically, as well.

The adjutants (*Leptotilus argala* and *nudifrons*), the cyrus, pelican, vultures of several kinds, and storks, habitually rise here during fine weather, if there is a wind. At first they rise by flapping the wings vigorously, and, when 200 or 300 feet up, gradually begin to sail in huge right- or left-hand spirals, rising 30 or 40 feet at each lap. When seen thus, the wings are rigidly extended, and tail spread, the primary wing-feathers distinctly separated, and a loud musical tone is heard as the bird sweeps round and round overhead. If low down, they can be closely studied through a binocular, but if at a great height, I generally use a telescope, 3"·5 O.G., and terrestrial eye-piece, power 40. With this latter I can follow them, either in a group or singly, until each is a mere speck, and the elevation can be fairly calculated, when the spread of wing is often 8 or 9 feet × $1\frac{1}{2}$.

Our prevailing north-east wind, and also our south-west monsoon, are particularly steady air-drifts of, say, 5 or 6 to 10 miles per hour, and I should doubt very much, if, after 500 feet up, there is any variation in the speed at different heights. The lifting due to occasional waves should also here be out of the question, and under these conditions I would ask Lord Rayleigh where we could possibly obtain the "energy of position" if it is not from the kite-like action? In my former note, to which he refers, I think a small diagram illustrated the slow drift to leeward as the bird rose by sailing in spirals.

I should say, perhaps, that the birds maintain the kite-like incline, *all round the windward portion* of the spiral, thus 

and appear to gain elevation in sweeping round while thus at an angle; at first facing the breeze, and as they sweep round being side on, as per diagram, the centrifugal element coming into play. If very low down, indeed, the bird can be seen to rise distinctly as it wheels round the windward edge of the curve, and reaches its greatest elevation just as it turns tail to wind. Then it seems to shoot along down a slight incline with wings horizontal, and in rounding the leeward part of the spiral, the outer wing is again spiral, thus . The speed of the bird so greatly exceeding that of the wind, renders this necessary to enable it to turn.

Viewed from below, the tracks look circular, and often vary in size a good deal.

In all cases, so far as I have seen, these birds when rising, as they sail round and round, make leeway, say a mile for every 1000 feet rise, or less.

They never rise on a straight course, though they often descend in a straight line, and, after a time, flocks that drift over the hills recover their position over the plains by descending to windward.

But in all these cases of land-birds rising steadily as they sail round and round, there is no possibility of lifting by waves, as at sea, nor yet differences in the *speed of air strata* every 20 feet or so, as they sail at all heights, from 500 feet up to 10,000 or more.

The problem is much more clearly seen, and more wonderful, than in the case of the albatross, and, as far as I can see, the explanation which I gave years ago is the only one feasible. The momentum gained in descending a given distance *with the wind* is expended in lifting the bird kite-like as it turns and *faces* the wind, on the shorter windward course, where also a certain amount of centrifugal reaction comes into play and does some of the *work*. S. E. PEAL.

Sibsagar, Assam, August 8.

Bishop's Ring and Allied Phenomena.

Is there any connection between the sunset glows seen by Mr. S. E. Bishop in the Sandwich Islands (see p. 415) and the phenomena which have occurred lately in Western Europe? There has been for two months a feeble reappearance of a great corona round the sun. I do not know whether I can call it Bishop's ring, as it has generally been larger than that caused by the Krakatão dust, and also more dirty-looking in colour, and doubtless at a lower level. Bishop's ring itself had never altogether ceased to be visible about sunset and sunrise, but I had not certainly seen it at any time when the sun has had a considerable altitude for nearly two years, and then only in the clear Alpine air. On May 14 and August 26, 1888, there was indeed a great corona, but on the latter occasion the air was not very clear, so I concluded it probably had some other cause than volcanic dust; and there have been a few other occasions before that when I have had the same impression. On May 14, 1888, it is also questionable whether the circle was caused by volcanic dust.

The present corona appeared all at once on June 30 last, while I was at Macugnaga, at the foot of Monte Rosa; and since that date, both travelling in the Alps and here (since I came home on August 8) it has continued more or less visible at all times of the day when not obscured by mist or smoke, except from the afternoon of July 1 until the afternoon of the 2nd. It is plainest about sunset. It first appeared on cloudy streaks like very thin indefinite cirrus, but these disappeared during July 1. Since it appeared the sky has never been such a pure blue as it was before; this applies to all altitudes from sea-level to 10,000 feet.

I have not seen any remarkable sunset phenomena, so that it seems difficult to connect this circle with the phenomena noticed by Mr. Bishop a fortnight later than this commenced; still the connection is not impossible, as he mentions seeing the central (and by far the brightest) part of the corona.

Although it is improbable that the dust of Krakatão can have entirely settled to the ground, the above description will show that the present phenomenon can hardly be caused by it; but there must be a very extensive diffusion of some kind of dust—it may be one of the kinds alluded to by Prof. Tyndall and others in the discussion which occurred in your pages last year.

Sunderland, September 21. T. W. BACKHOUSE.

OBSERVATIONS OF TWILIGHT AND ZODIACAL LIGHT DURING THE TOTAL ECLIPSE OF THE SUN, DECEMBER 21, 1889.

THE attention of all friends of astronomy and meteorology, and especially of navigators on the ocean, and of meteorological observers in India and Mexico, is respectfully called to the fact that they may make valuable observations during the coming total eclipse of the sun. The observations referred to are of a most elementary character, and pertain to the phenomena of twilight and of the zodiacal light. The importance of the observations to be described was first appreciated by German meteorologists, and the following paragraphs are based upon suggestions made by Prof. Bezold and Dr. Zenker, of Berlin.

On December 22, 1889, at 47 minutes before noon, Greenwich time (which corresponds with 6 hours and 13 minutes a.m. of standard Eastern time) our globe becomes tangent to the long cone that constitutes the shadow of total darkness behind the moon, and we say that "totality has begun on the earth"; this occurs at a point in the Caribbean Sea, north of Venezuela, at about 15° N. lat. and 72½° W. long. In about three hours and a quarter this zone of total eclipse has passed the earth; its last point of contact, or tangency, occurs at a point on the eastern coast of Africa at about 5½° N. lat. and 49° E long. The path of totality on the earth's surface between these two limits is a narrow belt, about 100 miles broad, and astronomers will station themselves at favourable localities in order to observe the phenomena visible immediately around the sun, but meteorologists and local amateurs residing entirely outside the path of totality can also do good work.

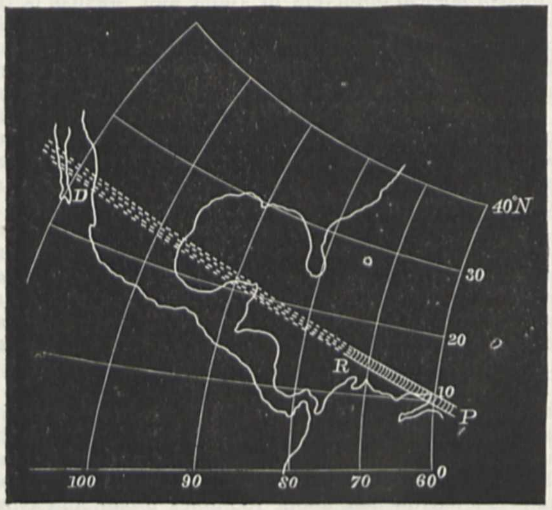
The accompanying diagrams, I., II., III., IV., will illustrate the conditions of the eclipse and the nature of the meteorological observations desired to be made. Thus in diagram I. we see the point R, or the locality where the totality begins to an observer on the earth; at that point the sun rises while it is totally eclipsed, and the band from R to P shows the path of the shadow as it flies eastward. Diagram II. takes up the path at its other end, and shows it still moving eastward from P towards S, where it finally leaves the earth, so that an observer at S sees the sun in his western horizon, but totally eclipsed as it is setting out of sight.

If we now imagine a vertical plane passing through the observer and the eclipsed sun, it will give us a section of the earth, its atmosphere, and the cone of darkness very much as shown in diagrams II. and IV. In diagram II. R represents, as before, the locality where the sun rises eclipsed, the moon is to the east of the observer, and the cone of darkness passes over him towards its apex, which is far off towards the west. Those who are located to the west of R, as at D, looking eastward, cannot see the sun because it is below their horizon; they can see only the light of the morning dawn where the sun is about to rise, but these observers, if they look directly toward the brightest part of the dawn, immediately over the sun, will, on this morning of December 22, find that they are looking right into the shadow of the cone, and will therefore observe that the light of the dawn is for a few minutes much feebler than usual.

Similarly diagram IV. shows the condition of things at the point S, where the observer at sunset has the cone of darkness imediately above him; observers to the east of his location, as at T, will be enjoying twilight, and as they look toward the west, immediately above the sun, they will be looking into the cone of darkness, and will find the twilight correspondingly diminished.

The distance from K to D in Fig. 1, and from S to T in diagram II., represents about 35° or 40° of the great circle, or over 2000 miles on the earth's surface; all persons located in this immense region—namely, all on vessels in the Caribbean Sea and Gulf of Mexico, the Arabian Sea—and all meteorological observers in Northern Mexico, Yucatan, Jamaica, and Western India, should be on the watch for phenomena of the following kind:—

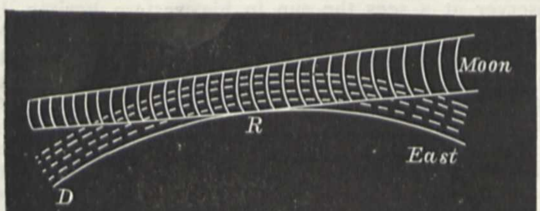
(1) Note when the moon's shadow becomes first or last



I.

visible by the darkness of the high cirrus, haze, or the tops of cumulus clouds or the tops of mountains.

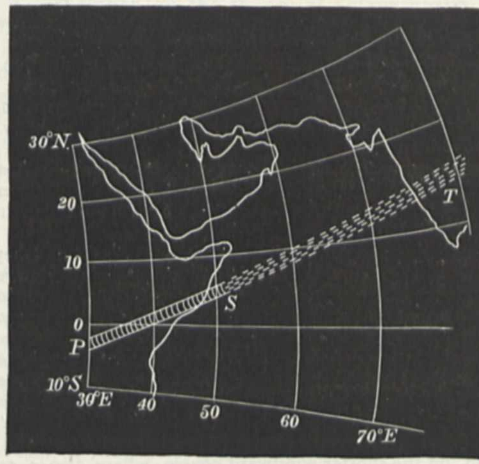
(2) When the darkness is at its maximum, its boundary appears as a curved band of colours convex to the horizon, disturbing the otherwise horizontal arrangement of the illumination that constitutes the dawn or twilight. Note the angular distance of this boundary above the horizon at several points to the right and left of the sun. Note also the colours and their arrangement, both in the twilight proper and in the shaded portions. Note especially how far to the right or left the disturbance of the twilight



II.

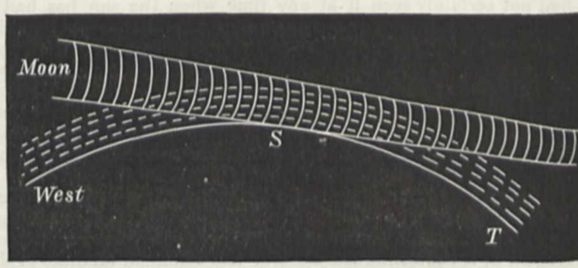
or dawn appears to extend. By distance, of course, is meant apparent angular distance; but if the observer is so located that distant mountain-tops are visible, he should note whether they also show any phenomena due to the moon's shadow.

(3) Owing to the fact that the observers at D and T are looking through a portion of atmosphere that does not receive its ordinary amount of light from the sun, they will be able to see objects that ordinarily would be hidden by the glare of twilight, such as fainter stars, and especially the Milky Way, and the zodiacal light. The latter object is one of special interest. It is desirable that observers should carefully record the appearance of zodiacal light for several days before and after the eclipse, as also on the 22nd itself, in order that we may distinguish between local atmospheric effects and those due to the eclipse. During the morning dawn in the region R D, and during the twilight S T, one should observe that the zodiacal light attains an unusual brilliancy and extent; the points to be noted are, first, how much further up from the horizon, or from the sun, is the beam of zodiacal light visible during the few minutes when the cone of shadow comes between it and the observer;



III.

again, how much nearer down toward the sun does it extend; and, again, how much broader is it toward the right and left? Does its colour appear changed? Sometimes several portions of the zodiacal light appear brighter than the neighbouring regions, therefore observe whether, as seen through the conical shadow, these differences disappear and the whole zodiacal light has a uniform gradation in intensity from the sun outward toward its extremity. Sometimes pulsations or waves of brightness and darkness have appeared to proceed from near the sun, reaching the extremity of the zodiacal light



IV.

in the course of five or ten seconds; note whether any such phenomena are visible.

The zodiacal light has been, on good authority, observed very high above the east and west horizon, so that some maintain that it is continuous through the zenith, and therefore surrounds the earth. This view may possibly be tested by observers located anywhere near the equator and a long way off from the path of totality. Therefore everyone should keep a record of the zodiacal light if he is so located as to see it at all during a few days before and after the eclipse and several hours before sunrise and after sunset. If observations show that the brightness of the zodiacal light is materially diminished during totality, in any part of the region where the moon's shadow darkens the atmosphere, this will go far to show that the zodiacal light originates in the earth's atmosphere; but if, as seen through the shaded air, the zodiacal light appears brighter than ever, it would follow that its location is far from us, and that it is an appendage of the sun.

(4) The observers of the zodiacal light should not fail to record the phenomena sometimes seen on the opposite side of the horizon, and called *Gegenschein*, or the anti-zodiacal light. Similarly, observers of the twilight phenomena should record the appearances in the horizon at the opposite side of the sun, or the so called anti-twilight arc, or band.

(5) Observers to whom the sun is beyond the horizon, and for whom the atmosphere between them and the sun is not illumined owing to the presence of the moon's shadow, will have a good opportunity, for a few minutes, to see any faint comet that may have been hidden to astronomers by the glare of the sunlight, and, if such should be seen, they should record the apparent altitude and azimuth of the nucleus.

The diagrams I. and III. trace the shadow-cone westward to South California and eastward to India, but this should not prevent observers still further west on the Pacific, or east over India and Japan, from recording and reporting such phenomena as they may observe.

Washington, August. CLEVELAND ABBE.

THE BRITISH ASSOCIATION.

SECTION D.

BIOLOGY.

OPENING ADDRESS BY PROF. J. S. BURDON SANDERSON, M.A., M.D., LL.D., F.R.SS. L. & E., PRESIDENT OF THE SECTION.

IT has long ceased to be possible in the course of an annual address in Section D to give an account even of the most important advances which have been made during the preceding twelve months in the various branches of knowledge which are now included under the term Biology. One reason is that each of the biological subjects has acquired such vast dimensions; the other, that the two main branches—Morphology, which strives to explain why plants and animals have assumed the forms and structure which they possess, and Physiology, which seeks to understand how the living organism works—have now diverged from each other so widely as regards subject and method, that there seems to be danger of complete separation of the one from the other.

From this sundering of sciences which a generation ago were intimately united, however inevitable it may be, Physiology chiefly suffers, as being even to the naturalist less attractive and interesting. The study of form and structure has the great advantage that it brings the observer into direct relation with objects which excite his curiosity without requiring too great an effort to understand them. This was the case even when Anatomy was mainly descriptive, and Zoology and Botany occupied themselves chiefly with classification and with definition of species. How much more is it the case now that Anatomy, Zoology, and Botany have become built into one system, of which the Doctrine of Evolution is the corner stone! Morphology, the name now given to this system, has, if I am not mistaken, this advantage over all other subjects of scientific study—that while attractive to the beginner, it is perfectly satisfactory to the mature student. It derives its perfectness from its subject—the *order* of the plant and animal world. For inasmuch as its fundamental conception is the development of all organisms, however complicated, from elementary forms, and as the theoretical development of the plant and animal world (in other words the science of morphology), claims to be nothing more than a synthesis of the observed facts of its actual development, the science is co-ordinate and conterminous with living nature, and strives after a perfection which is that of nature itself.

Physiology is without this source of attractiveness. Its first lessons present difficulties to the beginner which, unless he is contented (as, indeed, ordinary students are) to accept as true what he does not understand, are, to say the least, discouraging; while to the more mature student, who has mastered more or less some part of the subject, it fails to present a system of knowledge of which all the parts are interdependent and can be referred to one fundamental principle, comparable to that of development or evolution.

It is easy to understand that this must be so if we consider the present position of the subject, and the nature of the work which the physiologist has to do. That work is of two kinds. He has first to determine what are the chemical and physical endowments of living matter in general, and of each of the varieties of living matter which constitute the animal and plant organism in particular. Then, these having been investigated, he has to determine how these processes are localized so as to constitute the special function of each structure, and the relation between structure and process in each case. The order I have indicated is the logical order, but in the actual progress of physiology this order has not been followed, *i.e.* there has not been a correlation of structure with previously investigated process, for in former days physiologists spoke of assimilation, secretion, contraction, and the like, as functions of muscles, glands, or other parts, without recognizing their ignorance of their real nature. But now, no one who is awake to the tendencies of thought and work in physiology, can fail to have observed that the best minds are directed with more concentration than ever before to those questions which relate to the elementary endowments of living matter, and that if they are still held in the background it is rather because of the extreme difficulty of approaching them than from any want of appreciation of their importance.

It is to some of these questions that I am anxious to draw the attention of the Section to-day. I feel that I have set myself a difficult task, but think that, even should I succeed very partially, the attempt may be a useful one. And I am encouraged by the consideration that the interest they possess is one which is common to plant and animal physiology, and that if we really understood them, they would furnish a key, not only to the phenomena of nutrition and growth, but even to those of reproduction and development, and by the belief that it is in the direction of elementary physiology, which means nothing more than the study of the endowments of living material, that the advance of the next twenty years will be made.

Nearly fifty years ago, J. R. Mayer's[1] treatise on the relation between organic motion and the exchange of material in living organisms was published in Germany. Although its value was more appreciated by physicists than by biologists, it was in its purpose, as well as in its subject-matter, physiological. In it Mayer showed for the first time that certain functions of the animal body, which up to that time had been considered most vital, are strictly within reach of measurement, *i.e.* referable to physical standards of quantity. He was even able to demonstrate that those quantitative relations between different kinds of energy which physicists were then only beginning to recognize, held good as regards the processes peculiar to the living organism.

Almost immediately after the appearance of this now celebrated work, a series of discoveries were made in physiology, which constituted the period we are now considering an epoch. Mayer himself had proved that muscles in doing work and producing heat do not do so at the expense of their own substance. But this fact could not be understood until Bernard showed that sugar is one of the most important constituents of the blood, and its storage and production a chief function of the liver. Helmholtz next succeeded in proving what Johannes Müller[2] had declared to be nearly impossible—namely, that the time occupied by the propagation of a motor impulse from the brain to a muscle could be measured, and showed it to be proportional to the distance traversed. Next, du Bois-Reymond investigated the electrical phenomena of living beings, and marshalled them under a physical theory which stood its ground against the severest criticism for more than a generation. And finally, the hydrodynamic principles relating to the circulation, set forth by Dr. Thomas Young in his Croonian Lecture forty

[1] J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel" (Heilbronn, 1845).
[2] Müller's "Physiology," translation of second edition, p. 25.

years before, were demonstrated experimentally by Ludwig, at the very time when Helmholtz was giving definite form to the great natural philosopher's theory of colour perceptions.

The effect of these discoveries was to produce a complete revolution in the ways of thinking and speaking about the phenomena of life. The error of the past had been to believe that, although the heart resembled a pump, although digestion could be imitated in the laboratory, and comparisons of vital with physical processes cou'd be used for illustration, it was always wrong to identify them. But, inasmuch as it had been learned that sensation is propagated along a nerve just as sound is propagated through the air, only with something like a tenth of the velocity, that the relations between the work done, the heat produced, and the fuel used, can be investigated in the living body just as they are in the steam-engine, it now came to be felt that, in other similar cases, what had been before regarded as peculiarly vital might be understood on physical principles, and that for, the future the word "vital" as distinctive of physiological processes might be abandoned altogether. In looking back, we have no difficulty in seeing that the lines of investigation which were then initiated by such men as Helmholtz, Ludwig, Brücke, du Bois-Reymond, Donders, Bernard, are those along which, during the succeeding generation, the science of physiology advanced; nor can anyone who is acquainted with the literature of that time doubt that these leaders of physiological thought knew that they were the beginners of a new epoch. But such an epoch cannot occur again. We have adopted once for all the right, *i.e.* the scientific method, and there is not the least possibility of our recurring to the wrong. We have no new departure, no change of front in prospect; but even times which are not epochal have their tendencies, and I venture to submit to you, that in physiology the tendency of the present time is characterized by the concentration of the best efforts of the best minds on what I have already referred to as elementary questions. The work of investigating the special functions of organs, which during the last two decades has yielded such splendid results, is still proceeding, and every year new ground is being broken and new and fruitful lines of experimental inquiry are being opened up; but the further the physiologist advances in this work of analysis and differentiation, the more frequently does he find his attention arrested by deeper questions relating to the essential endowments of living matter, of which even the most highly differentiated functions of the animal or plant organism are the outcome. In our science the order of progress has been hitherto and will continue to be the reverse of the order of Nature. Nature begins with the elementary and ends with the complex (first the amœba, then the man). Our mode of investigation has to begin at the end. And this not merely for the historical reason that the first stimulus to physiological inquiry was man's reasonable desire to know himself, but because differentiation actually involves simplification. For just as in manufactures it is the effect of division of labour that less is required of each workman, so in an organism which is made up of many organs, the function of each is simpler.

Physiology, therefore, first studies man and the higher animals and proceeds to the higher plants, then to invertebrates and cryptogams, ending where development begins. From the beginning her aim has been to correlate function with structure, at first roughly, afterwards, when, as I have explained, her methods of observation became scientific, more and more accurately—the principle being *that every appreciable difference of structure corresponds to a difference of function*; and conversely that each endowment of a living organ must be explained, if explained at all, as springing from its structure.

It is not difficult to see whither this method must eventually lead us. For inasmuch as function is more complicated than structure, the result of proceeding, as Physiology normally does, from structure to function, must inevitably be to bring us face to face with functional differences which have no structural difference to explain them. Thus, for example, if the physiologist undertakes to explain the function of a highly differentiated organ like the eye, he finds that up to a certain point, provided that he has the requisite knowledge of dioptrics, the method of correlation guides him straight to his point. He can mentally or actually construct an eye which will perform the functions of the real eye, in so far as the formation of a real image of the field of vision on the retina is concerned, and will be able thereby to understand how the retinal picture is transferred to the organ of consciousness. Having arrived at this point he begins to correlate the known structure of the retina with what is required of it, and finds that the number of objects which he can discriminate in the field of vision is as numerous as, but not more numerous than, the parts of the retina, *i.e.* the cones which are concerned in discriminating them. So far he has no difficulty; but the method of correlation fails him from the moment that he considers that each object point in the field of vision is coloured, and that he is able to discriminate not merely the number and the relations of all the object points to each other, but the colour of each separately. He then sees at once that each cone must possess a plurality of endowments for which its structure affords no explanation. In other words, in the minute structure of the human retina, we have a mechanism which would completely explain the picture of which I am conscious, were the objects composing it possessed of one objective quality only, being colourless, but it leaves us without explanation of the differentiation of colour.

Similarly, if he is called upon to explain the function of a secreting gland, such, *e.g.*, as the liver, there is no difficulty in understanding that, inasmuch as the whole gland consists of lobules which resemble each other exactly, and each lobule is similarly made up of cells which are all alike, each individual cell must be capable of performing all the functions of the whole organ. But when by exact experiment we learn that the liver possesses not one function but many—when we know that it is a storehouse for animal starch, that each cell possesses the power of separating waste colouring-matter from the blood, and of manufacturing several kinds of crystallizable products, some of which it sends in one direction and others in the opposite—we find again that the correlation method fails us, and that all that our knowledge of the minute structure has done for us is to set before us a question which, though elementary, we are quite unable to answer.

By multiplying examples of the same kind, we should in each case come to the same issue, namely, *plurality of function with unity of structure*, the unity being represented by a simple structural element—be it retinal cone or cell—possessed of numerous endowments. Whenever this point is arrived at in any investigation, structure must for the moment cease to be our guide, and in general two courses or alternatives are open to us. One is to fall back on that worn-out *Deus ex machinâ*, protoplasm, as if it afforded a sufficient explanation of everything which cannot be explained otherwise, and accordingly to defer the consideration of the functions which have no demonstrable connection with structure as for the present beyond the scope of investigation; the other is, retaining our hold of the fundamental principle of correlation, to take the problem in reverse, *i.e.* to use analysis of function as a guide to the ultra-microscopical analysis of structure.

I need scarcely say that of these two courses the *first* is wrong, the *second* right, for in following it we still hold to the fundamental principle *that living material acts by virtue of its structure*, provided that we allow the term structure to be used in a sense which carries it beyond the limits of anatomical investigation, *i.e.* beyond the knowledge which can be attained either by the scalpel or the microscope. We thus (as I have said) proceed from function to structure, instead of the other way.

The departure from the traditions of our science which this change of direction seems to imply is indeed more apparent than real. In tracing the history of some of the greatest advances, we find that the recognition of function has preceded the knowledge of structure. Haller's discovery of irritability was known and bore fruit, long before anything was known of the structure of muscle. So also, at a later period, Bichat was led by his recognition of the physiological differences between what he termed the functions of organic and animal life, to those anatomical researches which were the basis of the modern science of Histology. Again, in much more recent times, the investigation of the function of gland cells, which has been carried on with such remarkable results by Prof. Heidenhain in Germany, and with equal success by Mr. Langley in this country, has led to the discovery of the structural changes which they undergo in passing from the state of repose to that of activity: nor could I mention a better example than that afforded (among many others relating to the physiology of the nervous system) by Dr. Gaskell's recent and very important discovery of the anatomical difference between cerebro-spinal nerves of different functions. We may therefore anticipate that the future of physiology will differ from the past chiefly in this respect—that whereas hitherto the greater part of the work has consisted in the interpretation of facts arrived at in the first instance by anatomical methods of research, Histology, once the guide of Physiology, has now become her handmaid.

During the last ten or fifteen years Histology has carried her methods of research to such a degree of perfection that further improvement scarcely seems possible. As compared with these subtle refinements, the "minute anatomy" of thirty years ago appears coarse—the skill for which we once took credit seems but clumsiness. Notwithstanding, the problems of the future from their very nature lie as completely out of reach of the one as of the other. It is by different methods of investigation that our better equipped successors must gain insight of those vital processes of which even the ultimate results of microscopical analysis will ever be, as they are now, only the outward and visible sign.

In what has preceded, I have endeavoured to show that at present the fundamental questions in physiology, the problems which most urgently demand solution, are those which relate to the endowments of apparently structureless living matter, and that the most important part of the work of the immediate future will be the analysis of these endowments. With this view, what we have to do is, first, to select those cases in which the vital process offers itself in its simplest form, and is consequently best understood; and, secondly, to inquire how far in these particular instances we may, taking as our guide the principle I have so often mentioned as fundamental, viz. the correlation of structure with function, of mechanism with action, proceed in drawing inferences as to the mechanism by which these vital processes are in these simplest cases actually carried out.

The most distinctive peculiarity of living matter as compared with non-living is that it is ever changing while ever the same, *i.e.* that life is a state of ceaseless change. For our present purpose I must ask you, first, to distinguish between two kinds of change which are equally characteristic of living organisms—namely, those of growth and decay on the one hand, and those of nutrition on the other. Growth the biologist calls evolution. Growth means the unfolding, *i.e.* development, of the latent potentialities of form and structure which exist in the germ, and which it has derived by inheritance. A growing organism is not the same to-day as it was yesterday, and consequently not quite the same now as it was a minute ago, and never again will be. This kind of change I am going to ask you to exclude from consideration altogether at this moment, for in truth it does not belong to Physiology, but rather to Morphology, and to limit your attention to the other kind which includes all other vital phenomena. I designated it just now as nutrition, but this word expresses my meaning very inadequately. The term which has been used for half a century to designate the sum or complex of the non-developmental activities of an organism is "exchange of material," for which Prof. Foster has given the very acceptable substitute Metabolism. Metabolism is only another word for "change," but in using it we understand it to mean that, although an organism in respect of its development may never be what it has been, the phases of alternate activity and repose which mark the flow of its life-stream are recurrent. Life is a Cyclosis in which the organism returns after every cycle to the same point of departure, ever changing yet ever the same.

It is this antithesis which constitutes the essential distinction between the two great branches of biology, the two opposite aspects in which the world of life presents itself to the inquiring mind of man. Seen from the morphological side, the whole plant and animal kingdom constitutes the unfolding of a structural plan which was once latent in a form of living material of great apparent simplicity. From the physiological side this apparently simple material is seen to be capable of the discharge of functions of great complexity, and therefore must possess corresponding complexity of mechanism. It is the nature of this invisible mechanism that physiology thirsts to know. Although little progress has as yet been made, and little may as yet be possible, in satisfying this desire, yet, as I shall endeavour to show you, the existing knowledge of the subject has so far taken consistent form in the minds of the leaders of physiological thought that it is now possible to distinguish the direction in which the soberest speculation is tending.

The *non-developmental* vital functions of protoplasm are the absorption of oxygen, the discharge of carbon dioxide and water and ammonia, the doing of mechanical work, the production of heat, light, and electricity. All these, excepting the last, are known to have chemical actions as their inseparable concomitants. As regards electricity, we have no proof of the dependence of the electrical properties of plants and animals on chemical action. But all the other activities which have been mentioned are fundamentally chemical.

Let us first consider the relation of oxygen to living matter and vital process. For three-quarters of a century after the fundamental discoveries of Lavoisier and Priestley (1772-76), the accepted doctrine was that the effete matter of the body was brought to the lungs by the circulation and burnt there, of which fact the carbon dioxide expired seemed an obvious proof. Then came the discovery that arterial blood contained more oxygen than venous blood, and consequently that oxygen must be conveyed as such by the blood-stream to do its purifying work in all parts of the body, this advance in the understanding of the process being crowned a few years later by the discovery of the oxygen-carrying properties of the colouring-matter of the blood, in which the present President of the Royal Society took so prominent a part. Finally, between 1872 and 1876, as the result of an elaborate series of investigations of the respiratory process, the proof was given by Pflüger[1] that the function of oxygen in the living organism is not to destroy effete matter either here or there, but rather to serve as a food for protoplasm, which, so long as it lives, is capable of charging itself with this gas, absorbing it with such avidity, that, although its own substance retains its integrity, no free oxygen can exist in its neighbourhood. This discovery, of which the importance is comparable with that of Lavoisier, can best be judged of by considering its influence on other fundamental conceptions of the vital process. The generally accepted notion of effete matter waiting to be oxidized was associated with a more general one, viz. that the elaborate structure of the body was not permanent, but constantly undergoing decay and renewal. What we have now learnt is, that the material to be oxidized comes as much from the outside as the oxygen which burns it, though the reaction between them, *i.e.* the oxidation, is intrinsic, *i.e.* takes place within the living molecular framework.

Protoplasm, therefore, understanding by the term the visible and tangible presentation to our senses of living material, comes to consist of two things—namely, of framework and of content—of channel and of stream—of acting part which lives and is stable, and of acted-on part which has never lived and is labile, that is, in a state of metabolism, or chemical transformation.

If such be the relation between the living framework and the stream which bathes it, we must attribute to this living, stable, acting part, a property which is characteristic of the bodies called in physiological language ferments, or enzymes, the property which, following Berzelius, we have for the last half-century expressed by the word *catalytic;* and use, without thereby claiming to understand it, to indicate a mode of action in which the agent which produces the change does not itself take part in the decompositions which it produces.

I have brought you to this point as the outcome of what we know as to the essential nature of the all-important relation between oxygen and life. In botanical physiology the general notion of a stable catalyzing framework, and of an interstitial labile material, which might be called catalyte, has been arrived at on quite other grounds. This notion is represented in plant physiology by two words, both of which correspond in meaning—Micellæ, the word devised by Nägeli, and the better word Tagmata, substituted for it by Pfeffer. Nägeli's word has been adopted by Prof. Sachs as the expression of his own thought in relation to the ultra-microscopical structure of the protoplasm of the plant cell. His view is that certain well-known properties of organized bodies require for their explanation the admission that the simplest *visible* structure is itself made up of an arrangement of units of a far inferior order of minuteness. It is these hypothetical units that Nägeli has called micellæ.

Now, Nägeli in the first instance confounded the micellæ with molecules, conceiving that the molecule of living matter must be of enormous size.[2] But, inasmuch as we have no reason for believing that any form of living material is chemically homogeneous, it was soon recognized, perhaps first by Pfeffer, but eventually also by Nägeli himself, that a micella, the ultimate

[1] *Pflüger's Archiv*, vol. vi., 1872, p. 43. and vol. x., 1875, p. 251, "Ueber die physiologische Verbrennung in den lebendigen Organismen."

[2] Nägeli, "Theorie der Gährung; Beitrag zur Moleculär Physiologie," p. 121 (1879).

element of living material, is not equivalent to a molecule, however big or complex, but must rather be an arrangement or phalanx of molecules of different kinds. Hence the word tagma, first used by Pfeffer,[1] has come to be accepted as best expressing the notion. And here it must be noted that each of the physiologists to whom reference has been made regards the micellæ, not as a mere aggregate of separate particles, but as connected together so as to form a system, a conception which is in harmony with the view I gave you just now from the side of animal physiology, of catalyzing framework and interstitial catalyzable material.

To Prof. Sachs, this porous constitution of protoplasm serves to explain the property of vital turgescence—that is, its power of charging itself with aqueous liquid—a power which Sachs estimates to be so enormous that living protoplasm may, he believes, be able to condense water which it takes into its interstices to less than its normal volume. For our present purpose it is sufficient for us to understand that to the greatest botanical thinkers, as well as to the greatest animal physiologists, the ultimate mechanism by which life is carried on is not, as Prof. Sachs[2] puts it, "slime," but "a very distensible and exceedingly fine network."

And now let us try to get a step further by crossing back in thought from plants to animals. At first sight, the elementary vital processes of life seem more complicated in the animal than in the plant, but they are, on the contrary, simpler; for plant protoplasm, though it may be structurally homogeneous, is dynamically polyergic—it has many endowments—whereas in the animal organism there are cases in which a structure has only one function assigned to it. Of this the best examples are to be found among so-called excitable tissues, viz. those which are differentiated for the purpose of producing (along with heat) mechanical work, light, or electricity. In the life of the plant these endowments, if enjoyed at all, are enjoyed in common with others.

By the study, therefore, of muscle, of light organ, and of electrical organ, the vital mechanism is more accessible than by any other portal. About light organs we as yet know little, but the little we know is of value; of electrical organs rather more; about muscle a great deal.

To the case of muscle, Engelmann, one of the best observers and thinkers on the elementary questions which we have now before us, has transferred the terminology of Nägeli and Pfeffer as descriptive of the mechanism of its contraction. Muscular protoplasm differs from those kinds of living matter to which I have applied the term "polyergic," in possessing a molecular structure comparable with that of a crystal in the respect, that each portion of the apparently homogeneous and transparent material of which it consists resembles every other.

With this ultra-microscopical structure, its structure as investigated by the microscope may be correlated, the central fact being that, just as a muscular fibre can be divided into cylinders by cross-sections, so each such cylinder is made up of an indefinite number of inconceivably minute cylindrical parts, each of which is an epitome of the whole. These, Engelmann, following Pfeffer, calls ino-tagmata. So long as life lasts each minute phalanx has the power of keeping its axis parallel with those of its neighbours, and of so acting within its own sphere as to produce, whenever it is awakened from the state of rest to that of activity, a fluxion from poles to equator. In other words, muscle, like plant protoplasm, consists of a stable framework of living catalyzing substance, which governs the mechanical and chemical changes which occur in the interstitial catalyzable material, with this difference, that here the ultra-microscopical structure resembles that of a uniaxial crystal,[3] whereas in plant protoplasm there may be no evidence of such arrangement.

According to this scheme of muscular structure, the contraction, *i.e.* the change of form which, if allowed, a muscle undergoes when stimulated, has its seat not in the system of tagmata but in the interstitial material which surrounds it, and consists in the migration of that labile material from pole to equator, this being synchronous with explosive oxidation, sudden disengagement of heat and change in the electrical state of the living substance. Let us now see how far the scheme will help us to an understanding of this marvellous concomitance of chemical, electrical, and mechanical change.

It is not necessary to prove to you that the discharge of carbon dioxide and the production of heat which we know to be associated with that awakening of a muscle to activity which we call stimulation, are indices of oxidation. If we take this fact in connection with the view that has just been given of the mechanism of contraction, it is obvious that there must be in the sphere of each tagma an accumulation of oxygen and oxidizable material, and that concomitantly with or antecedently to the migration of liquid from pole to equator, these must come into encounter. Let us for a moment suppose that a soluble carbohydrate is the catalyzable material, that this is accumulated equatorially, and oxygen at the poles, and consequently that between equator and poles water and carbon dioxide, the only products of the explosion, are set free. That the process is really of this nature is the conclusion to which an elaborate study of the electrical phenomena which accompany it has led one of the most eminent physiologists of the present time, Prof. Bernstein.[1] To this I wish for a moment to ask your attention.

Prof. Bernstein's view of the molecular structure of muscular protoplasm is in entire accordance with the theory of Pflüger and with the scheme of Engelmann, with this addition, that each ino-tagma is electrically polarized when in a state of rest, depolarized at the moment of excitation or stimulation, and that the axes of the tagmata are so directed that they are always parallel to the surface of the fibre, and consequently have their positive sides exposed. In this amended form the theory admits of being harmonized with the fundamental facts of muscle-electricity—namely, that cut surfaces are negative to sound surfaces, and excited parts to inactive—provided that the direction of the hypothetical polarization is from equator to pole, *i.e.* that in the resting state the poles of each tagma are charged with negative ions, the equators with positive; and consequently that the direction of the discharge in the catalyte at the moment that the polarization disappears is from pole to equator.

Time forbids me even to attempt to explain how this theory enables us to express more consistently the accepted explanations of many collateral phenomena, particularly those of electrotonus. I am content to show you that it is not impossible to regard the three phenomena—viz. chemical explosion, sudden electrical change, and change of form—as all manifestations of one and the same process—as products of the same mechanism.

In plants, in certain organs or parts in which movement takes place, as in muscles in response to stimulation, the physiological conditions are the same or similar, but the structural very different; for the effect is produced not by a change of form, but by a diminution of volume of the excited part, and this consists not of fibres, but of cells. The way in which the diminution of volume of the whole organ is brought about is by diminution of the volume of each cell, an effect which can obviously be produced by flow of liquid out of the cell. At first sight therefore the differences are much more striking than the resemblances.

But it is not so in reality, for the more closely we fix our attention on the elementary process rather than on the external form, the stronger appears the analogy—the more complete the correspondence. The state of turgor, as it has been long called by botanical physiologists, by virtue of which the framework of the protoplasm of the plant retains its content with a tenacity to which I have already referred, is the analogue of the state of polarization of Bernstein. As regards its state of aggregation, it can scarcely be doubted that, inasmuch as the electrical concomitants of excitation of the plant cell so closely correspond with those of muscle, here also the tagmata are cylindrical, and have their axes parallel to each other. Beyond this we ought perhaps not to allow speculation to carry us, but it is scarcely possible to refrain from connecting this inference with the streaming motion of protoplasm which in living plant cells is one of the indices of vitality. If, as must I think be supposed, this movement is interstitial, *i.e.* due to the mechanical action of the moving protoplasm on itself, we can most readily understand its mechanism as consisting in rhythmically recurring phases of close and open order in the direction of the tagmatic axes.

In submitting this hypothesis I do not for a moment forget that the facts relating to the contractility of plant cells have as yet been insufficiently investigated. No one has as yet shown that when the leaf of the sensitive plant falls, or that of the fly-trap closes on its prey, heat is developed or oxidation takes

[1] Pfeffer, "Pflanzenphysiologie," p. 12 (Leipzig, 1881).
[2] Sachs, "Experimental-Physiologie," p. 443 (1865).
[3] Brücke, "Vorlesungen," second edition, vol. ii. p. 497.

[1] Bernstein, "Neue Theorie der Erregungsvorgänge und electrischen Erscheinungen an den Nerven- und Muskelfasern," *Untersuchungen aus dem Physiologischen Institut* (Halle, 1888).

place, but it does not seem to me very rash to anticipate that if it were possible to make the experiment to-morrow it would be found to be so.

I have thus endeavoured (building on two principles in physiology, firstly that of the constant correlation of mechanism and action, of structure and function, and secondly the identity of plant and animal life both as regards mechanism and structure; and on two experimentally ascertained elementary relations, viz. the relation of living matter or protoplasm to water on the one hand, and to oxygen and food on the other) to present to you in part the outline or sketch of what might, if I had time to complete it, be an adequate conception of the mechanism and process of life as it presents itself under the simplest conditions. To complete this outline, so far as I can to-day, I have but one other consideration to bring before you, one which is connected with the last of my four points of departure—that of the relation of oxygen to protoplasm, a relation which springs out of the avidity with which, without being oxidized or even sensibly altered in chemical constitution, it seizes upon oxygen and stores it for its own purposes. The consideration which this suggests is that if the oxygen and oxidizable material are constantly stored, they must either constantly or at intervals be discharged, and inasmuch as we know that in every instance without exception in which heat is produced or work is done, these processes have discharge of water and of carbon dioxide for their concomitants, we are justified in regarding these discharges as the sign of expenditure, the charging with oxygen as the sign of restitution. In other words, a new characteristic of living process springs out of those we have already had before us—namely, that it is a constantly recurring alternation of opposite and complementary states, that of activity or discharge, that of rest or restitution.

Is it so, or is it not? In the minds of most physiologists the distinction between the phenomena of discharge and the phenomena of restitution (*Erholung*) is fundamental, but beyond this, unanimity ceases. Two distinguished men, one in Germany and one in England—I refer to Prof. Hering and Dr. Gaskell—have taken, on independent grounds, a different view to the one above suggested, according to which, life consists, not of alternations between rest and activity, charge and discharge, loading and exploding, but between two kinds of activity, two kinds of explosion, which differ only in the direction in which they act, in the circumstance that they are antagonistic to each other.

Now when we compare the two processes of rest, which as regards living matter means restitution, and discharge, which means action, with each other, they may further be distinguished in this respect, that, whereas restitution is autonomic, *i.e.* goes on continuously like the administrative functions of a well-ordered community, the other is occasional, *i.e.* takes place only at the suggestion of external influences; that, in other words, the contrast between action and rest is (in relation to protoplasm) essentially the same as between waking and sleeping.

It is in accordance with this analogy between the alternation of waking and sleeping of the whole organism, and the corresponding alternation of restitution and discharge, of every kind of living substance, that physiologists by common consent use the term Stimulus (*Reiz, Prikkeling*), meaning thereby nothing more than that it is by external disturbing or interfering influence of some kind that energies stored in living material are (for the most part suddenly) discharged. Now, if I were to maintain that restitution is not autonomic, but determined, as waking is, by an external stimulus—that it differs from waking only in the direction in which the stimulation acts, *i.e.* in the tendency towards construction on the one hand, towards destruction on the other—I should fairly and as clearly as possible express the doctrine which, as I have said, the two distinguished teachers I have mentioned, viz. Dr. Gaskell[1] and Prof. Hering, have embodied in words which have now become familiar to every student. The words in question, "anabolism," which being interpreted means winding up, and "catabolism," running down, are the creation of Dr. Gaskell. Prof. Hering's equivalents for these are "assimilation," which, of course, means storage of oxygen and oxidizable material, and "disassimilation," discharge of these in the altered form of carbon dioxide and water. But the point of the theory which attaches to them lies in this, that that wonderful power which living material enjoys of continually building itself up out of its environment, is, as I have already suggested, not autonomic, but just as dependent on occasional and external influences or stimuli, as we know the disintegrating processes to be; and accordingly Hering finds it necessary to include under the term stimuli not only those which determine action, but to create a new class of stimuli which he calls *Assimilations-Reize*, those which, instead of waking living mechanism to action, provoke it to rest.

It is unfortunately impossible within the compass of an address like the present to place before you the wide range of experimental facts which have led two of the strongest intellects of our time to adopt a theory which, when looked at *a priori*, seems so contradictory. I must content myself with mentioning that Hering was led to it chiefly by the study of one of the examples to which I referred in my introduction—namely, the colour-discriminating functions of the retina; Dr. Gaskell by the study of that very instructive class of phenomena which reveal to us that among the channels by which the brain maintains its sovereign power as supreme regulator of all the complicated processes which go on in the different parts of the animal organism, there are some which convey only commands to action, others commands to rest, the former being called by Gaskell catabolic, the latter anabolic. To go further than this would not only wear out your patience but would carry me beyond the limits I proposed to myself, viz. the mechanism of life in its simplest aspects. I therefore leave the subject here, adding one word only. The distinction which has suggested to their authors the words on which I have been commenting is a real one, but it implies rather the interference with each other of the simultaneous operation of two regulating mechanisms, than an antagonism between two processes of opposite tendencies carried on by the same mechanism; or, putting it otherwise, that the observed antagonism is between one nervous mechanism and another, and not between two antagonistic functions of the same living material.

Without attempting to recapitulate, I have a word to say by way of conclusion on a question which may probably have suggested itself to some of my audience.

I have indicated to you that although scientific thought does not, like speculative, oscillate from side to side, but marches forward with a continued and uninterrupted progress, the stages of that progress may be marked by characteristic tendencies; and I have endeavoured to show that in physiology the questions which concentrate to themselves the most lively interest are those which lie at the basis of the elementary mechanism of life.

The word Life is used in physiology in what, if you like, may be called a technical sense, and denotes only that state of *change with permanence* which I have endeavoured to set forth to you. In this restricted sense of the word, therefore, the question "What is Life?" is one to which the answer is approachable; but I need not say that in a higher sense—higher because it appeals to higher faculties in our nature—the word suggests something outside of mechanism, which may perchance be its cause rather than its effect.

The tendency to recognize such a relation as this is what we mean by vitalism. At the beginning of this discourse I referred to the anti-vitalistic tendency which accompanied the great advance of knowledge that took place at the middle of the century. But even at the height of this movement there was a reaction towards vitalism, of which Virchow,[1] the founder of modern pathology, was the greatest exponent. Now, a generation later, a tendency in the same direction is manifesting itself in various quarters. What does this tendency mean? It has to my mind the same significance now that it had then. Thirty years ago the discovery of the cell as the basis of vital function was new, and the mystery which before belonged to the organism was transferred to the unit, which while it served to explain everything was itself unexplained. The discovery of the cell seemed to be a very close approach to the mechanism of life, but now we are striving to get even closer, and with the same result. Our measurements are more exact, our methods finer; but these very methods bring us to close quarters with phenomena which, although within reach of exact investigation, are as regards their essence involved in a mystery which is the more profound the more it is brought into contrast with the exact knowledge we possess of surrounding conditions.

If what I have said is true, there is little ground for the apprehension that exists in the minds of some that the habit of

[1] See Gaskell in *Ludwig's Festschrift*, and Hering, "Zur Theorie der Vorgänge in der lebendigen Substanz," pp. 1–22 (Prag, 1888).

[1] Virchow, "Alter und Neuer Vitalismus," *Archiv für pathol. Anat* 1856, vol. ix. p. 1. See also Rindfleisch, "Ärztliche Philosophie," pp. 10–1 Wurzburg, 1888).

scrutinizing the mechanism of life tends to make men regard what can be so learned as the only kind of knowledge. The tendency is now certainly rather in the other direction. What we have to guard against is the mixing of two methods, and so far as we are concerned the intrusion into our subject of philosophical speculation. Let us willingly and with our hearts do homage to "divine Philosophy," but let that homage be rendered outside the limits of our science. Let those who are so inclined, cross the frontier and philosophize; but to me it appears to be more conducive to progress that we should do our best to furnish professed philosophers with such facts relating to structure and function as may serve them as aids in the investigation of those deeper problems which concern man's relations to the past, the present, and the unknown future.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY PROF. SIR WILLIAM TURNER, M.B., LL.D., F.R.SS.L. & E., PRESIDENT OF THE SECTION.

TWENTY-SIX years have passed by since the British Association for the Advancement of Science last assembled in this city. Many of the incidents of that meeting are still fresh in my memory, the more vividly, perhaps, because it was the first meeting of the Association that I had attended. The weather, so important a factor in most of our functions, was dry and bright. The visitor, instead of being enshrouded in that canopy of mist and smoke which so often meets the traveller as he approaches your city, was greeted with light and sunshine. The cordial welcome and reception so freely granted by the community, and more especially the princely yet gracious hospitality exercised by the President, your eminent townsman, now Lord Armstrong, are all deeply imprinted on my memory. But, apart from these attractions, which added so much to the amenities of the occasion, the meeting was one of deep interest to all those Members and Associates who were engaged in biological study.

Lyell's famous book on the "Antiquity of Man" had been published shortly before. The essays on the "Origin of Species" by natural selection, by Charles Darwin and Alfred Russel Wallace, had appeared only five years earlier in the Journal of the Linnean Society, and in 1859 Darwin's treatise on the "Origin of Species," in which its illustrious author summarized the facts he had collected and the conclusions at which he had arrived, had been published. Although no President of the British Association had up to that time given his adhesion to the new theory, yet it was clear that men were beginning to see, in many instances perhaps only dimly, how the theory of evolution by natural selection was destined to work a remarkable change, amounting almost to a revolution, in our conceptions of biological questions generally, and their applicability to the study of man.

At that time Anthropology had not assumed so definite a position in the work of the Association as it now possesses. Neither a Department nor a Section was devoted to it, and the subjects which it embraces were scattered abroad, either in the Department of Anatomy and Physiology, in the Section of Geography and Ethnology, in that of Geology, or in that of Statistics. It is true that a vigorous attempt was made about that time to give it a more independent position, but it was not until the Association met in Nottingham, in 1866, that it was assigned a definite Department, and at the Montreal meeting, in 1884, Anthropology assumed the dignity of a Section.

But although the youngest Section of the Association, the Science of Man is not the youngest of the sciences. Long before the British Association came into existence, Man, in his physical, racial, geological, and psychological aspects, had been studied by hosts of able and industrious inquirers. All that the Association has done in establishing a special Section of Anthropological Science has been to bring together, as it were, into a single focus all those workers who apply themselves to the study of man in his various aspects.

As presiding over the proceedings of the Section on this occasion, it is a part of my duty to open its public business with an address. For me, as doubtless for many of those who have preceded me in this honourable office, one's mind has been somewhat exercised in the choice of a subject. In a branch of biological science so vast as Anthropology, in which the room for selection is so ample, the difficulty of making a choice is perhaps still further increased. As a professional anatomist, whose life's work it has been to study the structure of the human body in its normal aspects, to inquire into the variations which it exhibits in different individuals, and to compare its structure with that of various forms of animal life, it at first occurred to me that an address on the physical characteristics of some of the races of men would be appropriate. But further consideration led me to think that such a subject would be too technical for a general audience, and that it might perhaps be productive of greater interest on the part of my auditors if I selected a topic which, whilst strictly scientific in all its bearings, yet appeals more distinctly to the popular mind, and is now attracting attention. Hence I have chosen the subject of Heredity, by which I mean that special property through which the peculiarities of an organism are transmitted to its descendants throughout successive generations, so that the offspring, in their main features, resemble their parents.

The subject of Heredity, if I may say so, is in the air at the present time. The journals and magazines, both scientific and literary, are continually discussing it, and valuable treatises on the subject are appearing at frequent intervals. But though so important a topic of existing scientific thought and speculation, it is by no means a new subject, and certain of its aspects were under discussion so far back as the time of Aristotle. The prominence which it has assumed of late years is in connection with its bearing on the Darwinian Theory of Natural Selection, and, consequently, biologists generally have had their attention directed to it. But in its relations to Man, his structure, functions, and diseases, it has long occupied a prominent position in the minds of anatomists, physiologists, and physicians. That certain diseases, for example, are hereditary was recognized by Hippocrates, who stated generally that hereditary diseases are difficult to remove, and the influence which the hereditary transmission of disease exercises upon the duration of life is the subject of a chapter in numerous works on practical medicine, and forms an important element in the valuation of lives for life insurance.

The first aspect of the question which has to be determined is whether any physical basis can be found for Heredity. Is there any evidence that the two parents contribute each a portion of its substance to the production of the offspring so that a physical continuity is established between successive generations? The careful study, especially during the last few years, of the development of a number of species of animals mostly but not exclusively among the Invertebrata, by various observers, of whom I may especially name Bütschli, Fol, E. Van Beneden, and Hertwig, has established the important fact that the young animal arises by the fusion within the egg or germ-cell of an extremely minute particle derived from the male parent with an almost equally minute particle derived from the germ-cell produced by the female parent. These particles are technically termed in the former case the *male pronucleus*, in the latter the *female pronucleus*, and the body formed by their fusion is called the *segmentation nucleus*. These nuclei are so small that it seems almost a contradiction in terms to speak of their magnitude; rather one might say their minimitude, for it requires the higher powers of the best microscopes to see them and follow out the process of conjugation. But notwithstanding their extreme minuteness, the pronuclei and the segmentation nucleus are complex both in chemical and molecular structure. From the segmentation nucleus produced by the fusion of the pronuclei with each other, and from corresponding changes which occur in the protoplasm of the egg which surrounds it, other cells arise by a process of division, and these in their turn also multiply by division. These cells arrange themselves in course of time into layers which are termed the germinal or embryonic layers. From these layers arise all the tissues and organs of the body, both in its embryonic and adult stages of life. The starting-point of each individual organism—*i.e.* of each new generation—is therefore the segmentation nucleus. Every cell in the adult body is derived by descent from that nucleus through repeated division. As the segmentation nucleus is formed by the fusion of material derived from both parents, a physical continuity is established between parents and offspring. But this physical continuity carries with it certain properties which cause the offspring to reproduce, not only the bodily configuration of the parent, but other characters. In the case of Man we find along with the family likeness in form and features a correspondence in temperament and disposition, in the habits and mode of life, and sometimes in the tendency to particular diseases. This transmission of characters from parent to offspring is summarized in the well-known expression that "like begets like," and it rests upon a physical basis.

The size of the particles which are derived from the parents, called the male and female pronuclei, the potentiality of which is so utterly out of proportion to their bulk, is almost inconceivably small when compared with the magnitude of the adult body. Further, by the continual process of division of the cells, the substance of the segmentation nucleus is diffused throughout the body of the new individual produced through its influence, so that each cell contains but an infinitesimal particle of it. The parental dilution, if I may so say, is so attenuated as to surpass the imagination of even the most credulous believer in the attenuation of drugs by dilution. And yet these particles are sufficient to stamp the characters of the parents, of the grandparents, and of still more remote ancestors on the offspring, and to preserve them throughout life, notwithstanding the constant changes to which the cells forming the tissues and organs of the body are subjected in connection with their use and nutrition. So marvellous, indeed, is the whole process, that even the exact contributions to recent knowledge on the fusion of the two pronuclei, instead of diminishing our wonder, have intensified the force of the expression "*magnum hereditatis mysterium*."

In considering the question of how new individuals are produced, one must keep in mind that it is not every cell in the body which can act as a centre of reproduction for a new generation, but that certain cells, which we name germ-cells and sperm-cells, are set aside for that purpose. These cells, destined for the production of the next generation, form but a small proportion of the body of the animal in which they are situated. They are, as a rule, marked off from the rest of the cells of its body at an early period of development. The exact stage at which they become specially differentiated for reproductive purposes varies, however, in different organisms. In some organisms, as is said by Balbiani to be the case in *Chironomus*, they apparently become isolated before the formation of the germinal layers is completed; but, as a rule, their appearance is later, and in the higher organisms not until the development of the body is relatively much more advanced.

The germ-cells after their isolation take no part in the growth of the organism in which they arise, and their chief association with the other cells of its body is that certain of the latter are of service in their nutrition. The problem, therefore, for consideration is the mode in which these germ or reproductive cells become influenced, so that after being isolated from the cells which make up the bulk of the body of the parent they can transmit to the offspring the characters of the parent organism. Various speculations and theories have been advanced by way of explanation. The well-known theory of Pangenesis, which Charles Darwin with characteristic moderation put forward as merely a provisional hypothesis, assumes that *gemmules* are thrown off from each different cell or unit throughout the body which retain the characters of the cells from which they spring; that the gemmules aggregate themselves either to form or to become included within the reproductive cells; and that in this manner they and the characters which they convey are capable of being transmitted in a dormant state to successive generations, and to reproduce in them the likeness of their parents, grandparents, and still older ancestors.

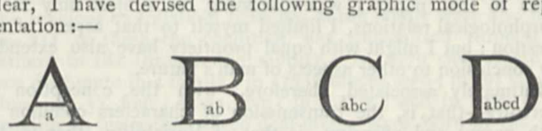
In 1872, and four years afterwards, in 1876, Mr. Francis Galton published most suggestive papers on Kinship and Heredity (Proc. Roy. Soc. Lond., 1872, and Journ. Anthrop. Inst., vol. v., 1876). In the latter of these papers he developed the idea that "the sum-total of the germs, gemmules, or whatever they may be called," which are to be found in the newly fertilized ovum, constitute a *stirp*, or root; that the germs which make up the stirp consist of two groups—the one which develops into the bodily structure of the individual, and which constitutes, therefore, the personal structure; the other, which remains latent in the individual, and forms, as it were, an undeveloped residuum; that it is from these latent or residual germs that the sexual elements intended for producing the next generation are derived, and that these germs exercise a predominance in matters of heredity; further, that the cells which make up the personal structure of the body of the individual exercise only in a very faint degree any influence on the reproductive cells, so that any modifications acquired by the individual are barely, if at all, inherited by the offspring.

Subsequent to the publication of Mr. Galton's essays, valuable contributions to the subject of Heredity have been made by Profs. Brooks, Jaeger, Naegeli, Nussbaum, Weismann, and others. Prof. Weismann's theory of Heredity embodies the same fundamental idea as that propounded by Mr. Galton; but as he has employed in its elucidation a phraseology which is more in harmony with that generally used by biologists, it has had more immediate attention given to it. As Weismann's essays have, during the present year, been translated for and published by the Clarendon Press (Oxford, 1889), under the editorial superintendence of Messrs. Poulton, Schönland, and Shipley, they are now readily accessible to all English readers.

Weismann asks the fundamental question, "How is it that a single cell of the body can contain within itself all the hereditary tendencies of the whole organism?" He at once discards the theory of pangenesis, and states that in his belief the germ-cell, so far as its essential and characteristic substance is concerned, is not derived at all from the body of the individual in which it is produced, but directly from the parent germ-cell from which the individual has also arisen. He calls his theory the *continuity of the germ-plasm*, and he bases it upon the supposition that in each individual a portion of the specific germ-plasm derived from the germ-cell of the parent is not used up in the construction of the body of that individual, but is reserved unchanged for the formation of the germ-cells of the succeeding generation. Thus, like Mr. Galton, he recognizes that in the stirp or germ there are two classes of cells destined for entirely distinct purposes: the one for the development of the *soma* or body of the individual, which class he calls the *somatic* cells; the other for the perpetuation of the species, *i.e.* for reproduction.

In further exposition of his theory Weismann goes on to say, as the process of fertilization is attended by a conjugation of the nuclei of the reproductive cells—the pronuclei referred to in an earlier part of this address—that the nuclear substance must be the sole bearer of hereditary tendencies. The two uniting nuclei would contain the germ-plasms of the parents, and this germ-plasm also would contain that of the grandparents as well as that of all previous generations.

To make these somewhat abstract propositions a little more clear, I have devised the following graphic mode of representation:—



Let the capital letters A, B, C, D, &c., express a series of successive generations. Suppose A to be the starting-point, and to represent the somatic or personal structure of an individual; then *a* may stand for the reproductive cells, or germ-plasm, from which the offspring of A, viz. B, is produced. B, like A, has both a personal structure and reproductive cells or germ-plasm, the latter of which is represented by the letters *ab*, which are intended to show that whilst belonging to B they have a line of continuity with A. C stands for an individual of the third generation, in which the reproductive plasm is indicated by *abc*, to express that, though within the body of C, the germ-plasm is continuous with that of both *b* and *a*. D also contains the reproductive cells, *abcd*, which are continuous with the germ-plasm of the three preceding generations, and so on.

It follows, therefore, from this theory that the germ-plasm possesses throughout the same complex chemical and molecular structure, and that it would pass through the same stages when the conditions of development are the same, so that the same final product would arise. Each successive generation would have therefore an identical starting-point, so that an identical product would arise from all of them.

Weismann does not absolutely assert that an organism cannot exercise a modifying influence upon the germ-cells within it; yet he limits this influence to such slight effect as that which would arise from the nutrition and growth of the individual, and the reaction of the germ-cell upon changes of nutrition caused by alteration in growth at the periphery, leading to some change in the size, number, and arrangements of its molecular units. But he throws great doubt upon the existence of such a reaction, and he, more emphatically than Mr. Galton, argues against the idea that the cells which make up the somatic or personal structure of the individual exercise any influence on the reproductive cells. From his point of view the structural or other properties which characterize a family, a race, or a species are derived solely from the reproductive cells through continuity of their germ-plasm, and are not liable to modification by the action on them of the organs or tissues of the body of the individual organism in which they are situated. To return for one moment to my graphic illustration in elucidation of this part of the theory. The cells which make up the personal structure of A or B would exercise no effect upon the character of the reproductive cells *a* or *ab* contained within them. These latter would not be modified or changed in their properties by the action of the individual organism A or B. The individual B would be in hereditary descent, not from A + *a*, but only from *a*, with which its germ-plasma *ab* would be continuous, and through which the properties of the family, race, or species would be transmitted to C, and so on to other successive generations.

The central idea of Heredity is permanency; that like begets like, or, as Mr. Galton more fitly puts it, that "like *tends* to produce like." But though the offspring conform with their parents in all their main characteristics, yet, as everyone knows, the child is not absolutely like its parents, but possesses its own character, its own individuality. It is easy for anyone to recognize that differences exist amongst men when he compares one individual with another; but it is equally easy for those who make a special study of animals to recognize individual differences in them also. Thus a pigeon or canary fancier distinguishes without fail the various birds in his flock, and a shepherd knows every sheep under his charge. But the anatomist tells us that these differences are more than superficial—that they also pervade the internal structure of the body. In a paper which I read to the meeting of this Association in Birmingham so long ago as 1865,[1] after relating a series of instances of variation in structure observed in the dissections of a number of human bodies, I summarized my conclusion as follows: "Hence, in the development of each individual, a morphological specialization occurs both in internal structure and external form by which distinctive characters are conferred, so that each man's structural individuality is an expression of the sum of the individual variations of all the constituent parts of his frame."

As in that paper I was discussing the subject only in its morphological relations, I limited myself to that aspect of the question; but I might with equal propriety have also extended my conclusion to other aspects of man's nature.

Intimately associated, therefore, with the conception of Heredity—that is, the transmission of characters common to both parent and offspring—is that of Variability—that is, the appearance in an organism of certain characters which are unlike those possessed by its parents. Heredity, therefore, may be defined as the perpetuation of the like; Variability, as the production of the unlike.

And now we may ask, Is it possible to offer any feasible explanation of the mode in which variations in organic structure take their rise in the course of development of an individual organism? Anything that one may say on this head is of course a matter of speculation, but certain facts may be adduced as offering a basis for the construction of an hypothesis, and on this matter Prof. Weismann makes a number of ingenious suggestions.

Prior to the conjugation of the male and female pronuclei to form the segmentation nucleus a portion of the germ-plasm is extruded from the egg to form what are called the *polar bodies*. Various theories have been advanced to account for the significance of this curious phenomenon. Weismann explains it on the hypothesis that a reduction of the number of ancestral germ-plasms in the nucleus of the egg is a necessary preparation for fertilization and for the development of the young animal. He supposes that by the expulsion of the polar bodies one-half the number of ancestral germ-plasms is removed, and that the original bulk is restored by the addition of the male pronucleus to that which remains. As precisely corresponding molecules of this plasm need not be expelled from each ovum, similar ancestral plasms are not retained in each case; so that diversities would arise even in the same generation and between the offspring of the same parents.

Minute though the segmentation nucleus is, yet microscopic research has shown that it is not a homogeneous structureless body, but is built up of different parts. Most noteworthy are

[1] Transactions of Sections, p. 111, 1865, and Trans. Roy. Soc. Edinburgh, vol. xxiv., 1865.

the presence of extremely delicate threads or fibrils, called the *chromatin filaments*, which are either coiled on each other, or intersect to form a network-like arrangement. In the meshes of this network a viscous—and, so far as we yet know, structureless—substance is situated. Before the process of division begins in the segmentation nucleus these filaments swell up and then proceed to arrange themselves at first into one and then into two star-like figures before the actual division of the nucleus takes place.[1] It is obvious, therefore, that the molecules which enter into the formation of the segmentation nucleus can move within its substance, and can undergo a readjustment in size and form and position. But this readjustment of material is, without doubt, not limited to those relatively coarse particles which can be seen and examined under the microscope, but applies to the entire molecular structure of the segmentation nucleus. Now it must be remembered that the cells of the embryo from which all the tissues and organs of the adult body are derived are themselves descendants of the segmentation nucleus, and they will doubtless inherit from it both the power of transmitting definite characters and a certain capacity for readjustment both of their constituent materials and the relative positions which they may assume towards each other. One might conceive, therefore, that if in a succession of organisms derived from common ancestors the molecular particles were to be of the same composition and to arrange themselves in the segmentation nucleus and in the cells derived from it on the same lines, these successive generations would be alike; but if the lines of adjustment and the molecular constitution were to vary in the different generations, then the products would not be quite the same. Variations in structure, and to some extent also in the construction of parts, would arise, and the unlike would be produced.

In this connection it is also to be kept in mind that in the higher organisms, and, indeed, in multicellular organisms generally, an individual is derived, not from one parent only, but from two parents. Weismann emphasizes this combination as the cause of the production of variations and the transmission of hereditary individual characters. If the proportion of the particles derived from each parent and the forces which they exercise were precisely the same in any individual case, then one could conceive that the product would be a mean of the components provided by the two parents. But if one parent were to contribute a larger proportion than the other to the formation of a particular organism, then the balance would be disturbed, the offspring in its character would incline more to one parent than to the other, according to the proportion contributed by each, and a greater scope for the production of variations would be provided. These differences would be increased in number in the course of generations, owing to new combinations of individual characters arising in each generation.

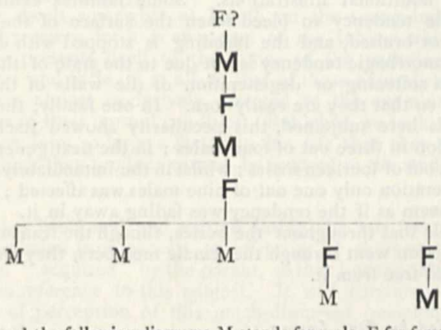
As long as the variations which are produced in an organism are collectively within a certain limitation, they are merely individual variations, and express the range within which such an organism, though exhibiting differences from its neighbours, may yet be classed along with them in the same species. It is in this sense that I have discussed the term Variability up to the present stage of this address. Thus all those varieties of mankind which, on account of differences in the colour of the skin, we speak of as the white, black, yellow races and red-skins are men, and they all belong to that species which the zoologists term *Homo sapiens*.

But the subject of Variability cannot, in the present state of science, be confined in its discussion to the production of individual variations within the limitations of a common species. Since Charles Darwin enunciated the proposition that favourable variations would tend to be preserved, and unfavourable ones to be destroyed, and that the result of this double action, by the accumulation of minute existing differences, would be the formation of new species by a process of natural selection, this subject has attained a much wider scope, has acquired increased importance, and has formed the basis of many ingenious speculations and hypotheses. As variations, when once they have arisen, may be hereditarily transmitted, the Darwinian theory might be defined as Heredity modified and influenced by Variability.

This is not the place to enter on a general discussion of the Darwinian theory, and even if it were, the time at our disposal

[1] The observations more especially of Flemming, E. Van Beneden, Strasburger, and Carnoy may be referred to in connection with the changes which take place in nuclei prior to and in connection with their division.

would not admit of it. But there are some aspects of the theory which would need to be referred to in connection with the subject now before us. It may be admitted that many variations which may arise in the development of an individual, and which are of service to that individual, would tend to be preserved and perpetuated in its offspring by hereditary transmission. But it is also without question that variations which are of no service, and, indeed, are detrimental, to the individual in which they occur, are also capable of being hereditarily transmitted. This statement is amply borne out in the study of those important defects in bodily structure which pathologists group together under the name of Congenital Malformations. I do not require to go into much detail on this head, or to cite cases in which the congenital defect can only be exposed by dissection, but may refer, by way of illustration, to one or two examples in which the defect is visible on the surface of the body. The commonest form of malformation the hereditary transmission of which has been proved is where an increase in the number of digits on the hands or feet, or on both, occurs in certain families, numerous instances of which have now been put on record. But in other families there is an hereditary tendency to a diminution in the number of digits or to a defect in the development of those existing. I may give an illustration which occurred in the family of one of my pupils, the deformity in which consisted in a shortening or imperfect growth of the metacarpal bone of the ring finger of the left hand, so that the length of that finger was much below the normal. This family defect was traceable throughout six generations, and perhaps even in a seventh, and was, as a rule, transmitted alternately from the males to the females of the family (*Journ. Anat. and Phys.*, vol. xviii. p. 463)—

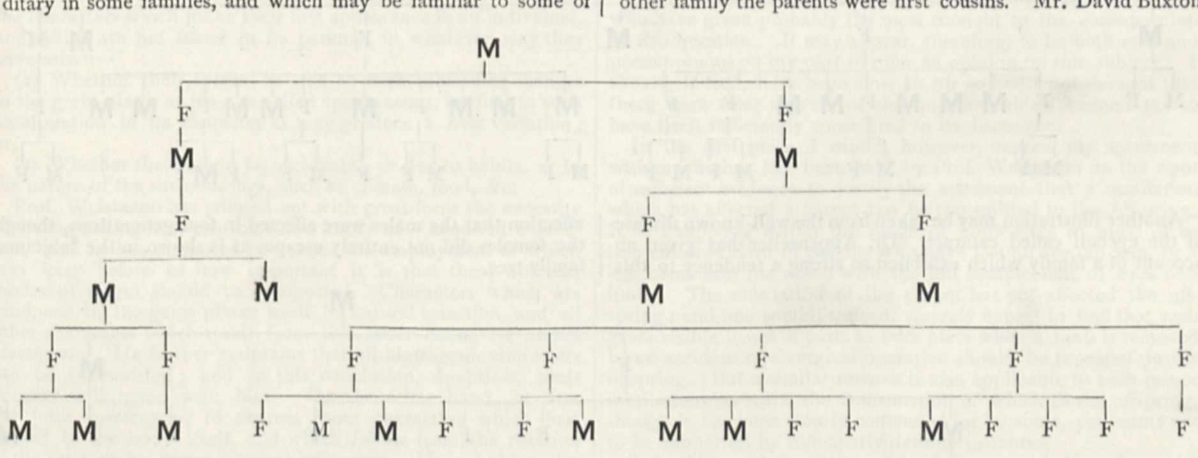


In this and the following diagrams M stands for male, F for female, whilst the block type (**M** or **F**) marks the individual or generation in which the variation occurred.

Another noticeable deformity which is known to be hereditary in some families, and which may be familiar to some of my auditors, is that of imperfect development of the upper lip and roof of the mouth, technically known as hare-lip and cleft palate.

These examples illustrate what may be called the coarser kinds of hereditary deformity, where the redundancies or defects in parts of the body are so gross as at once to attract attention. But modifications or variations in structure that can be transmitted from parent to offspring are by no means limited to changes which can be detected by the naked eye. They are sometimes so minute as to be determined rather by the modifications which they occasion in the function of the organ than by the ready recognition of structural variations. One of the most interesting of these is the affection known as Daltonism, or colour-blindness, which has distinctly been shown to be hereditary, and which is due, apparently in the majority of cases, to a defect in the development of the retina, or of the nerve of sight which ends in it, though in some instances they may be occasioned by defective development of the brain itself. Dr. Horner has related a most interesting family history (cited in "Die Allgemeine Pathologie," by Dr. Edwin Klebs, Jena, 1887), in which the colour-blindness was traced through seven generations. In this family the males were the persons affected, though the peculiarity was transmitted through the females, who themselves remained unaffected. The family tree showed that in the sixth generation seven mothers had children. Their sons, collectively nine in number, were all colour-blind with the exception of one son, while none of their nine daughters showed the hereditary defect. (See diagram below.)

The eye is not the only organ of sense which exhibits a tendency to the production of hereditary congenital defects. The ear is similarly affected, and intimately associated with congenital deafness is an inability to speak articulately, which occasions the condition termed Deaf-mutism. Statisticians have given some attention to this subject, both as regards its relative frequency and its hereditary character. The writer of the article "Vital Statistics," in the Report of the Irish Census Commissioners during the decades ending 1851, 1861, 1871, has discussed at some length the subject of congenital deaf-mutism, and has produced a mass of evidence which proves that it is often hereditarily transmitted. In the Census Report for 1871 (vol. lxxii. Part II., "Report on the Status of Disease," p. 1, 1873), 3297 persons were returned as belonging to this class, and in 393 cases the previous or collateral branches of the family were also mute. In 211 of these the condition was transmitted through the father; in 182 through the mother. In 2579 cases there was one deaf-mute in a family; in 379 instances, two; in 191 families, three; in 53, four; in 21, five; in 5, six; and in each of two families no fewer than seven deaf-mutes were born of the same parents. In one of these two families neither hereditary predisposition nor any other probable physiological or pathological reason was assigned to account for the peculiarity, but in the other family the parents were first cousins. Mr. David Buxton,



who has paid great attention to this subject (*Liverpool Medico-Chirurg. Journ.*, July 1857; January 1859), states that the probability of congenital deafness in the offspring is nearly seven times greater when both parents are deaf than when only one is so; in the latter case the chance of a child being born deaf is less than three-quarters per cent.; in the former, the chances are that 5 per cent. of the children will be deaf-mutes. Mr. Buxton refers to several families where the deaf-mutism has been transmitted through three successive generations, though in some instances the affection passes over one generation to reappear in the next. He also relates a case of a family of sixteen persons, eight of whom were born deaf and dumb, and one at least of the members of which transmitted the affection to his descendants as far as the third generation. There can be little doubt that congenital deaf-mutism, in the great majority of instances, is associated with a defective development, and therefore a structural variation of the organ of hearing, though in some cases, perhaps, the defect may be in the development of the brain itself.

Although a sufficient number of cases has now been put on record to prove that in some families one or other kind of congenital deformity may be hereditarily transmitted, yet I do not wish it to be supposed that congenital malformations may not arise in individuals in whom no hereditary tendency can be traced. It is undoubtedly true that family histories are in many cases very defective, and frequently cannot be followed back for more than one, or, at the most, two generations; so that it is not unlikely that an hereditary predisposition may exist in many instances where it cannot be proved. Still, allowing even for a considerable proportion of such cases, a sufficient number will remain to warrant the statement that malformations or variations in structure which have not been displayed by their ancestors may arise in individuals belonging to a particular generation.

The variations which I have spoken of as congenital malformations arise, as a rule, before the time of birth, during the early development of the individual; but there is an important class of cases, in which the evidence for hereditary transmission is more or less strong, which may not exhibit their peculiarities until months, or even years, after the birth of the individual. This class is spoken of as hereditary diseases, and the structural and functional changes which they produce exercise most momentous influences. Sometimes these diseases may occasion changes in the tissues and organs of the body of considerable magnitude, but at other times the alteration is much more subtle, is molecular in its character, requires the microscope for its determination, or is even incapable of being recognized by that instrument.

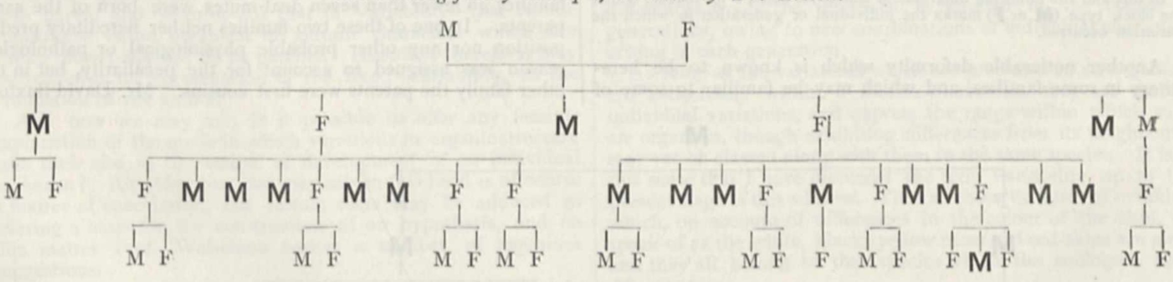
Had one been discussing the subject of hereditary disease twenty years ago, the first example probably that would have been adduced would have been tuberculosis, but the additions to our knowledge of late years throw some doubt upon its hereditary character. There can, of course, be no question that tubercular disease propagates itself in numerous families from generation to generation, and that such families show a special susceptibility or tendency to this disease in one or other of its forms. But whilst fully admitting the predisposition to it which exists in certain families, there is reason to think that the structural disease itself is not hereditarily transmitted, but that it is directly excited in each individual in whom it appears by a process of external infection due to the action of the tubercle bacillus. Still, if the disease itself be not inherited, a particular temperament which renders the constitution liable to be attacked by it is capable of hereditary transmission.

Sir James Paget,[1] when writing on the subject of cancer, gives statistics to show that about a quarter of the persons affected were aware of the existence of the same disease in other members of their family, and he cites particular instances in which cancer was present in two and even four generations. He had no doubt that the disease can be inherited—not, he says, that, strictly speaking, cancer or cancerous material is transmitted, but a tendency to the production of those conditions which will finally manifest themselves in a cancerous growth. The germ from the cancerous parent must be so far different from the normal as after the lapse of years to engender the cancerous condition.

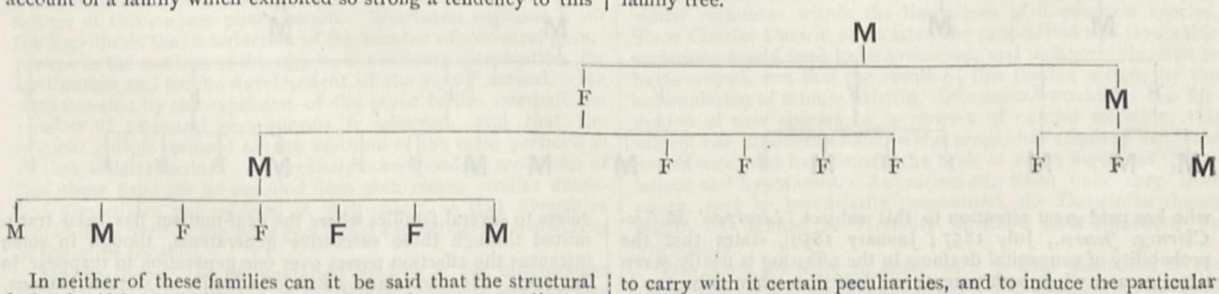
Heredity is also one of the most powerful factors in the production of those affections which we call gout and rheumatism. Sir Dyce Duckworth, the latest systematic writer on gout, states that in those families whose histories are the most complete and trustworthy the influence is strongly shown, and occurs in from 50 to 75 per cent. of the cases; further, that the children of gouty parents show signs of articular gout at an age when they have not assumed those habits of life and peculiarities of diet which are regarded as the exciting causes of the disease.

Some interesting and instructive family histories, in which the hereditary transmission of a particular disease through several generations has been worked out, are recorded by Prof. Klebs in his "Allgemeine Pathologie." I may draw from these one or two additional illustrations. Some families exhibit a remarkable tendency to bleed when the surface of the body is injured or bruised, and the bleeding is stopped with difficulty. The hæmorrhagic tendency is not due to the state of the blood, but to a softening or degeneration of the walls of the blood-vessels, so that they are easily torn. In one family, the tree of which is here subjoined, this peculiarity showed itself in one generation in three out of four males; in the next generation, in thirteen out of fourteen males; whilst in the immediately succeeding generation only one out of nine males was affected; so that it would seem as if the tendency was fading away in it. It is remarkable that throughout the series, though the transmission of the affection went through the female members, they themselves remained free from it.

The bleeding family Mampel, recorded by Dr. Lossen.



Another illustration may be taken from the well-known disease of the eyeball called cataract. Dr. Appenzeller has given an account of a family which exhibited so strong a tendency to this affection that the males were affected in four generations, though the females did not entirely escape, as is shown in the subjoined family tree.



In neither of these families can it be said that the structural lesion itself is transmitted, but that the tendency or predisposition to produce it is inherited. The germ-plasm, therefore, in these individuals must have been so modified from the normal as to carry with it certain peculiarities, and to induce the particular form of disease which showed itself in each family.

[1] "Lectures on Surgical Pathology," third edition, revised and edited by the author and W. Turner (London, 1870).

In connection with the tendency to the transmissibility of either congenital malformations or diseases, consanguinity in the parents, although by no means a constant occurrence, is a factor which in many cases must be taken into consideration.[1] If we could conceive both parents to be physiologically perfect, then it may be presumed that the offspring would be so also; but if there be a departure in one parent from the plane of physiological perfection, then it may safely be assumed that either the immediate offspring or a succeeding generation will display a corresponding departure in a greater or less degree. Should both parents be physiologically imperfect, we may expect the imperfections if they are of a like nature to be intensified in the children. It is in this respect, therefore, that the risk of consanguineous marriages arises, for no family can lay claim to physiological perfection.

When we speak of tendencies, susceptibilities, proclivities, or predisposition to the transmission of characters, whether they be normal or pathological, we employ terms which undoubtedly have a certain vagueness. We are as yet quite unable to recognize, by observation alone, in the germ-plasm any structural change which would enable us to say that a particular tendency or susceptibility will be manifested in an organism derived from it. We can only determine this by following out the life-history of the individual. Still it is not the less true that these terms express a something of the importance of which we are all conscious. So far as Man is concerned, the evidence in favour of a tendency to the transmission of both structural and functional modifications which are either of dis-service, or positively injurious, or both, is quite as capable of proof as that for the transmission of characters which are likely to be of service. Hence useless as well as useful characters may be selected and transmitted hereditarily.

I have dwelt somewhat at length on the transmissibility of useless characters, for it is an aspect of the subject which more especially presents itself to the notice of the pathologist and physician; and little, if at all, to that of those naturalists whose studies are almost exclusively directed to the examination of organisms in their normal condition. But when we look at Man, his diseases form so large a factor in his life that they and the effects which they produce cannot be ignored in the study of his nature.

Much has been said and written during the last few years of the transmission from parents to offspring of characters which have been "acquired" by the parent, so that I cannot altogether omit some reference to this subject. It will conduce to one's clearness of perception of this much-discussed question if one defines at the outset in what sense the term "acquired characters" is employed; and it is the more advisable that this should be done, as the expression has not always been used with the same signification. This term may be used in a wide or in a more restricted sense. In its wider meaning it may cover all the characters which make their first appearance in an individual, and which are not found in its parents, in whatever way they have arisen—

(1) Whether their origin be due to such molecular changes in the germ-plasm as may be called spontaneous, leading to such an alteration in its character as may produce a new variation; or,

(2) Whether their origin be accidental, or due to habits, or to the nature of the surroundings, such as climate, food, &c.

Prof. Weismann has pointed out with great force the necessity of distinguishing between these two kinds of "acquired characters," and he has suggested two terms the employment of which may keep before us how important it is that these different modes of origin should be recognized. Characters which are produced in the germ-plasm itself by natural selection, and all other characters which result from this latter cause, he names *blastogenic*. He further maintains that all blastogenic characters can be transmitted; and in this conclusion, doubtless, most persons will agree with him. On the other hand, he uses the term *somatogenic* to express those characters which first appear in the body itself, and which follow from the reaction of the *soma* under direct external influences. He includes under this head the effects of mutilation, the changes which follow from increased or diminished performance of function, those directly due to nutrition, and any of the other direct external influences which act upon the body. He further maintains that the somatogenic characters are not capable of transmission from parent to offspring, and he suggests that in future discussions on this subject the term "acquired characters" should be restricted to those which are somatogenic.

Thus one might say that blastogenic characters arising in the germ would be acquired in the individual by the action of the germ upon the soma; so that if we return again to the graphic illustration previously employed, the germ-plasm represented by the small italic letters *abcd* would act upon the soma represented by the capital letters A, B, C, D. Somatogenic characters, again, arising in the soma, would be acquired by the action of the soma A, B, C, D, upon the contained germ-plasm *abcd*. But whether those acquired characters expressed by the term somatogenic can or can not be transmitted has been fruitful of discussion.

That the transmission of characters so acquired can take place is the foundation of the theory of Lamarck, who imagined that the gradual transformation of species was due to a change in the structure of a part of an organism under the influence of new conditions of life, and that such modifications could be transmitted to the offspring. It was also regarded as of importance by Charles Darwin, who stated[1] that all the changes of corporeal structure and mental power cannot be exclusively attributed to the natural selection of such variations as are often called spontaneous, but that great value must be given to the inherited effects of use and disuse, some also to the modification in the direct and prolonged action of changed conditions of life, also to occasional reversions of structure. Herbert Spencer believes[2] that the natural selection of favourable varieties is not in itself sufficient to account for the whole of organic evolution. He attaches a greater importance than Darwin did to the share of use and disuse in the transmission of variations. He believes that the inheritance of functionally produced modifications of structure takes place universally, and that as the modification of structure by function is a *vera causa* as regards the individual, it is unreasonable to suppose that it leaves no traces in posterity.

On the other hand, there are very eminent authorities who contend that the somatogenic acquired characters are not transmissible from parent to offspring. Mr. Francis Galton, for example, gives a very qualified assent to this proposition. Prof. His, of Leipzig, doubts its validity. Prof. Weismann says that there is no proof of it. Mr. Alfred Russel Wallace, in his most recent work,[3] considers that the direct action of the environment, even if we admit that its effects on the individual are transmitted by inheritance, are so small in comparison with the amount of spontaneous variation of every part of the organism that they must be quite over-shadowed by the latter. Whatever other causes, he says, have been at work, natural selection is supreme to an extent which even Darwin himself hesitated to claim for it.

There is thus a conflict of opinion amongst the authorities who have given probably the most thought to the consideration of this question. It may appear, therefore, to be both rash and presumptuous on my part to offer an opinion on this subject. I should, indeed, have been slow to do so had I not thought that there were some aspects of the question which seemed not to have been sufficiently considered in its discussion.

In the first place, I would, however, express my agreement with much that has been said by Prof. Weismann on the want of sufficient evidence to justify the statement that a mutilation which has affected a parent can be transmitted to the offspring. It is, I suppose, within the range of knowledge of most of us that children born of parents who have lost an eye, an arm, or a leg, come into the world with the full complement of eyes and limbs. The mutilation of the parent has not affected the offspring; and one would, indeed, scarcely expect to find that such gross visible losses of parts as take place when a limb is removed by an accident or a surgical operation should be repeated in the offspring. But a similar remark is also applicable to such minor mutilations as scars, the transmission of which to the offspring, though it has been stoutly contended for by some, yet seems not to be supported by sufficiently definite instances.

I should search for illustrations of the transmission of somatogenic characters in the more subtle processes which affect living organisms, rather than those which are produced by violence

[1] I may especially refer for a discussion of this subject to an admirable essay, by Sir Arthur Mitchell, K.C.B., "On Blood Relationship in Marriage considered in its Influence upon the Offspring."

[1] Preface to second edition of "Descent of Man," 1885; also "Origin of Species," first edition.

[2] "Factors of Organic Evolution," *Nineteenth Century*, 1886.

[3] "Darwinism," p. 443 (London, 1889).

and accident. I shall take as my example certain facts which are well known to those engaged in the breeding of farm-stock or of other animals that are of utility to or are specially cultivated by man.

I do not refer to the influence on the offspring of impressions made on the senses and nervous system of the mother, the first statement of the effects of which we find in the book of Genesis, where Jacob set peeled rods before the flocks in order to influence the colour and markings of their young; though I may state that I have heard agriculturists relate instances from their own experience which they regarded as bearing out the view that impressions acting through the mother do influence her offspring. But I refer to what is an axiom with those who breed any particular kind of stock, that to keep the strain pure, there must be no admixture with stock of another blood. For example, if a shorthorned cow has a calf by a Highland sire, that calf, of course, exhibits characters which are those of both its parents. But future calves which the same cow may have when their sires have been of the shorthorned blood, may, in addition to shorthorn characters, have others which are not shorthorned but Highland. The most noteworthy instance of this transmission of characters acquired from one sire through the same mother to her offspring by other sires is that given in the often-quoted experiment by a former Lord Morton.[1] An Arabian mare in his possession produced a hybrid the sire of which was a quagga, and the young one was marked by zebra-like stripes. But the same Arabian had subsequently two foals, the sire of which was an Arab horse, and these also showed some zebra-like markings. How, then, did these markings characteristic of a very different animal arise in these foals, both parents of which were Arabians? I can imagine it being said that this was a case of reversion to a very remote striped ancestor, common alike to the horse and the quagga. But, to my mind, no such far-fetched and hypothetical explanation is necessary. The cause of the appearance of the stripes seems to me to be much nearer and more obvious. I believe that the mother had acquired, during her prolonged gestation with the hybrid, the power of transmitting quagga-like characters from it, owing to the interchange of material which had taken place between them in connection with the nutrition of the young one. For it must be kept in mind that in placental mammals an interchange of material takes place in opposite directions, from the young to the mother as well as from the mother to the young.[2] In this way the germ-plasm of the mother, belonging to ova which had not yet matured, had become modified whilst still lodged in the ovary. This acquired modification had influenced her future offspring, derived from that germ-plasm, so that they in their turn, though in a more diluted form, exhibited zebra-like markings. If this explanation be correct, then we have an illustration of the germ-plasm having been directly influenced by the soma, and of somatogenic acquired characters having been transmitted.

But there are other facts to show that the isolation of the germ-cells or germ-plasm from the soma cells is not so universal as might at the first glance be supposed. Weismann himself admits that in the Hydroids the germ-plasm is present in a very finely divided, and therefore invisible, state, in certain somatic cells in the beginning of embryonic development, and that it is then transmitted through innumerable cell generations to those remote individuals of the colony in which sexual products are formed. The eminent botanist Prof. Sachs states that in the true mosses almost any of the cells of the roots, leaves, and shoot axes may form new shoots and give rise to independent living plants. Plants which produce flowers and fruit may also be raised from the leaves of the *Begonia*. I may also refer to what is more or less familiar to everybody, that the tuber of the potato can give rise to a plant which bears flowers and fruit. Now in all these cases the germ-plasm is not collected in a definite receptacle isolated from the soma, but is diffused through the cells of the leaves of the *Begonia* or amidst those of the tuber of the potato, and the propagation of the potato may take place through the tuber for several generations without the necessity of having to recur to the fruit for seed. It seems difficult, therefore, to understand why, in such cases, the nutritive processes which affect and modify the soma cells should not also react upon the germ-plasm, which, as Weismann admits, is so intimately associated with them.

[1] Philosophical Transactions, 1881; also Darwin's "Animals and Plants under Domestication," first edition, vol. i. p. 403, 1868.

[2] See, for example, Essays by Profs. Harvey and Gusserow and Mr. Savory; also my "Lectures on the Comparative Anatomy of the Placenta" (Edinburgh, 1876).

Those who uphold the view that characters acquired by the soma cannot be transmitted from parents to offspring undoubtedly draw so large a cheque on the bank of hypothesis that one finds it difficult, if not impossible, to honour it. Let us consider for one moment all that is involved in the acceptance of this theory, and apply it in the first instance to Man. On the supposition that all mankind have been derived from common ancestors through the continuity of the germ-plasm, and that this plasm has undergone no modification from the *persona* or *soma* of the succession of individuals through whom it has been transmitted, it would follow that the primordial human germ-plasm must have contained within itself an extraordinary potentiality of development—a potentiality so varied that all those multiform variations in physical structure, tendency to disease, temperament, and other characters and dispositions which have been exhibited by all the races and varieties of men who either now inhabit or at any period in the world's history have inhabited the earth, must have been included in it. But if we are to accept the theory of Natural Selection, as giving a valid explanation of the origin of new species, then the non-transmissibility of somatogenic acquired characters has a much more far-reaching significance. For if all the organisms, whether vegetable, animal, or human, which have lived upon the earth have arisen by a more or less continuous process of evolution from one or even several simple cellular organisms, it will follow, as a logical necessity of the theory, that these simple organisms must have contained in their molecular constitution a potentiality of evolution into higher and more complex forms of life, through the production of variations, without the intermediation of any external force or influence acting directly upon the soma. Further, this must have endured throughout a succession of countless individual forms and species, extending over we know not how many thousands of years, and through the various geological and climatic changes which have affected the globe.

The power of producing these variations would therefore, on this theory, have been from the beginning innate to the germ-plasm, and uninfluenced in any way by its surroundings. Variations would have arisen spontaneously in it, and, for anything that we know, as it were by accident, and without a definite purport or object. But whether such variations would be of service or dis-service could not be ascertained until after their appearance in the soma had subjected them to the test of the conditions of life and the environment.

Let us now glance at the other side of the question. All biologists will, I suppose, accept the proposition that the individual soma is influenced or modified by its environment or surroundings. Now, if on the basis of this proposition the theory be grafted that modifications or variations thus produced are capable of so affecting the germ-plasm of the individual in whom the variation arises as to be transmitted to its offspring—and I have already given cases in point—then such variations might be perpetuated. If the modification is of service, then presumably it will add to the vitability of the individual, and through the interaction between the soma and the germ-plasm, in connection with their respective nutritive changes, will so affect the latter as to lead to its being transmitted to the offspring. From this point of view the environment would, as it were, determine and regulate the nature of those variations which are to become hereditary, and the possibility of variations arising which are likely to prove useful becomes greater than on the theory that the soma exercises no influence on the germ-plasm. Hence I am unable to accept the proposition that somatogenic characters are not transmitted, and I cannot but think that they form an important factor in the production of hereditary characters.

To reject the influence which the use and disuse of parts may exercise both on the individual and on his offspring is like looking at an object with only a single eye. The morphological aspect of organic structure is undoubtedly of fundamental importance. But it should not be forgotten that tissues and organs, in addition to their subjection to the principles of development and descent, have to discharge certain specific purposes and functions, and that structural modifications arise in them in correlation with the uses to which they are put, so as to adapt them to perform modified duties. It may be difficult to assign the exact value which physiological adaptation can exercise in the perpetuation of variations. If the habit or external condition which has produced a variation continues to be practised, then, in all probability, the variation would be intensified in successive generations. But should the habit cease or the external condition be changed, then, although the variation might continue to be for a time perpetuated by descent, it would probably become less strongly marked and perhaps ultimately disappear. One could also conceive that the introduction of a new habit or external condition the effect of which would be to produce a variation in a direction different from that which had originally been acquired, would tend to neutralize the influence of descent in the transmission of the older character.

By accepting the theory that somatogenic characters are transmitted we obtain a more ready explanation how men belonging to a race living in one climate or part of the globe can adapt themselves to a climate of a different kind. On the theory of the non-transmissibility of these acquired characters, long periods of years would have to elapse before the process of adaptation could be effected. The weaker examples, on this theory, would have had to have died out, and the racial variety would require to have been produced by the selection of variations arising slowly, and requiring one knows not how many hundreds or thousands of years to produce a race which could adapt itself to its new environment. We know, however, that this process of the dying out of the weakest and the selection of the strongest is not necessary to produce a race which possesses well-recognizable physical characters. For most of us can, I think, distinguish the nationality of a citizen of the United States by his personal appearance, without being under the necessity of waiting to hear his speech and intonation.

It may perhaps be thought that, in selecting the subject of Heredity for my address, and in treating it, as I have to a large extent done, in its general biological aspects, I have infringed upon the province of Section D. But I am not prepared to admit that any such encroachment has been made. Man is a living organism, with a physical structure which discharges a variety of functions, and both structure and functions correspond in many respects, though with characteristic differences, with those which are found in animals. The study of his physical frame cannot therefore be separated from that of other living organisms, and the processes which take place in the one must also be investigated in the other. Hence we require, in the special consideration of the physical framework of Man, to give due weight to those general features of structure and functions which he shares in common with other living organisms. But whatever may have been the origin of his frame, whether by evolution from some animal form or otherwise, we can scarcely expect it ever to attain any greater perfection than it at present possesses.

The physical aspect of the question, although of vast importance and interest, yet by no means covers the whole ground of Man's nature, for in him we recognize the presence of an element beyond and above his animal framework.

Man is also endowed with a spiritual nature. He possesses a conscious responsibility which enables him to control his animal nature, to exercise a discriminating power over his actions, and which places him on a far higher and altogether different platform than that occupied by the beasts which perish. The kind of evolution which we are to hope and strive for in him is the perfecting of this spiritual nature, so that the standard of the whole human race may be elevated and brought into more harmonious relation with that which is holy and divine.

REPORTS.

Report (Second) of the Committee appointed for the purpose of Collecting Information as to the Disappearance of Native Plants from their Local Habitats. Prof. Hillhouse, Secretary.

As intimated at the close of the Report for 1887,[1] the Committee has given its attention in the first instance to Scotland, and appends hereto such portion of the materials placed at its disposal as, for any reason, it considers desirable to publish. It has excluded a considerable number of plants of little interest, and especially such as the records show to be recent introductions, casuals, escapes, &c., the loss of which is only a return, therefore, to an earlier, but still recent, state. There is little doubt that the list, even thus restricted, will be considerably amplified hereafter.

The plants recorded are numbered in accordance with the "London Catalogue," eighth edition, in which the distribution census of each plant will be found. Nearly all of the records are on the authority of some competent botanist resident in the locality, and whose initials, or some distinguishing initials, are appended. As has been pointed out by more than one correspondent, scarce plants occasionally well-nigh disappear in particular seasons, and hence the records of other than frequent visitors are not fully reliable.

The attention of botanists is particularly drawn to the records under the numbers 52, 264, 374, 406, 570, 575, 687, 910, 932, 993, 1018, 1020, 1478, 1695, and 1772, as giving examples of divers ways, often very curious and interesting, in which plants can become extinct.

The attention of the Committee's correspondents has been, in the main, confined to complete or threatened extinction; but in addition to this there is a general consensus of opinion that the rarer and more conspicuous Alpine plants are less abundant than they used to be. Amongst the localities specially mentioned are Clova and Ben Lawers; such plants (in addition to those given in the list) as *Saxifraga cernua*, *Alsine rubella*, *Gentiana nivalis*, &c., are notably less frequent than twenty years ago. Strange rumours have been communicated to the Committee as to the disappearance of plants from accessible habitats within the range of some of the deer "forests," but it is unable to verify these statements. Most of the correspondents agree, however, that the injudicious action of botanists themselves, and of botanical exchange clubs, has been a potent factor in the changes which have taken place. It is too often forgotten that the very rarity of a plant is the sign, and in great degree also the measure, of the acuteness of its struggle for existence, and that when a plant is in a state of unstable equilibrium with its environment, a small disturbance may have disproportionately great effects.

It will be observed that the "dealer" and "collector" figure largely, especially in connection with the disappearance of ferns. Thus one of the correspondents indicates (and offers to name) a dealer who has extirpated, or well-nigh extirpated, a considerable number of species in the district of Dumfries, and whose conduct he had brought under the notice of the local Natural History Society, of which the correspondent is Secretary. "He had also removed and sold almost all of the plants of *Nymphæa alba* from the lochs of this district before discovery; but now, I am happy to say, he is forbidden access to any estate in this district under penalty of prosecution for trespass." The attention of Natural History Societies may well be drawn to this case, as it happily illustrates at the same time one phase of the disease and a cure.

"Summer visitors" do not appear to be directly responsible for much damage, as their wanderings are probably over too restricted an area to produce much effect. There is no doubt, however, that they provide the larger portion of the customers of the "collector," and so are indirectly answerable for his ravages. The temptation to bring home some rare and beautiful fern, like *Aspidium* (*Polystichum*) *Lonchitis*, as a relic of a northern trip, is too great to be resisted, though something may possibly be done by persuading tourists that equally good plants, taken up with all proper care, and at a season when transplanting is not dangerous, can be obtained from any great fern nursery, for a price which is practically lower, often much lower, than that charged upon some Highland railway platform or roadside.

The Committee feels, however, that neither local dealers nor their customers are as a rule amenable to any ordinary appeal or to sentimental considerations, and would suggest therefore that the local Natural History Societies or Field Clubs should keep careful guard over any rare plants to be found within their respective spheres of action, and by appeal to the owner, or in other preferable way, should endeavour to effect their preservation. At the same time, many correspondents draw attention to the insertion by gardening periodicals of the advertisements of collecting dealers, and express the hope that the amount of revenue derived from these advertisements is not so great as to negative the possibility that the gardening journals may be induced, by discontinuing their insertion, to strike a heavy blow at a process which is depriving many districts of our land of one of their chief natural beauties.

39 *Trollius europæus*, L. Extinct in Mid-Aberdeen, &c. (W. W. and J. M.).

52. *Nymphæa alba*, L. Almost extirpated from lochs in the district round Dumfries by a dealer (J. W.). Has disappeared from the district of Birnie, near Elgin, by drainage (G. and T. A.).

[1] The Committee was unable to report in 1888, having lapsed by accident.

58. *Meconopsis cambrica*, Vig. Believed to be extirpated from banks of Water of Leith and Currie, Midlothian (G. A. P.).

59. *Glaucium flavum*, Crantz. Recorded in 1776 for sea-shore at Bay of Nigg, near Aberdeen, but not since 1800 (J. W. H. T.). Found sixty years ago at Montrose Links; not now (R. B.).

184. *Dianthus Armeria*, L. Occurred, though not abundantly, in rough pasture near Glencarse Station, Perthshire; has been entirely destroyed through the cultivation of the ground (F. B. W.). This was one of its most northern stations.

207. *Lychnis viscaria*, L. Blackford Hill, Midlothian; now very rare (G. A. P.). Arthur's Seat, Edinburgh; supposed to be extirpated (G. A. P.),

208. *Lychnis alpina.* Is now becoming rare in its habitats on Clova Mountains (G. A. P.).

263. *Hypericum perforatum*, L. Formerly grew plentifully near Cromarty Nursery, but has ceased to exist, as the ground is now u ed for agricultural purposes (T. A.). This was one of its most northern stations.

264. *Hypericum quadrangulum*, L. Has wholly disappeared from the vicinity of Fortrose, Ross-shire, having been eaten by cattle or trodden down (T. A.). This was one of its most northern stations.

368. *Lotus pilosus*, Beeke. Extinct round Alford, Mid-Aberdeenshire, from cultivation (W. W.).

374. *Oxytropis uralensis*, D.C. Grew in abundance near Invergordon, Ross-shire, but on one occasion the medical man of the town saw a man digging it up with a trowel, and it is now extinct (T. A.).

375. *Oxytropis campestris*, D.C. Rocks at Bradoony, Clova; now very rare; extirpated from all accessible parts of the rocks (G. A. P.).

406. *Lathyrus niger*, Wimm. Has well-nigh disappeared from its station at Killiecrankie Pass owing to the late guide to the Pass showing it to all tourists. An appeal to the proprietor might save the rest of the specimens, of which very few stations exist (F. B. W.).

501. *Agrimonia Eupatoria*, L. Becoming very scarce in Glen Urquhart, Inverness-shire (Gr.). This was one of its most northern stations.

525. *Pyrus Aria*, Sm. One specimen only (? *P. fennica*, L.) known in Arran; now lost through injury (G. A. P.). Lost also from one or two other stations on the Western Highlands, and now very rare in Scotland.

570. *Sedum reflexum*, L. Found freely on a wall at Birnie, Elgin; disappeared through repairs (G.). Not native.

575. *Drosera anglica*, Huds. Extinct in Kincardine (M.). Extinct round Alford, Mid-Aberdeen, through drainage (W. W.).

577. *Hippuris vulgaris*, L. Extinct round Alford, Mid-Aberdeen, but still appears on the borders of Banffshire.

611. *Eryngium maritimum*, L. Found in the early part of the century on the sandy coast at St Cyrus, near Montrose, and at St. Fergus, Peterhead, but extinct in both localities from unknown causes (J. W. H. T. and R. B.).

687. *Linnæa borealis*, Gronov. Has been cleared from near Dingwall, Ross-shire, owing to the wood in which it grew having been cut down and the ground cultivated (T. A.). Formerly grew at Kingsmills, but has been destroyed through cultivation (G. A., *fide* T. A.). These are two of the most northern British stations.

812. *Silybum Marianum*, Gaertn. Has gone from the rocks near Tarbet-ness Lighthouse, Ross-shire (D.). This plant is very rare in Scotland.

887. *Lactuca* (*Mulgedium*) *alpinum.* This plant was found (probably abnormally) on the Coreen Hills at about 700 feet, but is now extinct (W. W.).

910. *Vaccinium Oxycoccos*, L. Formerly grew in a piece of mossy land on the uplands north of Mealfourvouny, a hill of Old Red Sandstone conglomerate above 3000 feet, but whether the plants were of recent introduction or last survivors, they have disappeared (Gr.).

926. *Phyllodoce taxifolia*, Salisb. (*Menziesia cærulea*). The only British habitat of this plant is the Sow of Athol, and it has now been nearly extirpated, for sale (K. and F. B. W.). The habitat is within sight of a gamekeeper's house, so that its protection would be easy if the Duke of Athol, the owner, could be moved to that effect.

929. *Pyrola media*, Sw. Has disappeared from White Hills, Colvend, Kirkcudbrightshire, through sheep grazing (J. M. A.).

932. *Moneses grandiflora*, Salisb. (*Pyrola uniflora*, L.). Extirpated from Woodhead Hill, Traqueer, Dumfriesshire (J. W.). Once not uncommon on the Muirhead of Scone; now very rare, from extirpation by botanists and others (F. B. W.). Formerly abundant within 4 miles of Forres; now extirpated; also from the wood at Brodie, near Forres, from the wood being cut down, and from Coul Woods, near Strathpeffer. It is also disappearing from Rothiemurchen, in this case from the rapacity of collectors (K.).

945. *Primula scotica*, Hook. Marsh near Edinburgh, Pentland Hills; practically extirpated (G. A. P.).

984. *Asperugo procumbens*, L. Has not been found for some years near the village of Balnahuish, on the Dornoch Firth (D.). This was its most northern station.

993. *Mertensia maritima*, Don. Shingle at Bay of Nigg, Aberdeen; almost extirpated from shingle being removed to form concrete blocks used in building a pier some years ago (J. W. H. T.).

1006. *Echium vulgare*, L. Nearly extinct, through cultivation, in the Black Isle, between Inverness and Fortrose, Ross-shire (T. A.).

1018. *Atropa Belladonna*, L. Has disappeared from Renlop Abbey, near Birnie, by extraction, on account of the accidents it had caused (G.). Has not been seen for some years at the Old Kutt, near Ganlude (T. A.). This eliminates two of the few Scottish stations.

1020. *Hyoscyamus niger*, L. Appeared in two or three places in the neighbourhood of Avoch, a fishing village on the Moray Firth, but disappeared in a few years. Informant "thinks it would come up again if the ground were deeply trenched. Some years ago an old elm was blown down and the root blasted, and for two succeeding summers *H. niger* grew luxuriantly in the hole caused by the tearing up of the root of the tree" (S. R., *fide* T. A.).

1092. *Utricularia vulgaris*, and 1094, *U. minor*, L. Extinct in Central Aberdeen (J. M. and W. W.).

1161. *Ajuga pyramidalis.* Has disappeared from In. Achilty, Dingwall, Ross-shire (T. A.).

1424. *Paris quadrifolia*, L. There is one station near the town of Inverness; nearly extinct, through the publicity of its habitat, this being one of the chief resorts of the population (T. A.). This is one of its most northern stations.

1431. *Juncus balticus*, Willd. Loch of Park, and Links north of Aberdeen; never plentiful, and not seen for some years. Cause of disappearance doubtful (J. W. H. T.).

1457. *Sparganium ramosum*, Curtis; *S. simplex*, Huds.; *S. affine*, Sch.; and *S. minimum*, Fr. All apparently extinct in Mid-Aberdeen (W. W.).

1478. *Scheuchzeria palustris*, L. The only Scottish station for this plant, a marsh near Methven (known botanically as "Methven bog"), has been lost; perhaps from the outlet becoming blocked, so that more water collected than the plant could stand, but more probably from the settlement there of a large colony of about 3000 black-headed gulls, the result being the destruction of all but the rankest vegetation (chiefly *Carex ampullacea*). Very careful searching during the last three years has failed to show a trace of the plant (F. B. W.).

1590. *Carex limosa*, L. Has disappeared from Maxwell-town Loch, Kirkcudbrightshire, through drainage (J. M. A.).

1695. *Melica uniflora*, Retz. Is not now found by the side of the burn at Golspie, Sutherland, probably from the hollow, caused by the upturned stool of a large tree which has been blown over, draining the spot where it grew (J.). This was its most northern Scottish station.

1766. *Cryptogramme crispa*, R. Br. (Parsley fern). Extirpated from several localities in the vicinity of Dumfries (J. W.). Abundant thirty years ago on an ancient hill-fortress near Brechin; now extirpated by traders (R. B.).

1772. *Asplenium viride*, Huds. Nearly extinct in district of Black Isle, between Inverness and Fortrose, through drainage and cultivation (T. A.). Has been extirpated from its old habitats in Glen Urquhart, Inverness-shire, by an itinerant fern-collector who squatted in the neighbourhood and took all he could find; but new habitats have been discovered (Gr.).

1773. *Asplenium Trichomanes*, L. Not now found in the woods of Knockespock Clatt, Mid-Aberdeen (W. W.).

1776. *Asplenium germanicum*, Weiss. Nearly eradicated from Stenton Rock, near Dunkeld (F. B. W.).

1777. *Asplenium septentrionale*, Hull. Probably extirpated, or nearly so, from Arthur's Seat, Edinburgh (G. A. P.). Nearly eradicated from Stenton Rock, near Dunkeld (F. B. W.).

1779. *Athyrium alpestre*, Milde. Now very rare in Clova Mountains, and mostly in accessible places (G. A. P.).

1781. *Ceterach officinarum*, Desv. Almost extirpated from Orchard-town Tower, Kirkcudbrightshire, by fern-hunters (J. M. A.). Used to grow on the walls of Drumlanrig Castle, one of the seats of the Duke of Buccleuch, Dumfriesshire, but not now found there (T. A.).

1782. *Scolopendrium vulgare*, Symons. Almost extirpated from several places in Kirkcudbrightshire by fern-hunters (J. M. A.). Extirpated from several places in the vicinity of Dumfries (J. W.) On the burns falling into Loch Ness there is now only one in which this plant is to be found, owing to the ravages of the itinerant fern-collector referred to under 1772. It still exists, however, in inaccessible stations (Gr.).

1783. *Woodsia ilvensis*, R. Br. Well-nigh extirpated by fern-hunters from the Moffat district (J. W.).

1787. *Cystopteris montana*. This plant, though not at present really uncommon round Aberfeldy, will not improbably be made very scarce by fern-collectors. It has disappeared altogether from one of the stations in which it was first found in Britain (F. B. W.).

1788. *Polystichum Lonchitis*, Roth. Almost extinct on Meal-fourvouny Mountain, Inverness-shire, through the action of fern-collectors, and especially of the one referred to under 1772 and 1782 (Gr.). Has been cleared from the Raven's Rock, near Strathpeffer, Dingwall, Ross-shire, by summer visitors (T. A.). Was plentiful near Castleton, Braemar, formerly, but the guides learned that they could sell it at a shilling a plant, and it is now difficult to get (T. A.).

1803. *Phegopteris* (*Polypodium*) *Robertiana*, A. Br.; *Polypodium calcareum*, Sm. Once abundant in the *débris* of an old limestone quarry near Aberfeldy, but now nearly eradicated. Fern-hunting visitors and tourists are largely to blame for this, but the destruction has been completed by persons who collect ferns for sale. That the species is not altogether lost in the district is, however, shown by the fact that a few weeks ago a local fern-hunter was offering plants for sale, and at the same time plants of 1787, *Cystopteris montana* (F. B. W., July, 1887).

1806. *Osmunda regalis*, L. Has disappeared from Ballingear Glen, New Galloway, and from other places, as Colvend, through the ravages of fern-hunters (J. M. A.). Extirpated from several localities in the vicinity of Dumfries (J. W.). Has entirely disappeared from Loch of Park, and nearly from the cliffs south of Aberdeen, in both of which localities it was formerly plentiful. Fern-collectors are mainly responsible (J. W. H. T.).

1809. *Botrychium Lunaria*, Sw. Formerly very local in the Pentlands; now extirpated (G. A. P.).

1818. *Equisetum hyemale*, L. Extinct in Mid-Aberdeen (J. M.).

Report of the Committee appointed for the purpose of co-operating with the Scottish Meteorological Society in making Meteorological Observations on Ben Nevis. Mr. Buchan, Secretary.

The work of carrying on the observations hourly, by night as well as day, has been carried on by Mr. Omond and his assistants during the year with the same enthusiasm and unbroken continuity as in time past; and the five daily observations in connection with the Ben Nevis Observatory have been made at Fort William by Mr. Livingston with the greatest regularity and care.

As in the previous year, the state of the health of the observers, occasioned by their continuous residence at the top of the mountain, where exercise in the open air is practically impossible during the greater part of the year, rendered it again necessary to give them relief during the winter and spring. The services of Mr. Drysdale were again secured for six months; Mr. R. C. Mossman, the Society's observer for Edinburgh, gave his services as observer for six weeks in April and May; and Mr. McDonald, Edinburgh, has given a month's service as observer in July and August of this year. Messrs. Omond and Rankin, during the time they were relieved from the work of the Observatory, took part in the work of the office of the Scottish Meteorological Society, and gave material assistance, more particularly in the reduction, preparation for press, and discussion of the Ben Nevis observations.

The photographing of clouds and other meteorological phenomena has been actively prosecuted at the Observatory, and results of considerable interest and importance have been already obtained. Selections from the photographs were exhibited by the Scottish Meteorological Society and by the Royal Meteorological Society during the winter session. Of these photographs four are submitted with this Report—viz. (1) a photograph of St. Elmo's fire; (2) a cloud photographed at midnight of June last year; (3) a remarkably fine photograph of a cloud, partly made up of flattened masses, which is occasionally formed in mountainous districts; and (4) photographs of crystals on the Observatory and instruments outside.

Mr. Rankin has extended and amplified his investigation of the cases of St. Elmo's fire recorded at the Observatory; and the results, which are interesting and suggestive, have been published in the Journ. Scot. Meteor. Soc.

Mr. Omond has entered on an investigation of the relations of the wind direction on the top of Ben Nevis to the sea-level isobaric of the district at the time, and to the storms advancing on the Atlantic towards North-Western Europe, as shown on the daily weather charts of the northern hemisphere published by the Meteorological Institutes of Germany and Denmark.

This is properly only the commencement of a large discussion of the Ben Nevis observations in some of their more practical aspects, which will be undertaken and pushed forward next year on the plan referred to in last year's Report, as rapidly as the means at the disposal of the Directors of the Observatory will admit.

This season (1889) the snow disappeared from the summit of the mountain in the middle of May, being about a month earlier than in any previous year, and seven weeks earlier than in 1885; and during the month of June the spring near the Observatory, and about 60 feet lower down, frequently ran dry, so that for some time water had to be carried on horseback a distance of two and a half miles.

The Directors have had under consideration a proposed systematic observation of the numbers of dust particles in the atmosphere with the instrument recently invented by Mr. John Aitken, and they are of opinion that the Ben Nevis Observatory is the best place for making the observations in the most satisfactory manner. Mr. Aitken will himself superintend the construction of the two instruments which are required, and will see to the placing of the stationary one in the Observatory, and its connections with the atmosphere outside, in suitable positions, and give directions as to the portable one designed as a check instrument, and for observations made at various distances from the Observatory. Application has been made for a grant from the Government Research Fund to aid in carrying on this novel and important research.

Mr. Aitken recently visited the Observatory, and ten observations of the numbers of dust particles on the top of the mountain were made by Mr. Omond and himself, with the results that the numbers per cubic centimetre rose from 350 at noon to 500 at 3 p.m. This result of the first observation is interesting and suggestive. The purest air previously obtained by Mr. Aitken anywhere was on the Ayrshire coast, and on that occasion the numbers were 1260 per cubic centimetre. It may be also observed that the numbers on Ben Nevis rose from noon to 3 p.m., the observations being made at the time of the day when aërial currents from lower levels ascend along the heated sides of the mountain to the Observatory.

In January last the Directors accepted an offer from the Meteorological Council that, on being satisfied that provision had been made for the maintenance of a Low Level Observatory at Fort William, they would supply and erect in the Observatory the self-registering instruments and otherwise complete the ordinary outfit of meteorological instruments, and make an annual grant of £250 towards its maintenance, and also continue the grant of £100 yearly under the present arrangement.

Since last Report, the Directors have received a legacy of £500 bequeathed to the Observatory, by the late Mr. R. M. Smith, who was one of the Directors; and a grant of £1000 from the Association of the Edinburgh International Exhibition of 1886 from the Surplus Fund of the Exhibition. A suitable site for the Low Level Observatory was procured in Fort William, and plans of the buildings were prepared by their architects, Messrs. Sydney Mitchell and Wilson, which were submitted to the Directors and the Meteorological Council, and approved of. The building is now well advanced, and it is expected that the Observatory will be opened towards the end of the autumn.

The Directors of the Observatory and your Committee in their successive Reports from 1884 insisted on the absolute necessity of combining the double observation for all forecasting purposes, and inquiries in connection therewith; in other words, of combining with the observation at the top of Ben Nevis that made at the same instant near sea-level at Fort William. With the opening of the Low Level Observatory in November will commence a new era in the work of the Ben Nevis Observatory. It will then be possible, by the double set of horizontal meteorological gradients and vertical meteorological gradients thus obtainable, to examine more fully and rigorously the atmospheric changes which precede, accompany, and follow the passage across these islands of the cyclones and anticyclones of North-Western Europe.

The minimum temperature on Ben Nevis for the year was 7°·2, being the lowest yet observed since the Observatory was opened in 1883. The maximum was 61°·1 in June, which closely agrees with the maxima of previous years, except that of 1887, which rose to 67°·0. It is also to be noted that so late as September temperature rose to 57°·6.

The registrations of the sunshine recorder showed 970 hours of sunshine during the year, the smallest number of hours for any month being 8 for November, and the largest 250 in June, being nearly half the possible sunshine. The number of hours for the four years now observed, beginning with 1885, were 680, 576, 898, and 970. The contrast of the sunshine of 1886 with that of 1888 is thus very striking.

The amount of rainfall for the year was 132·46 inches, the month of least rainfall, 3·76 inches, being June, and of greatest, 20·60 inches, being November. The number of days on which precipitation was *nil*, or less than the hundredth of an inch, was 118. The number of rainless days for the last three years have been 159, 128, and 118. From all the observations yet made, it is seen that a fall, equalling at least 1·00 a day, has occurred on an average of one day in nine.

Atmospheric pressure was this year again above the annual average, the mean at sea-level being 29·889, or 0·055 higher. The lowest mean at the Observatory, 25·035 inches, occurred in March, and the highest, 25·590 inches, in September; the difference being 0·555 inch. At sea-level at Fort William the extreme monthly means were 29·636 inches in November, and 30·132 in September; the difference being 0·496 inch.

NOTES.

MR. GRIESBACH, of the Geological Survey of India, shortly proceeds to Beluchistan to exploit for coal; and Mr. Oldham, also of the Geological Survey, is also going to Beluchistan to investigate and report upon oil fields which are believed to exist there.

THE Iron and Steel Institute has assembled this year in Paris at the rooms of the National Industry Encouragement Society, and its first sitting was held on the 24th inst., under the presidency of Sir J. Kitson. The paper which attracted most attention on the first day was one by M. Schneider, of Creuzot, and M. Hersent, ex-President of the French Civil Engineers' Society, on the Channel Bridge, which gave an elaborate account of the scheme. The route chosen as the line, stretching over the shallowest parts of the Channel and connecting the shores where closest to each other, commences at a point near Cape Gris Nez, passes over the Colbart and Varne banks, and terminates near Folkestone. The Colbart and Varne banks are situated near the centre of the Channel, about 6 kilometres apart, the depth of the water at that point not exceeding 7 or 8 metres at low water, and they are separated from each other by a depression about 25 to 27 metres deep. Between the Varne and the British coast the depth does not exceed 29 metres, but near Colbart the bottom sinks somewhat abruptly down to 40 metres. It then attains 55 metres about midway across, when it begins gradually to rise. In these parts the chief difficulties would be encountered in laying the foundations. The result of repeated experiments is that the ground is found to be sufficiently solid to support very extensive works, and the borings lately made in connection with the proposed tunnel have confirmed preceding experiments as to the position and nature of the bottom as published by M. de Gamond. The metal proposed to be used is steel. The amount of metal and machinery to be provided would represent an aggregate weight of about 1,000,000 tons. The spans of metal would be 500 metres in length across the Channel, supported on columns resting at different depths on the bottom of the sea. A rough calculation gives 380,000,000 francs for masonry supports, and 480,000,000 francs for the metallic superstructure—in all, 860,000,000 francs, or £34,400,000. The time required for the undertaking was fixed at about ten years.

THE eleventh Congress of the Sanitary Institute was opened at Worcester on the 24th inst. In connection with the Congress an exhibition of sanitary appliances and apparatus is being held at the Skating Rink, where also lectures on cookery are given by Dr. Strange. The Presidential address was delivered by Mr. G. W. Hastings, M.P., and referred mainly to recent legislation affecting sanitary science.

THE International Oriental Congress, which was held this year during the first and second weeks of the present month, in Stockholm and Christiania, was well attended, and was especially noticeable for the enlightened and warm interest taken in the proceedings by the King. Representatives of Oriental learning from the chief countries were His Majesty's personal guests, the members of the Congress present were on several occasions specially entertained by him, and in other marked ways the King showed his desire to honour science and learning in the persons of the assembled Oriental scholars. The *Times* is the only one of the English daily papers in which the proceedings have been followed regularly, and in the last letter on the subject, its Correspondent, who has been far from a prophet of smooth things in reference to all the proceedings, says that "this eighth International Oriental Congress was favoured above all its predecessors by the right royal splendour with which the ruler of the two countries entertained his guests, by the warm interest which the citizens took in the foreign *savants*, by the care and kindly forethought with which all the arrangements for our comfort had been planned and were carried out, and last (not least) by the grand and lovely natural features of the places which the members visited. Perhaps at future Congresses care will be taken that there be less of empty Oriental parade, by which no palpable literary object can be gained, and that greater facilities be given for placing without delay within the reach of members an abstract of the proceedings in each Section. However, in the face of such boundless hospitality and such personal sacrifices on the part of our hosts, it would be ungracious were we to take exception to what are after all but small matters of detail."

A LARGE number of papers of great philological and general interest were read, as will be readily gathered from the following list of the Sections, with their respective Presidents and Vice-Presidents:—Section I. Modern Semitic: Presidents—Baron Kremer, of Vienna; M. Schefer, of Paris; M. de Goeje, of Leyden. Section II. Ancient Semitic: President—M. Fehr, of Stockholm; Vice-Presidents—M. Chivolson, of St. Petersburg; M. Oppert, of Paris. Section III.: Presidents—M. Max Müller, of Oxford; M. Weber, of Berlin; M. Spiegel, of Erlangen. Section IV.: President—Brugsch Pasha; Vice-Presidents—M. Lieblein, M. Reinisch. Section V.: President—M. Schlegel, of Leyden; Vice-President—M. Cordier, of Paris. Section VI.: President—M. Kern, of Leyden; Vice-President: Mr. R. N. Cust, of London.

A CORRESPONDENT sends us the following instance of "Science as she is wrote," which he thinks worth preserving. It is extracted from an eloquent description of the icebergs near the coast of Newfoundland, which appeared in the *Daily Telegraph* of September 17, and was signed "Edwin Arnold":—"The icebergs are unfortunately most to be expected in those summer months when alone the navigation is open. The first heats of the brief but hot Arctic sunshine set in rapid motion the glaciers of Labrador and Greenland. These vast storehouses of gathered and consolidated snow glide to the edge of the tremendous precipices of the Winter Lands, and, falling over them in monstrous masses, crash into the deep water with shocks which send thunder-peals through the still Polar air, and perturb the ocean far and near with rolling waves. Then, committed by this awful launch to the southward-going currents, the great broken glittering mass goes solemnly sailing away in the unwonted sunshine. As it floats, the water, warmer than the air, melts its lower portion gradually, and detached pieces also fall from the visible part, until equilibrium becomes destroyed, and the colossal block capsizes with a second shock, startling the ocean for leagues around."

WE have received from the Mansion House a copy of the pamphlet relating to the Mansion House Fund for the Pasteur Institute. Besides a full report of the meeting held on July 1, it contains an excellent introduction describing the aims and needs of the fund, written by the Lord Mayor, and statistics of the work done by anti-rabic institutions in France and elsewhere. The last paragraph of the Lord Mayor's introduction is itself an ample justification of the appeal which he makes, for in it he reports that during July and August he sent over to Paris seventeen poor persons who had been bitten by mad dogs. There they have been treated free of charge, as usual by M. Pasteur. The Honorary Secretary to the Fund is Dr. Rueffer, 26 Torrington Square, London.

THE syllabus of the eighth course of lectures and demonstrations for sanitary officers at the Sanitary Institute during the coming winter has been issued. The introductory lecture, on the general history, principles, and methods of hygiene, by Sir Edwin Chadwick, will be delivered on October 8, and the course will be continued every Tuesday and Friday at 8 p.m. until November 15. Amongst the lectures are : water supply, by Dr. Louis Parkes ; drainage and construction, by Prof. H. Robinson ; ventilation, by Sir Douglas Galton ; sanitary appliances, by Prof. Corfield ; scavenging, by Mr. Percy Boulnois ; food, by Mr. Cassal ; infectious diseases and methods of disinfection, by Mr. Shirley F. Murphy ; general powers and duties of inspectors of nuisances, by Mr. J. F. J. Sykes ; and sanitary law, by Mr. Wynter Blyth.

THE carrier pigeon has just been turned to a curious use in Russia, according to the *Novoe Vremya*. It is to convey negatives of photographs taken in a balloon. The first experiment was made from the cupola of the Cathedral of Isaac, and the subject photographed was the Winter Palace. The plates were packed in envelopes impenetrable to the light, and then tied to the feet of the pigeons, who safely and quickly carried them to the station at Volkovo.

THE current issue of the *Kew Bulletin* opens with a memorandum on the use of the flowers of *Calligonum* as an article of food in North-Western India. From a note from Mr. Duthie, of the Botanical Department of Northern India, it seems that the flowers of the *Calligonum polygonoides*, locally known as *phog*, are gathered and used by the poorer classes. They are either mixed with flour, in the proportion of a third *phog* to flour, or are eaten separately with salt and condiments, to which those who can afford it add a little glue. The flowers are swept up as they fall, and are kept for a night in a closed earthenware vessel, so as to fade. Sometimes they are kneaded up in the thin atta, about a fourth flowers to three-fourths atta, and baked in cakes and eaten. The flowers sent home have been analyzed by Prof. Church, who states that the chief peculiarity of these flowers, from a dietetic point of view, is their richness in nitrogenous compounds, and that there is rather a close resemblance in composition between *phog* and the seeds of the edible amaranths and buckwheats, only sugar replaces starch. There are also a short note from Dr. Ernst, of Caracas, Venezuela, on the earliest mention of coca, and a memorandum on the *Buazé* fibre, which the natives living around Lake Ngami use to make fishing-nets. The efforts which are being made to catalogue and publish the notices of Chinese plants scattered through botanical literature and to enumerate in systematic order the species of which specimens are to be found in the British Museum and in Kew, are described. Finally, there are two notes on vine cultivation and the Phylloxera, one being the substance of the report, made by Mr. Dyer in 1881, of the conclusions of the International Congress at Bordeax, which he attended as the representative of certain Colonial Governments ; and the other a report on the vineyards of the Cape Colony, by M. Mouillefert, Professor of Viticulture in the French School of Agriculture at Grignon.

ACCORDING to the Naples Correspondent of the *Daily News*, the scientific excursion to the volcanic regions of Italy, which commenced on the 14th inst., under the direction of Dr. H. J. Johnston-Lavis, of Naples, is exciting great interest. The affair is under the special patronage of Signor Boselli, the Minister of Public Instruction, of several Italian communes, and of the London and Italian Geological Societies. The general director is Dr. Johnston-Lavis, and the secretary in London is Dr. J. Fullerton. The visits to the various notable places will be personally conducted by Prof. Struver, of Rome ; Prof. Silvestri, of Catania ; Profs. Sacchi and Bassani, of Naples ; Dr. Johnston-Lavis, and several others. The English, Belgians, and Italians who joined in the excursion were received at Naples, which was the starting-point, on the Sunday, by the Syndic of Naples, who also intends to give a banquet to the visitors, tickets for the theatres, and to show them other attentions during their visit to the city. Serious business commenced on the 16th inst., with a voyage to the Æolian Islands and Sicily the culminating point of this part of the programme being an ascent of Etna, which is fixed for the 29th inst. A special steamer or steam yacht will be either engaged or lent for this journey. The return to Naples will take place on the 30th inst. From the 1st to the 14th of October the head-quarters will be Naples. During this time excursions will be made to the Phlegiæan Fields, Vesuvius, the craters of Campania, the islands, and the coast towns of the Gulf, and visits paid to the sights of the city, the museums, while lectures, conversaziones, and garden parties will afford relaxation from the harder scientific work. On the 15th and 16th the volcanic groups of Monte Rocca-Monfina and the celebrated monastery of Monte Cassino will be visited, and from the 17th to the 29th Rome and its volcanic neighbourhood will be examined. Among the notable spots to be visited are Capo di Bove, Monte Tuscolo, Monte Porzio, Rocca-di-Papa, Monte Cavo, Lago di Albano, Nemi, Monte Mario, Bagno di Tivoli, Tivoli, Monte Cimmino, Monte Venere, and the crater of Vico, Frascati, and many famous villas. On the 30th of October the members will separate at Viterbo, going each their own way. The Minister of Public Instruction has accorded many facilities, such as reduced fares to the intending travellers, and the director and secretaries are using every effort to render the tour a complete success. Apart from the scientific interest of the excursion, it will afford an unexampled opportunity for becoming acquainted with some of the most beautiful and picturesque parts of Italy and Sicily.

IN a Report presented to the Foreign Office by the British Consul at Florence, there is a very full account of the forests on the north-eastern boundary of Italy, which in that region are superintended by a large staff of officials. In the mountainous regions the maintenance of the forests is an absolute necessity, but, unfortunately, many slopes have become quite denuded through reckless cutting in the past. The actual extent of forest area has, however, increased since the sixteenth century, but energetic measures are now needed to prevent many of the woods deteriorating any further. Complaints are made by those who are anxious to preserve the forests that the peasants are not taught the value of them, and that in the elementary schools the principles of woodcraft and agriculture are never taught. Of the whole area under timber, the State owns only 3·42 per cent., while the *communes* possess 79·93 per cent., and private owners 16·63 per cent. As a rule the communal management is very reckless, and the forests have suffered so much that it has been seriously considered that the woods should be divided up amongst the peasants; but it is more than probable that this remedy would prove worse than the disease. The timber in the Cadore country is said to be the best in the world. Amongst the trees of the region are—the common Norway spruce, which flourishes at an altitude of 2400 metres above the level of the Adriatic, and grows to a height of about 30 metres; a variety of the Norwegian spruce, resembling the beech in its fibres, with harder wood, shorter leaves, and smaller cones than the common spruce; the common silver fir, which grows in the mountains 1500 metres above the Adriatic, and reaches a height of 25 metres; the common larch, the Scotch fir, the Mugho pine, the Swiss stone pine, the beech, and the walnut.

FROM the general results of the Swiss census of December 1, 1888, which have already been worked out, it seems that the total population is 2,934,055, against 2,846,102 in 1880. The German-speaking element increased from 2,030,792 in 1880 to 2,092,562, which, taking into account the normal growth of the population, was no relative increase, the proportion in both cases being precisely 71·3 per cent. of the whole. The French, on the other hand, increased from 608,007 to 637,940, which was also a relative increase of 21·4 to 21·7 per cent.; while the Italian declined actually as well as relatively, the numbers being 161,923 in 1880 and 156,602 in 1888, or 5·7 and 5·3 per cent. respectively. The decline of the Italians in the Cantons of Uri and Schwyz is explained by the return home of a large number of Italian workmen engaged in the St. Gothard Railway; but it is not so easy to explain why there is a large decrease in the Germans in the Cantons of Berne and Neuchâtel, while the French have increased. In general the French increase in Switzerland seems to be at the expense of the Germans, while the German element recovers its place at the expense of the Italian.

WE have received a "Guide to Technical and Commercial Education," drawn up by the executive Committee of the Dundee and District Association for the Promotion of Technical and Commercial Education. It is intended to fulfil one of the objects for which the Association was promoted, viz. "To draw up a syllabus for the district, in which shall be suggested such courses of education as shall be most suitable for particular trades; and further, to indicate (*a*) the number of years required for the complete education, (*b*) the total cost, and (*c*) the institutions in which the necessary instruction may be obtained." We draw special attention to this "Guide," as it cannot fail to be of the utmost use to similar Associations elsewhere, especially as it is drawn up with reference to the Science and Art examinations and those of the City Guilds. A definite order of study is suggested and recommended in a number of subjects, such as commerce, civil, mechanical, and electrical engineering, architecture, art industries, textiles, ship-building, gas manufacture, &c. The courses have evidently been compiled with great care and patience, and with the aid of practical men in the various subjects treated.

AMONGST the books to be published by Messrs. Crosby Lockwood and Son during the coming publishing season are:—"The Art of Paper Manufacture," by Alexander Watt; "A Hand-book on Modern Explosives," by M. Eissler; "Engineering Estimates, Costs, and Accounts," by a General Manager; "The Mechanical Engineer's Office Book," by Nelson Foley, second edition; "The Practical Engineer's Hand-book," by Walter S. Hutton, third edition; "Electric Light, its Production and Use," by J. W. Urquhart, third edition; "The Fields of Great Britain," a text-book of agriculture adapted to the Syllabus of the Science and Art Department, by Hugh Clements, second edition. And the following new editions in Weale's Rudimentary Scientific Series:—"Metallurgy of Iron," by H. Bauerman; "The Mineral Surveyor and Valuer's Complete Guide," by W. Lintern; "Stationary Engine Driving," by Michael Reynolds; "Irrigation and Water Supply," by Prof. John Scott.

MR. T. J. P. JODRELL, of Yeardsley, Cheshire, sometime of Stratton Street, London, whose death, as recently announced in the *Times*, took place on the 3rd inst., in his eighty-second year, was the founder of the Jodrell Fund, a capital sum of £6000 having been given by him to the Royal Society in the year 1876 for scientific purposes.

THE additions to the Zoological Society's Gardens during the past week include a White-fronted Lemur (*Lemur albifrons* ♂) from Madagascar, presented by Mr. C. O. Pelly; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. H. B. Wedlake; a Brown Bear (*Ursus arctus*) from Russia, presented by Mr. Frank Dugdale; a Crested Porcupine (*Hystrix cristata*) from Africa, presented by Mrs. Lucas-Shadwell; three African Lepidosirens (*Protopterus annectans*) from the River Gambia, West Africa, deposited; a Burchell's Zebra (*Equus burchelli* ♀), from South Africa, four Larger Tree Ducks (*Dendrocygna major*), three Indian Tree Ducks (*Dendrocygna javanica*) from India, a Tuberculated Iguana (*Iguana tuberculata*) from Brazil, purchased; a Red Kangaroo (*Macropus rufus* ♂), two Cockateels (*Calopsitta novæ-hollandiæ*), a Crested Pigeon (*Ocyphaps lophotes*), a Nicobar Pigeon (*Calænas nicobarica*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1889 *e* (DAVIDSON).—The following ephemeris for Greenwich midnight for this object is in continuation of that given in NATURE for August 29 (p. 424):—

1889.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
Sept. 29	17 14 19	32 6·5 N.	0·1635	0·1940	0·03
Oct. 3	17 21 53	32 38·5	0·1829	0·2064	0·02
7	17 29 28	33 8·2	0·2011	0·2186	0·02
11	17 37 7	33 36·1 N.	0·2182	0·2306	0·02

The brightness at discovery is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 SEPTEMBER 29—OCTOBER 5.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 29

Sun rises, 5h. 59m.; souths, 11h. 50m. 11·7s.; daily decrease of southing, 19·5s.; sets, 17h. 41m.: right asc. on meridian, 12h. 23·9m.; decl. 2° 35′ S. Sidereal Time at Sunset, 18h. 16m.

Moon (at First Quarter October 2, 2h.) rises, 11h. 8m.; souths, 15h. 40m.; sets, 20h. 4m.: right asc. on meridian, 16h. 14·4m.; decl. 18° 29′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 30	13 15	18 0	13 49·4	14 59 S.
Venus.....	2 44	9 46	16 48	10 19·9	11 18 N.
Mars......	2 49	9 51	16 53	10 24·8	11 18 N.
Jupiter....	13 35	17 27	21 19	18 1·6	23 30 S.
Saturn....	2 23	9 34	16 45	10 7·0	12 56 N.
Uranus ...	7 22	12 46	18 10	13 19·8	7 48 S.
Neptune..	19 50*	3 39	11 28	4 11·2	19 24 N.

* Indicates that the rising is that of the preceding evening.

Oct.	h.	
1	11	Jupiter in conjunction with and 0° 39′ south of the Moon.
1	13	Venus in conjunction with and 0° 22′ south of Mars.
4	1	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
S Ceti	0 18·4	9 57 S.	Oct. 1,		*M*
U Cassiopeiæ	0 40·2	47 39 N.	,, 3,		*M*
U Cephei	0 52·5	81 17 N.	,, 5,	3 26	*m*
Algol	3 1·0	40 32 N.	,, 4,	3 45	*m*
λ Tauri	3 54·5	12 11 N.	,, 5,	0 59	*m*
U Boötis	14 49·2	18 9 N.	Sept. 30,		*M*
S Herculis	16 46·8	15 8 N.	,, 29,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 29,	20 52	*m*
			Oct. 4,	21 38	*m*
Y Sagittarii	18 14·9	18 55 S.	Sept. 29,	21 0	*m*
R Scuti	18 41·6	5 20 S.	Oct. 3,		*M*
β Lyræ	18 46·0	33 14 N.	,, 4,	0 30	*M*
R Cygni	19 33·8	49 57 N.	,, 4,		*M*
S Aquilæ	20 6·5	15 18 N.	,, 3,		*m*
R Delphini	20 9·6	8 45 N.	,, 1,		*M*
T Vulpeculæ	20 46·8	27 50 N.	Sept. 30,	23 0	*m*

M signifies maximum: *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °		
Near η Aurigæ	75	41 N.	October 2.	Swift.
	225	52 N.	October 2.	Slow, bright.
,, δ Draconis	290	68 N.		Swift.

GEOGRAPHICAL NOTES.

On the 12th inst. the administration of the Congo State received intelligence by way of Zanzibar that Mr. H. M. Stanley, on leaving the basin of the Albert Nyanza, endeavoured to make his way southwards by passing to the west of the Victoria Nyanza, but was unsuccessful. He then went northwards, and reached the eastern shore of the lake, Emin Pasha accompanying him. Mr. Stanley made a long stay on the borders of the lake, awaiting supplies from Msalala and Jabora, for which he had sent. He left Emin Pasha on the eastern shore of the lake several months ago, and proceeded in the direction of Mombassa. Mr. Stanley is expected to reach the eastern coast of Africa towards the end of October next.

According to information received at Lloyd's from Tromsö, dated September 9, the German travellers Kukenthal and Walter, belonging to the Bremen Arctic Expedition, who were shipwrecked last spring in the *Bertina*, have arrived safely at Tromsö.

M. Joseph Martin, the French explorer, known in connection with his late expedition to Eastern Siberia, recently left Pekin with a small escort for Tibet, intending to proceed along the Great Wall, subsequently passing through the towns of Liang-Chow and Sining and the province of Koko-Nor, where he expects to arrive next spring. The object of the expedition is of a purely scientific character.

Mr. Frederick Jeppe, of Pretoria, has recently issued, through Dulau and Co., an excellent map of the Transvaal and neighbouring territories on the scale of 15·78 miles to an inch. It includes not only the Transvaal, but the Orange Free State and all the countries between these and the coast, from Delagoa Bay down to Pondoland. It goes north to close on the Zambesi, including Matabeleland, Bechuanaland, Griqualand West, and the northern parts of Cape Colony. There are, moreover, a number of special insert maps. The physical features, mountains, rivers, &c., are laid down clearly and in detail. The gold-fields are coloured yellow, the topography is almost exhaustive, and the map is really a gazetteer of the extensive and important region which it embraces. Mr. Jeppe gives a list of the various authorities which he has used in the compilation of his map, and these are the best and latest available.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, September 9.—M. Des Cloizeaux, President, in the chair.—On the fixation of atmospheric nitrogen, by M. Berthelot.—Observations on the formation of ammonia and volatile azotised compounds at the expense of vegetable earth and of plants, by the same. He traces the researches, initiated by him six years ago, establishing the fixation, by earth and plants, of free nitrogen of the air, with the aid of mineral matters and living organisms. Analysis of the liquid condensed within a bell jar inclosing earth, or earth with vegetation, proves the exhalation (of ammonia, &c.) above referred to; and like the ptomaïnes, &c., produced by animals in a closed space, the products are toxical to the organisms yielding them.—On the nitrification of ammonia, by M. Th. Schlœsing. Small quantities of gaseous nitrogen (negligible in agricultural practice), are liberated during the oxidation of ammonia in soil. The author shows that the nitrification of ammonia put into a soil in the form of sulphate, may be effected very quickly, when favoured by the nature of the soil, its humidity and its temperature. In slow combustion of the organic matter of soil, through the agency of the nitric ferment, much more oxygen is used in burning the carbon and hydrogen, than in nitrification of the nitrogen. But in a soil enriched with ammonia, the activity of the ferment is much increased, in conveying oxygen to the ammonia, and it seeks from organic matter only the carbon needed for its development and multiplication.—On the bacteriological study of the lesions of contagious peripneumonia of the ox, by M. S. Arloing. He distinguishes a bacillus and three kinds of micrococci.—On some observations made at the Observatory of Algiers, by M. Ch. Trépied. The separation of the nucleus of Brooks's comet, affirmed by the Mount Hamilton observers, could not be certainly made out. This Observatory, begun in the spring of 1885, on a height (350 metres) overlooking Algiers, has now all its instruments except a photographic equatorial. M. Trépied notes that the telescopic image of a star, during the sirocco, becomes a continuous luminous spot, the intensity diminishing outwards; an effect, doubtless, of dust.—Observations of Brooks's comet and its companion, made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Rambaud and Sy.—The spectro-photography of the invisible parts of the solar spectrum, by M. Ch. V. Zenger. He describes as advantageous combinations, prisms of quartz and anethol; of quartz and calcareous spar; of the latter and sulphide of carbon; and of rock salt and anethol. One prism of rock salt, with two of anethol, gives nine times more dispersion, and the red part is six times more dispersed between A and D, than by a 60° prism of rock salt.—Researches on sulphites, by M. P. J. Hartog.—On a new monobromized camphor; on the constitution of monosubstituted derivatives of camphor, by M. P. Cazeneuve. The new compound is obtained similarly to the chlorine compound, got by the action of hypochlorous acid, and has similar properties.—On phenoldisulphonic acid, by M. S. Allain-Le Canu.—Influence, on bare soil, of gypsum and clay, on the conservation of nitrogen, the fixation of atmospheric nitrogen, and nitrification, by M. Péchard. The sulphate of lime retains the ammonia in the state of sulphate, and contributes indirectly to the production of nitric acid, by keeping the nitrogen in a form easily nitrifiable; also directly, (in a way not well understood) by its power of deoxidation and reoxidation. Gypsum and clay, both added to sandy soil, concur in fixing ammonia; the former keeps the fixing power of the latter active by removing its ammonia in the state of sulphate easily nitrifiable (clay alone is rather adverse to nitrification).—Manufacture of red glasses for windows (twelfth and thirteenth centuries), by MM. Ch. Er. Guignet, and L. Magne. A microscopic examination of these old glasses shows that various effects were obtained by making two glasses act on each other. In one case of interior twisted marbling, *e.g.*, a yellowish glass (charged with iron protoxide) reddened only at its contact with the enveloping mass of greenish-blue glass (copper-oxide). In another case (parallel marbling), each pellicle of yellow glass is reddened at its two faces. M. Henrivaux has adopted a similar method at St. Gobain.

September 16.—M. Des Cloizeaux, President, in the chair.—On an adynamic gyrostatic constitution for the ether, by Sir William Thomson. He describes a system of small spheres, connected by rods, with terminal cups moving on the spheres, and, at their middle part, two gyroscopes, with outer rings at right angles to each other.—On an application of the electric transmission of force, made at Bourganeuf, by M. Marcel Deprez. Further details are given of the system, which has worked well since May. The high tension generator, driven by a turbine, has two rings on one shaft, excited by two rectilinear inductors parallel to the axis, having the four poles quite free. The receiver is similar. The machines for light are of the Gramme type; and with a line resistance of 23 ohms, about 50 per cent. of the force imparted to the generator is recovered in light.—Determination of the microbe producing contagious peripneumonia of the ox, by M. S. Arloing. Of the four he describes, he finds the *Pneumobacillus liquefaciens* (as he calls it) the essential element in the virus.—Observations of Brooks's comet and its companion, at Algiers Observatory, by M. Rambaud.—On the occultations of Jupiter's satellites, by M. Ch. André. With three different telescopes at Lyons, the time of contact determined differed to the extent of 2½ minutes; immersion being noted earlier, and emersion later, with the smaller instrument; also the apparent complete visibility of the satellite, continued after contact (as others have observed), is referred to. This is thought to be due to a zone of diffracted light, spread by the object-glass round the geometrical image of the planet, covering the focal image of the satellite.—On the calculations of Maxwell, relative to movement of a rigid ring round Saturn, by M. O. Callandreau.—On the heat of vaporization of carbonic acid near the critical point, by M. E. Mathias. He uses the heat of dilution of sulphuric acid in the water of the calorimeter, as a compensating source of heat, and finds Clapeyron's formula satisfactorily verified. At the critical point the latent heat, L, is rigorously *nil*.—On the use of the new Edison phonograph as a universal acoumeter, by M. Lichtwitz. With it, one may form *phonograms*, to serve as acoumetric scales, with vowels, consonants, syllables, words and phrases, &c., according to their intensity and acoustic value (as determined by O. Wolf). The sound-source being nearly constant, could be used to compare the hearing of different patients, or the same patient at different times. A set of uniform phonograms could be got by placing phonographs at a fixed distance from a reproducing instrument. Thus aurists in all countries could compare results.—Catadioptric objectives applied to celestial photography, by M. Ch. V. Zenger. Two correction lenses of magnesium glass, of the same focal length, one concave and the other convex, are inserted, the focal length of the system being identical with that of the spherical mirror. The time of exposure is reduced to a third or a quarter, for stars of a given size.—Some supplementary thermal data, by M. J. Ossipoff. Thermal formation of salts of phenylene diamines, by M. Léo Vignon. Comparing the heat of neutralization of the three diamines by hydrochloric acid, he finds orthophenylene diamine to show less than the meta isomer; which, again, shows less than the para. The bisubstituted derivatives of benzine studied by Berthelot and Werner present a similar case.—On the alcoholic fermentation of honey and the preparation of hydromel, by M. G. Gastine. Solutions of honey generally give but poor alcoholic fermentation. The author verified an idea that this is because the ferments, in a medium so poor in mineral and azotized matters, miss the conditions necessary to their evolution.—Physiological action of the poison of the terrestrial salamander, by MM. Phisalix and Langlois. The characteristic symptom is convulsion; and the poison acts successively on the cortical, bulbar, and medullary cells. Temperature rises rapidly, and dyspnœa occurs, followed by asphyxia. Arterial tension is increased.—Cyclone of Jougne, on July 13, 1889, by M. Ch. Dufour. This appeared at 1.15 p.m., on a very hot, calm, cloudy day, in the canton of Doubs, and tore along eastwards 6 km., with a rattle like thunder, lasting two to three minutes. Of many trees uprooted, those at the outset lay mostly east to west; those further on, mostly west to east. The width of region devastated grew from 100 to 250 metres. The intensity seems to have varied in this space, and to have been greater on the right than on the left side (probably through the velocity of translation being added to that of gyration in the former case). Curiously, the weather changed at the time of the cyclone, from dry and warm to cold and wet.

STOCKHOLM.

Royal Academy of Sciences, September 11.—A new arrangement of the species of the cod-fishes, by Prof. A. F. Smitt.—On types of weather-maps, and on the latest dispositions as to the circulation of the meteorological observations of the Meteorological State Institute to the public in general, by Prof. R. Rubenson.—On the genus Prisciturben, Kunth, by Prof. G. Lindström.—Analytic construction of the integrals of a linear homogeneous differential equation of a circular ring, which does not include any singular place, by Prof. G. Mittag-Leffler.—Analytic construction of the invariants of a linear homogeneous differential equation, by the same.—Contribution to the history of the mathematical studies in Sweden during the sixteenth century, by Dr. G. Eneström.—On the constitution of the cumenyl-propin-acid, by Prof. O. Widman.—On hydro-canel-carbon-acid and some of its derivatives, by the same.—A contribution to the question of the re-adjustment of the atoms within the propyl group, by the same.—Derivatives of the ortho-amid-benzyl-alcohol, by Prof. Widman and Dr. Söderbom.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Hints to Travellers, 6th Edition (Royal Geographical Society).—Travels in France by Arthur Young during the Years 1787, 1788, and 1789: M. Betham-Edwards (Bell).—Contributions to Canadian Palæontology, vol. i. Part 2: J. F. Whiteaves (Montreal).—The Fauna of British India, including Ceylon and Burma; Fishes, vol. ii.: F. Day (Taylor and Francis).—The Hand-book of Jamaica for 1889-90 (Stanford).—A Treatise on Analytical Mechanics; vol. ii., Dynamics of a Material System, 2nd edition: B. Price (Oxford, Clarendon Press).—Animal Biology, 2nd edition: C. Lloyd Morgan (Rivingtons).—Notes on the Pinks of Western Europe: F. N. Williams (West).—Simple Shorthand: W. Heather (Groombridge).—The Birds in my Garden: W. T. Greene (R.T.S.).—First Mathematical Course (Blackie).—An Elementary Text-book of Geology: W. J. Harrison (Blackie).—On the Motion of the Heart and Blood in Animals: W. Harvey; Willis's Translation, revised and edited by A. Bowie (Bell).—The Rotifera or Wheel-Animalcules; Supplement: C. T. Hudson and P. H. Gosse (Longmans).—The British Moss-Flora, Part 12: R. Braithwaite (published by the Author).—A Monograph of the Horny Sponges: R. von Lendenfeld (Trübner).—Records of the Geological Survey of New South Wales, vol. i. Part 2, 1889 (Sydney, Potter).

CONTENTS.

PAGE