THURSDAY, SEPTEMBER 8, 1881

## THE STUDENT'S DARWIN

*The Student's Darwin.* By Edward B. Aveling, D.Sc., Fellow of University College, London. International Library of Science and Freethought. Vol. II. (London: Annie Besant and Charles Bradlaugh, 1881.)

SEVERAL months ago we reviewed the first volume of this series, and now in reviewing the second we are still of opinion that the promoters of the series are mistaken, so far as they may have the interests of science at heart, in associating their endeavours to render science popular with their systematic onslaught against theistic belief. In itself science has no necessary relation to any such belief; it is neither theistic nor atheistic; it is simply extra-theistic. It is but an extension of common experience, and as such has to deal only with the facts of ordinary knowledge without at any point being able to escape from the sphere of the phenomenal; in so far as any inferences are extended from this domain they are not scientific but metaphysical. Therefore, although it may be of use in the interests of "Freethought" to represent science as not merely neutral but negative in its bearings upon religion, the attempt to do so is detrimental to the interests of science; so far as it may be successful it can only tend to increase the suspicious dislike of scientific knowledge which large masses of the general public are already too apt to harbour. Still, as the leading object of the "International Library" is no doubt that of advancing anti-theistic dogma, its promoters are probably careless whether in so doing they are either loyal or just to the cause of science, under whose banner and in whose name they profess to march.

But beyond recording our dissent from the unreasonable and, from our point of view, pernicious association of "Science" with "Freethought" which is being carried through the "International Library," we have nothing further to do with this matter; in these columns we have only to deal with the science, and so shall not again refer to the freethought, although it may be noted as a curious illustration of the contrast between "the solid ground of nature" and the quicksands of speculative thinking, that one of our most recent reviews was that of a book by Dr. Lauder Brunton, who is certainly no less an authority in science than Dr. Aveling, and whose whole object was seen to be the exact reverse of that which appears in "The Student's Darwin,"—viz., to show that Darwinism is *not* opposed to theism. For ourselves, it is needless to add, we hold that the theory of evolution resembles all other scientific theories in having no point of legitimate contact with any ulterior question of metaphysics, further than that of removing from metaphysics certain erroneous arguments previously based upon imperfect knowledge.

Dr. Aveling has been a diligent student of Mr. Darwin's books, and on reading his epitome of them, even in the most cursory way, one is more than ever amazed at the enormous fertility of Mr. Darwin's work. At every page one feels how meagre the epitome is—often little better than an index—and yet for more than 300 pages the index runs on showing as in a sketch what the mind of one man has accomplished, till the reader who is able to remember how many and minute are the details which the index embraces is glad to agree with an introductory remark of the writer, "It is well that all of us should know at least the outline of the work that has been done by this man. For as the name of Chaucer marks the fourteenth, and the name of Shakespeare the sixteenth century, so probably will the name of Charles Darwin mark the nineteenth century in the years to come."

The object of the "Student's Darwin" is thus, as its author says, to furnish a brief summary of the main results of Mr. Darwin's labours, and as the abstract has on the whole been well made, it ought to be found useful for any one who has not time to read for himself the originals. It would have been desirable to have gone less into mere description of species, and more fully into the theory of their origin; for no one who is likely to read the book will profit by the former, while the chief object of the "Student's Darwin" ought to be that of rendering a careful and complete abstract of Darwinism. Yet this is far from being the case in the book before us When, for instance, we have the arguments from Classification, Morphology, Development, and Rudimentary Organs all compressed into less than two pages, it is evident that the analysis is becoming much too scanty; and in fact no one depending for his information upon this analysis alone could form any just idea of the mass of evidence in favour of evolution and natural selection which Mr. Darwin has collected. This fault is the less pardonable, because it cannot be pleaded in excuse for it that the author is pressed for space, seeing that throughout the book he every here and there devotes a paragraph or two to bad attempts at "fine writing," which, besides being blemishes from a literary point of view, absorb a number of pages which might have been profitably devoted to a further exposition of what he properly terms "the *magnum opus*."

Dr. Aveling, however, everywhere exhibits a just estimate of Mr. Darwin's powers, as a few quotations may suffice to show. "From these pages" (*i.e.* those of the Monograph of the Cirripedia) "the student will turn with renewed reverence for the great generaliser, who is so patient and so completely master of detail." "Preconceived notions are not for him. He states the arguments for the conclusions that would strengthen the position of the great theory of evolution only less clearly than he states those that tell against that theory. No man was ever more of judge than he; no man was ever less of advocate. . . . The obligations of Charles Darwin to other workers in the same field as himself are always paid with a cordiality and courtesy that must be as gratifying to them as they are natural to him." "Only thirty-four years and the man who has produced the new thoughts is still among us! To-day they form part of the accepted creed of scientific thinkers. . . . To those who remember how few of the great have beheld with their own patient eyes their own greatness in some faint degree recognised during their own lives, their own thoughts accepted as true guides by the thoughtful, assuredly there is cause for comfort here." "Looking back over them again" (*i.e.* the whole series of works), "we cannot fail to be impressed with those two large attributes of genius that are especially his—unrivalled powers of observation, unrivalled powers of generalisation. And the homage that we pay him to-day is, I am assured, but the feeblest of utterances as compared with the heartfelt gratitude and wondering praise that will be the reward of this great thinker in those future times when the very lowliest in the land shall have full grasp of the meaning of his teaching," &c.

On the whole, the "Student's Darwin" deserves to be successful in its object of popularising Mr. Darwin's work. The great bar to its usefulness will be its needlessly aggressive tone towards religion, which is sure greatly to lessen a circulation which it might otherwise have had. GEORGE J. ROMANES

## LETTERS TO THE EDITOR

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

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

### Tebbutt's Comet—Origination of its Proper Light

WHILE there seems now no doubt that the honour of being the discoverer of the great comet of 1881 belongs without question to that life-long and most persevering observer, as well as successful computer, of comets, in Australia, Mr. John Tebbutt, three communications which chance to have arrived here this morning from different countries contain most diverse ideas of the nature of that portion of the comet's light which universal spectroscopic observation proves is inherent to the comet itself, indicating the existence there of carburetted gas of some kind, and is quite distinct from the concomitant weak reflection of solar light.

1. In Abbé and Chanoine Moigno's *Les Mondes* for August 25, that excellent physicist in Paris, M. Jamin, is represented as stating that the comet's carburetted gas could be rendered "properly" luminous only in two modes, viz., either by combustion or electric discharges. "If by combustion," says he, "how did it first take fire? what keeps up the fire perpetually? and how are the materials of the comet kept, in such a fire, from becoming red-hot, and then giving out quite a different spectrum to any that has yet been observed?" Wherefore he concludes that the cause of the "proper" light of the comet is the illumination of its constituent molecules by electric discharge, as in the gas-vacuum tubes of our laboratories.

2. But next comes a pamphlet from that accomplished spectroscopist and astronomer, Prof. C. A. Young of Princeton, New Jersey, U.S., setting forth that the bands of carburetted gas seen in the comet's spectrum do most admirably and exactly agree with the combustion-bands of coal-gas and air, as seen in a Bunsen-burner or a blowpipe flame, or in the blue base of all carbo-hydrogen flames known; while they do, on the contrary, most eminently, markedly, and distinctly disagree from the bands of the spectra of the same gases as seen in gas vacuum-tubes when illumined by electric discharge. And this conclusion of the eminent American physicist is confirmed by a pamphlet just received from M. Fiévez, the spectroscopic observer of the re-organised Royal Observatory of Brussels; as was also announced at the very time of the comet's appearance by the present most acutely observing Astronomer-Royal at Greenwich.

3. What then! Is M. Jamin's theory of the comet's proper light being entirely due to electrical illumination utterly overthrown, and the celestial phenomenon given over to a process of combustion, the mere mention of the necessary details of which suffices to show it ridiculous and impossible?

4. Not yet, I venture to think. We ought to discriminate in such a case most carefully between electricities of different intensities and different temperatures. Something too of that kind, and even much to the purpose of this cometary case, I had the honour of setting forth to the Royal Society, Edinburgh, last year, in a paper which is now being printed for their *Transactions*. For it was shown therein that, when using an induction-coil capable of giving sparks of such intensity as to be five inches long in the open air, a gas vacuum-tube of olefiant gas showed only the carburetted bands which Prof. Young alludes to as being absolutely *not* the bands which the spectrum of the comet exhibited. But when a smaller coil was employed, and more particularly when its outer helix of long thin wire was replaced by another of short thick wire (specially prepared for the experiment), and the sparks thereby lowered in intensity to such a degree as from 1·3 of an inch, to be capable of only passing through 0·2 inch of air, then, when employed to illuminate the same olefiant gas vacuum-tube, besides the bands seen before (but now more faintly), another set of bands appeared, which were exactly those of the combustion of coal-gas and air, of Bunsen burners, blowpipe flames, blue base of all carbo-hydrogen flames, and finally—*teste* Prof. C. A. Young, M. Fiévez, the Astronomer-Royal, W. H. M. Christie, and others—of Tebbutt's great comet of 1881.

5. From this condensation of testimonies I presume that no other conclusion is to be drawn than that the electrical discharges permeating the whole length of a comet's tail must be something exceedingly weak in intensity;—and the gentlemen who employ electrically lit-up gas vacuum-tubes in their laboratories must do their spiriting with them in future much more gently, if they would really arrive at what goes on in cometary existences. The following exception, too, duly mentioned by Prof. Young, to his general rule, seems to tend in the same direction. For he states "that while the evidence as to the identity of the flame and comet spectra is almost overwhelming, the peculiar ill-defined appearance of the cometary bands at the time of the comet's greatest brightness is, however, something which he has not yet succeeded in imitating with the flame spectrum."

6. "Certainly not," we may add to this most honest confession; for as the comet's greater brightness near its perihelion passage could hardly be due to anything else than a temporary increase in the intensity of its illuminating electric currents, that would tend to bring out the *tube-set* of carburetted bands to interfere with, and spoil the neatness and sharpness of, the so-called *flame*-bands, and would certainly imply a quality or temperature which does not exist in any known simple flame, but is found in the spark of even the smallest induction coil, unless some special means are taken to damp down its intensity.

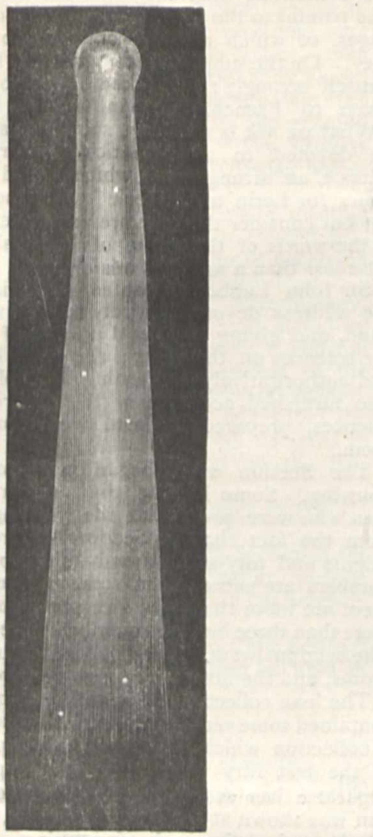
I have long wished at this Observatory to try a whole course of electric illuminations, as of the old friction machine, Holtz's machine, modern dynamo-machine, coils in variety, and whatever is capable of giving out electricity in any visible luminous shape; but the state of miserable starvation in which this Royal Observatory, Edinburgh, is kept throughout all its branches by Government, and their continued neglect of the applications of their own "Board of Visitors" to "endeavour to obtain justice to this Observatory"—the very words of the last public remit from the Board-meeting, of which the venerable Duncan McLaren, then M.P. for Edinburgh, was chairman—prevent any important apparatus being purchased, or even obtained on loan, to prosecute the inquiries which the science of the times demands. PIAZZI SMYTH,

Astronomer-Royal for Scotland

Royal Observatory, Edinburgh, August 29

### Schaeberle's Comet

SINCE my last remarks I have had an opportunity to examine this fine object with the 6″ Cooke equatorial. On Wednesday evening, the 24th, simultaneously with the Great Bear stars, it was easily seen as soon as twilight set in, near the horizon and considerably more to the west than on the 21st. With a comet eyepiece it presented, in spite of its low altitude, a sharp and well-defined figure. The nucleus was stellar-like, with, I thought, a still brighter minute central point. No jets of light proceeded directly from it, but it appeared surrounded by a circular nebulosity of greater extent than the base of the tail, and giving the headed form to the comet frequently seen in old drawings of these objects. The tail was straight, long, and luminous, with a central ray of condensed light which gave it a cylindrical look. When first examined three small stars were involved in the tail without any apparent diminishing of their brightness; while two others below served to define the limit of the tail's visibility in the comet eyepiece. This measured two degrees only, but both it and the nucleus were of a peculiarly fine pale blue tint. I send a drawing of the telescopic appearance of the comet at 8h. 40m. On the nights of the 27th and 28th the comet was again examined at about 8h. 30m. Under a lower power Kellner the appearance was that of a round comet with a central nucleus and circular coma. The tail was to be seen, but was quite faint, and as before was less at the base than the width of



Sketch of Schaeberle's Comet, August 24, 8h. 40m. in 6″ Cooke equatorial.

the coma. Dr. de Konkoly I see has examined the spectrum of this comet, and found it a faint continuous one, with three tolerably bright lines, at following positions:—

| | | Estimated brightness. |
|---|---|---|
| I. | 5601 ± 2·5 ... ... ... ... | 0·4 |
| II. | 5161 ± 0·9 ... ... ... ... | 1·0 |
| III. | 4753 ± 0·6 ... ... ... ... | 0·8 |

Schaeberle's Comet, August 28, 8h. 32m.

The appearance of this comet throughout has been peculiarly distinguished from that class in which jets of light streaming from the nucleus in front fall back to form the tail or a brigh margin to it. These, as far as I have seen, have been absent.

Guildown, August 31 J. RAND CAPRON

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### Comet *b* 1881

M. CH. FIEVEZ, the Astronomer adjoint at the Royal Observatory, Brussels, has been good enough to send me a copy of his note on the analysis of the light of this comet, made with the 15-inch Merz-Cooke equatorial, provisionally installed at the Avenue Cortenberg. The polariscopic observations demonstrate that the polarisation of the nucleus was strong (très nette et bien accentuée), while that of the tail was very weak. These observations were made at several days interval, from 11h. till midnight. Sky polarisation was scarcely sensible. The spectroscopic observations proved the spectrum of the comet to consist of four bands of intensity in the following order: green, blue, violet, and yellow, with wave-lengths 5160, 4780, 4200 (about), and 5620. The original appearance of these bands was modified as the comet receded from the sun, their edges towards the red then becoming more and more defined. The nucleus presented a brilliant continuous spectrum, in which however the Fraunhofer lines were not recognised. The conclusions arrived at by M. Fievez were as follows:—That a great part of the light of the comet was inherent to it, while the other part was reflected solar light. That the strong polarisation of the nucleus indicated a marked state of condensation of the matter composing it. That the spectrum differed little from that of other comets. Lastly, that the marked modifications in the brilliancy of the continuous spectrum, and in the appearance of the spectrum bands indicated a progressive diminution in the comet's temperature. The chief interest in the above observations attaches to the feeble polarisation detected in the tail as compared with that found by Prof. A. W. Wright and Mr. Cowper Ranyard, and in the absence of the Fraunhofer lines, which were measured by Dr. N. de Konkoly, and also photographed by Dr. Huggins. Whence, we may ask, arises the divergence of conclusions arrived at by M. Fievez and Prof. Wright respectively, the one considering that the principal part of the comet's light is from itself, the other that it is reflected sunlight, and why were the Fraunhofer lines seen in the one case, and not in the other? The answer lies, I think, not with the instruments employed, but rather in the interesting probability of change in the comet's structure or condition during the time of its examination. A comparison of the many observations recorded during its stay with us may possibly lead to important discoveries in this direction. I am much interested to see that Prof. C. A. Young informs us that the green band was seen by observers at Princeton split up into fine sharp lines coinciding with those seen in the flame spectrum, a result to be expected, but hitherto not attained. J. RAND CAPRON

Guildown, September 3

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THE comet at present visible was examined by me with the spectroscope on the 8¼-inch refractor on Saturday evening, August 27. The three principal hydrocarbon bands were plainly visible, the central one being the brightest, and on comparing them with the spectrum of a spirit-lamp flame the coincidence of the least refrangible sides of the bands in the two spectra was sensibly complete. The nucleus gave a narrow continuous spectrum, and I could see no trace of such a spectrum except from that point. I could see no other band in the spectrum except the three above mentioned, but the proximity of the comet to the horizon may have something to do with this.

GEORGE M. SEABROKE

Temple Observatory, Rugby, August 29

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### A Pink Rainbow

I SPENT Sunday, August 21, at Mr. Tennyson's house, Aldworth, near Haslemere. The house stands on an elevated ledge of the Blackdown range, looking over the Weald towards the Brighton Downs, between east and south-east. About sunset the deep red of the south-eastern sky attracted our attention, and while we were looking at it we saw stretching across it a well-marked rainbow, but of a uniform red or pink colour, which Mrs. Tennyson compares, in a note I have just had from her, to a "pink postage-stamp"—not the one now in use, but the last discarded one. This was seen distinctly by Mrs. Tennyson, Mr. Hallam Tennyson, and myself for, I think, more than a minute. Mr. Hallam went to call his father, who was in another room, but before he came, "the bow," to quote Mr. Tennyson's words, "had assumed its usual colours, which were, however, very faint." Mrs. Tennyson says the pink colour "was visible for a very little time just at sunset, and then I saw a dull olive green at the lower edge." After that, as Mr. Tennyson says, we all saw the vanishing ghost, as it were, of an ordinary rainbow. The actual uniform redness came just at sunset, as marked in the almanac we consulted—ten minutes past seven. A. M.

August 26

### The Glacial Period

PLEASE correct an error in the notice of my paper on the Glacial Period (NATURE, vol. xxiv. p. 364). It is on the western slope of New Zealand that the glaciers reach to the highest mean annual temperature (10° C., or 50° F.) as well as to the lowest level. *Apropos* of my studies on this subject, I should be very glad to meet some of the British glacialists at Venice, at the third International Geographical Congress, and discuss some points of interest with them. As there is, a few days later, an International Geological Congress at Bologna, it will be the easier for geologists to make a short stay at Venice before. The Geographical Congress begins on September 15.

St. Petersburg, August 13-25 A. WOEIKOF

## *THE BRITISH ASSOCIATION*

THE Jubilee Meeting of the British Association has come to a close, and whether we take the test of work done, or of the numbers present as members or associates, it must be admitted that it has been a great success. While in 1879, in the densely populated town of Sheffield, the total was 1404, and at Swansea last year 915, the number has risen this year to 2533, which includes 22 foreign members, 510 ladies, and 1173 associates. Of course York does not supply the whole of the latter: many come from Leeds, Sheffield, and Scarborough, and the surrounding towns. Seven times previously has the number been greater; the maximum (3335) having been attained at Newcastle-on-Tyne in 1863. As regards work done, it may be mentioned that on Friday nearly a hundred papers were announced for reading in the various sections. One of the laws of the Jewish jubilee festival was that the land should remain untilled for a year; but we have reversed this, and only cultivated our scientific soil the more. Sir David Brewster, in the original letter which laid the foundations of the society, suggested York as the most central city of the three kingdoms, but he first inquired "if York will furnish the accommodation necessary for so large a meeting, which might perhaps consist of 100 individuals." Apparently therefore he did not contemplate the admission of associates, or the use of the Association as a means of scattering broadcast the results of the scientific year, but rather regarded it as a means whereby the cultivators of science might become better acquainted with each other at a time when communication with London was far more difficult, and intercourse through scientific publications far more restricted than now. But the first meeting numbered 350 members, and included some of the most representative men of science of the day. On this occasion the presidential address lasted five minutes.

The proceedings commenced on Wednesday, August 31, by the reading of the Report of the Council, in which it was announced that Mr. P. L. Sclater had resigned the office of general secretary, and that he would be succeeded by Mr. F. M. Balfour of Cambridge. Mr. G. E. Gordon has also retired from the assistant secretaryship, and is to be succeeded by Prof. Bonney, with the title of secretary and a salary of 300*l.* per annum, with 25*l.* for travelling expenses. Mr. Spottiswoode succeeds Sir Philip de Malpas Grey Egerton as trustee.

The new members of council are Messrs. Warren De La Rue, A. Vernon Harcourt, G. W. Hastings, J. C. Hawkshaw, and G. Prestwich.

Sir John Lubbock's address was listened to by a very crowded audience. The Exhibition Hall is a fine building, and was prettily decorated, but its acoustic properties are somewhat deficient, and the unsteady electric light was painful to the eyes. The address occupies fifty octavo pages, of which nearly twenty were omitted during delivery. On the subject of education the President expressed himself strongly; he asked that more time should be given to French, German, science, and mathematics. "What we ask is that, say, six hours a week each should be devoted to mathematics, modern languages, and science, an arrangement which would still leave twenty hours for Latin and Greek"; and he added, "we cannot but consider that our present system of education is, in the words of the Duke of Devonshire's Commission, little less than a national misfortune."

Sir John Lubbock adopted a judicious mean between the address devoted entirely to one subject on the one hand, and giving a general *résumé* of the progress of all the sciences on the other; for while he spoke in detail and authoritatively concerning the biological sciences, he also furnished accounts of the progress of the physical sciences, prepared by men well competent to discuss them.

The Section work began in earnest on Thursday morning. Some idea of the number of representative men who were present at the meeting may be gathered from the fact that in Section A there are ten vice-presidents and fifty-seven members of committee, and these numbers are exceeded in some of the sections; so that there are more than fifty vice-presidents of sections, and more than three hundred members of sectional committees. The sections were housed in capacious and very suitable rooms, and the attendance was very good.

The loan collection of scientific apparatus, although it contained some very interesting examples, was by no means a collection which represents the experimental progress of the last fifty years, and the appeal for historical apparatus has scarcely been responded to. The exhibition was shown at the Thursday *soirée*, and remained open till the end of the week of meeting. A good catalogue of thirty-two pages was prepared. We may particularly notice some beautifully-finished telescopes and transit instruments, and an electric chronograph exhibited by Messrs. T. Cooke and Sons; a model of the Vienna 27-inch refractor and its dome by Mr. Howard Grubb; and a very old telescope constructed by Abraham Sharp. The Manchester Literary and Philosophical Society exhibited some of the apparatus used by John Dalton in his researches; and the Science and Art Department sent astrolabes and sun-dials of the sixteenth, seventeenth, and eighteenth centuries. A few instruments were sent by foreign makers. Dr. Stone exhibited a large syren fitted with a key-board and worked at an uniform rate by clockwork. A quantity of physiological apparatus was exhibited by Dr. Burdon-Sanderson and Mr. G. B. Thistleton. Mr. Francis Galton exhibited and explained his composite photographic process, "a method of superposing the images of separate portraits and thence creating a face, the sum of all the components employed; it has a curious air of individuality about it, but is a perfectly ideal face, like all, but exactly resembling none." Dr. Tempest Anderson, one of the local secretaries, exhibited some ophthalmic appliances. The North-Eastern Railway Company exhibited an interesting meteorite which fell on March 14 last between the Middlesbrough and Ormesby stations of the Guisborough line. It is of the stony tufaceous type, and weighs three and a half pounds.

On Friday afternoon several manufactories were visited, also the gas-works and water-works. Messrs. Cooke's works were of especial interest, particularly the processes connected with the grinding of lenses and the graduation of circles by means of a large dividing-engine, the great circle of which is marked with divisions, each of which is equal to five minutes of arc. Saturday afternoon was as usual devoted to excursions, but the steady downpour of rain did much to mar the enjoyment. Several people in the vicinity of York have entertained the members very hospitably, and have thrown open their houses. On Monday the usual meteorological breakfast took place; forty persons were present, and meteorology was the chief order of the day in Section A. In the evening Mr. Spottiswoode gave a discourse on "The Electric Discharge." The Red Lion Club met on Tuesday before the *soirée*.

Southampton has been chosen as the place of meeting in 1882, and Dr. C. W. Siemens has been elected president. A vigorous contest for the honour of receiving the Association took place yesterday between six towns:—Leicester, Nottingham, Southport, Oxford, Birmingham, and Aberdeen. The claims of each town were stated by delegates, and afterwards votes were taken by a show of hands. Birmingham withdrew. The President of the Royal Society, Sir Joseph Hooker, and Professors Acland, H. J. S. Smith, and Prestwich, strongly advocated the claims of Oxford, and the show of hands was declared to be in its favour. Southport was second on the list. Worcester has lodged a claim for 1884.

Altogether more than three hundred papers or reports have been read.

Eighteen papers were put on the list of Section A for Tuesday; twenty-eight in the Geological Section, thirteen in that of Anthropology, and fifteen in Mechanical Science. Thus the work has never flagged at all.

At the Committee Meeting on Wednesday Capt. Bedford Pim gave notice of motion that the meeting be held in Canada in 1885.

The following grants have been made:—

| | £ |
|---|---|
| The Council—Exploration of Mountain District of Eastern Equatorial Africa ... | 100 |
| *A—Mathematics and Physics* | |
| Mr. G. H. Darwin—Lunar Disturbance of Gravity ... | 15 |
| Dr. A. Schuster—Meteoric Dust ... | 20 |
| Prof. Sylvester—Fundamental Invariants (partly renewed) | 80 |
| Mr. R. H. Scott—Synoptic Charts of the Indian Ocean ... | 50 |
| Prof. G. C. Foster—Standards for Use in Electrical Measurements (partly renewed) ... | 100 |
| *B—Chemistry* | |
| Prof. Dewar—Present State of Knowledge of Spectrum Analysis ... | 5 |
| Prof. Balfour Stewart—Calibration of Mercurial Thermometers ... | 20 |
| Prof. Roscoe—Wave-lengths Tables of Spectra of Elements | 50 |
| Dr. Hugo Müller—Chemical Nomenclature ... | 10 |
| Prof. Odling—Photographing the Ultra-Violet Spark Spectra ... | 25 |
| *C—Geology* | |
| Dr. J. Evans—Record of the Progress of Geology ... | 100 |
| Prof. Ramsay—Earthquake Phenomena of Japan ... | 25 |
| Dr. H. C. Sorby—Conditions of Conversion of Sedimentary Materials into Metamorphic Rocks ... | 10 |
| Prof. W. C. Williamson—Fossil Plants of Halifax ... | 15 |
| Dr. Sorby—Conversion of Sediments into Metamorphic Rocks ... | 10 |
| Prof. A. C. Ramsay—Geological Map of Europe ... | 25 |
| Prof. E. Hull—Circulation of Underground Waters ... | 15 |
| Prof. W. C. Williamson—Tertiary Flora associated with the Basalts of the North of Ireland ... | 20 |
| Dr. Sorby—British Fossil Polyzoa ... | 10 |
| Prof. Leith-Adams—Carboniferous Limestone Caves in South Ireland ... | 10 |
| Prof. Green—Exploration of Raygill Fissure ... | 20 |
| *D—Biology* | |
| Mr. F. M. Balfour—Table at the Zoological Stations at Naples ... | 80 |
| Dr. Burdon-Sanderson—Albuminoid Substances of Serum | 10 |
| Dr. Pye Smith—Influence of Bodily Exercise on the Elimination of Nitrogen ... | 50 |
| Dr. M. Foster—Zoological Station in Scotland ... | £40 |
| Mr. J. Cordeaux—Migration of Birds ... | 15 |
| Lieut.-Col. Godwin-Austen—Natural History of Socotra | 100 |
| Mr. Staniton—Record of Zoological Literature ... | 100 |
| Mr. Sclater—Natural History of Timorlaut ... | 100 |
| Prof. Flower—Photographs of Typical Races ... | 10 |
| *Statistics* | |
| Mr. F. Galton—Anthropometrics ... | 50 |

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## SECTION A

### MATHEMATICAL AND PHYSICAL

OPENING ADDRESS BY SIR WILLIAM THOMSON, F.R.S., PROFESSOR OF NATURAL PHILOSOPHY IN THE UNIVERSITY OF GLASGOW, PRESIDENT OF THE SECTION

*On the Sources of Energy in Nature Available to Man for the Production of Mechanical Effect*

DURING the fifty years' life of the British Association, the Advancement of Science for which it has lived and worked so well has not been more marked in any department than in one which belongs very decidedly to the Mathematical and Physical Section—the science of Energy. The very name energy, though first used in its present sense by Dr. Thomas Young about the beginning of this century, has only come into use practically after the doctrine which defines it had, during the first half of the British Association's life, been raised from a mere formula of mathematical dynamics to the position it now holds of a principle pervading all nature and guiding the investigator in every field of science.

A little article communicated to the Royal Society of Edinburgh a short time before the commencement of the epoch of energy under the title "On the Sources Available to Man for the Production of Mechanical Effect"[1] contained the following:—

"Men can obtain mechanical effect for their own purposes by working mechanically themselves, and directing other animals to work for them, or by using natural heat, the gravitation of descending solid masses, the natural motions of water and air, and the heat, or galvanic currents, or other mechanical effects produced by chemical combination, but in no other way at present known. Hence the stores from which mechanical effect may be drawn by man belong to one or other of the following classes:—

"I. The food of animals.

"II. Natural heat.

"III. Solid matter found in elevated positions.

"IV. The natural motions of water and air.

"V. Natural combustibles (as wood, coal, coal-gas, oils, marsh-gas, diamond, native sulphur, native metals, meteoric iron).

"VI. Artificial combustibles (as smelted or electrically-deposited metals, hydrogen, phosphorus).

"In the present communication, known facts in natural history and physical science, with reference to the sources from which these stores have derived their mechanical energies, are adduced to establish the following general conclusions:—

"1. *Heat radiated from the sun* (sunlight being included in this term) *is the principal source of mechanical effect available to man.*[2] From it is derived the whole mechanical effect obtained by means of animals working, water-wheels worked by rivers, steam-engines, galvanic engines, windmills, and the sails of ships.

"2. The motions of the earth, moon, and sun, and their mutual attractions, constitute an important source of available mechanical effect. From them all, but chiefly no doubt from the earth's motion of rotation, is derived the mechanical effect of water-wheels driven by the tides.

"3. The other known sources of mechanical effect available to man are either terrestrial—that is, belonging to the earth, and available without the influence of any external body—or meteoric—that is, belonging to bodies deposited on the earth from external space. Terrestrial sources, including mountain quarries and mines, the heat of hot springs, and the combustion of native sulphur, perhaps also the combustion of inorganic native combustibles, are actually used, but the mechanical effect

[1] Read at the Royal Society of Edinburgh on February 2, 1852 (*Proceedings* of that date).

[2] A general conclusion equivalent to this was published by Sir John Herschel in 1833. See his "Astronomy," edit. 1849, § (399).

obtained from them is very inconsiderable, compared with that which is obtained from sources belonging to the two classes mentioned above. Meteoric sources, including only the heat of newly-fallen meteoric bodies, and the combustion of meteoric iron, need not be reckoned among those available to man for practical purposes."

Thus we may summarise the natural sources of energy as Tides, Food, Fuel, Wind, and Rain.

Among the practical sources of energy thus exhaustively enumerated, there is only one not derived from sun-heat—that is the tides. Consider it first. I have called it *practical*, because tide-mills exist. But the places where they can work usefully are very rare, and the whole amount of work actually done by them is a drop to the ocean of work done by other motors. A tide of two metres' rise and fall, if we imagine it utilised to the utmost by means of ideal water-wheels doing with perfect economy the whole work of filling and emptying a dock-basin in infinitely short times at the moments of high and low water, would give just one metre-ton per square metre of area. This work done four times in the twenty-four hours amounts to 1-1620th of the work of a horse power. Parenthetically, in explanation, I may say that the French metrical equivalent (to which in all scientific and practical measurements we are irresistibly drawn, notwithstanding a dense barrier of insular prejudice most detrimental to the islanders),—the French metrical equivalent of James Watt's "horse-power" of 550 foot-pounds per second, or 33,000 foot-pounds per minute, or nearly two million foot-pounds per hour, is 75 metre-kilogrammes per second, or 4½ metre-tons per minute, or 270 metre-tons per hour. The French ton of 1000 kilogrammes used in this reckoning is 0·984 of the British ton.

Returning to the question of utilising tidal energy, we find a dock area of 162,000 square metres (which is a little more than 400 metres square) required for 100 horse-power. This, considering the vast costliness of dock construction, is obviously prohibitory of every scheme for economising tidal energy by means of artificial dock-basins, however near to the ideal perfection might be the realised tide-mill, and however convenient and non-wasteful the accumulator—whether Faure's electric accumulator, or other accumulators of energy hitherto invented or to be invented—which might be used to store up the energy yielded by the tide-mill during its short harvests about the times of high and low water, and to give it out when wanted at other times of the six hours. There may however be a dozen places possible in the world where it could be advantageous to build a sea-wall across the mouth of a natural basin or estuary, and to utilise the tidal energy of filling it and emptying it by means of sluices and water-wheels. But if *so* much could be done, it would in many cases take only a little more to keep the water out altogether, and make fertile land of the whole basin. Thus we are led up to the interesting economical question, whether is forty acres (the British *agricultural* measure for the area of 162,000 square metres) or 100 horse-power more valuable. The annual cost of 100 horse-power night and day, for 365 days of the year, obtained through steam from coals, may be about ten times the rental of forty acres at 2*l*. or 3*l*. per acre. But the value of land is essentially much more than its rental, and the rental of land is apt to be much more than 2*l*. or 3*l*. per acre in places where 100 horse-power could be taken with advantage from coal through steam. Thus the question remains unsolved, with the possibility that in one place the answer may be *one hundred horse-power*, and in another *forty acres*. But, indeed, the question is hardly worth answering, considering the rarity of the cases, if they exist at all, where embankments for the utilisation of tidal energy are practicable.

Turning now to sources of energy derived from sun-heat, let us take the wind first. When we look at the register of British shipping and see 40,000 vessels, of which about 10,000 are steamers and 30,000 sailing ships, and when we think how vast an absolute amount of horse-power is developed by the engines of those steamers, and how considerable a proportion it forms of the whole horse-power taken from coal annually in the whole world at the present time, and when we consider the sailing ships of other nations, which must be reckoned in the account, and throw in the little item of windmills, we find that, even in the present days of steam ascendency, old-fashioned Wind still supplies a large part of all the energy used by man. But however much we may regret the time when Hood's young lady, visiting the fens of Lincolnshire at Christmas, and writing to her dearest friend in London (both sixty years old now if they are alive), describes the delight of sitting in a bower and looking over the wintry plain, not desolate, because "windmills lend revolving animation to the scene," we cannot shut our eyes to the fact of a lamentable decadence of wind-power. Is this decadence permanent, or may we hope that it is only temporary? The subterranean coal-stores of the world are becoming exhausted surely, and not slowly, and the price of coal is upward bound—upward bound on the whole, though no doubt it will have its ups and downs in the future as it has had in the past, and as must be the case in respect to every marketable commodity. When the coal is all burned; or, long before it is all burned, when there is so little of it left and the coal-mines from which that little is to be excavated are so distant and deep and hot that its price to the consumer is greatly higher than at present, it is most probable that windmills or wind-motors in some form will again be in the ascendant, and that wind will do man's mechanical work on land at least in proportion comparable to its present doing of work at sea.

Even now it is not utterly chimerical to think of wind superseding coal in some places for a very important part of its present duty—that of giving light. Indeed, now that we have dynamos and Faure's accumulator, the little want to let the thing be done is cheap windmills. A Faure cell containing 20 kilogrammes of lead and minium charged and employed to excite incandescent vacuum-lamps has a light-giving capacity of 60-candle hours (I have found considerably more in experiments made by myself; but I take 60 as a safe estimate). The charging may be done uninjuriously, and with good dynamical economy in any time from six hours to twelve or more. The drawing-off of the charge for use may be done safely, but somewhat wastefully, in two hours, and very economically in any time of from five hours to a week or more. Calms do not last often longer than three or four days at a time. Suppose then that a five days storage-capacity suffices (there may be a little steam-engine ready to be set to work at any time after a four-days' calm, or the user of the light may have a few candles or oil-lamps in reserve, and be satisfied with them when the wind fails for more than five days). One of the twenty kilogramme cells charged when the windmill works for five or six hours at any time, and left with its 60 candle-hours' capacity to be used six hours a day for five days, gives a 2-candle light. Thus thirty-two such accumulator cells so used would give as much light as four burners of London 16-candle gas. The probable cost of dynamo and accumulator does not seem fatal to the plan, if the windmill could be had for something comparable with the prime cost of a steam-engine capable of working at the same horse-power as the windmill when in good action. But windmills as hitherto made are very costly machines, and it does not seem probable that, without inventions not yet made, wind can be economically used to give light in any considerable class of cases, or to put energy into store for work of other kinds.

Consider, lastly, rain-power. When it is to be had in places where power is wanted for mills or factories of any kind, water-power is thoroughly appreciated. From time immemorial, water-motors have been made in large variety for utilising rain-power in the various conditions in which it is presented, whether in rapidly-flowing rivers, in natural waterfalls, or stored at heights in natural lakes or artificial reservoirs. Improvements and fresh inventions of machines of this class still go on, and some of the finest principles of mathematical hydrodynamics have, in the lifetime of the British Association, and, to a considerable degree, with its assistance, been put in requisition for perfecting the theory of hydraulic mechanism and extending its practical applications.

A first question occurs: Are we necessarily limited to such natural sources of water-power as are supplied by rain falling on hill-country, or may we look to the collection of rain-water in tanks placed artificially at sufficient heights over flat country to supply motive power economically by driving water-wheels? To answer it: Suppose a height of 100 metres, which is very large for any practicable building, or for columns erected to support tanks; and suppose the annual rainfall to be three-quarters of a metre (30 inches). The annual yield of energy would be 75 metre-tons per square metre of the tank. Now one horse-power for 365 times 24 hours is 236,500 foot-tons; and therefore (dividing this by 75) we find 3153 square metres as the area of our supposed tank required for a continuous supply of one horse-power. The prime cost of any such structure, not to speak of the value of the land which it would cover, is utterly prohibitory of any such plan for utilising the motive power of rain. We may or may not look forward hopefully to the time when windmills will again "lend revolving animation" to a dull flat country; but we certainly need not be afraid that the scene will be marred by forests of iron columns taking the place of natural trees, and gigantic tanks overshadowing the fields and blackening the horizon.

To use rain-power economically on any considerable scale we must look to the natural drainage of hill country and take the water where we find it either actually falling or stored up and ready to fall when a short artificial channel or pipe can be provided for it at moderate cost. The expense of aqueducts, or of underground water-pipes, to carry water to any great distance—any distance of more than a few miles or a few hundred yards—is much too great for economy when the yield to be provided for is *power;* and such works can only be undertaken when the *water itself* is what is wanted. Incidentally, in connection with the water supply of towns, some part of the energy due to the head at which it is supplied may be used for power. There are however but few cases (I know of none except Greenock) in which the energy to spare over and above that devoted to bringing the water to where it is wanted, and causing it to flow fast enough for convenience at every opened tap in every house or factory, is enough to make it worth while to make arrangements for letting the water-power be used without wasting the water-substance. The cases in which water-power is taken from a town supply are generally very small, such as working the bellows of an organ, or "hair-brushing by machinery," and involve simply throwing away the used water. The cost of energy thus obtained must be something enormous in proportion to the actual quantity of the energy, and it is only the smallness of the quantity that allows the convenience of having it when wanted at any moment, to be so dearly bought.

For anything of great work by rain-power, the water-wheels must be in the very place where the water supply with natural fall is found. Such places are generally far from great towns, and the time is not yet come when great towns grow by natural selection beside waterfalls, for power; as they grow beside navigable rivers, for shipping. Thus hitherto the use of water-power has been confined chiefly to isolated factories which can be conveniently placed and economically worked in the neighbourhood of natural waterfalls. But the splendid suggestion made about three years ago by Mr. Siemens in his presidential address to the Institution of Mechanical Engineers, that the power of Niagara might be utilised, by transmitting it electrically to great distances, has given quite a fresh departure for design in respect to economy of rain-power. From the time of Joule's experimental electromagnetic engines developing 90 per cent. of the energy of a Voltaic battery in the form of weights raised, and the theory of the electromagnetic transmission of energy completed thirty years ago on the foundation afforded by the train of experimental and theoretical investigations by which he established his dynamical equivalent of heat in mechanical, electric, electro chemical, chemical, electro-magnetic, and thermo-elastic phenomena, it had been known that potential energy from any available source can be transmitted electromagnetically by means of an electric current through a wire, and directed to raise weights at a distance, with unlimitedly perfect economy. The first large-scale practical application of electro-magnetic machines was proposed by Holmes in 1854, to produce the electric light for lighthouses, and persevered in by him till he proved the availability of his machine to the satisfaction of the Trinity House and the delight of Faraday in trials at Blackwall in April, 1857, and it was applied to light the South Foreland lighthouse on December 8, 1858. This gave the impulse to invention; by which the electro-magnetic machine has been brought from the physical laboratory into the province of engineering, and has sent back to the realm of pure science a beautiful discovery, that of the fundamental principle of the dynamo, made triply and independently, and as nearly as may be simultaneously in 1867 by Dr. Werner Siemens, Mr. S. A. Varley, and Sir Charles Wheatstone; a discovery which constitutes an electro-magnetic analogue to the fundamental electrostatic principle of Nicholson's revolving doubler, resuscitated by Mr. C. F. Varley in his instrument "for generating electricity" patented in 1860; and by Holtz in his celebrated electric machine; and by myself in my "replenisher" for multiplying and maintaining charges in Leyden jars for heterostatic electrometers, and in the electrifier for the siphon of my recorder for submarine cables.

The dynamos of Gramme and Siemens, invented and made in the course of these fourteen years since the discovery of the fundamental principle, give now a ready means of realising economically on a large scale for many important practical applications, the old thermo-dynamics of Joule in electro-magnetism; and, what particularly concerns us now in connection with my present subject, they make it possible to transmit electro-magnetically the work of waterfalls through long insulated conducting wires, and use it at distances of fifties or hundreds of miles from the source, with excellent economy—better economy, indeed, in respect to proportion of energy used to energy dissipated than almost anything known in ordinary mechanics and hydraulics for distances of hundreds of yards instead of hundreds of miles.

In answer to questions put to me in May, 1879,[1] by the Parliamentary Committee on Electric Lighting, I gave a formula for calculating the amount of energy transmitted, and the amount dissipated by being converted into heat on the way, through an insulated copper conductor of any length, with any given electromotive force applied to produce the current. Taking Niagara as example, and with the idea of bringing its energy usefully to Montreal, Boston, New York, and Philadelphia, I calculated the formula for a distance of 300 British statute miles (which is greater than the distance of any of those four cities from Niagara, and is the radius of a circle covering a large and very important part of the United States and British North America), I found almost to my surprise that even with so great a distance to be provided for, the conditions are thoroughly practicable with good economy, all aspects of the case carefully considered. The formula itself will be the subject of a technical communication to Section A in the course of the Meeting on which we are now entering. I therefore at present restrict myself to a slight statement of results.

1. Apply dynamos driven by Niagara to produce a difference of potential of 80,000 volts between a good earth connection and the near end of a solid copper wire of half an inch (1·27 centimetres) diameter, and 300 statute miles (483 kilometres) length.

2. Let resistance by driven dynamos doing work, or by electric lights, or, as I can now say, by a Faure battery taking in a charge, be applied to keep the remote end at a potential differing by 64,000 volts from a good earth-plate there.

3. The result will be a current of 240 webers through the wire taking energy from the Niagara end at the rate of 26,250 horse-power, losing 5250 (or 20 per cent.) of this by the generation and dissipation of heat through the conductor and 21,000 horse-power (or 80 per cent. of the whole) on the recipients at the far end.

4. The elevation of temperature above the surrounding atmosphere, to allow the heat generated in it to escape by radiation and be carried away by convection is only about 20° Centigrade; the wire being hung freely exposed to air like an ordinary telegraph wire supported on posts.

5. The striking distance between flat metallic surfaces with difference of potentials of 80,000 volts (or 5,000 Daniell's) is (Thomson's "Electrostatics and Magnetism," § 340) only 18 millimetres, and therefore there is no difficulty about the insulation.

6. The cost of the copper wire, reckoned at 8*d.* per lb., is 37,000*l.*; the interest on which at 5 per cent. is 1900*l.* a year. If 5250 horse-power at the Niagara end costs more than 1900*l.* a year, it would be better economy to put more copper into the conductor; if less, less. I say no more on this point at present, as the economy of copper for electric conduction will be the subject of a special communication to the Section.

I shall only say in conclusion that one great difficulty in the way of economising the electrical transmitting power to great distances (or even to moderate distances of a few kilometres) is now overcome by Faure's splendid invention. High potential, as Siemens, I believe, first pointed out, is the essential for good dynamical economy in the electric transmission of power. But what are we to do with 80,000 volts when we have them at the civilised end of the wire? Imagine a domestic servant going to dust an electric lamp with 80,000 volts on one of its metals! Nothing above 200 volts ought on any account ever to be admitted into a house or ship or other place where safeguards against accident cannot be made absolutely and for ever trustworthy against all possibility of accident. In an electric workshop 80,000 volts is no more dangerous than a circular saw.

[1] Printed in the Parliamentary Blue Book Report of the Committee on Electric Lighting, 1879.

Till I learned Faure's invention I could but think of step-down dynamos, at a main receiving-station, to take energy direct from the electric main with its 80,000 volts, and supply it by secondary 200 volt dynamos or 100 volt dynamos, through proper distributing wires, to the houses and factories and shops where it is to be used for electric lighting, and sewing-machines, and lathes, and lifts, or whatever other mechanism wants driving power. Now the thing is to be done much more economically, I hope, and certainly with much greater simplicity and regularity, by keeping a Faure battery of 40,000 cells always being charged direct from the electric main, and applying a methodical system of removing sets of 50, and placing them on the town-supply circuits, while other sets of 50 are being regularly introduced into the great battery that is being charged, so as to keep its number always within 50 of the proper number, which would be about 40,000 if the potential at the emitting end of the main is 80,000 volts.

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## SECTION D

### BIOLOGY

#### *Department of Anthropology*

OPENING ADDRESS BY PROF. W. H. FLOWER, LL.D., F.R.S., PRES. Z.S., V.P. ANTHROP. INST., &C., CHAIRMAN OF THE DEPARTMENT

IT is impossible for us to commence the work of this section of the Association without having vividly brought to our minds the loss which has befallen us since our last meeting—the loss of one who was our most characteristic representative of the complex science of anthropology—one who had for many years conducted with extraordinary energy, amidst multifarious other avocations, a series of researches into the history, customs, and physical characters of the early inhabitants of our island, for which he was so especially fitted by his archæological, historical, and literary, as well as his anatomical knowledge, and who was also the most popular and brilliant expositor, to assemblies such as meet together on these occasions, of the results of those researches. I need scarcely say that I refer to Prof. Rolleston.

Within the last few months the study of our subject in this country has received an impulse from the publication of a book —small in size, it is true, but full of materials for thought and instruction—the "Anthropology" of Mr. E. B. Tylor, the first work published in English with that title, and one very different in its scope and method from the old ethnological treatises.

The immense array of facts brought together in a small compass, the terseness and elegance of the style, the good taste and feeling with which difficult and often burning questions are treated, should give this book a wide circulation among all classes, and thorougly familiarise both the word and the subject to English readers.

The origin and early history of man's civilisation, his language, his arts of life, his religion, science, and social customs in the primitive conditions of society, are subjects in which, in consequence of their direct continuity with the arts and sciences, religious, political, and social customs among which we all live, by which we are all influenced, and about which we all have opinions, every person of ordinary education can and should take an interest. In fact, really to understand all these problems in the complex condition in which they are presented to us now, we ought to study them in their more simple forms, and trace them as far as may be to their origins.

But, as the author remarks, this book is only an introduction to anthropology, rather than a summary of all that it teaches; and some, even those that many consider the most important, branches of the subject are but lightly touched upon, or wholly passed over.

In one of the estimates of the character and opinions of the very remarkable man and eminent statesman, whose death the country was mourning last spring, it was stated: "Lord Beaconsfield had a deep-rooted conviction of the vast importance of race, as determining the relative dominance both of societies and of individuals" (*Spectator*, April 23, 1881); and with regard to the question of what he meant by "race," we have a key in the last published work of the same acute observer of mankind: "Language and religion do not make a race—there is only one thing which makes a race, and that is blood" ("Endymion," vol. ii. p. 205). Now "blood" used in this sense is defined as "kindred; relation by natural descent from a common ancestor; consanguinity" (Webster's "Dictionary"). The study of the true relationship of the different races of men is then not only interesting from a scientific point of view, but of great importance to statesmanship in such a country as this, embracing subjects representing almost every known modification of the human species, whose varied and often conflicting interests have to be regulated and provided for. It is to want of appreciation of its importance that many of the inconsistencies and shortcomings of the government of our dependencies and colonies are due, especially the great inconsistency between a favourite English theory and a too common English practice—the former being that all men are morally and intellectually alike, the latter being that all are equally inferior to himself in all respects: both propositions egregiously fallacious. The study of race is at a low ebb indeed when we hear the same contemptuous epithet of "nigger" applied indiscriminately by the Englishman abroad to the blacks of the West Coast of Africa, the Kaffirs of Natal, the Lascars of Bombay, the Hindoos of Calcutta, the aborigines of Australia, and even the Maoris of New Zealand!

But how is he to know better? Where in this country is any instruction to be had? Where are the books to which he may turn for trustworthy information? The subject, as I have said, is but slightly touched upon in the last published treatise on anthropology in our language. The great work of Pritchard, a compendium of all that was known at the time it was written, is now almost entirely out of date. In not a single university or public institution throughout the three kingdoms is there any kind of systematic teaching, either of physical or of any other branch of anthropology, except so far as comparative philology may be considered as bearing upon the subject. The one society of which it is the special business to promote the study of these questions, the Anthropological Institute of Great Britain and Ireland, is, I regret to say, far from flourishing. An anthropological museum, in the proper sense of the word, either public or private, does not exist in this country.

What a contrast is this to what we see in almost every other nation in Europe! At Paris there is, first, the Muséum d'Histoire Naturelle, where man, as a zoological subject—almost entirely neglected in our British Museum—has a magnificent gallery allotted to him, abounding not only in illustrations of osteology, but also in models, casts, drawings, and anatomical preparations showing various points in his physical or natural history, which is expounded to the public in the free lectures of the venerable Prof. Quatrefages and his able coadjutor, Dr. Hamy; there is also the vigorous Society of Anthropology, which is stated in the last annual report to number 720 members, showing an increase of forty-four during the year 1880, and which is forming a museum on a most extensive scale; and, finally, the School of Anthropology, founded by the illustrious Broca, whose untimely death last year, instead of paralysing, seems to have stimulated, the energies of colleagues and pupils into increased activity. In this school, supported partly by private subscriptions, partly by the public liberality of the Municipality of Paris, and of the Department of the Seine, are laboratories in which all the processes of anthropological manipulation are practised by students and taught to travellers. Here all the bodies of persons of outlandish nationalities dying in any of the hospitals of Paris are dissected by competent and zealous observers, who carefully record every peculiarity of structure discovered, and are thus laying the foundation for an exhaustive and trustworthy collection of materials for the comparative anatomy of the races of man. Here, furthermore, are lectureships on all the different branches. Biological and anatomical anthropology, ethnology, prehistoric, linguistic, social, and medical anthropology are all treated of separately by eminent professors who have made these departments their special study. The influence of so much activity is spreading beyond the capital. The foundation of an anthropological society at Lyons has been announced within the present year.

In Germany, although there is not at present any institution organised like the school at Paris, the flourishing state of the Berlin Ethnological Society, which also reports a large increase in the number of its members, the various other societies and journals, and the important contributions which are continually being made from the numerous intellectual centres of that land of learning, all attest the interest which the study of man excites there. In Italy, in the Scandinavian kingdoms, in Russia, and even in Spain, there are signs of similar activity. A glance at the recent periodical literature of America, especially the publications of the Smithsonian Institution, will show how strongly the scientific work of that country is setting in the same direction.

It is true that a very great proportion of the energies of the societies, institutions, and individuals who cultivate this vast subject are, in all these lands, as it is indeed to so great an extent in our own, devoted to that branch which borders upon the old and favourite studies of archæology and geology. The fascinating power of the pursuit of the earliest traces of man's existence upon the earth, with the possibilities of obtaining some glimpses of his mode of origin, is attested in the devotion seen everywhere in museums, in separate publications, and in journals, to pre-historic anthropology.

But, though the study of man's origin and earliest appearances upon the earth, and that of the structural modifications to which in course of time he has arrived, or the study of races, are intimately related, and will ultimately throw light upon one another, I venture to think that the latter is the more pressing of the two, as it is certainly the more practically important; and hence the necessity for greater attention to physical anthropology. In seeking for a criterion upon which to base our study of races, in looking for essential proofs of consanguinity of descent from common ancestors in different groups of men, I have no hesitation in saying that we must first look to their physical or anatomical characters, next to their moral and intellectual characters—for our purpose more difficult of apprehension and comparison—and, lastly, as affording hints, often valuable in aid of our researches, but rarely to be depended upon, unless corroborated from other sources, to language, religion, and social customs.

The study of the physical or anatomical character of the races of man is unfortunately a subject beset with innumerable difficulties. It can only be approached with full advantage by one already acquainted with the ordinary facts of human anatomy, and with a certain amount of zoological training. The methods used by the zoologist in discriminating species and varieties of animals, and the practice acquired in detecting minute resemblances and differences that an ordinary observer might overlook, are just what are required in the physical anthropologist.

As the great problem which is at the root of all zoology is to discover a natural classification of animals, so the aim of zoological anthropology is to discover a natural classification of man. A natural classification is an expression of our knowledge of real relationship, of consanguinity—of "blood," as the author of "Endymion" expresses it. When we can satisfactorily prove that any two of the known groups of mankind are descended from the same common stock, a point is gained. The more such points we have acquired, the more nearly shall we be able to picture to ourselves, not only the present, but the past distribution of the races of man upon the earth, and the mode and order in which they have been derived from one another.

The difficulties in the way of applying zoological principles to the classification of man are vastly greater than in the case of most animals; the problem being, as we shall see, one of much greater complexity. When groups of animals become so far differentiated from each other as to represent separate species, they remain isolated; they may break up into further subdivisions—in fact, it is only by further subdivision that new species can be formed; but it is of the very essence of species, as now universally understood by naturalists, that they cannot recombine, and so give rise to new forms. With the varieties of man it is otherwise. They have never so far separated as to answer to the physiological definition of species. All races are fertile one with another, though perhaps in different degrees. Hence new varieties have constantly been formed, not only by the segmentation, as it were, of a portion of one of the old stocks, but also by various combinations of those already established.

Neither of the old conceptions of the history of man, which pervaded the thought, and form the foundation of the works of all ethnological writers up to the last few years, rest on any solid basis, nor account for the phenomena of the present condition and distribution of the species.

The one view—that of the monogenist—was that all races, as we see them now, are the descendants of a single pair, who, in a comparatively short period of time, spread over the world from one common centre of origin, and became modified by degrees in consequence of changes of climate and other external conditions. The other—that of the polygenist—is that a certain number of varieties or species (no agreement has been arrived at as to the number, which is estimated by different authorities at from three to twenty or more) have been independently created in different parts of the world, and have perpetuated the distinctive characters as well as the geographical position with which they were originally endowed.

The view which appears best to accord with what is now known of the characters and distribution of the races of man, and with the general phenomena of nature, may be described as a modification of the former of these hypotheses.

Without entering into the difficult question of the method of man's first appearance upon the world, we must assume for it a vast antiquity—at all events as measured by any historical standard. Of this there is now ample proof. During the long time he existed in the savage state—a time compared to which the dawn of our historical period was as yesterday—he was influenced by the operation of those natural laws which have produced the variations seen in other regions of organic nature. The first men may very probably have been all alike; but, when spread over the face of the earth, and become subject to all kinds of diverse external conditions—climate, food, competition with members of his own species or with wild animals—racial differences began slowly to be developed through the potency of various kinds of selection acting upon the slight variations which appeared in individuals in obedience to the tendency implanted in all living things.

Geographical position must have been one of the main elements in determining the formation and the permanence of races. Groups of men isolated from their fellows for long periods, such as those living on small islands, to which their ancestors may have been accidentally drifted, would naturally, in course of time, develop a new type of features, of skull, of complexion or hair. A slight set in one direction, in any of these characters, would constantly tend to intensify itself, and so new races would be formed. In the same way different intellectual or moral qualities would be gradually developed and transmitted in different groups of men. The longer a race thus formed remained isolated, the more strongly impressed and the more permanent would its characteristics become, and less liable to be changed or lost, when the surrounding circumstances were altered, or under a moderate amount of intermixture from other races—the more "true," in fact, would it be. On the other hand, on large continental tracts, where no "mountains interposed make enemies of nations," or other natural barriers form obstacles to free intercourse between tribe and tribe, there would always be a tendency towards uniformity, from the amalgamation of races brought into close relation by war or by commerce. Smaller or feebler races have been destroyed or absorbed by others impelled by superabundant population or other causes to spread beyond their original limits; or sometimes the conquering race has itself disappeared by absorption into the conquered.

Thus, for untold ages, the history of man has presented a shifting kaleidoscopic scene; new races gradually becoming differentiated out of the old elements, and, after dwelling a while upon the earth, either becoming suddenly annihilated or gradually merged into new combinations; a constant destruction and reconstruction; a constant tendency to separation and differentiation, and a tendency to combine again into a common uniformity—the two tendencies acting against and modifying each other. The history of these processes in former times, except in so far as they may be inferred from the present state of things, is a difficult study, owing to the scarcity of evidence. If we had any approach to a complete palæontological record, the history of man could be reconstructed: but nothing of the kind is forthcoming. Evidences of the anatomical characters of man as he lived on the earth during the time when the great racial characteristics were being developed, during the long antehistoric period in which the negro, the Mongolian, and the Caucasian were being gradually fashioned into their respective types, is entirely wanting, or, if any exists, it is at present safely buried in the earth, perhaps to be revealed at some unexpected time and in some unforeseen manner.

It will be observed, and perhaps observed with perplexity by some, that no definition has as yet been given of the oft-recurring word "race." The sketch just drawn of the past history of man must be sufficient to show that any theory implying that the different individuals composing the human species can be parcelled out into certain definite groups, each with its well-marked and permanent limits separating it from all others, has no scientific foundation; but that in reality these individuals are aggregated into a number of groups of very different value in a zoological sense, with characters more or less strongly marked and permanent, and often passing insensibly into one another. The great groups are split up into minor subdivisions, and filling up the gaps between them all are intermediate or intercalary forms, derived either from the survival of individuals retaining the generalised or ancestral characters of a race from which two branches have separated and taken opposite lines of modification, or from the reunion of members of such branches in recent times. If we could follow those authors who can classify mankind into such divisions as trunks, branches, races, and sub-races, each having its definite and equivalent meaning, our work would appear to be greatly simplified, although perhaps we should not be so near the truth we are seeking. But being not yet in a position to define what amount of modification is necessary to constitute distinction of race, I am compelled to use the word vaguely for any considerable group of men who resemble each other in certain common characters transmitted from generation to generation.

In approaching the question of the classification of the races of man from a physical point of view, we must bestow great care upon the characters upon which we rely in distinguishing one group from another. It is well known in zoology that the modifications of a single organ or system may be of great value, or may be quite useless according as such modifications are correlated with others in different organs or systems, or are mere isolated examples of variation in the economy of the animal without structural changes elsewhere. The older ornithologists associated in one order all the birds with webbed feet, and the order thus constituted, *Natatores* or *Palmipedes*, which received the great sanction of Cuvier, still stands in many zoological compilations. Recent investigations into the anatomy of birds have shown that the species thus associated together show no other sign of natural affinity, and no evidence of being derived from the same stock. In fact, there is tolerably good proof that the webbing of the feet is a merely adaptive character, developed or lost, present or absent, irrespective of other structural modifications. In the same way, when anthropology was less advanced than it is now, it was thought that the distinction between long and short-headed, dolichocephalic and brachycephalic, people, pointed out by Retzius, indicated a primary division of the human species; but it was afterwards discovered that, although the character was useful otherwise, it was one of only secondary importance, as the long-headed as well as the short-headed group both included races otherwise of the strongest dissimilarity.

In all classifications the point to be first ascertained is the fundamental plan of construction; but in cases where the fundamental plan has undergone but little modification, we are obliged to make use of what appear trivial characters, and compensate for their triviality by their number. The more numerous the combinations of specialised characters, by which any species or race differs from its congeners, the more confidence we have in their importance. The separation of what is essential from what is incidental or merely superficial in such characters lies at the root of all the problems of this nature that zoologists are called upon to solve; and in proportion as the difficulties involved in this delicate and often perplexing discrimination are successfully met and overcome will the value of the conclusions be increased. These difficulties, so familiar in zoology, are still greater in the case of anthropology. The differences we have to deal with are often very slight; their significance is at present very little understood. We go on expending time and trouble in heaping up elaborate tables of measurements, and minutely recording every point that is capable of description, with little regard to any conclusions that may be drawn from them. It is certainly time now to endeavour, if possible, to discriminate characters which indicate deep-lying affinity from those that are more transient, variable, or adaptive, and to adjust, as far as may be, the proper importance to be attached to each.

It is, however, quite to be expected that, in the infancy of all sciences, a vast amount of labour must be expended in learning the methods of investigation. In none has this been more conspicuous than in the subject under consideration. Many have come to despair, for instance, of any good commensurate with the time it occupies, coming of the minute and laborious work involved in craniometry. This is because nearly all our present methods are tentative. We have not yet learnt, or are only beginning to learn, what lines of investigation are profitable and what are barren. The results, even as far as we have gone, are, however, quite sufficient, in my opinion, to justify perseverance. I am, however, not so sure whether it be yet time to answer the demand, so eager and so natural, which is being made in many quarters, for the formulation of a definite plan of examination, measurement, and description to which all future investigation should rigidly adhere. All steps to promote agreement upon fundamental points are to be cordially welcomed, and meetings or congresses convened for such a purpose will be of use by giving opportunities for the impartial discussion of the relative value of different methods; but the agreement will finally be brought about by the general adoption of those measurements and methods which experience proves to be the most useful, while others will gradually fall into disuse by a kind of process of natural selection.

The changes and improvements which are being made yearly, almost monthly, in instruments and in methods, show what we should lose if we were to stop at any given period, and decree in solemn conclave that this shall be our final system, this instrument and this method shall be the only one used throughout the world, that no one shall depart from it. We scarcely need to ask how long such an agreement would be binding. The subject is not sufficiently advanced to be reduced to a state of stagnation such as this would bring it to.

To take an example from what is perhaps the most important of the anatomical characters by which man is distinguished from the lower animals, the superior from the inferior races of man; the smaller or greater projection forwards of the lower part of the face in relation to the skull proper, or that which contains the brain. From the time when Camper drew his facial angle, to the present day, the readiest and truest method of estimating this projection has occupied the attention of anatomists and anthropologists, and we are still far from any general agreement. Every country, every school, has its own system, so different that comparison with one another is well nigh impossible. This is undoubtedly an evil; but the question is whether we should all agree to adopt one of the confessedly defective systems now in vogue, or whether we should not rather continue to hope for, and endeavour to find, one which may not be subject to the well-known objections urged against all.

We want, especially in this country, more workers, trained and experienced men who will take up the subject seriously, and devote themselves to it continuously. Of such we may say, without offence to those few who have done occasional excellent work in physical anthropology, but whose chief scientific activity lies in other fields, we have not one. In the last number of the French *Revue d'Anthropologie*, a reference caught my eye to a craniometrical method in use by the "English school" of anthropologists. It was a reference only to a method which I had ventured to suggest, but which, as far as I know, has not been adopted by any one else. A school is just what we have not, and what we want—a body of men not only willing to learn, but able to discuss, to criticise, to give their approval to, or reduce to its proper level, the results put forth by our few original investigators and writers. The rapidity with which any one of the most slender pretensions who ventures into the field (I speak from painful experience) is raised to be an oracle among his fellows is one of the most alarming proofs of the present barrenness of the land.

Another most urgent need is the collection and preservation of the evidences of the physical structure of the various modifications of man upon the earth. Especially urgent is this now, as we live in an age in which, in a far greater degree than any previous one, the destruction of races, both by annihilation and absorption, is going on. The world has never witnessed such changes in its ethnology as those now taking place, owing to the rapid extension of maritime discovery and maritime commerce, which is especially affecting the island population among which, more than elsewhere, the solution of the most important anthropological problems may be looked for. If we have at present neither the knowledge nor the leisure to examine and describe, we can at least preserve from destruction the materials for our successors to work upon. Photographs, models, anatomical specimens, skeletons or parts of skeletons, with their histories carefully registered, of any of the so-called aboriginal races, now rapidly undergoing extermination or degeneration, will be hereafter of inestimable value. Drawings, descriptions, and measurements are also useful, though in a far less degree, as allowance must always be made for imperfections in the methods as well as the capacity of the artist or observer. Such collections must be made upon a far larger scale than has hitherto been attempted, as, owing to the difficulties already pointed out in the classification of man, it is only by large numbers that the errors arising from individual peculiarities or accidental admixture can be obviated, and the prevailing characteristics of a race or group truly ascertained. It is only in an institution commanding the resources of the nation that such a collection can be formed, and it may therefore be confidently hoped that the trustees of the British Museum will appropriate some portion of the magnificent new building, which has been provided for the accommodation of their natural history collections, to this hitherto neglected branch of the subject.

I have mentioned two of the needs of anthropology in this country—more workers and better collections: there is still a third—that of a society or institution in which anthropologists can meet and discuss their respective views, with a journal in which the results of their investigations can be laid before the public, and a library in which they can find the books and periodicals necessary for their study. All this ought to be provided by the Anthropological Institute of Great Britain and Ireland, which originated in the amalgamation of the old Ethnological and Anthropological Societies. But, as I intimated some time ago, the Institute does not at the present time flourish as it should; its meetings are not so well attended as they might be; the journal is restricted in its powers of illustration and printing by want of funds; the library is quite insufficient for the needs of the student.

This certainly does not arise from any want of good management in the Society itself. Its afairs have been presided over and administered by some of the most eminent and able men the country has produced. Huxley, Lubbock, Busk, Evans, Tylor, and Pitt-Rivers have in succession given their energies to its service, and yet the number of its members is falling away, its usefulness is crippled, and its very existence seems precarious. Some decline to join the Institute, others leave it upon the plea that, being unable from distance or other causes to attend the meetings, they cannot obtain the full return for their subscriptions; others on the ground that the journal does not contain the exact information which they require.

There surely is to be found a sufficient number of persons who are influenced by different considerations, who feel that anthropological science is worth cultivating, and that those who are laboriously and patiently tracing out the complex problems of man's diversity and man's early history are doing a good work, and ought to be encouraged by having the means afforded them of carrying on their investigations and of placing the results of their researches before the world—who feel, moreover, that there ought to be some central body, representing the subject, which may, on occasion, influence opinion or speak authoritatively on matters often of great practical importance to the nation.

There must be many in this great and wealthy country who feel that they are helping a good cause in joining such a society, even if they are not individually receiving what they consider a full equivalent for their small subscription—many who feel satisfaction in helping the cause of knowledge, in helping to remove the opprobrium that the British Anthropological Society alone of those of the world is lacking in vitality, and in helping to prevent this country from falling behind all the nations in the cultivation of a science in which, for the strongest reasons, it might be expected to hold the foremost place. It is a far more grateful task to maintain, extend, and if need be improve, an existing organisation, than to construct a new one. I feel, therefore, no hesitation in urging upon all who take interest in the promotion of the study of anthropology to rally round the Institute, and to support the endeavours of the present excellent president to increase its usefulness.

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### *Department of Anatomy and Physiology*

OPENING ADDRESS BY J. BURDON-SANDERSON, M.D., LLD., F.R.S., PROFESSOR OF PHYSIOLOGY IN UNIVERSITY COLLEGE, LONDON, VICE-PRESIDENT OF THE SECTION

### *On the Discoveries of the Past Half-Century relating to Animal Motion*

THE two great branches of Biology with which we concern ourselves in this section, Animal Morphology and Physiology, are most intimately related to each other. This arises from their having one subject of study—the living animal organism. The difference between them lies in this, that whereas the studies of the anatomist lead him to fix his attention on the organism itself, to us physiologists it, and the organs of which it is made up, serve only as *vestigia*, by means of which we investigate the vital processes of which they are alike the causes and consequences.

To illustrate this I will first ask you to imagine for a moment that you have before you one of those melancholy remainders of what was once an animal—to wit, a rabbit—which one sees exposed in the shops of poulterers. We have no hesitation in recognising that remainder as being in a certain sense a rabbit; but it is a very miserable vestige of what was a few days ago enjoying life in some wood or warren, or more likely on the sand-hills near Ostend. We may call it a rabbit if we like, but it is only a remainder—not the thing itself.

The anatomical preparation which I have in imagination placed before you, although it has lost its inside and its outside, its integument and its viscera, still retains the parts for which the rest existed. The final cause of an animal, whether human or other, is muscular action, because it is by means of its muscles that it maintains its external relations. It is by our muscles exclusively that we act on each other. The articulate sounds by which I am addressing you are but the results of complicated combinations of muscular contractions—and so are the scarcely appreciable changes in your countenances by which I am able to judge how much, or how little, what I am saying interests you.

Consequently the main problems of physiology relate to muscular action, or, as I have called it, animal motion. They may be divided into two—namely (1) in what does muscular action consist—that is, what is the process of which it is the effect or outcome? and (2) how are the motions of our bodies co-ordinated or regulated? It is unnecessary to occupy time in showing that, excluding those higher intellectual processes which, as they leave no traceable marks behind them, are beyond the reach of our methods of investigation, these two questions comprise all others concerning animal motion. I will therefore proceed at once to the first of them—that of the process of muscular contraction.

The years which immediately followed the origin of the British Association exceeded any earlier period of equal length in the number and importance of the new facts in morphology and in physiology which were brought to light; for it was during that period that Johannes Müller, Schwann, Henle, and, in this country, Sharpey, Bowman, and Marshall Hall, accomplished their productive labours. But it was introductory to a much greater epoch. It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of "vitalism." Before that time, even the greatest biologist—*e.g.* J. Müller—recognised that the knowledge they possessed both of vital and physical phenomena was insufficient to refer both to a common measure. The method, therefore, was to study the processes of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all, unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. Let us inquire for a moment what causes have conduced to the change.

The most efficient cause was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the Conservation of Energy. In the application of this great principle to physiology, the men to whom we are indebted are, first and foremost, J. R. Mayer, of whom I shall say more immediately; and secondly, to the great physiologists still living and working among us, who were the pupils of J. Müller—viz. Helmholtz, Ludwig, Du Bois-Reymond, and Brücke.

As regards the subject which is first to occupy our attention, that of the *process* of muscular contraction, J. R. Mayer occupies so leading a position that a large proportion of the researches which have been done since the new era, which he had so important a share in establishing, may be rightly considered as the working out of principles enunciated in his treatise[1] on the relation between organic motion and exchange of material. The most important of these were, as expressed in his own words: (1) "That the chemical force contained in the ingested food and in the inhaled oxygen is the source of the motion and heat which are the two products of animal life; and (2) that these products vary in amount with the chemical process which produces them." Whatever may be the claims of Mayer to be regarded as a great discover in physics, there can be no doubt

[1] J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel: ein Beitrag zur Naturkunde," Heilbronn, 1845.

that as a physiologist he deserves the highest place that we can give him, for at a time when the notion of the correlation of different modes of motion was as yet very unfamiliar to the physicist, he boldly applied it to the phenomena of animal life, and thus re-united physiology with natural philosophy, from which it had been rightly, because unavoidably, severed by the vitalists of an earlier period.

Let me first endeavour shortly to explain how Mayer himself applied the principle just enunciated, and then how it has been developed experimentally since his time.

The fundamental notion is this: the animal body resembles, as regards the work it does and the heat it produces, a steam-engine, in which fuel is continually being used on the one hand, and work is being done and heat produced on the other. The using of fuel is the chemical process, which in the animal body, as in the steam-engine, is a process of oxidation. Heat and work are the useful products, for as, in the higher animals, the body can only work at a constant temperature of about 100° F., heat may be so regarded.

Having previously determined the heat and work severally producible by the combustion of a given weight of carbon, from his own experiments and from those of earlier physicists, Mayer calculated that if the oxidation of carbon is assumed to represent approximately the oxidation process of the body, the quantity of carbon actually burnt in a day is far more than sufficient to account for the day's work, and that of the material expended in the body not more than one-fifth was used in the doing of work, the remaining four-fifths being partly used, partly wasted in heat production.

Having thus shown that the principles of the correlation of process and product hold good, so far as its truth could then be tested, as regards the whole organism, Mayer proceeded to inquire into its applicability to the particular organ whose function it is "to transform chemical difference into mechanical effect"—namely, muscle. Although, he said, a muscle acts under the direction of the will, it does not derive its power of acting from the will any more than a steamboat derives its power of motion from the helmsman. Again (and this was of more importance, as being more directly opposed to the prevalent vitalism), a muscle, like the steamboats use in the doing of work, not the material of its own structure, or mechanism, but the fuel—*i.e.* the nutriment—which it derives directly from the blood which flows through its capillaries. "The muscle is the instrument by which the transformation of force is accomplished, not the material which is itself transformed." This principle he exemplified in several ways, showing that if the muscle of our bodies worked, as was formerly supposed, at the expense of their own substance, their whole material would be used up in a few weeks, and that in the case of the heart, a muscle which works at a much greater rate than any other, it would be expended in as many days—a result which necessarily involved the absurd hypothesis that the muscular fibres of our hearts are so frequently disintegrated and reintegrated that we get new hearts once a week.

On such considerations Mayer founded the prevision, that, as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the limits of the chemical process either in the whole animal body or in a single muscle during a given period, and to measure the production of heat and the work done during the same period, the result would show a quantitative correlation between them.

If the time at our disposal permitted, I should like to give a short account of the succession of laborious investigations by which these previsions have been verified. Begun by Bidder and Schmidt in 1851,[1] continued by Pettenkofer and Voit,[2] and by the agricultural physiologists[3] with reference to herbivora, they are not yet by any means completed. I must content myself with saying that by these experiments the first and second parts of this great subject—namely, the limits of the chemical process of animal life and its relation to animal motion under different conditions—have been satisfactorily worked out, but that the quantitative relations of heat production are as yet only insufficiently determined.

Let me sum up in as few words as possible how far what we have now learnt by experiment justifies Mayer's anticipations, and how it falls short of or exceeds them. First of all, we are as certain as of any physical fact that the animal body in doing work does not use its own material—that, as Mayer says, the oil to his lamp of life is food; but in addition to this we know what he was unaware of, that what is used is not only not the living protoplasm itself, but is a kind of material which widely differs from it in chemical properties. In what may be called commercial physiology—*i.e.*, in the literature of trade puffs—one still meets with the assumption that the material basis of muscular motion is nitrogenous; but by many methods of proof it has been shown that the true "Oel in der Flamme des Lebens" is not proteid substance, but sugar, or sugar-producing material. The discovery of this fundamental truth we owe first to Bernard (1850-56), who brought to light the fact that such material plays an important part in the nutrition of every living tissue; secondly, to Voit (1866), who in elaborate experiments on carnivorous animals, during periods of rest and exertion, showed that, in comparing those conditions, no relation whatever shows itself between the quantity of proteid material (flesh) consumed, and the amount of work done; and finally to Frankland, Fick, and his associate Wislicenus, as to the work-yielding value of different constituents of food, and as to the actual expenditure of material in man during severe exertion. The subjects of experiment used by the two last-mentioned physiologists were themselves; the work done was the mountain ascent from Interlaken to the summit of the Faulhorn; the result was to prove that the quantity of material used was proportional to the work done, and that that material was such as to yield water and carbonic acid exclusively.

The investigators to whom I have just referred aimed at proving the correlation of process and product for the whole animal organism. The other mode of inquiry proposed by Mayer, the verification of his principle in respect of the work-doing mechanism—that is to say, in respect of muscle taken separately—has been pursued with equal perseverance during the last twenty years, and with greater success; for in experimenting on a separate organ, which has no other functions excepting those which are in question, it is possible to eliminate uncertainties which are unavoidable when the conditions of the problem are more complicated. Before I attempt to sketch the results of these experiments, I must ask your attention for a moment to the discoveries made since Mayer's epoch, concerning a closely related subject, that of the Process of Respiration.

I wish that I had time to go back to the great discovery of Priestley (1776), that the essential facts in the process of respiration are the giving off of fixed air, as he called it, and the taking in of dephlogisticated air, and to relate to you the beautiful experiments by which he proved it; and then to pass on to Lavoisier (1777), who, on the other side of the Channel, made independently what was substantially the same discovery a little after Priestley, and added others of even greater moment. According to Lavoisier, the chemical process of respiration is a slow combustion which has its seat in the lungs. At the time that Mayer wrote, this doctrine still maintained its ascendency, although the investigations of Magnus (1838) had already proved its fallacy. Mayer himself knew that the blood possessed the property of conveying oxygen from the lungs to the capillaries, and of conveying carbonic acid gas from the capillaries to the lungs, which was sufficient to exclude the doctrine of Lavoisier. Our present knowledge of the subject was attained by two methods—viz., first, the investigation of the properties of the colouring matter of the blood, since called "hæmoglobin," the initial step in which was made by Prof. Stokes in 1862; and secondly, the application of the mercurial air-pump as a means of determining the relations of oxygen and carbonic acid gas to the living blood and tissues. The last is a matter of such importance in relation to our subject that I shall ask your special attention to it. Suppose that I have a barometer of which the tube, instead of being of the ordinary form, is expanded at the top into a large bulb of one or two litres capacity, and that, by means of some suitable contrivance, I am able to introduce, in such a way as to lose no time and to preclude the possibility of contact with air, a fluid ounce of blood from the artery of a living animal into the vacuous space—what would happen? Instantly the quantity of blood would be converted into froth, which would occupy the whole of the large bulb. The colour of the froth would at first be scarlet, but would speedily change to crimson. It would soon subside, and we should then have the cavity which was before vacuous occupied by the blood and its gases—namely, the oxygen, carbonic acid gas, and nitrogen previously contained in it. And if we had the means (which

[1] Bidder and Schmidt, "Die Verdauungssäfte und der Stoffwechsel," Leipzig, 1852.
[2] Pettenkofer and Voit, *Zeitschr. f. Biologie*, passim, 1866–80.
[3] Henneberg and Stohmann, "Beiträge zur Begründung einer rationellen Futterung der Wiederkäuer," Brunswick and Göttingen, 1860–70.

actually exist in the gas-pump) of separating the gaseous mixture from the liquid, and of renewing the vacuum, we should be able to determine (1) the total quantity of gases which the blood yields, and (2), by analysis, the proportion of each gas.

Now, with reference to the blood, by the application of the "blood-pump," as it is called, we have learnt a great many facts relating to the nature of respiration, particularly that the difference of venous from arterial blood depends not on the presence of "effete matter," as used to be thought, but on the less amount of oxygen held by its colouring matter, and that the blood which flows back to the heart from different organs, and at different times, differs in the amount of oxygen and of carbonic acid gas it yields, according to the activity of the chemical processes which have their seat in the living tissues from which it flows.[1] But this is not all that the blood-pump has done for us. By applying it not merely to the blood, but to the tissues, we have learnt that the doctrine of Lavoisier was wrong, not merely as regards the place, but as regards the nature of the essential process in respiration. The fundamental fact which is thus brought to light is this, that although living tissues are constantly and freely supplied with oxygen, and are in fact constantly tearing it from the hæmoglobin which holds it, yet they themselves yield no oxygen to the vacuum. In other words, the oxygen which living protoplasm seizes upon with such energy that the blood which flows by it is compelled to yield it up, becomes so entirely part of the living material itself that it cannot be separated even by the vacuum. It is in this way only that we can understand the seeming paradox that the oxygen, which is conveyed in abundance to every recess of our bodies by the blood-stream, is nowhere to be found. Notwithstanding that no oxidation-product is formed, it becomes latent in every bit of living protoplasm; stored up in quantity proportional to its potential activity—*i.e.* to the work, internal or external, it has to do.

Thus you see that the process of tissue respiration—in other words, the relation of living protoplasm to oxygen—is very different from what Mayer, who localised oxidation in the capillaries, believed it to be. And this difference has a good deal to do with the relation of Process to Product in muscle. Let us now revert to the experiments on this subject which we are to take as exemplification of the truth of Mayer's forecasts.

The living muscle of a frog is placed in a closed chamber, which is vacuous—*i.e.* contains only aqueous vapour. The chamber is so arranged that the muscle can be made to contract as often as necessary. At the end of a certain period it is found that the chamber now contains carbonic acid gas in quantity corresponding to the number of contractions the muscle has performed. The water which it has also given off cannot of course be estimated. Where do these two products come from? The answer is plain. The muscle has been living all the time, for it has been doing work, and (as we shall see immediately) producing heat. What has it been living on? Evidently on stored material. If so, of what nature? If we look for the answer to the muscle, we shall find that it contains both proteid and sugar-producing material, but which is expended in contraction we are not informed. There is, however, a way out of the difficulty. We have seen that the only chemical products which are given off during contraction are carbonic acid gas and water. It is clear, therefore, that the material on which it feeds must be something which yields, when oxidised, these products, and these only. The materials which are stored in muscle are oxygen and sugar, or something resembling it in chemical composition.

And now we come to the last point I have to bring before you in connection with this part of my subject. I have assumed up to this moment that heat is always produced when a muscle does work. Most people will be ready to admit as evidence of this, the familiar fact that we warm ourselves by exertion. This is in reality no proof at all.

The proof is obtained when, a muscle being set to contract, it is observed that at each contraction it becomes warmer. In such an experiment, if the heat capacity of muscle is known, the weight of the particular muscle, and the increase of temperature, we have the quantity of heat produced.

If you determine these data in respect of a series of contractions, arranging the experiments so that the work done in each contraction is measured, and immediately thereupon reconverted into heat, the result gives you the total product of the oxidation process in heat.

[1] Ludwig's first important research on this subject was published in 1862.

If you repeat the same experiment in such a way that the work done in each contraction is not so reconverted, the result is *less* by the quantity of heat corresponding to the work done. The results of these two experiments have been found by Prof. Fick to cover each other very exactly. I have stated them in a table[1] in which we have the realisation as regards a single muscle of the following forecast of Mayer's as regards the whole animal organism. "Convert into heat," he said, "by friction or otherwise, the mechanical product yielded by an animal in a given time, add thereto the heat produced in the body directly during the same period, and you will have the total quantity of heat which corresponds to the chemical processes." We have seen that this is realisable as regards muscle, but it is not even yet within reach of experimental verification as regards the whole animal.

I now proceed abruptly (for the time at our disposal does not admit of our spending it on transitions) to the consideration of the other great question concerning vital motion, namely the question how the actions of the muscles of an animal are so regulated and co ordinated as to determine the combined movements, whether rhythmical or voluntary, of the whole body.

As every one knows who has read the "Lay Sermons," the nature and meaning of these often unintentional but always adapted motions, which constitute so large a part of our bodily activity, was understood by Descartes early in the seventeenth century. Without saying anything as to his direct influence on his contemporaries and successors, there can be no doubt that the appearance of Descartes was coincident with a great epoch—an epoch of great men and great achievements in the acquirement of man's intellectual mastery over nature. When he interpreted the unconscious closing of the eyelids on the approach of external objects, the acts of coughing, sneezing, and the like as mechanical and reflected processes, he neither knew in what part of the nervous system the mechanisms concerned were situated, nor how they acted.[2] It was not until a hundred years after that Whytt and Hales made the fundamental experiments on beheaded frogs, by which they showed that the involuntary motions which such preparations execute cease when the whole of the spinal cord is destroyed—that if the back part of the cord is destroyed, the motions of the hind limbs, if the fore part, those of the fore limbs cease. It was in 1751 that Dr. Whytt published in Edinburgh his work on the involuntary motions of animals. After this the next great step was made within the recollection of living physiologists: a period to which, as it coincided with the event which we are now commemorating—the origin of the British Association—I will now ask your special attention.

Exactly forty-nine years ago Dr. Marshall Hall communicated to the Zoological Society of London the first account of his experiments on the reflex function of the spinal cord. The facts which he had observed, and the conclusions he drew from them, were entirely new to him, and entirely new to the physiologists to whom his communication was addressed. Nor can there be any reason why the anticipation of his fundamental discovery by Dr. Whytt should be held to diminish his merit as an original

[1] RELATION OF PRODUCT AND PROCESS IN MUSCLE

(Result of one of Fick's experiments)

| | |
|---|---|
| Mechanical product ... ... ... | 6670 grammemillimeters. |
| Its heat-value ... ... ... ... | 15·6 milligrammeunits. |
| Heat produced ... ... ... ... | 39·0 ,, |
| Total product reckoned as heat ... | 54·6 ,, |

[2] Descartes' scheme of the central nervous mechanism comprised all the parts which we now regard as essential to "reflex-action." Sensory nerves were represented by threads (filets) which connected all parts of the body to the brain ("Œuvres," par V. Cousin, vol. iv. p. 359); motor nerves by tubes which extended from the brain to the muscles; "motor centres" by "pores" which were arranged on the internal surface of the ventricular cavity of the brain and guarded the entrances to the motor tubes. This cavity was supposed to be kept constantly charged with "animal spirits" furnished to it from the heart by arteries specially destined for the purpose. Any "incitation" of the surface of the body by an external object which affects the organs of sense does so, according to Descartes, by producing a *motion* at the incited part. This is communicated to the pore by the thread and causes it to open, the consequence of which is that the "animal spirit" contained in the ventricular cavity enters the tube and is conveyed by it to the various muscles with which it is connected, so as to produce the appropriate motions. The whole system, although it was placed under the supervision of the "*âme raisonable*" which had its office in the pineal gland, was capable of working independently. As instances of this mechanism Descartes gives the withdrawal of the foot on the approach of hot objects, the actions of swallowing, yawning, coughing, &c. As it is necessary that, in the performance of these complicated motions, the muscles concerned should contract in succession, provision is made for this in the construction of the systems of tubes which represent the motor nerves. The weakness of the scheme lies in the absence of fact basis. Neither threads nor pores nor tubes have any existence.

investigator. In the face of historical fact it is impossible to regard him as the discoverer of the "reflex function of the spinal cord," but we do not the less owe him gratitude for the application he made of the knowledge he had gained by experiments on animals to the study of disease. For no one who is acquainted with the development of the branch of practical medicine which relates to the diseases of the central nervous system will hesitate in attributing the rapid progress which has been made in the diagnosis and treatment of these diseases, to the impulse given by Dr. Marshall Hall to the study of nervous pathology.

In the mind of Dr. Marshall Hall the word reflex had a very restricted meaning. The term "excito-motary function," which he also used, stood in his mind for a group of phenomena of which it was the sole characteristic that a sensory impression produced a motor response. During the thirty years which have elapsed since his death, the development of meaning of the word reflex has been comparable to that of a plant from a seed. The original conception of reflex action has undergone, not only expansion, but also modification, so that in its wider sense it may be regarded as the empirical development of the philosophical views of the animal mechanism promulgated by Descartes. Not that the work of the past thirty years by which the physiology of the nervous system has been constituted can be attributed for a moment to the direct influence of Descartes. The real epoch-maker here was Johannes Müller. There can be no doubt that Descartes' physiological speculations were well known to him, and that his large acquaintance with the thought and work of his predecessors conduced, with his own powers of observation, to make him the great man that he was; but to imagine that his ideas of the mechanism of the nervous system were inspired, or the investigations by which, contemporaneously with Dr. Marshall Hall, he demonstrated the fundamental facts of reflex action, were suggested by the animal automatism of Descartes, seems to me wholly improbable.

I propose, by way of conclusion, to attempt to illustrate the nature of reflex action in the larger sense, or, as I should prefer to call it, the Automatic Action of Centres, by a single example—that of the nervous mechanism by which the circulation is regulated.

The same year that J. R. Mayer published his memorable essay, it was discovered by E. H. Weber that, in the vagus nerve, which springs from the medulla oblongata and proceeds therefrom to the heart, there exist channels of influence by which the medulla acts on that wonderful muscular mechanism. Almost at the same time with this, a series of discoveries [1] were made relating to the circulation, which, taken together, must be regarded as of equal importance with the original discovery of Harvey. First, it was found by Henle that the arterial blood-vessels by which blood is distributed to brain, nerve, muscle, gland, and other organs, are provided with muscular walls like those of the heart itself, by the contraction or dilatation of which the supply is increased or diminished according to the requirements of the particular organ. Secondly, it was discovered simultaneously, but independently, by Brown-Séquard and Augustus Waller, that these arteries are connected by nervous channels of influence with the brain and spinal cord, just as the heart is. Thirdly, it was demonstrated by Bernard that what may be called the heart-managing channels spring from a small spot of grey substance in the medulla oblongata, which we now call the "heart-centre"; and a little later by Schiff, that the artery-regulating channels spring from a similar head-central office, also situated in the medulla oblongata, but higher up, and from subordinate centres in the spinal cord.

If I had the whole day at my disposal and your patience were inexhaustible, I might attempt to give an outline of the issues to which these five discoveries have led. As it is, I must limit myself to a brief discussion of their relations to each other, in order that we may learn something from them as to the nature of automatic action.

Sir Isaac Newton, who, although he knew nothing about the structure of nerves, made some shrewd forecasts about their action, attributed to those which are connected with muscles an alternative function. He thought that by means of motor nerves the brain could determine either relaxation or contraction of muscles. Now as regards ordinary muscles, we know that this is not the case. We can will only the shortening of a muscle, not its lengthening. When Brown-Séquard discovered the function of the motor nerves of the blood-vessels, he assumed that the same limitation was applicable to it as to that of muscular nerves in general. It was soon found, however, that this assumption was not true in all cases—that there were certain instances in which, when the vascular nerves were interfered with, dilatation of the blood-vessels, consequent on relaxation of their muscles, took place; and that, in fact, the nervous mechanism by which the circulation is regulated is a highly-complicated one, of which the best that we can say is that it is perfectly adapted to its purpose. For while every organ is supplied with muscular arteries, and every artery with vascular nerves, the influence which these transmit is here relaxing, there constricting, according (1) to the function which the organ is called upon to discharge; and (2) the degree of its activity at the time. At the same time the whole mechanism is controlled by one and the same central office, the locality of which we can determine with exactitude by experiment on the living animal, notwithstanding that its structure affords no indication whatever of its fitness for the function it is destined to fulfil. To judge of the complicated nature of this function we need only consider that in no single organ of the body is the supply of blood required always the same. The brain is during one hour hard at work, during the next hour asleep; the muscles are at one moment in severe exercise, the next in complete repose; the liver, which before a meal is inactive, during the process of digestion is turgid with blood, and busily engaged in the chemical work which belongs to it. For all these vicissitudes the tract of grey substance which we call the *vascular centre* has to provide. Like a skilful steward of the animal household, it has, so to speak, to exercise perfect and unfailing foresight, in order that the nutritive material which serves as the oil of life for the maintenance of each vital process, may not be wanting. The fact that this wonderful function is localised in a particular bit of grey substance is what is meant by the expression "automatic action of a centre."

But up to this point we have looked at the subject from one side only.

No state ever existed of which the administration was exclusively executive—no government which was, if I may be excused the expression, absolutely absolute. If in the animal organism we impose on a centre the responsibility of governing a particular mechanism or process, independently of direction from above, we must give that centre the means of being itself influenced by what is going on in all parts of its area of government. In other words, it is as essential that there should be channels of information passing inwards, as that there should be channels of influence passing outwards. Now what is the nature of these channels of information? Experiment has taught us not merely with reference to the regulation of the circulation, but with reference to all other automatic mechanisms, that they are as various in their adaptation as the outgoing channels of influence. Thus the vascular centre in the medulla oblongata is so cognisant of the chemical condition of the blood which flows through it, that if too much carbonic acid gas is contained in it, the centre acts on information of the fact, so as to increase the velocity of the blood-stream, and so promote the arterialisation of the blood. Still more strikingly is this adaptation seen in the arrangement by which the balance of pressure and resistance in the blood-vessels is regulated. The heart, that wonderful muscular machine by which the circulation is maintained, is connected with the centre, as if by two telegraph wires—one of which is a channel of influence, the other of information. By the latter the engineer who has charge of that machine sends information to headquarters whenever the strain on his machine is excessive, the certain response to which is relaxation of the arteries and diminution of pressure. By the former he is enabled to adapt its rate of working to the work it has to do.

If Dr. Whytt, instead of cutting off the head of his frog, had removed only its brain—*i.e.*, the organ of thought and consciousness—he would have been more astonished than he actually was at the result; for a frog so conditioned exhibits, as regards its bodily movements, as perfect adaptiveness as a normal frog. But very little careful observation is sufficient to show the difference. Being incapable of the simplest mental acts, this true animal automaton has no notion of requiring food or of seeking it, has no motive for moving from the place it happens to

[1] The dates of the discoveries relating to this subject here referred to are as follows:—Muscular Structure of Arteries, Henle, 1841; Function of Cardiac Vagus, E. H. Weber, 1845; Constricting Nerves of Arteries, B. Séquard, 1852, Aug. Waller, 1853: Cardiac Centre, Bernard, 1858; Vascular Centre, Schiff, 1858; Dilating Nerves, Schiff, 1854; Eckhard, 1864; Lovén, 1866. Of the more recent researches by which the further elucidation of the mechanism by which the distribution of blood is adapted to the requirements of each organ, the most important are those of Ludwig and his pupils and of Heidenhain.

occupy, emits no utterance of pleasure or distress. Its life processes continue so long as material remains, and are regulated mechanically.

To understand this all that is necessary is to extend the considerations which have been suggested to us in our very cursory study of the nervous mechanism by which the working of the heart and of arteries is governed, to those of locomotion and voice. Both of these we know, on experimental evidence similar to that which enables us to localise the vascular centre, to be regulated by a centre of the same kind. If the behaviour of the brainless frog is so natural that even the careful and intelligent observer finds it difficult to attribute it to anything less than intelligence, let us ask ourselves whether the chief reason of the difficulty does not lie in this, that the motions in question are habitually performed intelligently and consciously. Regarded as mere mechanisms, those of locomotion are no doubt more complicated than those of respiration or circulation, but the difference is one of degree, not of kind. And if the respiratory movements are so controlled and regulated by the automatic centre which governs them, that they adapt themselves perfectly to the varying requirements of the organism, there is no reason why we should hesitate in attributing to the centres which preside over locomotion powers which are somewhat more extended.

But perhaps the question has already presented itself to your minds, What does all this come to? Admitting that we are able to prove (1) that in the animal body, Product is always proportional to Process, and (2), as I have endeavoured to show you in the second part of my discourse, that Descartes' dream of animal automatism has been realised, what have we learnt thereby? Is it true that the work of the last generation is worth more than that of preceding ones?

If I only desired to convince you that during the last half-century there has been a greater accession of knowledge about the function of the living organism than during the previous one, I might arrange here in a small heap at one end of the table the physiological works of the Hunters, Spallanzani, Fontana, Thomas Young, Benjamin Brodie, Charles Bell, and others, and then proceed to cover the rest of it with the records of original research on physiological subjects since 1831, I should find that, even if I included only genuine work, I should have to heap my table up to the ceiling. But I apprehend this would not give us a true answer to our question. Although, etymologically, Science and Knowledge mean the same thing, their real meaning is different. By science we mean, first of all, that knowledge which enables us to sort the things known according to their true relations. On this ground we call Haller the father of physiology, because, regardless of existing theories, he brought together into a system all that was then known by observation or experiment as to the processes of the living body. But in the "Elementa Physiologiæ" we have rather that out of which science springs than science itself. Science can hardly be said to begin until we have by experiment acquired such a knowledge of the relation between events and their antecedents, between processes and their products, that in our own sphere we are able to forecast the operations of nature, even when they lie beyond the reach of direct observation. I would accordingly claim for physiology a place in the sisterhood of the sciences, not because so large a number of new facts have been brought to light, but because she has in her measure acquired that gift of prevision which has been long enjoyed by the higher branches of natural philosophy. In illustration of this I have endeavoured to show you that every step of the laborious investigations undertaken during the last thirty years as to the process of nutrition, has been inspired by the previsions of J. R. Mayer, and that what we have learnt with so much labour by experiments on animals is but the realisation of conceptions which existed two hundred years ago in the mind of Descartes as to the mechanism of the nervous system. If I wanted another example I might find it in the previsions of Dr. Thomas Young as to the mechanism of the circulation, which for thirty years were utterly disregarded, until, at the epoch to which I have so often adverted, they received their full justification from the experimental investigations of Ludwig.

But perhaps it will occur to some one that if physiology founds her claim to be regarded as a science on her power of anticipating the results of her own experiments, it is unnecessary to make experiments at all. Although this objection has been frequently heard lately from certain persons who call themselves philosophers, it is not very likely to be made seriously here. The answer is, that it is contrary to experience. Although we work in the certainty that every experimental result will come out in accordance with great principles (such as the principle that every plant or animal is both, as regards form and function, the outcome of its past and present conditions, and that in every vital process the same relations obtain between expenditure and product as hold outside of the organism), these principles do little more for us than indicate the direction in which we are to proceed. The history of science teaches us that a general principle is like a ripe seed, which may remain useless and inactive for an indefinite period, until the conditions favourable to its germination come into existence. Thus the conditions for which the theory of animal automatism of Descartes had to wait two centuries, were (1) the acquirement of an adequate knowledge of the structure of the animal organism, and (2) the development of the sciences of physics and chemistry; for at no earlier moment were these sciences competent to furnish either the knowledge or the methods necessary for its experimental realisation; and for a reason precisely similar Young's theory of the circulation was disregarded for thirty years.

I trust that the examples I have placed before you to-day may have been sufficient to show that the investigators who are now working with such earnestness in all parts of the world for the advance of physiology, have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life, and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness.

The Science of Physiology has already afforded her aid to the Art of Medicine in furnishing her with a vast store of knowledge obtained by the experimental investigation of the action of remedies and of the causes of disease. These investigations are now being carried on in all parts of the world with great diligence, so that we may confidently anticipate that during the next generation the progress of pathology will be as rapid as that of physiology has been in the past, and that as time goes on the practice of medicine will gradually come more and more under the influence of scientific knowledge. That this change is already in progress we have abundant evidence. We need make no effort to hasten the process, for we may be quite sure that, as soon as science is competent to dictate, art will be ready to obey.

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## SECTION F

### GEOGRAPHY

### Opening Address by Sir Joseph D. Hooker, C.B., K.C.S.I., F.R.S., &c., President of the Section

#### *On Geographical Distribution*

It has been suggested that a leading feature of the sectional addresses to be delivered on the occasion of this, the fiftieth anniversary of the meetings of the British Association, should be a review of the progress made during the last half century in the branches of knowledge which the sections respectively represent.

It has further been arranged that, at so auspicious an epoch, the sections should, when possible, be presided over by past Presidents of the Association. This has resulted in almost every sectional chair being occupied by a President eminent as a cultivator of the science with which his section will be engaged, though not the one I have the honour of filling, which, from the fact of there being no professed geographer amongst the surviving past Presidents, has been confided to an amateur.

Under these circumstances I should be untrue to myself and to you, if I presumed to address you as one conversant with geography in any extended signification of the word, or if I attempted to deal with that important and attractive branch of it, topographical discovery, which claims more or less exclusively the time and attention of the geographers of this country. It is more fitting for me, and more in keeping with the objects of this Association, that I be allowed to discourse before you on one of the many branches of science the pursuit of which is involved in the higher aims of geographers, and which, as we are informed by an accomplished cultivator of the science, are integral portions of scientific geography.[1] Of these none is more important than that of the distribution of animals and plants, which further recommends itself to you on this occasion from being a subject that owes its great progress during the last half-century as much to the theories advanced by celebrated voyagers and travellers as to their observations and collections.

Before, however, I proceed to offer you a sketch of the progress made during the lifetime of the Association in this one branch, I must digress to remind you, however briefly, of the even greater advances made in others, in many cases through the direct or indirect instrumentality of the Association itself, acting in concert with the Royal and with the Royal Geographical Societies.

In topography the knowledge obtained during this half century has been unprecedently great. The veil has been withdrawn from the sources of the Nile, and the lake systems of Central Africa have been approximately localised and outlined. Australia, never previously traversed, has been crossed and recrossed in various directions. New Guinea has had its coasts surveyed, and its previously utterly unknown interior has been here and there visited. The topography of Western China and Central Asia, which had been sealed books since the days of Marco Polo, has been explored in many quarters. The elevations of the highest mountains of both hemispheres have been accurately determined, and themselves ascended to heights never before attained; and the upper regions of the air have been ballooned to the extreme limit beyond which the life-sustaining organs of the human frame can no longer perform their functions. In hydrography the depths of the great oceans have been sounded, their shores mapped, and their physical and natural history explored from the Equator to beyond both polar circles. In the Arctic regions the highest hitherto attained latitudes have been reached; Greenland has been proved to be an island; and an archipelago has been discovered nearer to the Pole than any other land. In the Antarctic regions a new continent has been added to our maps, crowned with one of the loftiest known active volcanoes, and the Antarctic ocean has been twice traversed to the 79th parallel. Nor have some of the negative results of modern exploration been less important, for the Mountains of the Moon and many lesser chains have been expunged from our maps, and there are no longer believers in the inland sea of Australia or in the open ocean of the Arctic pole. Of these and many others of the geographical discoveries of the last half-century full accounts will be laid before you, prepared for this section by able geographers; of whom Mr. Markham will contribute Arctic discovery; Sir Richard Temple, Asiatic; Lieut.-Col. Sir James Grant and Mr. H. Waller, African; Mr. Moseley, Australian; Mr. Trelawny Saunders, Syrian (including the Holy Land); the Hydrographer of the Admiralty will undertake the great oceans, and Mr. F. Galton will discuss the improvements effected in the instruments, appliances, and methods of investigation employed in geographical researches.

Of other branches of science which are auxiliary to scientific geography, the majority will be treated of in the sections of the Association to which they belong; but there are a few which I must not, in justice to the geographers who have so largely contributed to their advance, leave unnoticed.

Such is Terrestrial Magnetism,[2] which had as its first investigators two of our earliest voyagers, the ill-fated Hudson and Halley, who determined the magnetic dip in the north polar and tropical regions respectively. Theirs were the precursors of a long series of scientific expeditions, during which the dipping needle was carried almost from Pole to Pole, and which culminated in the establishment, mainly under the auspices of this Association, of the magnetic survey of Great Britain, of fixed magnetic observatories in all quarters of the globe, and of the Antarctic expedition of Sir James Ross, who, since the foundation of the Association, planted the dipping needle over the northern Magnetic Pole, and carried it within 200 miles of the southern one.

[1] Major-General Strachey, in a lecture delivered before the Royal Geographical Society (*Proceedings*, vol. xxxi. p. 179, 1877), discusses, with just appreciation and admirable clearness, the interdependence of the sciences which enter into the study and aims of scientific geography, and which he enumerates under fourteen heads. This lecture contains the ablest review of the subject known to me. It might very well be entitled "The whole duty of the Geographer." Every traveller's outfit should include a copy of it, and one should accompany every prize given by the Geographical Society to students for proficiency in geographical knowledge.

[2] The subject of an able lecture "On the Magnetism of the Earth," delivered before the Royal Geographical Society by the Hydrographer of the Admiralty (*Proceedings*, vol. xxi. p. 20, 1876).

Nor is the geography of this half century less indebted to physicists, geologists, and naturalists. It is to a most learned traveller, and naturalist, Von Baer, that the conception is due that the westward deflection of all the South Russian rivers is caused by the revolution of the globe on its axis.[1] It was a geologist, Ramsay, who explained the formation of so many lake beds in mountain regions by the gouging action of glaciers. It was a physicist and mountaineer, Tyndal, who discovered those properties of ice upon which the formation and movement of glaciers depend. The greatest of naturalist-voyagers, Darwin, within the same half-century has produced the true theory of coral reefs and atolls, showed the relations between volcanic islands and the rising and sinking of the bottom of the ocean, and proved that along a coast line of 2480 miles the southern part of the continent of South America has been gradually elevated from the sea level to 600 feet above it. Within almost the same period Poulett Scrope and Lyell have revolutionised the theory of the formation of volcanic mountains, showing that these are not the long-taught upheavals of the crust of the earth, but are heaped up deposits from volcanic vents, and they have largely contributed to the abandonment of the venerable theory that mountain chains are sudden up-thrusts. Within the same period, the theory of the great oceans having occupied their present positions on the globe from very early geological times was first propounded by Dana,[2] the companion of Wilkes in his expedition round the world, and is supported by Darwin and by Wallace.

In Meteorology the advance is no less attributable to the labours of voyagers and travellers. The establishment of the Meteorological Office is due to the energy and perseverance of a great navigator, the late Admiral Fitzroy.

Another domain of knowledge that claims the strongest sympathies of the geographer is Anthropology. It is only within the last quarter of a century that the study of man under his physical aspect has been recognised as a distinct branch of science, and represented by a flourishing society, and by annual international congresses.

I must not conclude this notice without a passing tribute to a department of geography that has occupied the attention of too few of its cultivators. I mean that of literary research. Nevertheless, in this too the progress has been great; and I need only mention the publications of the Hakluyt Society, and two works of prodigious learning and the greatest value, "The Book of Marco Polo, the Venetian,"[3] and "A History of Ancient Geography,"[4] to prove to you that one need not to travel to new lands to be a profound and sagacious geographer.

I have asked you to accept the geographical distribution of organic beings as the subject which I have chosen for this address. It is the branch with which I am most familiar; it illustrates extremely well the interdependence of those sciences which the geographer should study, and as I have before observed, its progress has been in the main due to the labours of voyagers and travellers.

In the science of distribution, Botany took the lead. Humboldt, in one of his essays,[5] says that the germ of it is to be found in an idea of Tournefort, developed by Linnæus. Tournefort was a Frenchman of great learning, and, moreover, a great traveller. He was sent by the King of France in 1700 to explore the islands of Greece and mountains of Armenia, in the interests of the Jardin des Plantes, and his published narrative is full of valuable matter on the people, antiquities, and natural productions of the countries he visited. The idea attributed to him by Humboldt,[6] is that in ascending mountains we meet successively with vegetations that represent those of successively higher latitudes; upon which Humboldt observes: "Il ne fallut pas une grande sagacité pour observer que sur les pentes des hautes montagnes de l'Arménie, des végétaux des différentes latitudes se suivent comme les climats superposés l'un sur les autres"; but he goes on to remark, "cette idée de Tournefort developpée par Linné dans deux dissertations intéressantes (Stationes et Coloniæ Plantarum), renferment cependant le germe de la Géographie

[1] Von Baer, "Ueber ein allgemeines Gesetz in der Gestaltung der Flussbetten," *St. Petersb. Bull. Sc.* ii. (1860).

[2] Dana in *American Journal of Science*, ser. 2, vol. iii. p. 352 (1847), and various later publications.

[3] By Colonel Henry Yule, C.B. (ed. 1, 1871; ed. 2, 1875).

[4] By S. H. Bunbury (1879).

[5] "Sur les lois que l'on observe dans le distribution des formes végétales" (Mémoire lu à l'Institut de France, January 29, 1816).

[6] I have been unable to find any such idea expressed in Tournefort's works. Edward Forbes, however, also attributes the idea to Tournefort (Memoirs of the Geology Survey, vol i. p. 351).

Botanique." Tournefort's idea was, however, an advanced one for the age he lived in, and should not be judged by the light of the knowledge of a succeeding century. He had no experience of other latitudes than the few intervening between Paris and the Levant. Humboldt himself did not suspect the whole bearing of the idea on the principles of geographical distribution, and that the parallelism between the floras of mountains and of latitudes was the result of community of descent of the plants composing the floras, not that it was brought about by physical causes. The idea of the early part of the eighteenth century is, when rightly understood, found to be the forerunner of the matured knowledge of the middle of the nineteenth.

The labours of Linnæus, himself a traveller, and whose narratives give him high rank as such, paved the way to a correct study of botanical geography. Before his time little or no attention was paid to the topography of plants, and he was the first to distinguish, to lay down rules, and to supply models for these two important elements in their life-history — namely, their habitats or topographical localisation, and their stations, or the physical nature of their habitats. In his "Stationes Plantarum,"[1] Linnæus defines with precision twenty-four stations characterised by soil, moisture, exposure, climate, &c., which, with comparatively slight modifications and improvements, have been adopted by all subsequent authorities. Nor, indeed, was any marked advance in this subject made, till geological observation and chemical analysis supplemented its shortcomings. In his essay "De coloniis plantarum," published fourteen years after the "Stationes,"[2] he says, "Qui veram cunque et solidam plantarum scientiam aucupatur, patriam ipsarum ac sedem cujusque propriam haud sane ignorabit," and he proceeds to give an outline of the distribution of certain plants on the globe, according to climate, latitude, &c., and to indicate their means of transport by winds, birds, and other agencies. India (meaning the tropics of both worlds) he characterises as the region of palms; the temperate latitudes, of herbaceous plants; the northern, of mosses, algæ, and coniferæ; and America, of ferns;—thus preparing the way for the next great generaliser in the field.[3]

This was the most accomplished and prolific of modern travellers, Humboldt, who made botany a chief pursuit during all his journeys, and who seems, indeed, to have been devoted to it from a very early age. His first work was a botanical one, the "Flora Friburgensis," and we have it on his own authority that three years before its publication, when he was only just of age (in 1790) he communicated to his friend G. Förster, the companion of Cook in his second voyage, a sketch of a geography of plants. It was not, however, till his return from America that his first essay on Botanical Geography[4] appeared, which at once gave him a very high position as a philosophical naturalist. Up to the period of its appearance there had been nothing of the kind to compare with it for the wealth of facts, botanical, meteorological, and hypsometrical, derived from his own observations, from the works of travellers and naturalists, and from personal communication with his contemporaries, all correlated with consummate skill and discussed with that lucidity of exposition of which he was a master. The great feature of this essay is the exactness of the methods employed for estimating the conditions under which species, genera, and families are grouped geographically, and the precision with which they are expressed.

This was succeeded in 1815, and subsequently, by four other essays on the same subject. Of these the most valuable is the "Prolegomena,"[5] in which he dwells at length on the value of numerical data, and explains his "Arithmeticæ botanices," which consists in determining the proportion which the species of certain large families or groups of families bear to the whole number of species composing the floras in advancing from the Equator to the Poles, and in ascending mountains. Some kinds of plants, he says, increase in numbers relatively to others in proceeding from the Equator to the Poles, as ferns, grasses, amentiferous trees, &c.; others decrease, as Rubiaceæ, Malvaceæ, Compositæ, &c.; whilst others still, as Labiatæ, Cruciferæ, &c., find their maximum in temperate regions, and decrease in both directions. He adds that it is only by accurately measuring this decrease or increase that laws can be established, when it is found that these present constant relations to parallels of temperature.[1] Furthermore, he says that in many cases the whole number of plants contained in any given region of the globe may be approximately determined by ascertaining the number of species of such families.

The importance of this method of analysing the vegetation of a country in researches in geographical botany is obvious, for it affords the most instructive method of setting forth the relations that exist between a flora and its geographical position and climatal conditions.

Humboldt's labours on the laws of distribution were not limited to floras, they included man and the lower animals, cultivated and domesticated, as well as native; they may not be works of the greatest originality, but they show remarkable powers of observation and reflection, astonishing industry, conscientious exactitude in the collection of data, and sagacity in the use of them; he is indisputably the founder of this department of geographical science.

No material advance was made towards improving the laws of geographical distribution[2] so long as it was believed that the continents and oceans had experienced no great changes of surface or of climate since the introduction of the existing assemblages of animals and plants. This belief in the comparative stability of the surface was first dispersed by Lyell, who showed that a fauna may be older than the land it inhabits. To this conclusion he was led by the study of the recent and later tertiary molluscs of Sicily, which he found had migrated into that land before its separation from the continent of Italy. Just, he adds, as the plants and animals of the Phlegræan fields had colonised Monte Nuovo since that mountain was thrown up in the sixteenth century; whence, he goes on to say, we are brought to admit the curious result, that the fauna and flora of Val de Noto, and of some other mountain regions of Italy, are of higher antiquity than the country itself, having not only flourished before the lands were raised from the deep, but even before they were deposited beneath the waters.[3] The same idea occurred to Darwin, who, alluding to the very few species of living quadrupeds which are altogether terrestrial in habit, that are common to Asia and America, and to these few being confined to the extreme frozen regions of the North, adds, "We may safely look at this quarter (Behring's Straits), as the line of communication (now interrupted by the steady progress of geological change), by which the elephant, the ox, and the horse entered America, and peopled its wide extent."[4]

The belief in the stability of climatal conditions during the lifetime of the existing assemblages of animals and plants was also dispelled by the discovery, throughout the northern temperate regions of the old and new worlds, of Arctic and boreal plants on all their mountains, and of these fossilised on their lowlands, and which discoveries led to the recognition of the glacial period and glacial ocean.

The first and boldest attempt to press the results of geological and climatal changes into the service of botanical and zoological geography was that of the late Edward Forbes, a naturalist of genius, who, like Tournefort, chose the Levant as the field for his early labours. In the year 1846, Forbes communicated a paper to the Natural History section of this Association, on the distribution of endemic plants, especially those of the British

[1] *Amœnitates Academicæ*, vol. iv. p. 64, 1754.

[2] *Ibid.* vol. viii. p. 1, 1768.

[3] Between the dates of the writings of Linnæus and Humboldt, two notable works on geographical distribution appeared. One by Frid. Stromeyer ("Commentatio inauguralis sistens Historiæ Vegetabilium Geographicæ specimen," Göttingen, 1800), is an excellent syllabus of the points to be attended to in the study of distribution, but without examples; the other is a too general work by Zimmermann, entitled, "Specimen Zoologiæ Geographicæ, Quadrupedum Domicilia et Migrationes sistens," Lugd. Bat. 1777, which he followed by "Geographische Geschichte des Menschen und der allgemein verbreiteten vierfüssigen Thiere, nebst einer hieher gehörigen zoologischen Weltcharte, Leipzig, 1778-1783."

[4] "Essai sur la Géographie des Plantes," par A. de Humboldt et Aimé Bonpland; rédigée par A. de Humboldt, lu à la Classe des Sc. Phys. et Math. de l'Institut Nationale, 17 Nivôse de l'An 13. 1805.

[5] "De Distributione Geographica plantarum secundum Cœli temperiem et altitudinem Montium, Prolegomena." This appeared in quarto in the first volume of the "Nova Genera et Species Plantarum" in 1815, and separately in an octavo form in 1817. Humboldt's other works on geographical distribution are "Notationes ad Geographiam Plantarum spectantes," 1815; "Ansichten der Natur," 1808, and ed. 2, 1827; "Nouvelles Recherches sur les lois que l'on observe dans la Distribution des formes végétales" (1816); and an article with a similar title in the "Dictionnaire des Sciences Naturelles," vol. xviii p. 422, 1820.

[1] Humboldt's isothermal lines and laws of geographical distribution are obviously the twin results of the same researches, one physical, the other biological.

[2] I do not hereby imply that no progress was made in the knowledge of the facts of distribution, for, over and above many treatises on the distribution of the plants of local floras, there appeared, in 1816, Schouw's "Dissertatio de sedibus plantarum originariis"; which was followed in 1822 by his excellent "Grundtrack til el almendelig Plante-Geographie," of which the German edition is entitled, "Grundzüge einer allgemeinen Pflanzengeographie."

[3] "Principles of Geology," ed. 3, vol. iii. p. 376, 1834.

[4] *Journal of Researches in Geology and Natural History, &c.*, p. 151 1839.

Islands, considered with regard to geological changes.[1] In this paper the British flora is considered to consist of assemblages of plants from five distinct sources, which, with the exception of one, immigrated during periods when the British Isles were united to the continent of Europe, and have remained more or less localised in England, in Scotland, or in Ireland. Of these he considered the Pyrenean assemblage, which is confined to the west of Ireland, to be the oldest, and to have immigrated, after the eocene period, along a chain of now submerged mountains, that extended across the Atlantic from Spain to Ireland, and indeed formed the eastern boundary of an imaginary continent of miocene age, which extended to the Azores Islands, and beyond them. This, the "Atlantis" of speculative geologists, has long since been abandoned. The second assemblage is of plants characteristic of the South-West of France, which now prevail in Devon, Cornwall, and the Channel Islands; their immigration he assigns to a miocene date, probably corresponding to the red crag. The third assemblage is of plants of the North-East of France, which abound in the chalk districts of the South-Eastern counties of England; their immigration is referred to the era of the mammaliferous crag. The fourth is of Alpine plants now found on the mountains of Scotland, Wales, and England; these were introduced mainly by floating ice from Scandinavia during the glacial period, when the greater part of the British Isles were submerged, its mountain tops forming part of a chain of islands in the glacial sea that extended to the coast of Norway; this was during the newer pliocene period. Lastly, the Germanic plants were introduced during the upheaval of the British Islands from the glacial ocean, and as the temperature was gradually increasing; these are spread over the whole islands, though more abundant on the Eastern side. At the commencement of this immigration England was supposed to be continuous with the Germanic plains, from which it was subsequently severed by the formation of the English Channel. Also, at the commencement of this immigration, Ireland was assumed to be continuous with England, to be early severed by the formation of the Irish Sea; which severance, by interrupting the migration of Germanic types, accounts for the absence of so many British animals in the sister island.

I have thus briefly related Forbes' views, to show how profoundly he was impressed with the belief that geographical and climatal conditions were the all-powerful controllers of the migrations of animals and plants. Forbes was the reformer of the science of geographical distribution.[2]

Before the publication of the doctrine of the origin of species by variation and natural selection, all reasoning on their distribution was in subordination to the idea that these were permanent and special creations; just as, before it was shown that species were often older than the islands and mountains they inhabited, naturalists had to make their theories accord with the idea that all migration took place under existing conditions of land and sea. Hitherto the modes of dispersion of species, genera, and families had been traced; but the origin of representative species, genera, and families remained an enigma[3]; these could be explained only by the supposition that the localities where they occurred presented conditions so similar that they favoured the creation of similar organisms, which failed to account for representation occurring in the far more numerous cases where there is no discoverable similarity of physical conditions, and of their not occurring in places where the conditions are similar. Now under the theory of modification of species after migration and isolation, their representation in distant localities is only a question of time and changed physical conditions. In fact, as Darwin well sums up, all[1] the leading facts of distribution are clearly explicable under this theory; such as the multiplication of new forms; the importance of barriers in forming and separating zoological and botanical provinces; the concentration of related species in the same area; the linking together under different latitudes of the inhabitants of the plains and mountains, of the forests, marshes, and deserts, and the linking of these with the extinct beings which formerly inhabited the same areas; and the fact of different forms of life occurring in areas having nearly the same physical conditions.

With the establishment of the doctrine of the orderly evolution of species under known laws, I close this list of those recognised principles of the science of geographical distribution which must guide all who enter upon its pursuit. As Humboldt was its founder, and Forbes its reformer, so we must regard Darwin as its latest and greatest lawgiver. With their example, and their conclusions to guide, advance becomes possible whenever discovery opens new paths, or study and reflection retraverse the old ones.

And it was not long before palæontology brought to the surface new data for the study of the present and past physical geography of the globe.

This was the discovery in Arctic latitudes of fossil plants whose existing representatives are to be found only in warm temperate ones. To Arctic travellers and voyagers this discovery is wholly due. Of these I believe I am correct in saying that Sir John Richardson was the earliest, for he, in the year 1848, when descending the McKenzie River to the Polar Sea in search of the Franklin Expedition, found in lat. 65° N. beds of coal, besides shales full of leaves of forest-trees belonging to such genera as the maple, poplar, taxodium, oak, &c. In the narrative of his journey[2] Richardson mentions these fossils, and figures some of them; and in a subsequent work[3] he speaks of them as "leaves of deciduous trees belonging to genera which do not in the present day come so far north on the American continent by ten or twelve degrees of latitude." This discovery was followed, in 1853, by the still more remarkable one, by Capt. M'Clure and Sir Alexander Armstrong (during another search for Sir John Franklin), of pine cones and acorns imbedded in the soil of Banksland, in lat. 75° N., at an elevation of 300 feet above the sea level. And again in 1854 Dr. Lyall found extensive accumulations of similar fossils near Discoe in Greenland (lat. 70° N.), during the return of Sir Edward Belcher's searching expedition. Nor are these fossils confined to America: they have been found in Spitzbergen, in Siberia, and in many other localities within the Polar area as well as south of it, proving that forests of deciduous trees, in all respects like those of the existing forests of the warm temperate regions, approached to within ten degrees of the Pole. The first of these collections critically examined was Dr. Lyall's; it was communicated to Prof. Heer of Zurich, the highest authority on the flora of the Tertiary period, and described by him,[4] as were also subsequently all the other collections brought from the Arctic regions.[5]

The examination of these fossil leaves revealed the wonderful fact that, not only did they belong to genera of trees common to the forests of all the three northern continents, such as planes, beeches, ashes, maples, &c., but that they also included what are now extremely rare and even local genera, as sequoia, liquidamber, magnolia, tulip-trees, gingkos, &c., proving that the forests were of a more mixed character than any now existing. These results opened up a new channel for investigating the problem of distribution, and the first naturalist to enter it as a botanist, Dr. Asa Gray, who pursued it with brilliant results, embodied in a series of memoirs on the vegetation of the United States of America, and of which my notice must be most brief.

When studying the collections of Japanese plants brought by the officers of Wilkes' expedition, Dr. Gray found cumulative evidence of the strong affinity between the flora of Eastern Asia

[1] *British Association Reports*, 1845, pt. ii. p. 67, and *Annals and Magazine of Natural History*, vol. xvi. p. 126. This the author followed by a much fuller exposition of the subject, which appeared in the *Memoirs of the Geological Survey of the United Kingdom*, vol. i. p. 336 (1846), entitled "On the Connection between the distribution of the existing flora and fauna of the British Isles, and the geological changes which have affected their area, especially during the epoch of the northern drift." After many years interval I have re-read this Memoir with increased pleasure and profit. The stores of exact information which he collected concerning the plants, the animals, and the geology of Europe and North America, appear to me to be no less remarkable than the skill with which he correlated them and educed from the whole so many very original and in great part incontrovertible conclusions.

[2] I cannot dismiss the subject of the geography of the British flora without an allusion to the labours of Hewett Cottrell Watson, who, after a life devoted to the topography of British plants, was laid in the grave only a month ago. Watson was the first botanist who measured the altitudinal range of each species, and, by a rigidly statistical method, traced their distribution in every county, and grouped them according to their continental affinities, as well as by the physical conditions of their habitats.

[3] The representation of species Forbes alludes to as "an accident . . . which has hitherto not been accounted for" (*Mem.* Geol. Survey, vol. i. p. 351).

[1] Of the many pre-Darwinian writers on distribution who advocated the Lamarckian doctrine of evolution, I am not aware of any who suggested that it would explain the existence of representative species, or indeed any other of the phenomena of distribution. Von Baer, however, in the very year of the publication of the first edition of the "Origin of Species," expressed his conviction, chiefly grounded on the laws of geographical distribution, that forms now specifically distinct have descended from a single parent form. See "Origin of Species," ed. 5, Historical Sketch, p. 23.

[2] "Boat Voyage through Rupert's Land and in the Arctic Sea," vol. i. p. 186.

[3] "Polar Regions," p. 289.

[4] "Ueber die von Dr. Lyall in Grönland entdeckten fossilen Pflanzen." *Zürich Vierteljahrschr.* vol. vii. p. 176 (1862).

[5] "Flora fossilis Arctica."

and Eastern North America, to the exclusion of the western half of that continent; and also that Europe and Western Asia did not share in this affinity. But what especially attracted his attention was, that this affinity did not depend only on a few identical or representative genera, but upon many endemic genera of exceptional character, and often consisting of only two almost identical species. This led to a rigorous comparison of those plants with the fossils from the Arctic regions whose affinities had been determined by Heer, and with others which had been meanwhile accumulating in the United States, and had been described by Lesquereux; and the result was what I may call an abridged outline history of the flora of North America in its relations to the physical geography of that country, from the Cretaceous to the present time.

The latest researches which have materially advanced our knowledge of the laws of distribution are those of Prof. Blytt of Christiania. His essay on "The Immigration of the Norwegian Flora during alternately Rainy and Dry Periods" has for its object to define and localise the various assemblages of plants of which that flora is composed, and to ascertain their mother-country and the sequence of their introduction. The problem is that of Prof. Forbes, which I have already described to you, only substituting Norway for the British Isles. Both these authors invoke the glacial period to account for the dispersion of Arctic plants, both deal with a rising land, both assume that immigration took place over land; but Prof. Blytt finds another and most powerful controlling agent, in alternating periods of greater moisture and comparative drought, of which the Norwegian peat bogs afford ample proof. These bogs were formed during the rise of the land, as the cold of the glacial period declined. They are found at various heights above the sea in Norway; the most elevated of them are of course the oldest, and contain remains of the earliest immigrants. The lowest are the newest, and contain remains of the latest introduced plants only. The proofs of the alternating wet and dry seasons rest on the fact that the different layers of peat in each bog present widely different characters, contain the remains of different assemblages of plants, and these characters recur in the same order in all the bogs. First there is a layer of wet spongy peat, with the remains of bog-mosses and aquatic plants; this gradually passes upwards into a layer of dry soil containing the remains of many land plants, and prostrate trunks of trees, showing that the country was forested. To this succeeds wet spongy peat as before, to be again covered with dry peaty soil and tree trunks, &c., and so on. From an examination of the plant remains in these formations Prof. Blytt draws the following conclusions:—

The Norwegian flora began with an immigration of Arctic plants during a dry period, evidence of which he finds in the presence of the remains of these beneath the lowest layer of peat. As the climate became warmer and the land rose, a rainy period set in, accompanied by an immigration of sub-Arctic plants (juniper, mountain ash, aconites, &c.), which to a great extent replaced the Arctic flora, which is impatient of great wet. This was the period of the first peat-bog formation. It was followed by a dry period, during which the bogs gradually dried up; while with the increasing warmth, deciduous trees and their accompanying herbaceous vegetation were introduced. The succeeding rainy season produced a second peat-formation, killing and burying the deciduous trees, the increasing warmth at the same time bringing in the Atlantic flora, characterised by the holly, foxglove, and other plants now confined in Norway to the rainy Atlantic coast. To this succeeded a third period of drought, when the bogs dried up, and pine-forests with their accompanying plants immigrated into Norway, to be in like manner destroyed and buried by bog earth during the next following rainy period; and it was during these last alternations that the subboreal plants now affecting the lowest south-eastern districts of Norway were introduced; and the sub-Atlantic plants, the most southern of all the types which are confined to the extreme south of the country.

It would be premature to regard all Prof. Blytt's recurrent periods as irrefragably established, or his correlations of these with the several floras as fully proved; but there is no doubt, I think, that he has brought forward a *vera causa* to account for the alternation of dry country with wet country plants in Norway, and one that must have both actively promoted the first introduction of these into that country, as also influenced their subsequent localisation. It would strengthen Prof. Blytt's conclusions very much if his alternating periods of rain and drought should be found to harmonise with Mr. Croll's recurrent astronomical periods, and with Mr. Geikie's fluctuations of temperature during the decline of the Glacial epoch: so would also the finding in the bogs of Scotland a repetition of the conditions which obtain in those of Norway; and there are so very many points of resemblance in the physical geography and vegetation of these two countries that I do not doubt a comparison of their peat formations would yield most instructive results.

Thus far all the knowledge we have obtained of the agents controlling geographical distribution have been derived from observations and researches on northern animals and plants, recent and tertiary. Turning now to the southern hemisphere, the phenomena of distribution are much more difficult of explanation. Geographically speaking, there is no Antarctic flora except a few lichens and sea-weeds. The plants called Antarctic,[1] from their analogy with the Arctic, are very few in number, and nowhere cross the 62° of south latitude. They are, in so far as they are endemic, confined to the southern islands of the great southern ocean, and the mountains of South Chili, Australia, Tasmania, and New Zealand; whilst the few non-endemic are species of the nearest continents, or are identical with temperate northern or with sub-Arctic or even Arctic species. Like the Arctic flora, the Antarctic is a very uniform one round the globe, the same species, in many cases, especially the non-endemic, occurring on every island, though there are sometimes thousands of miles of ocean between the nearest of these. And, as many of the island plants reappear on the mountains above mentioned, far to the north of their island homes, it is inferred on these grounds, as well as on astronomical and geological, that there was a glacial period in the southern temperate zone as well as in the northern.

The south temperate flora is a fourfold one. South America, South Africa, Australia, and New Zealand contain each an assemblage of plants differing more by far amongst themselves than do the floras of Europe, North Asia, and North America; they contain, in fact, few species in common, except the Antarctic ones that inhabit their mountains. These south temperate plants have their representative species and genera on the mountains of the tropics, each in their own meridian only, and there they meet immigrants from all latitudes of the northern hemisphere. Thus the plants of Fuegia extend northward along the Andes, ascending as they advance. Australian genera reappear on the lofty mountain of Kini-balu in Borneo; New Zealand ones on the mountains of New Caledonia; and the most interesting herbarium ever brought from Central Africa, that of Mr. Joseph Thomson, from the highlands of the lake districts, contains many of the endemic genera, and even species of the Cape of Good Hope. Nor does the northern representation of the south temperate flora cease within the tropics; it extends to the middle north temperate zone; Chilian genera reappearing in Mexico and California; South African in North Africa, in the Canary Islands, and even in Asia Minor[2]; and Australian in the Khasia Mountains of East Bengal, in East China and Japan.

So too there is a representation of genera in the southern temperate continents, feeble numerically compared to what the north presents, but strong in other respects. This is shown by the families of Proteaceæ, Cycadeæ, and Restiaceæ, abounding in South Africa and Australia alone, though not a single species or even genus of these families is common to the two countries; by New Zealand, with a flora differing in almost every element from the Chilian, yet having a few species of both calceolaria and fuchsia, genera otherwise purely American; whilst as regards Australia and New Zealand, it is difficult to say which are the most puzzling, the contrasts or the similarities which their animal and vegetable productions present.

These features of the vegetation of the south temperate and Antarctic regions, though they simulate those of the north temperate and Arctic, may not originate from precisely similar causes. In the absence of such evidence as the fossil animals and plants of the north affords,[3] there is no proof that the Ant-

[1] For accounts of the Antarctic flora see the "Botany" of the Antarctic Expedition of Sir James Ross, where the relations of the floras of the southern hemisphere with the Antarctic are discussed in introductory chapters.

[2] *Pelargonium Endlicherianum* in the Taurus is a remarkable instance.

[3] The only fossil leaves hitherto found in higher southern latitudes are those of beeches, closely allied to existing southern species, brought by Darwin from Fuegia. In one locality alone beyond the forest region of the south have fossil plants been found; there were silicified trunks of trees in lava beds of Kerguelen's Island (discovered by myself forty years ago). It is deeply to be regretted that searches for shales containing fossils were not

arctic plants found on the south temperate Alps, or the south temperate plants found in the mountains of the tropics, originated in the south; though this appears probable from the absence in the south of so many of the leading families of plants and animals of the north, no less than from the number of endemic forms the south contains. These considerations have favoured the speculation of the former existence, during a warmer period than the present, of a centre of creation in the Southern Ocean, in the form of either a continent or of an archipelago, from which both the Antarctic and southern endemic forms radiated. I have myself suggested continental or insular extension[1] as a means of aiding that wide dispersion of species over the Southern Ocean, which it is difficult to explain without such intervention; and the discovery of beds of fossil trunks of trees in Kerguelen's Island, testifies to that place having enjoyed a warmer climate than its present one.

The rarity in the existing Archipelago (Kerguelen's Island, the Crozets, and Prince Edward's Island) of any of the endemic genera of the south temperate flora, or of representatives of them, is, however, an argument against such land, if it ever existed, having been the birthplace of that flora; and there are two reasons for adopting the opposite theory, that the southern flora came from the north temperate zone. Of these, one is the number of northern genera and species (which, from their all inhabiting north-east Europe, I have denominated Scandinavian)[2] that are found in all Antarctic and south temperate regions, the majority of them in Fuegia, the flora of which country is, by means of the Andes, in the most direct communication with the northern one. The other is the fact I have stated above, that the several south temperate floras are more intimately related to those of the countries north of them than they are to one another.

And this brings me to the latest propounded theoretical application of the laws of geographical distribution. It is that recently advanced by Mr. Thiselton Dyer, in a lecture "On Plant Distribution as a Field of Geographical Research"[3]; wherein he argues that the floras of all the countries of the globe may be traced back at some time of their history to the northern hemisphere, and that they may be regarded in point of affinity and specialisation as the natural results of the conditions to which they must have been subjected during recent geological times, on continents and islands with the configuration of those of our globe. This hypothesis derives its principal support from the fact that many of the most peculiar endemic plants of the south have representatives in the north, some of them living and all of them in a fossil state, whilst the northern endemic forms have not hitherto been found fossil in the southern regions. So that, given time, evolution, continental continuity, changes of climate and elevations of the land, and all the southern types may be traced back to one region of the globe, and that one palæontology teaches us is the northern.

A very similar view has been held and published at the same time by Count Saporta,[4] a most eminent palæontologist, in a suggestive essay entitled "L'Ancienne Végétation Polaire." Starting from Buffon's thesis, that the cooling of the globe having been a gradual process, and the Polar regions having cooled first, these must have first become fit for organic life, Count Saporta proceeds to assume that the termination of the azoic period coincided with a cooling of the waters to the point at which coagulation of albumen does not take place, when organic life appeared in the water itself. I have discussed Count Saporta's speculations elsewhere[5]; it is sufficient here to indicate the more important ones as bearing upon distribution. These are that the Polar area was the centre of origination of all the successive phases of vegetation that have appeared on the globe, all being developed in the north; and that the development of flowering plants was enormously augmented by the introduction during the latter part of the secondary period of flower-feeding insects, which brought about cross-fertilisation.

It remains to allude briefly to the most important general

made either by the *Challenger* expedition or by the various "transit of Venus expeditions" that have recently visited this interesting island.

[1] "Flora Antarctica," pp. 230, 240. See also Moseley in *Journ.* Linn. Soc. Botany, vol. xv. p. 485, and "Observations on the Botany of Kerguelen's Island," by myself, in the *Philosophical Transactions*, vol. 168, p. 15.

[2] See "Outlines of the Distribution of Arctic Plants," *Transactions* of the Linnean Society, vol. xxiii. p. 257. Read June, 1860.

3 *Proceedings* of the Royal Geographical Society, vol. xxii. p. 415 (1878).

4 *Comptes rendus* of the International Congress of Geographical Science, which met in Paris in 1875, but apparently not published till 1877.

5 Address of the President delivered at the anniversary meeting of the Royal Society of London, November 30, 1878.

works on distribution that have appeared since the foundation of this Association. Of these, the two which take the first rank are Prof. Alphonse de Candolle's "Géographie Botanique" and Mr. Wallace's "Geographical Distribution of Animals." Prof. de Candolle's work[1] appeared at a critical period, when the doctrine of evolution with natural selection had only just been announced, and before the great influence of geological and climatal changes on the dispersion of living species had been fully appreciated; nevertheless it is a great and truly philosophical work, replete with important facts, discussed with full knowledge, judgment, and scrupulous caution. Of its numerous valuable and novel features, two claim particular notice, namely, the chapters on the history of cultivated and introduced plants; and the further development of Humboldt's "Arithmeticæ Botanices," by taking into account the sums of temperatures as well as the maxima, minima, and means, in determining the amount of heat required to satisfy all the conditions of a plant's life, at the various periods of its existence, and especially the maturation of its seeds.

Of Mr. Wallace's great work, "The Geographical Distribution of Animals," I cannot speak with sufficient knowledge of the subject, and can only appreciate and echo the high praises accorded to it by zoologists for its scientific treatment of a vast subject.

The "Géographie Botanique" was followed by the late Dr. Grisebach's "Die Vegetation der Erde,"[2] which contains an admirable summary of the vegetation of the different regions of the globe as limited by their physical features, divested of all theoretical considerations.

For the largest treatment in outline of the whole subject of distribution, I must refer to the chapters of Darwin's "Origin of Species" which are devoted to it.

In reference to these and other works, very able and instructive discussions of the principles of geographical distribution are to be found in the presidential addresses delivered before the Linnean Society, in 1869, 1870, and 1872, by the veteran botanist, G. Bentham.

With Mr. Wallace's "Island Life" I must conclude this notice, and very fittingly, for besides presenting an admirable account of the origin and migrations of animals and vegetables in oceanic and continental islands, it contains a complete and comprehensive analysis of those past and present conditions of the globe, astronomical, geological, geographical, and biological, which have been the earlier and later directors and controllers of the ever-warring forces of organic nature. In this work Mr. Wallace independently advocates the view of the northern origin of both the faunas and floras of the world.

I conclude with the hope that I have made the subject of the distribution of organic life on the globe interesting to you as geographers, by showing on the one hand how much it owes its advance to the observations made and materials collected by geographical explorers, and on the other how greatly the student of distribution has, by the use he has made of these observations and materials, advanced the science of physical geography.

## SECTION G

### MECHANICAL SCIENCE

OPENING ADDRESS BY SIR W. ARMSTRONG, C.B., D.C.L., LL.D., F.R.S., PRESIDENT OF THE SECTION

THE astonishing progress which has been made in the construction and application of machinery during the half century which has elapsed since the nativity of the British Association for the Avancement of Science, is a theme which I might with much complacency adopt in this address, but instead of reviewing the past and exulting in our successes, it will be more profitable to look to the future and to dwell on our failures. It is but justice to say that by growing experience, by increasing facilities of manufacture, and by the exercise of much skill and ingenuity, we have succeeded in multiplying and expanding the applications of our chief motor, the steam-engine, to an extent that would have appeared incredible fifty years ago; but the

[1] Prof. Alph. de Candolle divides his subject into botanical geography and geographical botany; the distinction is obvious and sound, but the two expressions have been so long used and regarded as synonymous, and as embracing both branches, that they cannot now be limited each to one. Perhaps the terms topographical botany and geographical botany would prove more acceptable designations.

[2] Published in 1872. Translated into French under the title of "La Végétation du Globe," by P. de Tchihachef, Paris, 1875.

gratulation inspired by this success is clouded by the reflection that the steam-engine, even in its best form, remains to this day a most wasteful apparatus for converting the energy of heat into motive power. Our predecessors of that period had not the advantage of the knowledge which we possess of the true nature of heat, and the conditions and limits affecting its utilisation. In their time heat was almost universally regarded as a fluid which, under the name of caloric, was supposed to lie dormant in the interstices of matter until forced out by chemical or mechanical means. Although Bacon, Newton, Cavendish, and Boyle all maintained that heat was only internal motion, and although Davy and Rumford not only held that view, but proved its accuracy by experiment, yet the old notion of caloric continued to hold its ground, until in more recent times Joule, Meyer, Codling, and others put an end to all doubt on the subject, and established the all-important fact that heat is a mode of motion having, like any other kind of motion, its exact equivalent in terms of work. By their reasonings and experiments it has been definitely proved that the quantity of heat which raises the temperature of a pound of water 1° Fahrenheit, has a mechanical value equal to lifting 772 lbs. one foot high, and that conversely the descent of that weight from that height is capable of exactly reproducing the heat expended.

The mechanical theory of heat is now universally accepted, although a remnant of the old doctrine is displayed in the continued use of the misleading term "latent heat." According to the new theory, heat is an internal motion of molecules capable of being communicated from the molecules of one body to those of another, the result of the imparted motion being either an increase of temperature, or the performance of work. The work may be either external, as where heat, in expanding a gas, pushes away a resisting body, or it may be internal, as where heat pulls asunder the cohering particles of ice in the process of liquefaction, or it may be partly internal and partly external, as it is in the steam engine, where the first effect of the heat is to separate the particles of water into vapour, and the second to give motion to the piston. Internal as well as external work may be reconverted into heat, but until the reconversion takes place, the heat which did the work does not exist as heat, and it is delusive to call it "latent heat." All heat problems are comprised under the three leading ideas of internal work, external work, and temperature, and no phraseology should be used that conflicts with those ideas.

The modern theory of heat has thrown new light upon the theory of the steam-engine. We now know what is the mechanical value in foot-pounds of the heat evolved in the combustion of one pound of coal. In practice we can determine how much of that heat is transmitted to the water in the boiler, and we are taught how to calculate the quantity which in the process of vaporisation takes the form of internal work. We can determine how much disappears in the engine in the shape of external work, including friction, and the remainder, with the exception of the trifling quantity saved in the feed-water, we know to be lost. Taking a good condensing engine as an example, we may roughly say that, dividing the whole heat energy into ten equal parts, two escape by the chimney, one is lost by radiation and friction, six remain unused when the steam is discharged, and only one is realised in useful work. It may be fully admitted that the greater part of the aggregate loss is inevitable; but are we to suppose that the resources of science, ingenuity, and skill have been exhausted in the attainment of so miserable a result? Nothing but radical changes can be expected to produce any great mitigation of the present monstrous waste, and without presuming to say what measures are practicable and what are not, I will briefly point out the directions in which amelioration is theoretically possible, and shall afterwards advert to the question whether we may hope to evade the difficulties of the steam-engine by resorting to electrical methods of obtaining power.

To begin with the loss which takes place in the application of heat to the boiler; why is it that we have to throw away, at the very outset of our operations, twice as much heat as we succeed in utilising in the engine? The answer is, that in order to force a transmission of heat from the fire to the water in the boiler, a certain excess of temperature over that of the water must exist in the furnace and flues, and the whole of the heat below the required excess must pass away unused, except the trifling portion of it which disappears in the production of draught. Further, that since we cannot avoid admitting the nitrogen of the air along with the oxygen, we have to heat a large volume of neutral gas which has no other effect than to rob the fire. Considering what efforts have been made to facilitate the transmission of the heat by augmenting the evaporative surface, and using thin tubes as flues, it is vain to expect any great result from further perseverance in that direction, and unless a method can be devised of burning the fuel inside instead of outside the apparatus, so as to use the heated gases conjointly with the steam as a working medium in the engine, a remedy appears to be hopeless. We already practise internal combustion in the gas-engine, and it is clear that with gaseous fuel, at all events, we could associate such a mode of combustion with the vaporisation of water. We may even regard a gun as an engine with internally-burnt fuel, and here I may remark that the action of heat in a gun is strictly analogous to that of heat in a steam-engine. In both cases the heat is evolved from chemical combination, and the resulting pressures differ only in degree. The gun is the equivalent of the cylinder, and the shot of the piston, and the diagrams representing the pressure exerted in the two cases bear a close resemblance to each other. While the powder is burning in the gun we have a nearly uniform pressure, just as we have in the cylinder while the steam is entering, and in both cases the uniform pressure is followed by a diminishing pressure, represented by the usual curve of expansion. If in the steam-engine we allowed the piston to be blown out it would act as a projectile, and if in the gun we opposed mechanical resistance to the shot, we might utilise the effect in a quieter form of motive power. But it is a remarkable fact that such is the richness of coal as a store of mechanical energy that a pound of coal, even as used in the steam-engine, produces a dynamic effect about five times greater than a pound of gunpowder burnt in a gun. I cannot, however, on this account encourage the idea that steam may be advantageously substitute for gunpowder in the practice of gunnery.

And now to turn from the fire which is the birthplace of the motive energy, let us follow it in the steam, to the condenser, where most of it finds a premature tomb. From the point at which expansion commences in the cylinder the temperature and pressure of the steam begin to run down, and if we could continue to expand indefinitely, the entire heat would be exhausted, and the energy previously expended in separating the water into steam would be wholly given up in external effect; but this exhaustion would not be complete until the absolute zero of temperature was reached (viz. 461° below the zero of Fahrenheit). I do not mean to say that an ideally perfect engine necessarily involves unlimited expansion, seeing that if instead of discharging the steam at the end of a given expansion, we made the engine itself do work in compressing it, we might, under the conditions of Carnot's reversible cycle, so justly celebrated as the foundation of the theory of the steam-engine, recommence the action with all the unutilised heat in an available form. But an engine upon this principle could only give an amount of useful effect corresponding to the difference between the whole work done by the engine, and that very large portion of it expended in the operation of compression, and this difference viewed in relation to the necessary size of the engine, would be quite insignificant, and would in fact be wholly swallowed up in friction. Carnot did not intend to suggest a real engine, and his hypothesis therefore takes no cognisance of losses incident to the application of an actual fire to an actual boiler. His ideal engine is also supposed to be frictionless, and impervious to heat except at the point where heat has to be transmitted to the water, and there the condition of perfect conduction is assumed. In short an engine which would even approximately conform to the conditions of Carnot's cycle is an impossibility, and a perfect steam-engine is alike a phantom whether it be sought for in the cyclical process of Carnot, or under the condition of indefinite expansion. Practically we have to deal with a machine which, like all other machines, is subject to friction, and in expanding the steam we quickly arrive at a point at which the reduced pressure on the piston is so little in excess of the friction of the machine as to render the steam not worth retaining, and at this point we reject it. In figurative language we take the cream off the bowl and throw away the milk. We do save a little by heating the feed water, but this gain is very small in comparison with the whole loss. What happens in the condenser is, that all the remaining energy which has taken the form of internal work is reconverted into heat, but it is heat of so low a grade that we cannot apply it to the vaporisation of water. But although the heat is too low to vaporise water it is not too low to vaporise Ether. If instead of condensing by the external application of water we did so by the similar application of ether, as proposed and practised by

M. du Trembley twenty-five years ago, the ether would be vaporised, and we should be able to start afresh with high tension vapour, which in its turn would be expanded until the frictional limit was again reached. At that point the ether would have to be condensed by the outward application of cold water and pumped back, in the liquid state, to act over again in a similar manner. This method of working was extensively tried in France when introduced by M. du Trembley, and the results were sufficiently encouraging to justify a resumption of the trials at the present time, when they could be made under much more favourable conditions. There was no question as to the economy effected, but in the discussions which took place on the subject it was contended that equally good results might be attained by improved applications of the steam, without resorting to an additional medium. The compound engine of the present day does in fact equal the efficiency of Du Trembley's combined steam- and ether-engine, but there is no reason why the ether apparatus should not confer the same advantage on the modern engine that attended its application to the older form. The objections to its use are purely of a practical nature, and might very possibly yield to persevering efforts at removal.

I need scarcely notice the advantage to be derived from increasing the initial pressure of the steam so as to widen the range of expansion by raising the upper limit of temperature instead of reducing the lower one. It must be remembered however that an increase of temperature is attended with the serious drawback of increasing the quantity of heat carried off by the gases from the fire, and also the loss by radiation, so that we have not so much to gain by increase of pressure as is commonly imagined.

But even supposing the steam-engine to be improved to the utmost extent that practical considerations give us reason to hope for, we should still have to adjudge it a wasteful though a valuable servant. Nor does there appear to be any prospect of substituting with advantage any other form of thermodynamic engine, and thus we are led to inquire whether any other kind of energy is likely to serve us better than heat, for motive power.

Most people, especially those who are least competent to judge, look to electricity as the coming panacea for all mechanical deficiency, and certainly the astonishing progress of electricity as applied to telegraphy, and to those marvellous instruments of recent invention which the British Post Office claims to include in its monopoly of the electric telegraph, as well as the wonderful advance which electricity has made as an illuminating agent, does tend to impress us with faith in its future greatness in the realm of motive power as well.

The difference between heat and electricity in their modes of mechanical action is very wide. Heat acts by expansion of volume which we know to be a necessarily wasteful principle, while electricity operates by attraction and repulsion, and thus produces motion in a manner which is subject to no greater loss of effect than attends the motive action of gravity as exemplified in the ponderable application of falling water in hydraulic machines. If then we could produce electricity with the same facility and economy as heat, the gain would be enormous, but this, as yet at least, we cannot do. At present by far the cheapest method of generating electricity is by the dynamic process. Instead of beginning with electricity to produce power, we begin with power to produce electricity. As a secondary motor an electric engine may, and assuredly will, play an important part in future applications of power, but our present inquiry relates to a primary, and not a secondary, employment of electricity. Thus we are brought to the question, From what source, other than mechanical action, can we hope to obtain a supply of electricity sufficiently cheap and abundant to enable it to take the place of heat as a motive energy? It is commonly said that we know so little of the nature of electricity that it is impossible to set bounds to the means of obtaining it; but ignorance is at least as liable to mislead in the direction of exaggerated expectation as in that of incredulity. It may be freely admitted that the nature of electricity is much less understood than that of heat, but we know that the two are very nearly allied. The doctrine that heat consists of internal motion of molecules may be accepted with almost absolute certainty of its truth. The old idea of heat being a separate entity is no longer held except by those who prefer the fallacious evidence of their senses to the demonstrations of science. So also the old idea of electricity having a separate existence from tangible matter must be discarded, and we are justified in concluding that it is merely a strained or tensional condition of the molecules of matter. Although electricity is more prone to pass into heat than heat into electricity, yet we know that they are mutually convertible. In short I need scarcely remind you, that according to that magnificent generalisation of modern times, so pregnant with great consequences, and for which we are indebted to many illustrious investigators, we now know that heat, electricity, and mechanical action, are all equivalent and transposable forms of energy, of which motion is the essence.

To take a cursory view of our available sources of energy, we have, firstly, the direct heating power of the sun's rays, which as yet we have not succeeded in applying to motive purposes. Secondly we have water power, wind power, and tidal power, all depending upon influences lying outside of our planet. And thirdly we have chemical attraction or affinity. Beyond these there is nothing worth naming. Of the radiant heat of the sun I shall have to speak hereafter, and bearing in mind that we are in search of electricity as a cause, and not an effect, of motive power we may pass over the dynamical agencies comprised under the second head, and direct our attention to chemical affinity as the sole remaining source of energy available for our purpose. At present we derive motive power from chemical attraction through the medium of heat only, and the question is, can we with advantage draw upon the same source through the medium of electricity. The process by which we obtain our supply of heat from the exercise of affinity is that of combustion, in which the substances used consist, on the one hand, of those we call fuel, of which coal is the most important, and on the other, of oxygen, which we derive from the atmosphere. The oxygen has an immense advantage over every other available substance in being omnipresent and costless. The only money value involved is that of the fuel, and in using coal we employ the cheapest oxidisable substance to be found in nature. Moreover the weight of coal used in the combination is only about one-third of the weight of oxygen, so that we only pay upon one-fourth of the whole material consumed. Thus we have conditions of the most favourable description for the production of energy, in the form of heat, and if we could only use the affinities of the same substances with equal facility to evolve electric energy instead of heat energy, there would be nothing more to desire; but as yet there is no appearance of our being able to do this. According to our present practice we consume zinc, instead of coal, in the voltaic production of electricity, and not only is zinc thirty or forty times dearer than coal, but it requires to be used in about six-fold larger quantity in order to develop an equal amount of energy. Some people are bold enough to say that with our present imperfect knowledge of electricity we have no right to condemn all plentiful substances, other than coal, as impracticable substitutes for metallic zinc, but it is manifest that we cannot get energy from affinity, where affinity has already been satisfied. The numerous bodies which constitute the mass of our globe, and which we call earths, are bodies in this inert condition. They have already, by the union of the two elements composing them, evolved the energy due to combination, and that energy has ages ago been dissipated in space in the form of heat, never again to be available to us. As well might we try to make fire with ashes, as to use such bodies over again as sources of either heat or electricity. To make them fit for our purpose we should first have to annul their state of combination, and this would require the expenditure of more energy upon them than we could derive from their recombination. Water, being oxidised hydrogen, must be placed in the same category as the earths. In short the only abundant substances in nature possessing strong unsatisfied affinities are those of organic origin, and in the absence of coal, which is the accumulated product of a past vegetation, our supply of such substances would be insignificant. This being the case, until a means be found of making the combination of coal with oxygen directly available for the development of electric energy, as it now is of heat energy, there seems to be no probability of our obtaining electricity from chemical action at such a cost as to supplant heat as a motive agent.

But while still looking to heat as the fountain-head of our power, we may very possibly learn to transmute it, economically, into the more available form of electricity. One method of transformation we already possess, and we have every reason to believe there are others yet to be discovered. We know that when dissimilar metals are joined at opposite ends, and heated at one set of junctions while they are cooled at the other, part of the heat applied disappears in the process, and assumes the form of an electric current. Each couple of metals may be treated as the cell of a voltaic battery, and we may multiply them to any extent, and group them in series or in parallels, with the same results as are obtained by similar combinations of voltaic cells. The electricity so produced we term Thermo-electricity, and the apparatus by which the current is evolved is the thermo-electric battery. At present this apparatus is even more wasteful of heat than the steam-engine, but considering the very recent origin of this branch of electrical science, and our extremely imperfect knowledge of the actions involved, we may reasonably regard the present thermo-electric battery as the infant condition of a discovery, which, if it follow the rule of all previous discoveries in electricity, only requires time to develop into great practical importance. Now if we possessed an efficient apparatus of this description we could at once apply it to the steam-engine for the purpose of converting into electric energy the heat which now escapes with the rejected steam, and the gases from the fire. The vice of the steam-engine lies in its inability to utilise heat of comparatively low grade, but if we could use up the leavings of the steam-engine by a supplemental machine acting on thermo-electric principles, the present excessive waste would be avoided. We may even anticipate that in the distant future a thermo-electric engine may not only be used as an auxiliary, but in complete substitution of the steam-engine. Such an expectation certainly seems to be countenanced by what we may observe in animated nature. An animal is a living machine dependent upon food both for its formation and its action. That portion of the food which is not used for growth or structural repair, acts strictly as fuel in the production of heat. Part of that heat goes to the maintenance of the animal temperature, and the remainder gives rise to mechanical action. The only analogy between the steam-engine and this living engine is that both are dependent upon the combustion of fuel, the combustion in the one case being extremely slow, and in the other very rapid. In the steam-engine the motion is produced by pressure, but in the animal machine it is effected by muscular contraction. The energy which causes that contraction, if not purely electrical, is so much of that nature that we can produce the same effect by electricity. The conductive system of the nerves is also in harmony with our conception of an electrical arrangement. In fact a description of the animal machine so closely coincides with that of an electrodynamic machine actuated by thermo-electricity, that we may conceive them to be substantially the same thing. At all events, the animal process begins with combustion and ends with electrical action, or something so nearly allied to it as to differ only in kind. And now observe how superior the result is in nature's engine to what it is in ours. Nature only uses heat of low grade, such as we find wholly unavailable. We reject our steam, as useless, at a temperature that would cook the animal substance, while nature works with a heat so mild as not to hurt the most delicate tissue. And yet, notwithstanding the greater availability of high-grade temperature, the quantity of work performed by the living engine relatively to the fuel consumed, puts the steam-engine to shame. How all this is done in the animal organisation we do not yet understand, but the result points to the attainability of an efficient means of converting low-grade heat into electricity, and in striving after a method of accomplishing that object we shall do well to study nature, and profit by the excellence which is there displayed.

But it is not alone in connection with a better utilisation of the heat of combustion that thermo-electricity bears so important an aspect, for it is only the want of an efficient apparatus for converting heat into electricity, that prevents our using the direct heating action of the sun's rays for motive power. In our climate, it is true, we shall never be able to depend upon sunshine for power, nor need we repine on that account so long as we have the preserved sunbeams which we possess in the condensed and portable form of coal, but in regions more favoured with sun and less provided with coal the case would be different. The actual power of the sun's rays is enormous, being computed to be equal to melting a crust of ice 103 feet thick over the whole earth in a year. Within the tropics it would be a great deal more, but a large deduction would everywhere have to be made for absorption of heat by the atmosphere. Taking all things into account, however, we shall not be far from the truth in assuming the solar heat, in that part of the world, to be capable of melting annually, at the surface of the ground, a layer of ice 85 feet thick. Now let us see what this means in mechanical effect. To melt 1 lb. of ice requires 142·4 English units of heat, which, multiplied by 772, gives us 109,932 foot pounds as the mechanical equivalent of the heat consumed in melting a pound of ice. Hence we find that the solar heat, operating upon an area of one acre, in the tropics, and competent to melt a layer of ice 85 feet thick in a year, would, if fully utilised, exert the amazing power of 4000 horses acting for nearly nine hours every day. In dealing with the sun's energy we could afford to be wasteful. Waste of coal means waste of money and premature exhaustion of coal-beds. But the sun's heat is poured upon the earth in endless profusion—endless at all events in a practical sense, for whatever anxiety we may feel as to the duration of coal, we need have none as to the duration of the sun. We have therefore only to consider whether we can divert to our use so much of the sun's motive energy as will repay the cost of the necessary apparatus, and whenever such an apparatus is forthcoming we may expect to bring into subjection a very considerable proportion of the 4000 invisible horses which science tells us are to be found within every acre of tropical ground.

But whatever may be the future of electricity as a prime mover, either in a dominant or subordinate relation to heat, it is certain to be largely used for mechanical purposes in a secondary capacity, that is to say, as the offspring instead of the parent of motive power. The most distinctive characteristic of electricity is that which we express by the word "current," and this gives it great value in cases where power is required in a transmissible form. The term may be objected to as implying a motion of translation analogous to the flow of a liquid through a pipe, whereas the passage of electricity through a conductor must be regarded as a wave-like action communicated from particle to particle. In the case of a fluid current through a pipe, the resistance to the flow increases as the square of the velocity, while in the case of an electric current the resistance through a given conductor is a constant proportion of the energy transmitted. So far therefore as resistance is concerned electricity has a great advantage over water for the transmission of power. The cost of the conductor will however be a grave consideration where the length is great, because its section must be increased in proportion to the length to keep the resistance the same. It must also be large enough in section to prevent heating, which not only represents loss but impairs conductivity. To work advantageously on this system, a high electromotive force must be used, and this will involve loss by imperfect insulation, increasing in amount with the length of the line. For these reasons there will be a limit to the distance to which electricity may be profitably conveyed, but within that limit there will be wide scope for its employment transmissively. Whenever the time arrives for utilising the power of great waterfalls the transmission of power by electricity will become a system of vast importance. Even now small streams of water inconveniently situated for direct application may, by the adoption of this principle, be brought into useful operation.

For locomotive purposes also we find the dynamo-electric principle to be available, as instanced in the very interesting example presented in Siemens' electric railway, which has already attained that degree of success which generally foreshadows an important future. It forms a combined fixed engine and locomotive system of traction, the fixed engine being the generator of the power and the electric engine representing the locomotive.

Steam power may both be transmitted and distributed, by the intervention of electricity, but it will labour under great disadvantage when thus applied, until a thoroughly effective electric accumulator be provided, capable of giving out electric energy with almost unlimited rapidity. How far the secondary battery of M. Faure will fulfil the necessary conditions remains to be seen, and it is to be hoped that the discussions which may be expected to take place at this meeting of the British Association will enable a just estimate of its capabilities to be formed. The introduction of the Faure battery is at any rate a very important step in electrical progress. It will enable motors of small power, whatever their nature may be, to accomplish, by uninterrupted action, the effect of much larger machines acting for short periods, and by this means the value of very small streams of water will be greatly enhanced. This will be especially the case where the power of the stream is required for electric lighting, which, in summer, when the springs are low, will only be required during the brief hours of darkness, while in winter the longer nights will be met by a more abundant supply of water. Even the fitful power of wind, now so little used, will probably acquire new life when aided by a system which will not only collect, but equalise, the variable and uncertain power exerted by the air.

It would greatly add to the utility of the Faure battery if its weight and size could be considerably reduced, for in that case it might be applicable to many purposes of locomotion. We may easily conceive its becoming available in a lighter form for all sorts of carriages on common roads, thereby saving to a vast extent the labour of horses. Even the nobler animal that strides a bicycle, or the one of fainter courage that prefers the safer seat of a tricycle, may ere long be spared the labour of propulsion, and the time may not be distant when an electric horse, far more amenable to discipline than the living one, may be added to the bounteous gifts which science has bestowed on civilised man.

In conclusion I may observe that we can scarcely sufficiently admire the profound investigations which have revealed to us the strict dynamical relation of heat and electricity to outward mechanical motion. It would be a delicate task to apportion praise amongst those whose labours have contributed, in various degrees, to our present knowledge; but I shall do no injustice in saying that of those who have expounded the modern doctrine of energy, in special relation to mechanical practice, the names of Joule, Clausius, Rankine, and William Thomson, will always be conspicuous. But up to this time our knowledge of energy is almost confined to its inorganic aspect. Of its physiological action we remain in deep ignorance, and as we may expect to derive much valuable guidance from a knowledge of Nature's methods of dealing with energy in her wondrous mechanisms, it is to be hoped that future research will be directed to the elucidation of that branch of science which as yet has not even a name, but which I may provisionally term "Animal Energetics."

## *THE RISE AND PROGRESS OF PALÆONTOLOGY*[1]

THAT application of the sciences of biology and geology which is commonly known as palæontology took its origin in the mind of the first person who, finding something like a shell or a bone naturally imbedded in gravel or in rock, indulged in speculations upon the nature of this thing which he had dug out—this "fossil"—and upon the causes which had brought it into such a position. In this rudimentary form, a high antiquity may safely be ascribed to palæontology, inasmuch as we know that, 500 years before the Christian era, the philosophic doctrines of Xenophanes were influenced by his observations upon the fossil remains exposed in the quarries of Syracuse. From this time forth, not only the philosophers, but the poets, the historians, the geographers of antiquity occasionally refer to fossils; and after the revival of learning lively controversies arose respecting their real nature. But hardly more than two centuries have elapsed since this fundamental problem was first exhaustively treated; it was only in the last century that the archæological value of fossils—their importance, I mean, as records of the history of the earth—was fully recognised; the first adequate investigation of the fossil remains of any large group of vertebrated animals is to be found in Cuvier's "Recherches sur les Ossemens Fossiles," completed in 1822; and, so modern is stratigraphical palæontology, that its founder, William Smith, lived to receive the just recognition of his services by the award of the first Wollaston Medal in 1831.

But, although palæontology is a comparatively youthful scientific speciality, the mass of materials with which it has to deal is already prodigious. In the last fifty years the number of known fossil remains of invertebrated animals has been trebled or quadrupled. The work of interpretation of vertebrate fossils, the foundations of which were so solidly laid by Cuvier, was carried on, with wonderful vigour and success, by Agassiz, in Switzerland, by Von Meyer, in Germany, and last, but not least, by Owen in this country, while, in later years, a multitude of workers have laboured in the same field. In many groups of the animal kingdom the number of fossil forms already known is as great as that of the existing species. In some cases it is much greater; and there are entire orders of animals of the existence of which we should know nothing except for the evidence afforded by fossil remains. With all this it may be safely assumed that, at the present moment, we are not acquainted with a tithe of the fossils which will sooner or later be discovered. If we may judge by the profusion yielded within the last few years by the Tertiary formations of North America, there seems to be no limit to the multitude of Mammalian remains to be expected from that continent, and analogy leads us to expect similar riches in Eastern Asia whenever the Tertiary formations of that region are as carefully explored. Again, we have as yet almost everything to learn respecting the terrestrial population of the Mesozoic epoch—and it seems as if the Western Territories of the United States were about to prove as instructive in regard to this point as they have in respect of Tertiary life. My friend Prof. Marsh informs me that, within two years, remains of more than 160 distinct individuals of mammals, belonging to twenty species and nine genera, have been found in a space not larger than the floor of a good-sized room; while beds of the same age have yielded 300 reptiles, varying in size from a length of 60 feet or 80 feet to the dimensions of a rabbit.

The task which I have set myself to-night is to endeavour to lay before you, as briefly as possible, a sketch of the successive steps by which our present knowledge of the facts of palæontology and of those conclusions from them which are indisputable has been attained; and I beg leave to remind you, at the outset, that in attempting to sketch the progress of a branch of knowledge to which innumerable labours have contributed, my business is rather with generalisations than with details. It is my object to mark the epochs of palæontology, not to recount all the events of its history.

That which I just now called the fundamental problem of palæontology, the question which has to be settled before any other can be profitably discussed, is this,—What is the nature of fossils? Are they, as the healthy common sense of the ancient Greeks appears to have led them to assume without hesitation, the remains of animals and plants? Or are they, as was so generally maintained in the fifteenth, sixteenth, and seventeenth centuries, mere figured stones, portions of mineral matter which have assumed the forms of leaves and shells and bones, just as those portions of mineral matter which we call crystals take on the form of regular geometrical solids? Or, again, are they, as others thought, the products of the germs of animals and of the seeds of plants which have lost their way, as it were, in the bowels of the earth, and have achieved only an imperfect and abortive development? It is easy to sneer at our ancestors for being disposed to reject the first in favour of one or other of the last two hypotheses; but it is much more profitable to try to discover why they, who were really not one whit less sensible persons than our excellent selves, should have been led to entertain views which strike us as absurd. The belief in what is erroneously called spontaneous generation—that is to say, in the development of living matter out of mineral matter, apart from the agency of pre-existing living matter, as an ordinary occurrence at the present day—which is still held by some of us, was universally accepted as an obvious truth by them. They could point to the arborescent forms assumed by hoar-frost and by sundry metallic minerals as evidence of the existence in nature of a "plastic force" competent to enable inorganic matter to assume the form of organised bodies. Then, as every one who is familiar with fossils knows, they present innumerable gradations, from shells and bones which exactly resemble the recent objects, to masses of mere stone which, however accurately they repeat the outward form of the organic body, have nothing else in common with it; and, thence, to mere traces and faint impressions in the continuous substance of the rock. What we now know to be the results of the chemical changes which take place in the course of fossilization, by which mineral is substituted for organic substance, might, in the absence of such knowledge, be fairly interpreted as the expression of a process of development in the opposite direction—from the mineral to the organic. Moreover, in an age when it would have seemed the most absurd of paradoxes to suggest that the general level of the sea is constant, while that of the solid land fluctuates up and down through thousands of feet in a secular ground swell, it may well have appeared far less hazardous to conceive that fossils are sports of nature than to accept the necessary alternative, that all the inland regions and highlands, in the rocks of which marine shells had been found, had once been covered by the ocean. It is not so surprising, therefore, as it may at first seem, that although such men as Leonardo da Vinci and Bernard Palissy took just views of the nature of fossils, the opinion of the majority of their contemporaries set strongly the other way; nor even that error maintained itself long after the scientific grounds of the true interpretation of fossils had been stated, in a manner that left nothing to be desired, in the latter half of the seventeenth century. The person who rendered this good service to palæontology was Nicholas Steno,

[1] Discourse given at the York meeting of the British Association by Prof. T. H. Huxley, Sec. R. S. Revised by the author.

professor of anatomy in Florence, though a Dane by birth. Collectors of fossils at that day were familiar with certain bodies termed "glossopetræ," and speculation was rife as to their nature. In the first half of the seventeenth century, Fabio Colonna had tried to convince his colleagues of the famous Accademia dei Lincei that the glossopetræ were merely fossil sharks' teeth, but his arguments made no impression. Fifty years later Steno re-opened the question, and, by dissecting the head of a shark and pointing out the very exact correspondence of its teeth with the glossopetræ, left no rational doubt as to the origin of the latter. Thus far, the work of Steno went little further than that of Colonna, but it fortunately occurred to him to think out the whole subject of the interpretation of fossils, and the results of his meditations was the publication, in 1669, of a little treatise with the very quaint title of "De Solido intra Solidum naturaliter contento." The general course of Steno's argument may be stated in a few words. Fossils are solid bodies which by some natural process have come to be contained within other solid bodies—namely, the rocks in which they are imbedded; and the fundamental problem of palæontology, stated generally, is this—"Given a body endowed with a certain shape and produced in accordance with natural laws, to find in that body itself the evidence of the place and manner of its production."[1] The only way of solving this problem is by the application of the axiom that "like effects imply like causes," or as Steno puts it, in reference to this particular case, that "bodies which are altogether similar have been produced in the same way."[2] Hence, since the glossopetræ are altogether similar to sharks' teeth, they must have been produced by shark-like fishes; and since many fossil shells correspond, down to the minutest details of structure, with the shells of existing marine or freshwater animals, they must have been produced by similar animals; and the like reasoning is applied by Steno to the fossil bones of vertebrated animals, whether aquatic or terrestrial. To the obvious objection that many fossils are not altogether similar to their living analogues, differing in substance while agreeing in form, or being mere hollows or impressions, the surfaces of which are figured in the same way as those of animal or vegetable organisms, Steno replies by pointing out the changes which take place in organic remains imbedded in the earth, and how their solid substance may be dissolved away entirely, or replaced by mineral matter, until nothing is left of the original but a cast, an impression, or a mere trace of its contours. The principles of investigation thus excellently stated and illustrated by Steno in 1669, are those which have, consciously, or unconsciously, guided the researches of palæontologists ever since. Even that feat of palæontology which has so powerfully impressed the popular imagination, the reconstruction of an extinct animal from a tooth or a bone, is based upon the simplest imaginable application of the logic of Steno. A moment's consideration will show, in fact, that Steno's conclusion that the glossopetræ are sharks' teeth implies the reconstruction of an animal from its tooth. It is equivalent to the assertion that the animal of which the glossopetræ are relics had the form and organisation of a shark; that it had a skull, a vertebral column, and limbs similar to those which are characteristic of this group of fishes; that its heart, gills, and intestines presented the peculiarities which those of all sharks exhibit; nay, even that any hard parts which its integument contained were of a totally different character from the scales of ordinary fishes. These conclusions are as certain as any based upon probable reasonings can be. And they are so, simply because a very large experience justifies us in believing that teeth of this particular form and structure are invariably associated with the peculiar organisation of sharks, and are never found in connection with other organisms. Why this should be we are not at present in a position even to imagine; we must take the fact as an empirical law of animal morphology, the reason of which may possibly be one day found in the history of the evolution of the shark tribe, but for which it is hopeless to seek for an explanation in ordinary physiological reasonings. Every one practically acquainted with palæontology is aware that it is not every tooth nor every bone which enables us to form a judgment of the character of the animal to which it belonged, and that it is possible to possess many teeth, and even a large portion of the skeleton of an extinct animal, and yet be unable to reconstruct its skull or its limbs. It is only when the tooth or bone presents peculiarities which we know by previous experience to be characteristic of a certain group that we can safely predict that the fossil belonged to an animal of the same group. Any one who finds a cow's grinder may be perfectly sure that it belonged to an animal which had two complete toes on each foot, and ruminated; any one who finds a horse's grinder may be as sure that it had one complete toe on each foot and did not ruminate; but, if ruminants and horses were extinct animals of which nothing but the grinders had ever been discovered, no amount of physiological reasoning could have enabled us to reconstruct either animal, still less to have divined the wide differences between the two. Cuvier, in the "Discours sur les Révolutions de la Surface du Globe," strangely credits himself, and has ever since been credited by others, with the invention of a new method of palæontological research. But if you will turn to the "Recherches sur les Ossemens Fossiles" and watch Cuvier, not speculating, but working, you will find that his method is neither more nor less than that of Steno. If he was able to make his famous prophecy from the jaw which lay upon the surface of a block of stone to the pelvis of the same animal which lay hidden in it, it was not because either he, or any one else, knew, or knows, why a certain form of jaw is, as a rule, constantly accompanied by the presence of marsupial bones—but simply because experience has shown that these two structures are co-ordinated.

The settlement of the nature of fossils led at once to the next advance of palæontology—viz., its application to the deciphering of the history of the earth. When it was admitted that fossils are remains of animals and plants, it followed that, in so far as they resemble terrestrial or freshwater animals and plants, they are evidences of the existence of land or fresh water, and in so far as they resemble marine organisms, they are evidences of the existence of the sea at the time at which they were parts of actually living animals and plants. Moreover, in the absence of evidence to the contrary, it must be admitted that the terrestrial or the marine organisms implied the existence of land or sea at the place in which they were found while they were ye living. In fact, such conclusions were immediately drawn by everybody, from the time of Xenophanes downwards, who believed that fossils were really organic remains. Steno discusses their value as evidence of repeated alteration of marine and terrestrial conditions upon the soil of Tuscany in a manner worthy of a modern geologist. The speculations of De Maillet in the beginning of the eighteenth century turn upon fossils, and Buffon follows him very closely in those two remarkable works, the "Théorie de la Terre" and the "Époques de la Nature," with which he commenced and ended his career as a naturalist.

The opening sentences of the "Époques de la Nature" show us how fully Buffon recognised the analogy of geological with archæological inquiries. "As in civil history we consult deeds, seek for coins, or decipher antique inscriptions in order to determine the epochs of human revolutions and fix the date of moral events; so, in natural history, we must search the archives of the world, recover old monuments from the bowels of the earth, collect their fragmentary remains, and gather into one body of evidence all the signs of physical change which may enable us to look back upon the different ages of nature. It is our only means of fixing some points in the immensity of space and of setting a certain number of waymarks along the eternal path of time."

Buffon enumerates five classes of these monuments of the past history of the earth, and they are all facts of palæontology. In the first place, he says, shells and other marine productions are found all over the surface and in the interior of the dry land; and all calcareous rocks are made up of their remains. Secondly, a great many of these shells which are found in Europe are not now to be met with in the adjacent seas; and, in the slates and other deep-seated deposits, there are remains of fishes and of plants of which no species now exist in our latitudes, and which are either extinct or exist only in more northern climates. Thirdly, in Siberia and in other northern regions of Europe and of Asia, bones and teeth of elephants, rhinoceroses, and hippopotamuses occur in such numbers that these animals must once have lived and multiplied in those regions, although at the present day they are confined to southern climates. The deposits in which these remains are found are superficial, while those which contain shells and other marine remains lie much deeper. Fourthly, tusks and bones of elephants and hippopotamuses are found not only in the northern regions of the

[1] "De Solido intra Solidum," p 5.—"Dato corpore certâ figurâ prædito et juxta leges naturæ producto, in ipso corpore argumenta invenire locum et modum productionis detegentia."

[2] "Corpora sibi invicem omnino similia simili etiam modo producta sunt."

old world, but also in those of the new world, although, at present, neither elephants nor hippopotamuses occur in America. Fifthly, in the middle of the continents, in regions most remote from the sea, we find an infinite number of shells, of which the most part belong to animals of those kinds which still exist in southern seas, but of which many others have no living analogues; so that these species appear to be lost, destroyed by some unknown cause. It is needless to inquire how far these statements are strictly accurate; they are sufficiently so to justify Buffon's conclusions that the dry land was once beneath the sea; that the formation of the fossiliferous rocks must have occupied a vastly greater lapse of time than that traditionally ascribed to the age of the earth; that fossil remains indicate different climatal conditions to have obtained in former times, and especially that the polar regions were once warmer; that many species of animals and plants have become extinct; and that geological change has had something to do with geographical distribution.

But these propositions almost constitute the framework of palæontology. In order to complete it but one addition was needed, and that was made, in the last years of the eighteenth century, by William Smith, whose work comes so near our own times that many living men may have been personally acquainted with him. This modest land surveyor, whose business took him into many parts of England, profited by the peculiarly favourable conditions offered by the arrangement of our secondary strata to make a careful examination and comparison of their fossil contents at different points of the large area over which they extend. The result of his accurate and widely-extended observations was to establish the important truth that each stratum contained certain fossils which are peculiar to it; and that the order in which the strata, characterised by these fossils, are superimposed one upon the other is always the same. This most important generalisation was rapidly verified and extended to all parts of the world accessible to geologists; and, now, it rests upon such an immense mass of observations as to be one of the best established truths of natural science. To the geologist this discovery was of infinite importance, as it enabled him to identify rocks of the same relative age, however their continuity might be interrupted or their composition altered. But to the biologist it had a still deeper meaning, for it demonstrated that, throughout the prodigious duration of time registered by the fossiliferous rocks, the living population of the earth had undergone continual changes, not merely by the extinction of a certain number of the species which at first existed, but by the continual generation of new species, and the no less constant extinction of old ones.

Thus, the broad outlines of palæontology, in so far as it is the common property of both the geologist and the biologist, were marked out at the close of the last century. In tracing its subsequent progress I must confine myself to the province of biology, and, indeed, to the influence of palæontology upon zoological morphology. And I accept this limitation the more willingly as the no less important topic of the bearing of geology and of palæontology upon distribution has been luminously treated in the address of the President of the Geographical Section.

The succession of the species of animals and plants in time being established, the first question which the zoologist or the botanist had to ask himself was, What is the relation of these successive species one to another? And it is a curious circumstance that the most important event in the history of palæontology which immediately succeeded William Smith's generalisation was a discovery which, could it have been rightly appreciated at the time, would have gone far towards suggesting the answer, which was in fact delayed for more than half a century. I refer to Cuvier's investigation of the Mammalian fossils yielded by the quarries in the older Tertiary rocks of Montmartre, among the chief results of which was the bringing to light of two genera of extinct hoofed quadrupeds, the *Anoplotherium* and the *Palæotherium*. The rich materials at Cuvier's disposition enabled him to obtain a full knowledge of the osteology and of the dentition of these two forms, and consequently to compare their structure critically with that of existing hoofed animals. The effect of this comparison was to prove that the *Anoplotherium*, though it presented many points of resemblance with the pigs on the one hand, and with the ruminants on the other, differed from both to such an extent that it could find a place in neither group. In fact, it held, in some respects, an intermediate position, tending to bridge over the interval between these two groups, which in the existing fauna are so distinct. In the same way, the *Palæotherium* tended to connect forms so different as the tapir, the rhinoceros, and the horse. Subsequent investigations have brought to light a variety of facts of the same order, the most curious and striking of which are those which prove the existence, in the mesozoic epoch, of a series of forms intermediate between birds and reptiles—two classes of vertebrate animals which at present appear to be more widely separated than any others. Yet the interval between them is completely filled, in the mesozoic fauna, by birds which have reptilian characters on the one side, and reptiles which have ornithic characters, on the other. So, again, while the group of fishes termed ganoids is at the present time so distinct from that of the dipnoi, or mudfishes, that they have been reckoned as distinct orders, the Devonian strata present us with forms of which it is impossible to say with certainty whether they are dipnoi or whether they are ganoids.

Agassiz's long and elaborate researches upon fossil fishes, published between 1833 and 1842, led him to suggest the existence of another kind of relation between ancient and modern forms of life. He observed that the oldest fishes presented many characters which recall the embryonic conditions of existing fishes; and that, not only among fishes, but in several groups of the invertebrata which have a long palæontological history, the latest forms are more modified, more specialised, than the earlier. The fact that the dentition of the older tertiary ungulate and carnivorous mammals is always complete, noticed by Prof. Owen, illustrated the same generalisation.

Another no less suggestive observation was made by Mr. Darwin, whose personal investigations during the voyage of the *Beagle* led him to remark upon the singular fact, that the fauna which immediately precedes that at present existing in any geographical province of distribution presents the same peculiarities as its successor. Thus, in South America and in Australia, the later tertiary or quaternary fossils show that the fauna which immediately preceded that of the present day was, in the one case, as much characterised by edentates and in the other by marsupials as it is now, although the species of the older are largely different from those of the newer fauna.

However clearly these indications might point in one direction, the question of the exact relation of the successive forms of animal and vegetable life could be satisfactorily settled only in one way—namely, by comparing, stage by stage, the series of forms presented by one and the same type throughout a long space of time. Within the last few years this has been done fully in the case of the horse, less completely in the case of the other principal types of the ungulata and of the carnivora, and all these investigations tend to one general result—namely, that in any given series the successive members of that series present a gradually increasing specialisation of structure. That is to say, if any such mammal at present existing has specially modified and reduced limbs or dentition and complicated brain, its predecessors in time show less and less modification and reduction in limbs and teeth and a less highly developed brain. The labours of Gaudry, Marsh, and Cope furnish abundant illustrations of this law from the marvellous fossil wealth of Pikermi and the vast uninterrupted series of tertiary rocks in the territories of North America.

I will now sum up the results of this sketch of the rise and progress of palæontology. The whole fabric of palæontology is based upon two propositions: the first is, that fossils are the remains of animals and plants; and the second is, that the stratified rocks in which they are found are sedimentary deposits; and each of these propositions is founded upon the same axiom that like effects imply like causes. If there is any cause competent to produce a fossil stem, or shell, or bone, except a living being, then palæontology has no foundation; if the stratification of the rocks is not the effect of such causes as at present produce stratification, we have no means of judging of the duration of past time, or of the order in which the forms of life have succeeded one another. But, if these two propositions are granted, there is no escape, as it appears to me, from three very important conclusions. The first is that living matter has existed upon the earth for a vast length of time, certainly for millions of years. The second is that, during this lapse of time, the forms of living matter have undergone repeated changes, the effect of which has been that the animal and vegetable population at any period of the earth's history contains some species which did not exist at some antecedent period, and others which ceased to exist at some subsequent period. The third is that in the case of many groups of mammals and some of reptiles, in which one type can be followed through a considerable extent of geological time, the series of different forms by which the type is represented at successive intervals of this time is exactly such as it would be if they had been produced by the gradual modification of the earliest form of the series. These are facts of the history of the earth guaranteed by as good evidence as any facts in civil history.

Hitherto I have kept carefully clear of all the hypotheses to which men have at various times endeavoured to fit the facts of palæontology, or by which they have endeavoured to connect as many of these facts as they happened to be acquainted with. I do not think it would be a profitable employment of our time to discuss conceptions which doubtless have had their justification and even their use, but which are now obviously incompatible with the well-ascertained truths of palæontology. At present these truths leave room for only two hypotheses. The first is that, in the course of the history of the earth, innumerable species of animals and plants have come into existence, independently of one another, innumerable times. This, of course, implies either that spontaneous generation on the most astounding scale, and of animals such as horses and elephants, has been going on, as a natural process, through all the time recorded by the fossiliferous rocks; or it necessitates the belief in innumerable acts of creation repeated innumerable times. The other hypothesis is, that the successive species of animals and plants have arisen, the later by the gradual modification of the earlier. This is the hypothesis of evolution; and the palæontological discoveries of the last decade are so completely in accordance with the requirements of this hypothesis that, if it had not existed, the palæontologist would have had to invent it.

I have always had a certain horror of presuming to set a limit upon the possibilities of things. Therefore, I will not venture to say that it is impossible that the multitudinous species of animals and plants may have been produced one separately from the other by spontaneous generation, nor that it is impossible that they should have been independently originated by an endless succession of miraculous creative acts. But I must confess that both these hypotheses strike me as so astoundingly improbable, so devoid of a shred of either scientific or traditional support, that even if there were no other evidence than that of palæontology in its favour, I should feel compelled to adopt the hypothesis of evolution. Happily, the future of palæontology is independent of all hypothetical considerations. Fifty years hence, whoever undertakes to record the progress of palæontology will note the present time as the epoch in which the law of succession of the forms of the higher animals was determined by the observation of palæontological facts. He will point out that, just as Steno and as Cuvier were enabled from their knowledge of the empirical laws of coexistence of the parts of animals to conclude from a part to the whole, so the knowledge of the law of succession of forms empowered their successors to conclude, from one or two terms of such a succession, to the whole series, and thus to divine the existence of forms of life, of which, perhaps, no trace remains, at epochs of inconceivable remoteness in the past.

## *NOTES*

MOST of the foreign Governments have appointed their delegates to the International Congress of Electricians at Paris. Among the German delegates are M. Wiedemann, editor of *Wiedemann's Annalen*, Helmholtz, Du Bois-Reymond, and Weber, who, as we stated in our last issue, has received a medal in commemoration of the fiftieth anniversary of his professoriate in Halle. The name of Weber is the only one among living men which has been inscribed on the walls of the Palais de l'Industrie. The original instrument which Weber invented with Gauss in 1833 is exhibited in the German section. Amongst the names of English men of science who are said to have been delegated by the English Government are those of Sir William Thomson and Dr. Siemens. One, if not the principal, object of the deliberations of the Congress will be the adoption of a universal system of electric and magnetic measures, as advocated by the British Association. The work of the Commission which has been appointed by it will be discussed, and practical suggestions are to be made relating to it. It is supposed that the electrical and magnetic units are to be considered as a sequel to the metric system of weights and measures. Another question will relate to the laying of submarine cables, viz., the establishment of an international codex of signals for telegraphic steamers, and the necessity of adopting rules for parallel or transversal lines, liable to endanger the existing ones. But it does not appear that any allusion is to be made to the neutralisation in war time, although it has been recommended by M. Barthélemy St. Hilaire, the Minister of Foreign Affairs. All the sittings are to be private, to the exclusion of the public and Press, except a few lectures given by some members on selected topics. *Procès-verbaux* are to be written and published by a select body of authorised secretaries.

THE telephonic audition of the Opera at the Paris Electrical Exhibition is very popular. Not less than 1500 people are admitted by relays of twenty-four, during two minutes at a time, to enjoy it every opera night. It was contemplated to transmit the performances from the Théâtre Français on the same principle, but it has not been successful. The receipts of the Exhibition exceed 4000*l.* daily.

A SIXTH electrical paper has been started in Paris. It is a large folio issued every Saturday, and called *Moniteur officiel de l'Électricité*. It is conducted by M. Barbieny, a gentleman connected with the political Press, and who has founded several periodicals. Electricity has now more papers in Paris than general science.

THE will of the late Sir Josiah Mason of Birmingham ha just been proved. The personal estate was sworn to be of the value of 56,729*l.* The testator had no real property, having in his lifetime disposed of his real estate, worth upwards of 10,000*l.* per annum, either to his orphanage or college trustees, or his great nephew. After legacies and bequests amounting to 7500*l.*, the whole of the testator's personal estate by law applicable to charitable purposes is bequeathed to the trustees of the Mason Science College, for the general purposes of the institution. Elaborate provisions are made for charging the debts, annuities, and legacies on the property which cannot legally be bequeathed to charitable purposes, so as to secure the whole residue for the college.

DR. ARCHIBALD BILLING, M.A., F.R.S., the author of the "First Principles of Medicine," died in London on Friday, at the age of ninety. The deceased physician, who was a native of Ireland, was born in 1791, and was educated at Trinity College, Dublin, and at Oxford, graduating at the first-named University. While engaged at the London Hospital, he instituted the series of chemical lectures which have since become an established feature of the medical school at that institution, but resigned his appointment at the close of 1836, upon the establishment of the University of London. Dr. Billing was a large contributor to the medical Press. He was a member of a large number of learned societies, both in this country and on the Continent.

A CONVENTION of American photographers has recently concluded its sittings at New York. Before separating the members appointed a committee to consider the feasibility of forming an International Photographic Association, and to confer with foreign societies with that view. A report upon the subject will be presented at the next meeting of the Convention, which is to take place at Indianopolis.

THE American Association for the Advancement of Science, at its meeting last month in Cincinnati, took action in reference to the scandal of American degrees, by resolving to unite with the American Philological Association in presenting a memorial to all colleges in the United States empowered to confer degrees, stating the objections to conferring the degree of Ph.D. *honoris causâ*, and praying them to discontinue the practice, if it exists. It seems that the reprehensible practice has been growing of late in the United States. There are, it would seem, in the United

States, 360 institutions of a collegiate grade; these colleges and universities receive their charters from the Legislature of their several States, these charters giving them the unlimited right to confer degrees. The president of the college near Cincinnati told one of the speakers, with a face shining with pride, that his college gave seventeen different degrees. One of these was M.P., which in interpretation meant, not Member of Parliament, but Master of Penmanship. It would seem, moreover, that even the degree of S.D. (equivalent, we believe, to our D.Sc.) has actually been granted by some of these American institutions *honoris causâ*. We trust that the action of the American Association will have some influence with the peccant colleges; it will, at any rate, put people on their guard against American Ph.D.'s and S.D.'s, as well as D.D.'s.

IN the *Revue Scientifique* of September 3, Mr. G. Delaunay has a paper on the "Equality and Inequality of the Two Sexes," in which he endeavours to show that except in some of the lowest forms of animal life, and in the lowest stages of human society, the inferiority of the female sex to the male is unmistakable in all respects—that physically, mentally, and morally, woman is the inferior of man.

A HUGE mass of rock and earth fell the other day from a mountain side at Somnix in the Grisons, blocked up the course of the Jobel, an affluent of the Rhine, and converted the valley into a lake. The village of Surrheim, hard by, is in great danger.

THE additions to the Zoological Society's Gardens during the past week include two Malbrouck Monkeys (*Cercopithecus cynosurus*) from West Africa, presented by Mr. H. P. Sherlock; a Central American Agouti (*Dasyprocta isthmica*) from Central America, presented by Mr. J. E. Sharp; two Spotted Cavys (*Cœlogenys paca*) from South America, presented by Dr. Portella; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. James W. Duncan; two Domestic Pigeons (*Columba œnas*, var.) from Arabia, presented by Mr. Reginald Zohrab; three Common Chamæleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. Alfred R. Rogers; two Greater Sulphur-crested Cockatoos (*Cacatua galerita*) from Australia, deposited; a Black-headed Caique (*Caica melanocephala*) from Demerara, purchased. The additions to the Insectarium for the past week include *Attacus pernyi*, second brood of larvæ hatched; also *Attacus cynthia*, imago second brood, and the Death's-Head Moth (*Acherontia atropos*) larva, presented by Master Kingchurch; second brood of Ant Lions (Myrmeleons), and a brood of the Edible Snail (*Helix pomatia*) from specimens presented by Lord A. Russell, F.Z.S., in April last.

## SOCIETIES AND ACADEMIES

### PARIS

**Academy of Sciences**, August 29.—M. Decaisne in the chair.—M. Faye presented the first volume of his "Cours d'Astronomie de l'École Polytechnique," treating of the diurnal motion, the theory of instruments and errors, organisation of great observatories, mathematics and geodesy. The second volume will be devoted to the solar system.—Dioptric studies, by M. Zenger. He constructs tables which give, in algebraic form, the relation between the radii of curvature and refractive indices of two media forming the objective of a microscope or telescope. Any one may make his own telescope or microscope, without calculation, taking a lens of quartz or crown glass, and a mixture of aromatic substances giving it a dispersion twice as great, or equal for all spectral rays. The lens being corrected, it is combined with one or two other symmetrical lenses, according to the well-known process for getting an aplanetic and achromatic doublet or triplet.—MM. Tresca and Breguet were requested to represent the Academy at the inauguration of the monument to Frederic Sauvage in Boulogne on the 12th inst.—On a very old application of the screw as an organ of propulsion, by M. Govi. This was by Leonardo da Vinci, about the end of the fifteenth century. In one of his works is a sketch of a device for rising in the air, consisting of a helix formed of wire and cloth to be rotated about a vertical axis. He seems to have made small paper models actuated by thin slips of steel, twisted, then left to themselves. Another sketch shows that Leonardo da Vinci conceived the idea of the parachute.—On some new cases of equipotential figures, realised electro-chemically, by M. Guébhard.—On the absorption of ultra-violet rays by some media, by M. de Chardonnet. Two methods are described. The liquids which circulate in plants or impregnate roots and fruits show a great avidity for chemical rays. Fluorescence does not appear to be in direct ratio to the intensity of actinic absorption; thus, *e.g.* the decoction of radish is a less powerful absorbent than that of potatoes; yet the former is fluorescent, the latter not. White wine is weakly fluorescent, red wine lacks the property. The few animal substances studied gave very varied results. While blood, even very dilute, is a strong absorbent, the (fresh) aqueous humour of a calf's eye and the albumen of hen's eggs have no action on the chemical rays (at least up to 20 mm. thickness). Distilled water, alcohol, sulphuric ether, normal collodion, and solution of cane-sugar are also without action. Gelatine appropriates readily all the actinic rays. An object-glass of Dallmeyer projected an invisible spectrum 25 to 40 per cent. longer than one of Darlot, of Paris, of equal focus.—Figures produced by fall of a drop of water holding minium in suspension, by M. Decharme. Minium, in fine powder, is mixed with water and spread uniformly on a horizontal glass plate; then a drop of the mixture is let fall on this layer. Figures resembling those of the three systems Caladni observed on vibrating plates are produced; the three types usually coexist, but one or other may be made to predominate at will.—On the composition of buckwheat, by M. Lechartier. Marked differences appear between the crops of 1879 and of 1880. Thus the ashes of the straw in 1880 had twice as much potash as in 1879, and phosphoric acid was still more increased; and there was also more chlorine. The composition of the grain is little modified. The straw may contain more of mineral matter than the grain. Buck-wheat removes more of the fertilising principles from the soil than corn.—On hydrosulphurous acid; reply to M. Schutzenberger's note, by M. Bernthsen.—On the dissolution of silver in presence of alkaline iodides, by M. Ditte.—On the constitution of glyceric ether, and on the transformation of epichlorhydrine into normal propylic alcohol, by M. Silva.—On pyruvic alcohol and its derivatives, by M. Henry.—Action of triethylamine on epichlorhydrine; compounds of oxallyltriethylammonium, by M. Reboul.—Biological evolution of the pucerons of the alder tree, by M. Lichtenstein.—Observations on a new enunciation of the second law of Gay-Lussac concerning combinations of gas, by M. Garcia de la Cruz. He indicates some of the numerous exceptions to M. Verschaffel's proposition: "The space occupied by a gaseous compound is always double the space occupied by that one of the components which enters with less volume into the combination." This law he regards as less general than the laws of contraction long accepted.

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